University of Southampton

On the Problem of Indeterminacy in Fluvial Geomorphology

David William Wilson

Doctor of Philosophy

Department of Geography

September 2001

The work presented herein has been produced solely by the author whilst in postgraduate registration at the University of Southampton.

UNIVERSITY OF SOUTHAMPTON

ABSTRACT

FACULTY OF SCIENCE

GEOGRAPHY

Doctor of Philosophy

ON THE PROBLEM OF INDETERMINACY IN FLUVIAL GEOMORPHOLOGY

By David William Wilson

This thesis is concerned with the indeterminacy of morphology in alluvial river channel cross-section and river channel planform. Indeterminacy herein, is defined as the inability to determine the individual case at a given scale of measurement. In order to assess current theoretical approaches in geomorphology to fluvial indeterminacy and to provide a framework for future investigations, an original historical review of the aims of geomorphology is presented. It is concluded that geomorphology has always considered itself as scientific, and in particular, that many geomorphologists view the discipline as a physical science. Given that geomorphology wants to produce scientific knowledge, and this is the framework within which indeterminacy is to be investigated, an original review of what characterises scientific knowledge is presented. The primary conclusion of this review is that science is explanation by laws, of which there are three types: causal, aleatory statistical and chaos. Furthermore, it is concluded that a hallmark of scientific knowledge is its predictive capability, although the relationship between knowledge and prediction is problematic. In light of this conceptual framework, fluvial indeterminacy in geomorphology is approached by testing the three forms of law and their associated predictive statements.

The predictions that the three types of scientific law make are tested in experimental conditions using a flume, where the relationship between controls and morphology can be regulated. Causal laws which operate at the scale of the individual measurement fail predictive tests. It is not possible to predict the morphology of a specified point on a sediment surface, given values of controls, primarily because internal processes (which are as yet unknown) in the fluid-sediment system, obscure any relationship. The addition of variables does not make the system determinate. The proactive identification of chaos laws (which operate at the scale of the attractor and for which a new hypothesis is proposed in geomorphology, based on the chaos nature of turbulence) via fractal analysis also fails predictive tests. A non-linear dimension results, and it is also found that the pattern of dimension observed can be produced by a random process. Aleatory statistical laws, which operate at the scale of the ensemble, fail predictive tests at the reach level. Different morphology can be produced over the narrow range of controls, within the same spatial location. Aleatory statistical laws are found to function at the run level; given a constant discharge, the whole of the fluid-sediment system will produce statistically similar morphology and can be predicted.

The primary contribution of the thesis is to refocus attention on the indeterminate nature of channel morphology. If geomorphology is a science, a key aim for its development is to attain a more rigourous understanding of the complex and unpredictable nature of the relationship between the controls on rivers, and the morphology they produce. In particular, geomorphology must focus on the internal structure of the fluid-sediment system, which at present is poorly understood, and acts as a powerful mediator between the controls (such as discharge and sediment) and the final morphology of the system.

Contents

	List of Figures and Tables	v
	Preface	xiv
	Acknowledgements	xv
1.	A Statement of the Problem	1
1. 1.	Alluvial Channel Cross-Section and Channel Planform	2
1. 2.	Indeterminacy	2
1. 3.	Indeterminacy as the 'Core Area' for Fluvial Geomorphology	5
1. 4	The Aim of the Thesis	7
2.	The Scientific Nature of Geomorphology	8
2. 1.	Geomorphology as Scientific	9
2. 2.	Gilbert and Davis	9
2. 3.	The Fabric of Geology	16
2. 4.	Kittonian and Simpsonian Proponents	20
2. 4. 1.	Kittonian Proponents	20
2. 4. 2.	Simpsonian Proponents	24
2. 4. 3.	A Non-Scientific Approach to Geomorphology	28
3.	The Nature of Science	33
3. 1.	Introducing the Scientific Method	34
3. 2.	The Components of Science	36
3. 3.	Hypotheses	37
3. 4.	Laws - Causal and Statistical	40
3. 4. 1.	Causal Laws and Concepts	41
3. 4. 2.	Statistical Laws and Acausality	54
3. 4. 3.	Causal Laws - Fundamental to Nature or to Cognition?	67
3. 4. 3. 1.	Are Causal Laws the Fundamental Way in Which Nature Works?	67

3. 4. 3. 2.	Are Causal Laws Fundamental to Human Cognition?	75
3. 5.	Theory as an Established Framework of Scientific Knowledge	79
3. 6.	Prediction	82
4.	The Development of Chaos Theory	101
4. 1.	Introduction	102
4. 2.	Sensitive Dependence on Initial Conditions	103
4. 3.	Phase Space	108
4. 4.	Strange Attractors	111
4. 5.	Turbulence	118
4. 5. 1.	Reynolds Number and Navier-Stokes	118
4. 5. 2.	Statistical Turbulence	124
4. 5. 3.	*The* Strange Attractor	126
4. 5. 4.	Universality	132
5.	The Application of Laws to Alluvial River Cross-Section and Planform	139
5. 1.	The Application of Causal Laws in Fluvial Theory	140
5. 2.	Four Ideas that Support the Application of Causal Laws in Fluvial Theory	181
5. 2. 1.	Aleatory/Representative Variables	181
5. 2. 2.	Error	186
5. 2. 3.	Equifinality	188
5. 2. 4.	Equilibrium	194
5. 3.	The Application of Chaos Laws in Fluvial Theory	198
5. 3. 1	Previous Developments	198
5. 3. 2.	Lorenz, Culling and a New Hypothesis for Fluvial Geomorphology	207

6.	Experimental Design	209
6. 1.	The Predictive Statements of Causal, Aleatory Statistical and Chaos Laws	209
6. 2.	Experimental Design	210
6. 3.	Experimental Procedure	213
7.	Analysis and Discussion of the Fundamental Data Set.	220
7. 1.	The Fundamental Data Set	221
7. 2.	A Brief Description of the Fundamental Data Set	222
7. 3.	Testing the Causal Hypothesis for the Fundamental Data Set	225
7. 3. 1.	Linear Prediction of the Final Height at the Run Level	226
7. 3. 2.	Linear Prediction of the Final Height of Sections Within a Run	234
7. 3. 3.	The Influence of Percentage Sediment Saturation and Temperature on Linear Explanation of the Final Height of Sections Within a Run	242
7. 3. 4.	Residual Analysis, Transformations and Dependence	246
7. 4.	Testing the Aleatory Statistical Hypothesis for the Fundamental Data Set	271
7. 4. 1.	Outline of Statistical Tests	274
7. 4. 2.	Section Level Results	275
7. 4. 3.	Analytical Procedure for Analysis of Aleatory Test Results	280
7. 4. 4.	Section Level Analysis	283
7. 4. 5.	Run Level Results	287
7. 4. 6.	Run Level Analysis	291
7. 4. 7.	Discussion of Analysis at the Section Level	293
7. 4. 8.	Discussion of Analysis at the Run Level	297
7. 4. 9.	Reconciling the Section and the Run Level	302
7. 5.	Testing the Chaos Hypothesis for the Fundamental Data Set	305

8. Conclusions and Suggestions for Further Research 324

8. 1. A Summary of the Key Ideas to Emerge in this Thesis 324

8. 2. Suggestions for Future Research 329

8. 3. Concluding Remarks 330

Appendix 331

Section A - Experimental Equipment and Materials 331

Section B - The Fundamental Data Set: Final Height of Drainage Surface 332

Section C - Initial Height of Drainage Surface 346

Section D - Percentage Sediment Saturation 356

Section E - Temperature 356

Section F - Weight of Eroded Sediment 356

Section G - Table of all Runs Performed 357

References 358

Figures and Tables

Chapter One

Table 1. 2. 1.	Nine Process of Hey (1997) Requiring Equations	3

Chapter Two

Table 2. 1. 1.	Geomorphology as Science	10

Chapter Three

Figure 3. 2. 1.	The Components of Science	36
Figure 3. 4 .2. 1	Frequency Distributions of Particle Speed with Different Temperatures	58
Table 3. 5. 1.	Opposing Definitions of Theory	80
Table 3. 6. 1.	The Value of Prediction in Science	83

Chapter Four

Figure 4. 2. 1.	Lorenz's Weather Simulation	104
Figure 4. 2. 2.	Lorenz's Mogul Slope	104
Figure 4. 2. 3	Scatter Plot for Table 4. 2. 1.	106
Figure 4. 2. 4.	The Non-linearity of the Mogul Ski Slope	107
Figure 4. 3. 1.	Alluvial River System Represented in Three Dimensional Phase Space	109
Figure 4. 4. 1.	A Sink	112
Figure 4 .4. 2.	A Limit Cycle	112
Figure 4. 4. 3.	A Source	113
Figure 4. 4. 4.	A Saddle	114
Figure 4. 4. 5.	The Butterfly Attractor of Lorenz	115
Figure 4. 5. 1. 1.	Design of Reynolds Turbulence Experiment	119
Figure 4. 5. 3. 1.	Formation of Different Period Velocity with Increases in Reynolds Number.	127
Figure 4. 5. 3. 2.	Deviation of Landau - Hopf Bifurcations from Path of Equilibria	128
Figure 4. 5. 3. 3.	Formation of a Strange Attractor Using Three Dimensions	129

Figure 4. 5. 3. 4.	Power Spectra of Gollub and Swinney's Turbulence Experiment	130
Figure 4. 5. 3. 5.	The Taylor-Couette Time Delayed Strange Attractor	131
Figure 4. 5. 4. 1.	Period Doubling for the Logistic Difference Equation	134
Table 4. 2. 1.	Results for Stewart and Casti's Circle Experiment	105

Chapter Five

Figure 5. 1. 1.	At-a-Station Hydraulic Geometry	142
Figure 5. 1. 2.	Slope, Discharge and Channel Planforms	147
Figure 5. 1. 3.	Change in Quantitative Channel Pattern Index with Slope Given Constant Discharge	149
Figure 5. 1. 4.	Change in Quantitative Channel Pattern Index with Bankfull Discharge Given Constant Slope	149
Figure 5. 1. 5.	Change in Quantitative Channel Pattern Index with Stream Power	150
Figure 5. 1. 6.	Carlston's (1965) Plot of Mean Annual Discharge Against Width	152
Figure 5. 1. 7.	Ackers (1964) Discharge/ Slope Relationship	154
Figure 5. 1. 8.	Ackers and Charlton's (1970) Plot of Dimensionless Sediment Measures/Discharge Against Dimensionless Wavelength	156
Figure 5. 1. 9.	Richards (1977) Second Order Polynomial Function for Hydraulic Geometry	163
Figure 5. 1. 10.	Richards (1977) f and m Exponents for Riffles and Pools	165
Figure 5. 1. 11.	Relationship Between Sediment Size/Sorting and Channel Gradient	167
Figure 5. 1. 12.	Carson's (1984) Relationship Between Sediment/Discharge and Slope	173
Figure 5. 1. 13.	Changes in Channel Width with Valley Slope	175
Figure 5. 1. 14.	Changes in Mean Channel Depth with Valley Slope	175
Figure 5. 1. 15.	Changes in Channel Width/Depth Ratio with Valley Slope	176
Figure 5. 1. 16.	Plot of Shields (1996) Results for Sorry River in table 5. 1. 9 with Varying Discharge.	178
Figure 5. 1. 17.	Change in Flood Channel Planform with Valley Slope/Median Annual Flood	180

Figure 5. 1. 18.	Change in Low Water Channel Planform with Valley Slope/Effective Discharge	180
Figure 5. 2. 1. 1	Wolman and Miller's (1960) Concept of Frequency and Magnitude	183
Figure 5. 3. 1. 1.	Schumm and Khan's (1971) Nonlinear Model of Channel Development	198
Figure 5. 3. 1. 2.	Sequence of Bedforms with Increasing Power	201
Figure 5. 3. 1. 3.	Pits and Mounds in Karcz's (1973) Experiments	202
Figure 5. 3. 1. 4.	Flow Aligned Grooves in Karcz's (1973) Experiments	203
Table 5. 1. 1.	Ackers and Charlton's (1970) Different Equations for the Relationship Between Dimensionless Sediment/ Discharge and Meander Wavelength	157
Table 5. 1. 2.	Schumm's (1969) Regression Equations	159
Table 5. 1. 3.	Schumm's (1969) Qualitative Univariate Equations	160
Table 5. 1. 4.	Schumm's (1969) Bivariate Qualitative Equations	161
Table 5. 1. 5.	Osterkamp's (1978) Gradient-Discharge Relationships	166
Table 5. 1. 6.	Mosley's (1981) Variables of Cause and Effect	169
Table 5. 1. 7.	Mosley's (1981) Regression Equations for River Form	170
Table 5. 1. 8.	Shields (1996) Review of Hydraulic Geometry Equations.	177
Table 5. 1. 9.	Shields (1996) Different Results for Sorry River Using Selected Equations from table 5. 1. 8.	178
Table 5. 2. 1. 1.	Common Descriptors of Channel Morphology	196
Table 5. 2. 1. 2.	Common Measures of Discharge	197
Table 5. 2. 1. 3.	Common Measures of Sediment	197
Table 5. 3. 1	Lyapunov Exponents and Attractors Types in Three Dimensional Phase Space	205

Chapter Six

Figure 6. 2. 1.	Photograph of a Constructed Run, Looking Upstream	212
Figure 6. 3. 1.	First Stage of the Flume Experiments; 'Flat' Drainage Surface	215
Figure 6. 3. 2.	Second Stage of the Flume Experiments; Constrained Flow Excavating Central Portion of Saturated Drainage Surface	217
Figure 6. 3. 3.	Final Stage of the Flume Experiments; Eroded Surface and Relative Position of Test Sections	219
Table 6. 3. 1	Flume Test Section Locations	213

Chapter Seven

Figure 7. 2. 1.	Contour Plot of Section b1: Run Two	222
Figure 7. 2. 2.	Contour Plot of Section c2: Run Seven	222
Figure 7. 2. 3.	Contour Plot of Section b3: Run Four	223
Figure 7. 2. 4.	Contour Plot of Section c4: Run Five	223
Figure 7. 2. 5.	Contour Plot of Section b5: Run Three	224
Figure 7. 3. 4. 1	Examples of Nonlinearity and Non-Constant Variance in Residual-Fit Plots	247
Figure 7. 3. 4. 2.	Residual Plots for Multiple Regression of FHT: Data for Run 1	249
Figure 7. 3. 4. 3.	Residual Plots for Multiple Regression of FHT: Data for Run 2	250
Figure 7. 3. 4. 4.	Residual Plots for Multiple Regression of FHT: Data for Run 3	250
Figure 7. 3. 4. 5.	Residual Plots for Multiple Regression of FHT: Data for Run 4	251
Figure 7. 3. 4. 6.	Residual Plots for Multiple Regression of FHT: Data for Run 5	251
Figure 7. 3. 4. 7.	Residual Plots for Multiple Regression of FHT: Data for Run 6	252
Figure 7. 3. 4. 8.	Residual Plots for Multiple Regression of FHT: Data for Run 7	252
Figure 7. 3. 4. 9.	Residual Plots for Multiple Regression of FHT: Data for Run 8	253
Figure 7. 3. 4. 10.	Fitted Line Plot for Residuals versus Residuals-1: Data for Run 1	258

Figure 7. 3. 4. 11.	Fitted Line Plot for Residuals versus Residuals-1: Data for Run 2	258
Figure 7. 3. 4. 12.	Fitted Line Plot for Residuals versus Residuals-1: Data for Run 3	259
Figure 7. 3. 4. 13.	Fitted Line Plot for Residuals versus Residuals-1: Data for Run 6	259
Figure 7. 3. 4. 14.	Fitted Line Plot for Residuals versus Residuals-1: Data for Run 7	260
Figure 7. 3. 4. 15.	Residual Plot for Multiple Regression of FHT using FHT-1: Data for Run 1	262
Figure 7. 3. 4. 16.	Residual Plot for Multiple Regression of FHT using FHT-1: Data for Run 2	263
Figure 7. 3. 4. 17.	Residual Plot for Multiple Regression of FHT using FHT-1: Data for Run 3	263
Figure 7. 3. 4. 18.	Residual Plot for Multiple Regression of FHT using FHT-1: Data for Run 4	264
Figure 7. 3. 4. 19.	Residual Plot for Multiple Regression of FHT using FHT-1: Data for Run 5	264
Figure 7. 3. 4. 20.	Residual Plot for Multiple Regression of FHT using FHT-1: Data for Run 6	265
Figure 7. 3. 4. 21.	Residual Plot for Multiple Regression of FHT using FHT-1: Data for Run 7	265
Figure 7. 3. 4. 22.	Residual Plot for Multiple Regression of FHT using FHT-1: Data for Run 8	266
Figure 7. 4. 3. 1	Plot of Mean Run FHT Versus Mean Run PSS	281
Figure 7. 4. 8. 1.	Membership of PSS Distributions Proceeding to Membership of FHT Distributions	298
Figure 7. 4. 8. 2.	Membership of IHDS Distributions Proceeding to Membership of FHT Distributions	299
Figure 7. 4. 9. 1.	An Example of a Population with Defined *a priori* Probabilities	302
Figure 7. 5. 1.	Wireframe Contour Plot of Section c2, Run 4	307
Figure 7. 5. 2.	Fitted Line Plot of Dimension for Wireframe Plot of Section c2, Run Four	308
Figure 7. 5. 3.	Residual Data for Fitted Line Plot of Section c2, Run Four	308
Figure 7. 5. 4.	Fitted Line Plot of Dimension for Wireframe Plot of Section b3, Run Two.	309
Figure 7. 5. 5.	Residual Data for Fitted Line Plot of Section b3, Run Two.	309

Figure 7. 5. 6.	Fitted Line Plot for Lower Slope in Figure 7. 5. 4., Box Size 20 to 299	310
Figure 7. 5. 7.	Residual Data for Lower Slope in Figure 7. 5. 4., Box Size 20 to 299	311
Figure 7. 5. 8.	Fitted Line Plot for Intermediate Box Sizes (20 - 55)	312
Figure 7. 5. 9.	Residual Data for Intermediate Box Sizes (20 - 55)	312
Figure 7. 5. 10.	Image One Derived by Random Sampling of Data Points For section b1 Run One	318
Figure 7. 5. 11.	Image Two Derived by Random Sampling of Data Points For section b1 Run Four	318
Figure 7. 5. 12.	Image Three - Original Data for Section b1 Run One	319
Figure 7. 5. 13.	Image Four- Original Data for Section b1 Run Four	319
Table 7. 3. 1. 1.	Regression of Final Height on Distance From Source	226
Table 7. 3. 1. 2.	Regression of Final Height on Percentage Sediment Saturation	226
Table 7. 3. 1. 3.	Regression of Final Height on Initial Height of Drainage Surface	227
Table 7. 3. 1. 4.	Regression of Percentage Sediment Saturation on Distance From Source	229
Table 7. 3. 1. 5.	Regression of Initial Height of Drainage Surface on Distance From Source	229
Table 7. 3. 1. 6.	Regression of Initial Height of Drainage Surface on Percentage Sediment Saturation	230
Table 7. 3. 1. 7.	Regression of Final Morphology Height on Distance From Source, Percentage Sediment Saturation and Initial Height of Drainage Surface	232
Table 7. 3. 2. 1.	Regression of Final Height on Distance From Source for transect 0.6850 at individual measurement grids; temperature, discharge, slope and Percentage Sediment Saturation constant.	235
Table 7. 3. 2. 2.		235
Table 7. 3. 2. 3.		235
Table 7. 3. 2. 4.	Regression of Final Height on Initial Height of Drainage Surface for transect 0.6850 at individual measurement grids; temperature, discharge, slope and Percentage Sediment Saturation constant.	236
Table 7. 3. 2. 5.		236
Table 7. 3. 2. 6.		237

Table 7. 3. 2. 7.	Regression of Initial Height of Drainage Surface on Distance From Source for transect 0.6850 at individual measurement grids; temperature, discharge, slope and Percentage Sediment Saturation constant.	237
Table 7. 3. 2. 8.		238
Table 7. 3. 2. 9.		238
Table 7. 3. 2. 10.	Multiple Regression of Final Height on Distance From Source and Initial Height of Drainage Surface for transect 0.6850 at individual measurement grids; temperature, discharge, slope and Percentage Sediment Saturation constant.	239
Table 7. 3. 2. 11.		239
Table 7. 3. 2. 12.		239
Table 7. 3. 2. 13.		240
Table 7. 3. 3. 1.	Regression of Sectional Regression r^2 values for each run against Sectional Percentage Sediment Saturation Values for each run	242
Table 7. 3. 3. 2.	Regression of Sectional Regression $\hat{\beta}_1$ values for each run against Sectional Percentage Sediment Saturation Values for each run	243
Table 7. 3. 3. 3.	Regression of Sectional Regression r^2 values against Sectional Percentage Sediment Saturation Values over eight runs	244
Table 7. 3. 3. 4.	Regression of Sectional Regression $\hat{\beta}_1$ values against Sectional Percentage Sediment Saturation Values over eight runs	244
Table 7. 3. 3. 5.	Regression of Sectional Regression r^2 values against Temperature Values over eight runs	245
Table 7. 3. 3. 6.	Regression of Sectional Regression $\hat{\beta}_1$ values against Temperature Values over eight runs	245
Table 7. 3. 4. 1.	Functions for Transformations of the Dependent Variable.	254
Table 7. 3. 4. 2.	Regression of Final Morphology Height on Final Height-1	261
Table 7. 3. 4. 3.	Regression of FHT on FHT-1, DFS, PSS and IHDS	261
Table 7. 3. 4. 4.	RSS, df and F Values for Weisberg's (1982) Four Regression Models	267
Table 7. 4. 1.	Similar Shape and Mean but Different Variance	272
Table 7. 4. 2. 1.	P-values for the Anderson-Darling Test of Normality: FHT data at the Section Level	275

Table 7. 4. 2. 2.	P-values for Levenes Test for the Equivalence of Variance: FHT data at the Section Level	276
Table 7. 4. 2. 3.	P-values for unpooled twosample t-test for the equivalence of sample means: FHT data at the Section Level	277
Table 7. 4. 2. 4.	P-values for the Anderson-Darling Test of Normality: IHDS data at the Section Level	278
Table 7. 4. 2. 5.	P-values for Levenes Test for the Equivalence of Variance: IHDS data at the Section Level	278
Table 7. 4. 2. 6.	P-values for unpooled twosample t-test for the equivalence of sample means: IHDS data at the Section Level	279
Table 7. 4. 4. 1.	Scenarios for section FHT shape	283
Table 7. 4. 4. 2.	Regression Relationships for FHT shape: Scenarios Two, Three and Four	284
Table 7. 4. 4. 3.	Scenarios for section FHT Variance	284
Table 7. 4. 4. 4.	Regression Relationships for FHT variance: Scenarios Two, Three and Four	285
Table 7. 4. 4. 5.	Scenarios for section FHT Mean	285
Table 7. 4. 4. 6.	Regression Relationships for FHT mean: Scenarios Two, Three and Four	286
Table 7. 4. 4. 7.	Regression relationships between difference in PSS for section and difference in statistical properties of section FHT.	286
Table 7. 4. 4. 8.	Regression relationships between difference in Temperature and difference in statistical properties of section FHT.	286
Table 7. 4. 5. 1.	P-values for the Anderson-Darling Test of Normality: FHT data at the Run Level	287
Table 7. 4. 5. 2.	P-values for Levenes Test for the Equivalence of Variance: FHT data at the Run Level	287
Table 7. 4. 5. 3.	P-values for unpooled twosample t-test for the equivalence of sample means: FHT data at the Run Level	288
Table 7. 4. 5. 4.	P-values for the Anderson-Darling Test of Normality: IHDS data at the Run Level	288
Table 7. 4. 5. 5.	P-values for Levenes Test for the Equivalence of Variance: IHDS data at the Run Level	288
Table 7. 4. 5. 6.	P-values for unpooled twosample t-test for the equivalence of sample means: IHDS data at the Run Level	289

Table 7. 4. 5. 7.	P-values for the Anderson-Darling Test of Normality: PSS data at the Run Level	289
Table 7. 4. 5. 8.	P-values for Levenes Test for the Equivalence of Variance: PSS data at the Run Level	290
Table 7. 4. 5. 9.	P-values for unpooled twosample t-test for the equivalence of sample means: PSS data at the Run Level	290
Table 7. 4. 6. 1.	Scenarios for run FHT shape	291
Table 7. 4. 6. 2.	Regression relationships between difference in run FHT shape and statistical properties of run IHDS and PSS	291
Table 7. 4. 6. 3.	Scenarios for run FHT variance	292
Table 7. 4. 6. 4.	Regression relationships between difference in run FHT variance and statistical properties of run IHDS and PSS	292
Table 7. 4. 6. 5.	Scenarios for run FHT mean	292
Table 7. 4. 6. 6.	Regression relationships between difference in run FHT mean and statistical properties of run IHDS and PSS	293
Table 7. 5. 1.	Students t test on Distribution of Slopes (Dimensions) for Same Section Between Runs	315
Table 7. 5. 2	Students t test on Distribution of Slopes (Dimensions) for Different Runs	316
Table 7. 5. 3	Students t test on Distribution of Slopes (Dimensions) of Sections Within a Run	316
Table 7. 5. 4	Students t-test on Random Data and Original Data	320

Preface

The development of this thesis began with an undergraduate dissertation at King's College London. The experience of undertaking that piece of work was to form a strong impression of the unpredictable nature of river channel morphology, and the emergence of a view that current fluvial theory only succeeded in giving gross descriptions of channel behaviour.

At Southampton, the original task confronted was to explore the significance of information generated by the Environment Agency's River Habitat Survey. At the heart of this national initiative is the idea of river channel classification; that is, the notion that river channels can be placed into categories according to their controls and their morphology. Such classification assumes that a river's form corresponds directly to the controls acting upon it, but the validity of this assumption had been challenged in my own mind by the work undertaken at King's College. On the basis of these reservations about classification, it was decided to focus the thesis on an examination of the theoretical underpinning of classification, which led to the fundamental relationship between the controls and morphology of rivers.

Until the internal viva voce examination, the thesis was developing into an argument for the indeterminacy of river channels and the possible benefits of chaos theory for explaining the unpredictable behaviour of river morphology. However, in the internal viva, it was apparent that two fundamental assumptions were being made on my part. The first was the assumption of geomorphology's goals and geomorphologists' aspirations; in particular, that geomorphology is science and its practitioners are scientists. Secondly, I was presenting the inadequacy of current fluvial theory within an unspecified framework for scientific investigation; that is, I was assuming what science is and how it is practised.

The thesis presented here aims to meet the initial goal whilst addressing these two foundational assumptions; what form of knowledge does geomorphology want to produce and what are the hallmarks of that knowledge? These questions are tackled in depth, exploring the history and philosophy of geomorphology and science, but the practical problem of river indeterminacy and how geomorphologists may begin to understand it, remains the central issue. The thesis aims to present an original and rigourously defined framework for investigation in geomorphology and then examines indeterminacy within this framework. Both the framework for investigation and the phenomenon under investigation are important for the development of fluvial geomorphology. On this basis, it is possible to return ultimately to questions such as river classification with greater rigour and insight.

Acknowledgements

I would like to thank my beautiful wife Sarah Wilson, for her enduring patience, encouragement and support throughout the writing of this thesis. I'm grateful that her love for me is more predictable than fluvial geomorphology. I am indebted to my Mum (Mrs C. M. Wilson) for innumerable proof readings of this text, for housing us, and for help with the flume work. Thanks also to Mum and Dad Jones, for considerable financial support, Sunday lunches and help with the experimental work.

I would also like to acknowledge helpful discussions on chaos with Dr. Chillingworth (Department of Mathematics, University of Southampton) and on statistical analysis with Professor Welsh (Department of Mathematics, University of Southampton). Any errors in understanding presented in this thesis are entirely due to the author.

The author gratefully acknowledges receipt of an N.E.R.C. CASE studentship in partnership with the Environment Agency (award reference GT04/97/264/FS).

Finally, I would like to thank Dr. David Sear and Professor Mike Clark, for their input to this text, and for letting me undertake a thesis somewhat tangential to that originally set.

Chapter 1

A Statement of the Problem

Outline

The aim of this introductory chapter is to provide an outline of the central problem that this thesis aims to address. Firstly, it is stated that this thesis is concerned with the indeterminacy of river channel cross-section and river channel planform in alluvial channels, and definitions of these terms are given (1. 1.). The proposed equations required for an analytical solution to flows over a mobile sediment layer are outlined and it is highlighted that these equations cannot be solved and that alluvial river channel cross-section and alluvial river channel planform are consequently indeterminate. An example of indeterminate behaviour is taken from sediment transport equations and it is suggested that indeterminacy in alluvial channels exists for at least three reasons (1. 2). Two definitions of fluvial geomorphology are given; a broad definition of the discipline includes all processes and forms involving running water; a narrow definition of the discipline limits its objects of study to within channel processes and forms. It is contended that whichever definition is accepted, indeterminacy is a good candidate for the 'core area' of research in fluvial geomorphology, because indeterminacy is present wherever fluid flows pass over mobile sediment layers. Indeterminacy introduces uniqueness into fluvial phenomena and it is posited that this poses a considerable challenge to the formation of laws in the discipline (1. 3.). Finally then, based on the problem of indeterminacy and the need to form laws in fluvial geomorphology, the overarching question this thesis aims to answer is stated; "What scientific laws can best explain the indeterminacy of river channel cross-section and river channel planform?" This question results in three hypotheses, that are tested in this thesis: 1) "Causal laws can best explain the indeterminacy of alluvial river channel cross-section and alluvial river channel planform," 2) "Aleatory statistical laws can best explain the indeterminacy of alluvial river channel cross-section and alluvial river channel planform," 3) Chaos laws can best explain the indeterminacy of alluvial river channel cross-section and alluvial river channel planform." The chapter concludes by stating that the formation of this question and its hypotheses are based upon two tentative conclusions from the literature - that geomorphology is a science and that science is defined by several core elements (1. 4.). These two conclusions have important ramifications as to how indeterminacy is to be approached by fluvial geomorphology and how current understanding of indeterminacy is to be judged. Reciprocally, indeterminacy has ramifications for the scientific nature of fluvial geomorphology.

1. 1. Alluvial Channel Cross-Section and Channel Planform

The central focus of this thesis is the indeterminacy of morphology in alluvial river channel cross-section and river channel planform. Alluvial rivers are defined as those rivers that flow through their own sediment deposits and form their own channels. River channel cross-section and river channel planform are both fairly self-explanatory terms; channel cross-section describes the three-dimensional form of a river channel, if a line was drawn across the river channel. Its basic dimensions are width and depth. In practice, defining the channel cross-section is sometimes difficult in the field because the current channel may not seem to correspond to the current flow. For example, the flow at the time of measurement may be smaller than the dimensions of the channel and the investigator has difficulty in establishing what the dimensions of the channel should be labelled as. Channel planform is defined as the two-dimensional morphology seen from a 'birds-eye' view of the river channel and includes straight, meandering and braided channel patterns. Channel cross-section and planform are closely related.

1. 2. Indeterminacy

Indeterminacy may be defined as the inability to specify the individual case at a given scale of measurement, given some understanding of what controls the phenomenon under investigation (see Leopold and Langbein; 1963). This statement pre-empts much of the discussion given in Chapter Three about what constitutes a legitimate description of the 'individual case.'[1]

Within a river channel, a system of components and associated equations has been formulated, which, if solved, would provide a framework for determining the cross-section and planform of alluvial rivers. Principally, a given flow within a stream must conform to three physical relations; continuity, flow resistance and sediment transport equations (Leopold and Langbein, p. 189; 1962: Knighton, p. 88; 1984) - continuity is the conservation of mass of a body of water - flow resistance is the force acting upon a given body of water and sediment transport is the transfer of lithological mass. These three components revolve in a cycle of feedbacks where each is dependent on the other, in all directions, at any point in space and time, with each component containing several degrees of freedom. The extent of these degrees of freedom results in the three principal components of continuity, flow resistance and sediment transport and their associated equations requiring further constituent equations to determine their behaviour.

[1] **Chapter three** discusses the view that science sees statistical distributions and strange attractors as individual cases and that a fundamental understanding of nature can be gained from understanding nature through determinacy of this kind.

Hey (1975: 1979: 1982: 1997) and Hey and Thorne (1984: 1986) have developed a model which at its last evolvement (Hey (1997)) identified nine process equations which must be solved in order to determine the cross-section and channel planform for river channels with mobile boundaries (see Table 1. 2. 1.).

Table 1. 2. 1.- Nine Processes of Hey (1997) Requiring Equations

- Average flow velocity
- Mean depth
- Channel slope
- Width
- Maximum flow depth
- Bedform wavelength
- Bedform amplitude
- Sinuosity
- Meander arc length

Two methods of determining sediment transport provide a practical illustration of the difficulties in solving the nine process equations proposed by Hey (1997). The Hjulstrom curve defines sediment transport in terms of velocity, whilst Shield's diagram relates entrainment to shear stress, which is indirectly dependent on depth through flow structure. If the components of the continuity equation (width, depth and velocity) can be prescribed for a given flow, then a solution exists for that flow's sediment transport, according to these two schemes.

To describe continuity, flow resistance must be considered. Resistance affects velocity and concomitantly width and depth. If resistance is described, it can be related to continuity and then to an appropriate equation. However, continuity affects resistance in that, depending on how its components are distributed, the flow alters resistance to itself on deformable boundaries by moulding bedforms (Simons *et al.*; 1965). Resistance is also dependent on the continuity equation through the hydraulic radius component, which in turn requires the solution of bank erosion variables and hence resistance again, as erosion is dependent on entrainment. In addition, once sediment entrainment occurs, the energy budget of the stream is altered (Gilbert; 1880) (Hickin; 1972) meaning that relationships between continuity and sediment transport adjust. Richards expresses this situation in the following terms:

"Achieving a point of entry into this system for the purpose of analysis is extremely difficult, since the interaction is complete. Searching for simply - directed relationships is also fraught with difficulty. Depth and velocity are functions of roughness, which is partly a function of channel geometry, which is in turn partly a function of depth and velocity."

Richards (p. 894; 1973)

The system of equations proposed by Hey (1997) cannot be solved analytically. The alluvial channel is thus termed indeterminate, because, although investigators know the relevant equations for the cross sections and planforms of alluvial channels, they are unable to solve them; "... deterministic solutions become impossible in sand-bed channels where sediment transport, bedforms and roughness are interrelated," (Richards, p. 67; 1977).[2]

Indeterminacy exists in Hey's model for at least three related reasons; 1) for a given magnitude of a set of causes or controls, many combinations of the nine degrees of freedom are possible. Many configurations are available because all nine variables are capable of adjustment by different magnitudes (see Cole and King, p. 529; 1968). Leopold and Langbein (p. 189; 1963) writing on fluvial processes, stated that "the applicable physical laws may be satisfied by a large number of combinations of values of interdependent variables ... Any individual case then, cannot be forecast ... The result of an individual case is indeterminate."[3] Similarly, Simons *et al.* (p. 315; 1965) commented that "... Many significant aspects of flow in open channels, after nearly two centuries of study, are still only vaguely understood. The principal reasons for this lack of progress are the complex and interrelated nature of the variables..."

2) There is no fundamental equation that links all of the nine individual equations together. For example, imagine that the magnitude of the constituents of the continuity equation (width, depth and velocity) and the magnitude of sediment transport are initially fixed. When flow begins in the channel, from these initial conditions, the channel will adjust, but it is not possible to determine in what manner it will adjust, because the equation that links all the nine degrees of freedom of Hey (1997) is not known - "... an additional item of information is required: a relationship between the flow parameters, fluid and sediment properties, and the hydraulic roughness of the channel," (Task; 1971). Thomas Maddock Junior summarised this situation when he wrote that "...the requisite number of equations is not available and probably never will be." (Maddock; 1969).

[2] See also Cole and King (pp. 529 - 530; 1968), Kondolf and Downs (p. 130 and 132; 1996) and Clifford (p. 84; 1996) for statements on indeterminacy.

[3] This statement anticipates further philosophical discussion on the nature of science given in **Chapter Three**. It implies that although the individual case is not forecastable, the collective properties may be. The validity of this suggestion depends on the scientific viewpoint adopted. A clear statement of what constitutes scientific understanding is essential for commenting on approaches in fluvial geomorphology.

3) Indeterminacy exists, because the nine variables of Hey (1997) adjust mutually and instantaneously. The causes of river form are also part of the effect - the causes do not act at a distance from the river system, but are part of the system, so that when a cause acts, it produces an effect, which in turn affects how the cause behaves *ad infinitum.* The problem of sediment transport used earlier, highlights this difficulty. The Hjulstrom curve suggests that the velocity of water flow is the cause of sediment transport. If investigators performed an experiment to find out how much sediment could be transported by a flow with given velocity, the flow would change in velocity due to the formation of resistant bedforms and the very act of transporting sediment would also change the velocity of the flow. Any action within the fluvial system changes the nature of the system itself. In this sense, the fluvial system seems to fall under the remit of the Heisenberg Uncertainty Principle.[4] Phrased more colloquially, any answer to an investigator's question changes the question asked. Stewart (p. 358; 1997) provides an apt analogy. Writing about the philosophical problems that confront Laplace's Vast Intellect[5], Stewart writes; "... if Vast Intellect were part of the universe, then every time It pondered the value of $dx_{7345232115}/dt$, It would change the very thing It was pondering."

1. 3. Indeterminacy as the 'Core Area' for Fluvial Geomorphology

Two definitions of fluvial geomorphology appear to be present in the literature. The first definition restricts fluvial activity to rivers and processes acting within the river channel; "Fluvial - pertaining to a river: Fluvial Processes - a set of mechanisms that operate as a result of water flow within a stream channel," (Allaby and Allaby, p. 211; 1999: see also Whittow, p.200; 1984). This definition was adopted by Knighton (1984) and Summerfield (1991) in their discussion of fluvial processes and form. The second definition is much broader; "Fluvial - running water including both overland-flow and stream-flow (McKnight, p. 585; 1999: see also Tinkler, p. 182; 1985). This definition was taken by Leopold *et al.* (p. vii; 1964) who wrote that fluvial processes were concerned with '... landform development under processes associated with running water.' In accordance with the above quote, their text 'Fluvial Processes in Geomorphology' included chapters on weathering, overland flow, erosion, hillslope characteristics and processes, channel form and process and the initiation and growth of channel networks. Other texts on fluvial processes have also used this wider definition (Chorley; 1969: Pitty; 1979b).

[4] See **p. 56 this thesis** for a description of Heisenberg's Uncertainty Principle.

[5] See **p. 48 this thesis** for a description of Laplace's Vast Intellect.

Gregory (p. 278; 2000) in a survey of physical geography wrote that '... there are core areas for each of the sub-branches ...' of the discipline. Whichever definition of fluvial geomorphology is accepted, this thesis contends that indeterminacy is a good candidate for inclusion in the core area and core problem focus of fluvial geomorphology. Indeterminacy poses the problem of uniqueness in rivers; Shields Jr. (p. 26; 1996) wrote that "Natural streams are conveyors of sediment as well as water and are inherently dynamic. Since so many variables are involved, the adage 'A river is the best model of itself', is apt, despite the self contradiction involved;" Culling (p. 352; 1987a) makes a related point using the observation of Heraclitus, the Greek philosopher, who opined that "You cannot step twice into the same river," (see also Russell, p. 63; 1961).

This uniqueness stems from the three reasons given above; 1) Different rivers or the same river can assume many different configurations of cross-section or planform morphology for the same magnitude of cause or control[6] 2) There is no fundamental equation to link form and process together and 3) Cause and effect interact, meaning that it is difficult for an investigator to isolate the contribution of a variable to morphology.

Uniqueness means that fluvial geomorphology risks being an exclusively empirical activity, where the controls on any given river are observed and recorded and the cross-section and planform morphology for any given river are observed and recorded. In contrast, a scientific activity has laws[7] which bridge the gap between controls and effect, or in the case of fluvial geomorphology, between control and channel cross-section and channel planform. A scientific activity, using laws, could determine the form without having to observe it. It is suggested that a major task for fluvial geomorphology is to find laws that can adequately incorporate the uniqueness that indeterminacy involves.

The problem of indeterminacy is of particular significance within alluvial channels (the narrow definition of fluvial geomorphology), where the formation of the channel's morphology and the morphology's subsequent influence on fluvial processes, is entirely dependent on indeterminate relations. However, indeterminate relationships are of significance in any environment where fluid flows interact with mobile sediment, and this means indeterminacy is present in those components that form the broader definition of fluvial geomorphology. It is suggested that the problem of indeterminacy forms a core research area for either definition of the discipline.

[6] See **Chapter Seven** where different morphology is produced at the individual datum level, and at the reach level in experimental conditions where the controls on river form are regulated.

[7] See **Sections 3. 4. 1** to **3. 4. 3. 2.** for a justification of the role of laws in science.

1. 4. The aim of the Thesis

Following on from the importance of indeterminacy and the conjecture that an important task for fluvial geomorphology is the formation of laws, this thesis proposes the overarching question - "What scientific laws best explain the indeterminacy of river channel cross-section and river channel planform?" In accordance with the account of physical science given in Chapter Three, three specific hypotheses result from this question:

Hypothesis 1:

"Causal relationships between entities best explain the indeterminacy of alluvial river channel cross-section and alluvial river channel planform."

Hypothesis 2:

"Aleatory[8] statistical relationships between entities best explain the indeterminacy of alluvial river channel cross-section and alluvial river channel planform."

Hypothesis 3:

"Chaos relationships between entities best explain the indeterminacy of alluvial river channel cross-section and alluvial river channel planform."

The aim of this thesis is to test these hypotheses in experimental conditions described in Chapter Six.

The formulation of the overarching question "What scientific laws best explain the indeterminacy of river channel cross-section and river channel planform?" and the resulting three hypotheses require considerable justification. This question and the resulting hypotheses have been proposed, based upon two tentative conclusions that have been derived from a review of the philosophy of geomorphology and the philosophy of science (Chapters 2 - 4). The first tentative conclusion is that geomorphologists think they are scientists and that geomorphology is a science (Chapter 2). The second tentative conclusion is that science is defined by, and works by, several key components and a set methodology, or rules that must apply, if a discipline is to be called a science (Chapters 3 and 4).

[8] Hacking (1975) uses this term to describe laws which are concerned with stable statistical frequencies - see **pp. 49** and **53 this thesis**.

Chapter 2

The Scientific Nature of Geomorphology

"We need always to take account of what has been experienced and understood by other people and other ages, before we conclude that here we stand and can do no other."

Polkinghorne (p. 59; 1991)

Outline

This Chapter deals with the first tentative conclusion used to form the overarching question and the resulting three hypotheses in Chapter One - "Geomorphologists think they are scientists and geomorphology is a science." This conclusion is essential for defining a framework of investigation for the problem of indeterminacy in alluvial channels and the criteria by which the success of investigations can be judged.

The views of several geomorphologists over a century of research are presented and it is suggested that the majority view of geomorphologists is that the discipline is a science (2. 1). It is suggested that the consensus on the scientific nature of geomorphology masks two distinct conceptions of geomorphology evident in the literature; the first conception is that geomorphology is equivalent to physical science (physics, chemistry) and makes use of all its associated components (hypotheses, laws etc.); the second conception is that geomorphology is a science, termed 'historical science,' that is logically different to physical science. These two schools of thought are examined. It is proposed that the eminent geomorphologists G. K. Gilbert and W. M. Davis were in agreement and held that explanation in geomorphology was by the methods of physical science, and in particular, scientific laws (2. 2.). It is then stated that the two philosophical conceptions of geomorphology as equivalent to physical science, and geomorphology as 'historical science' came from the development of process studies in the 1950's and its conflict with historical geology. It is contended that Kitts (1963) proposed the first view and Simpson (1963) proposed the latter and the different philosophies are labelled Kittonian and Simpsonian respectively (2. 3.). The views of geomorphologists who have adopted the Kittonian or Simpsonian philosophy are presented. Those geomorphologists who consider the discipline to be non - scientific and the reasoning behind this view, are also considered (2. 4). The chapter ends by advocating the philosophy of Kitts (1963) primarily because of the second tentative conclusion presented in Chapter Three that science is defined by several key components and any discipline which calls itself scientific must incorporate these components (summary).

2. 1. Geomorphology as Scientific

"The Landscape is a frame of mind..."

Culling (p. 358; 1987a)

Whether geomorphologists view geomorphology as scientific or not, is essential for approaching the problem of indeterminacy and for commenting on previous work on fluvial morphology. The idea of a discipline being scientific carries with it the notion of some sort of framework, or expectation, of the type of knowledge that is being generated. This chapter must establish where geomorphology's interests lie, and what type of knowledge it wants to produce, so that an appropriate investigation into indeterminacy can be made and a critique of previous work can be given.

Commentators on the philosophy and methodology of geomorphology, generally agree that their discipline is scientific. Table 2. 1. 1. shows the views of several practitioners of geomorphology. Table 2. 1. 1. seems to show that for at least a century, many influential Anglo-American geomorphologists have perceived themselves as possessing a scientific 'frame of mind' and view geomorphology as a legitimate member of the scientific community (Vitek and Giardino, p. xii; 1993). Rhoads and Thorn (p. 288; 1993) surmise the consensus given by table 2. 1. 1., stating that "The scientific view of geomorphology is clearly the majority opinion in the discipline."

However, the apparent agreement given in table 2. 1. 1. masks two distinct conceptions of geomorphic science evident in the literature. The first of these holds that geomorphology is equivalent to physical science (physics, chemistry) and makes use of its philosophy and methods. The second holds that geomorphology is logically different to physical science and belongs to a branch of science termed 'historical science.' The development of these two ideas is reviewed chronologically below, so that a complete description of the nature of geomorphology is given, and that an informed choice can be made as to which one of these two approaches is adopted by this thesis and the reasons why this is so.

2. 2. Gilbert and Davis

Johnson (1933) and Mackin (1963) taken together, identify Thomas Crowder Chamberlin, Grove Karl Gilbert and William Morris Davis as the three founders of the rational method in geomorphology. These men founded and developed 'multiple working hypotheses' as a framework for investigation in geology/geomorphology, with Chamberlin (1890: 1897: 1904) proposing the scheme for science and society in general, Gilbert (1886: 1896) developing the precise formulation of hypotheses by his advocacy of reasoning by analogy, and Davis (1911) furnishing the technique with the quality of deductive reasoning (see Johnson, p. 158; 1933 for a commentary on Davis's contribution).

Table 2. 1. 1.- Geomorphology as Science

Author	Statement
Kennedy, (p. 154; 1977)	"... fluvial geomorphology is one of the oldest established of the modern natural sciences..."
Bagnold, R. A. (p. 166; 1990)	"I should try to find ... an explanation of how the laws of nature operate to make rivers behave as they do"
Summerfield , M. A. (p. 3; 1991)	"Geomorphology is the science concerned with the form of the land surface and the processes which create it."
Rhoads, B. L and Thorn, C. E. (p. viii; 1996a)	"... it is reasonable to assert that most geomorphic knowledge is derived scientifically .. numerous articles ... agree on one point - that geomorphology is a science and, therefore, a scientific discipline."
Graf W. L. (p. 444; 1996)	"Fluvial geomorphology and the downstream impacts of dams is also a suitable venue for exploring science and policy"
Davis, W. M. (p. 168; 1900)	"Further illustration of the growing recognition of form as the chief object of physiographic study of the lands is seen in the use of the term, 'geomorphology', by some American writers; but more important than the term is the principle which underlies it. This is the acceptance of theorising as an essential part of investigation in geography, just as in other sciences."
Smith (p. 252;1993)	"Fluvial Geomorphology firmly established itself as a scientific subdiscipline..."
Leopold, L. B. and Langbein, W. B (p. 185; 1963)	"In Geomorphology, as in other branches of science..."
Mosley, P. M. and Zimpfer, G. L. (p. 381; 1976)	"Most geomorphologists currently work within the context of classical scientific method..."
Bauer B. O. (p. 399; 1996)	"Most geomorphologists believe that geomorphology is a science."
Strahler, A. N. (p. 74; 1992)	"Geomorphology ... a sine qua non of all scientific research in the natural sciences"
Haines - Young, R. H. and Petch, J. R. (p. vii; 1986)	"[a] framework for the understanding of scientific method and its application in physical geography."
Thornes, J. B. (p. 14; 1978)	"[there are] theories in geomorphology commonly developed from basic laws of physics or chemistry... virtually all the work on river mechanics falls into this class."
Chorley, R. J (p. 1; 1978)	"Geomorphology is the science which has for its objects of study the geometrical features of the earth's terrain..."
Cox. N. J (p. 426; 1985)	"[Richard Chorley's] many distinctive and influential contributions to geomorphology, especially to the history and methodology of the science."
Osterkamp, W. R. and Hupp, C. R. (p. 415; 1996)	Geomorphology ... developed from principles borrowed from basic science [physics, chemistry, biology].

Gilbert and Davis, arguably the two principal architects of geomorphology (Sack; 1992)[9] are sometimes thought of as having antithetical perspectives on the nature of the discipline (Strahler; 1950: see Sack; 1991), yet there are some common themes in their approach. The adoption and development of the method of multiple working hypotheses, by both Gilbert and Davis, have already been alluded to above, but, in addition, it can be construed that both these workers viewed geomorphology as an extension of physical science. That is, geomorphology was science in the normal sense of the word, replete with all the specialised facets usually associated with the scientific method, such as hypotheses, laws and theories.[10]

[9] The title 'geomorphology' was introduced during the lifetime of Davis (Davis, p. 168; 1900).

[10] See **Chapter Three** for definitions of these terms.

Gilbert (p. 286; 1886) held the view that nature consisted of complex chains of events; "Antecedent and consequent relations are therefore not merely linear, but constitute a plexus; and this plexus pervades nature." Gilluly (p. 220; 1963) suggests that Gilbert's use of the terms 'antecedent' and 'consequent' are noncommittal ways of expressing cause and effect, because in causal terminology, the relationship between cause and effect is strictly linear - a cause has a given effect[11] , whereas in Gilbert's conception of nature '... every antecedent has not one but many consequents..' (Gilluly, p. 220; 1963).

With field investigations in geomorphology, reasoning often proceeds from effects to causes and Gilbert (p. 287; 1886) himself reverses the procedure so that in terms of Gilluly's interpretation, 'every consequent has not one but many antecedents.' This recognition by Gilbert that a cause may have different effects and an effect may have different causes may be what prompted his adoption of the method of multiple working hypotheses (Baker and Pyne, p. 99; 1978). Gilbert writes about his solution to the problem of antecedents and consequents in the following terms:

> *"In the application of this method the student of a group of phenomena, instead of inventing and testing hypotheses one at a time, devises at an early stage as many as possible, and then treating them as rival claimants, assigns to himself the role of judge. Returning to the study of nature he seeks for special features which cannot consist with all the hypotheses, and may therefore serve to discriminate among them. Thus by a series of crucial tests he eliminates one after another of the tentative theories until but a single one remains, and he then proceeds to apply such tests as he may to the survivor."*
>
> Gilbert (p. 2; 1896)

At this juncture in the review of Gilbert's conception of geomorphology, a possible and perhaps helpful interpretation of Gilbert's work is an analogy to a detective solving a murder enquiry. When the investigation begins, the detective forms a list of various suspects - he does not draw up a list based on wild speculation, but on careful consideration of the circumstances, motive, opportunity etc. of various people. The detective does not believe that all or none of the suspects committed the crime, nor that the list of suspects could be as long or as imaginative as you like, because he will not be able to decide who did the deed. On the contrary, the suspects are rounded up based on preliminary evidence and their stories are evaluated by testing the truth or falsity of the consequences of those stories.[12] The plexus of suspects is due to the detective's ignorance which he believes will be removed by careful investigation.

[11] See **Section 3. 4. 1.** for an outline of causal thinking in science.

[12] The philosophies of induction and falsification are considered in **Section 3. 6.**

This is the sense in which Gilbert proposes that a cause can have different effects and an effect different causes. It is only a perception that the same effect has a different cause; at the start of an investigation it is easy to conceive several processes that may cause such an effect, but after formulating these conceptions as multiple hypotheses, they will be whittled down to one hypothesis which is judged to be the true cause of the effect by gaining evidence of the consequences of its application (see quote above (Gilbert, p. 2; 1896) where Gilbert states there is a soul survivor from the process of multiple hypotheses). Haines-Young and Petch (1984) suggest a similar schema.

Gilbert's practice of geomorphology proceeds in a vein similar to that of the detective, and importantly the method by which Gilbert formulates his hypotheses and tests their validity is in the manner common to physical science; the use of scientific laws. Gilbert proposed analogic reasoning for the selection of hypotheses from the '...infinite number of hypotheses available to explain any natural phenomena,' (Sherman, p. 104; 1996) and these analogies appealed to established information elsewhere; "... tentative explanations are always founded on accepted explanations of similar phenomena," (Gilbert, p. 145; 1896).

Invariably these 'accepted explanations' from which Gilbert drew his analogies and formulated his hypotheses, were the laws of physics and thermodynamics. Baker and Pyne (pp. 102 and 103; 1978) identify analogies to fixed beams, hydrostatic equilibrium, the principle of least action and the laws of thermodynamics in Gilbert's (1880) 'Report on the Geology of the Henry Mountains' and form parallels between the work of Gilbert and the work of the thermodynamicist, Willard Gibbs. Baker and Pyne (pp. 107 - 108; 1978) also point out that Gilbert (1890) in his 'Lake Bonneville' monograph attempted to derive the physics of wave processes and that "By equating geologic process with mechanical forces, Gilbert was able to organise those processes by the same principles that engineers were applying to structures and machines." Baker and Pyne (p. 113; 1978) surmise Gilbert as a "... consummate Newtonian ..."

It would appear that Gilbert reasoned by analogy to physical laws for the formulation of his hypotheses and then tested the implications if those laws applied in the natural world (see Gilbert; 1896 for a detailed example of this procedure). If a given physical law was proven to be applicable by the evidence, it became a law in geomorphology. For example, from his analogic reasoning in the 'Report on the Geology of the Henry Mountains', Gilbert (pp. 100 - 124; 1880) wrote on the sixth power law, the law of uniform slopes, the law of structure and the law of divides (see Bradley, p. 17; 1963: Higgins, p. 5; 1975: Baker and Pyne, p. 103; 1978).

Reasoning in geomorphology for Gilbert appears to have been by law. In terms of the detective analogy, laws provided candidates for suspects, laws provided evidence for or against themselves by the consequences of their claims which could be tested for truth or falsity in the field, and those laws which were found 'guilty' were established as suspects the next time an investigation arose.

William Morris Davis, *in principle*, had a similar view to Gilbert, of geomorphology as an extension of physical science, whereby understanding of geomorphic phenomena progressed by the testing of hypotheses and the formulation of laws and theories. Davis (1899) proposed the Geographical Cycle as a framework for understanding landforms. In this paper he writes of the need to adopt the practices and facets of physical science in order that geography may become a more rigourous discipline.

> *"The scheme* [the Geographical Cycle] *gains a very 'theoretical' flavour that is not relished by some geographers, whose work implies that geography, unlike all other sciences, should be developed by the use of only certain ones of the mental faculties, chiefly observation, description and generalisation. But nothing seems clearer to me than that geography has already suffered too long from the disuse of imagination, invention, deduction and the various other mental faculties that contribute to a well-tested explanation. It is like walking on one foot, or looking with one eye, to exclude from geography the 'theoretical' half of the brain-power which other sciences call upon as well as the practical half.*
>
> Davis (p. 484; 1899)

In the opening paragraph of this paper Davis (p. 481; 1899) states the three foundational points of the Geographical Cycle; "All the varied forms of the lands are dependent upon - or as the mathematician would say, are functions of three variable quantities, which may be called structure, process and time." This 'trio of geographical controls' may be interpreted as the causes of landforms (processes) working on given boundary conditions (structure) and the time over which these processes have operated.

This interpretation finds some support in the writings of Davis. Firstly Davis (p. 482; 1899) viewed structure, not as a process in geography, but as a given initial condition for any investigation; "The forces by which structures and attitudes have been determined do not come within the scope of geographical enquiry, but the structures acquired by the action of these forces serve as the essential basis for the genetic classification of geographical forms." Secondly, Davis viewed structure as a passive initial condition that set the context for processes:

> *"The fully developed scheme of the cycle recognises the passive mass of the earth's crust ... The surface of the land may be regarded as composed of a number of individual forms, whose general character depend on the rock structure which the processes of land - sculpture have worked upon ... the active process .. works on the landform and causes its change."*
>
> Davis (p. 367; 1899: p. 160; 1905: p. 312; 1909)

The synthesis of Davis, that landforms are a function of structure, process and time is in complete agreement with that of Gilbert. Although Gilbert's primary concern was the elucidation of the laws by which processes act and not explicitly with the changes in landforms through time, the basic *principles* that Davis laid down were in agreement with Gilbert and in fact would foreshadow much of the central concerns of modern geomorphology which addresses both the application of process laws and the change of landforms through time.[13] However, whereas Gilbert attempted to formulate laws for processes, "... Davis did not like the detailed study of processes" (Chorley *et al.*, p. 395; 1973) and seemed to hold to the view that the elucidation of the specific workings of process was not necessary, because the time elapsed since the initial conditions effectively summarises all of the complex processes working on the earth's surface:

"Process cannot, however, complete its work instantly and the amount of change from initial form is therefore a function of time ... the sequential forms existing at any one moment are so largely dependent on the amount of work that has been done upon them that they are susceptible to systematic description in terms of the stage of the cycle which they have reached .. [in] the trio of geographical controls [time is] the one of most frequent application and of most practical value in geographical description,"

Davis (p. 482; 1899: p. 150; 1905)

Davis (p. 483; 1899) did note that "... neither the amount nor the rate of change is a simple function of time," but despite this recognition, Davis explained landforms by a law of temporal evolution. Davis held that landforms would progress in a systematic sequence, such that if structure and process are similar for a given region, the morphology of that region is a function of time since the initial conditions were imposed:

"The sequence in the developmental changes of land-forms is, in its own way, as systematic as the sequence of changes found in the more evident development of organic forms .... Let it be understood that the type of forms thus established in a systematic series are not empirical intimations of actual landforms; they are imagined examples of rationally developed land forms deduced from a general explanation, the validity of which has been previously established by critical analysis ... as rationally developed forms they are transparent in space and time, they are known through and through, in the past and in the future, and all the members of a series are helpfully related to one another."

Davis (p. 485; 1905: pp. 77 - 78; 1915)

[13]Schumm and Lichty (1965) exemplify this movement to reconcile process changes over time.

Davis's use of a temporal law is different to Gilbert's development of process, only in its ontology (objects of study) rather than its epistemology (theory of knowledge). Davis's initial trio of structure, process and time, interpreted in terms of process laws acting on boundary conditions is similar to the work of Gilbert. All Davis did though by focusing on time, was to make both structure and process boundary conditions to a temporal law. Explanation in geomorphology was for Davis, as for Gilbert, the form of explanation used by the physical sciences and in particular, explanation by law. Apparent anomalies to Davis's law of temporal evolution could be explained by differences in process or structure, i.e. the boundary conditions on which the law operated (see Guelke; 1971: Bishop; 1980).

The scientific nature of Davis's work has been questioned (Chorley; 1965: Chorley *et al.*; 1973: Guelke; 1971: Pitty, p. 1; 1979a: Bishop; 1980) on the basis that it was a dogmatic framework for enquiry and that it was expressed in such a way as to be untestable. Perhaps the most memorable criticism though came from Strahler (1950a):

"Davis's treatment appealed then as it does now, to persons who have had little training in basic physical sciences, but who like scenery and outdoor life. As a cultural pursuit, Davis's method of analysis of landforms is excellent; as part of the basis for the understanding of human geography it is entirely adequate. As a branch of natural science it seems superficial and inadequate."

Strahler (p. 209; 1950a)

Although the limitations to Davis's scheme are considerable and involve large assumptions about the nature of structure and process, Davis, *in principle,* if not in practice, held the view that geomorphology was equivalent to physical science, that understanding was by laws, and that like his counterpart Gilbert, scientific knowledge was always tentative (see Davis, p. 1; 1926). Davis (1926) gave an almost Kuhnian account of geomorphology:

"For inasmuch as the great advances of physics in recent years and as the great advances of geology in the past have been made by outraging in one way or another a body of preconceived opinions, we may be pretty sure that the advances yet to be made in geology will at first be regarded as outrages upon the accumulated convictions of today, which we are too prone to regard as geologically sacred ... it may also come to be part of wisdom to ask ourselves in what way and how far our present conception of the earth must be modified in order to transform such outraging possibilities into reasonable actualities; for that is precisely the way in which ...outrages ... have gained an established place in our science."

Davis (pp. 464 and 465; 1926)

The fact that while in practice Davis seems to have held the Cycle of Erosion as 'geologically sacred', that Davis appears to have "... told his research students what to look for...," that the "... Davisian scheme as expressed by the master was so elastic that failure to comply with it simply meant lack of imagination," (Chorley *et al.*, p. 393; 1973) and that coupled with the 'loyalty/fanaticism of his disciples' the Cycle exerted a stranglehold on the discipline (Chorley *et al.*, pp. 753 -754; 1973), does not seem due to Davis's lack of appeal to the different facets of physical science, but merely because Davis and his followers were convinced that the Cycle was correct.

It is contended here that Davis held the same scientific views in *principle* as Gilbert and indeed as Strahler - that geomorphology promotes understanding of the earth by way of the method used in physical science[14] - and that the criticisms of Strahler and others were directed at how Davis practised geomorphology, rather than the idea of science he subscribed to.

2. 3. The Fabric of Geology

Following the work of Gilbert and Davis, it is generally held that the Cyclic view of Davis with its temporal law, rather than the timeless process laws of Gilbert, became the dominant concept for the understanding of landforms (Chorley; 1964: Bishop; 1980: Sack; 1991: 1992). The dominance of Davis's framework persisted until the work of '... hydrologists, hydraulic engineers and soil erosion specialists...' on various earth surface processes came to the attention of geomorphologists (Strahler, p. 210; 1950a: 1992). Works such as Gilbert (see above), Hjulstrøm (1935), Bagnold (1941), and Horton (1945) (see also Carlston, p. 864; 1965: Tinkler; 1985: Strahler 1992: Gregory 2000) coupled with a growing belief that process and form did not have a simple relationship to time (see Leighly; 1940: Chorley; 1964) spawned explicitly geomorphic papers that investigated the exact nature of processes and the laws that governed them (i.e. Strahler; 1950a: 1950b: 1952a: 1952b: 1954: 1956: 1958: Melton; 1958: Leopold; 1953: Leopold and Maddock Jr.; 1953 and Scheidegger; 1961). Advances in technology placed the detailed quantitative measurement of process laws on a firm foundation (Krumbein; 1962: Mackin, p. 137; 1963: Wilson; 1972: Albritton, p. 5; 1975).

Chorley (p. 31; 1965) writes that "... the cyclic approach to landforms ... in geomorphology, successfully isolated the subject from every other science except a small segment of historical geology." The developments in process studies and the possible challenge to techniques of historical geology such as denudation chronology to which the geographical cycle was intimately linked (Chorley, p. 32; 1965), may have prompted the collection of essays presented in Albritton (1963a).

[14] See **p. 20 this thesis** for Strahler's views on geomorphology and science.

Entitled the 'Fabric of Geology'[15] the aim of the volume was to "... re-examine the logical structure of geologic science from the ground up," or to answer the question "What is to be understood when geology is called historical science," (Albritton, p. v; 1963b). In this re-examination, the development of process laws and historical geology and their respective philosophies collided (see Mackin; 1963). The volume in essence proposed two important and enduring philosophies for geomorphology; the first by Kitts (1963) and the second by Simpson (1963). These are examined in turn below.

Kitts (1963) espoused the view that had been taken by both Gilbert and Davis, that geological/geomorphological science made use of the theories and the facets of physical science and was epistemologically equivalent to physical science; "The theory of geology is ... the theory of physics and chemistry," (Kitts, p. 60; 1963). The only difference between geological science and physical science was that in physical science the boundary conditions for the timeless laws that it discovered were known, whereas in geological/geomorphological science, changing boundary conditions had to be discovered for timeless laws.

Because geology/geomorphology had to specify boundary conditions, Kitts (1963) defined several types of laws that applied in geology. Probabilistic laws, which were not to be aspired to, at heart expressed universal timeless laws, but with the conditions (boundary conditions) needed for that law to be universal, not clearly known. Normic laws were where the boundary conditions that affect the universal law were known and if they were measured, then a universal law could be formed. Kitts thought that normic laws were the concern of geology, where the geologist's role was to establish the magnitude of the boundary conditions that resulted in a specific landform. Kitts emphasised that explanation in geology would focus on singularities and the specification of boundary conditions, which led to timeless laws producing unique landforms:

> *"One thing is certain and that is the geologist will not 'abandon the hold on the individual case' by allowing statistical statements to assume too important a role in his procedures, for ... it is the individual case which is his primary concern,"*
>
> Kitts (p. 52; 1963)

In essence Kitts proposed the first interpretation given to Davis's trio of structure, process and time;[16] that universal timeless process laws operate on different boundary conditions and that boundary conditions can change over time. This philosophy is termed Kittonian.

[15] Rhoads and Thorn (p. 90; 1994) have pointed to the significance of this volume and also the fact that most of the philosophical discussion concerning geomorphology has occurred within geology and not geography.

[16] See **p. 14 this thesis** for this interpretation.

Simpson (1963) proposed the view that a branch of science which he termed 'historical science' and included geology/geomorphology, was epistemologically and logically different to physical science (Watson; 1966). Somewhat ironically, a precursor to this philosophy was given by Melton, a pioneer of process geomorphology;

> *"Geology differs from physics and other exact sciences in the kind of variability encountered, as well as the amount. It should be acknowledged that the concept of the universe applicable to geology and physics is accordingly quite different; i.e. there is not just a single scientific 'world view' but two or perhaps many."*
>
> Melton (p. 445 - 446; 1958)

Simpson began by discussing the view that physical science was involved in discovering timeless laws, or in his terminology, 'immanent' laws. Historical science, including geology/geomorphology, however, whilst taking account of immanent laws was primarily concerned with discovering 'configurational' elements, or in modern parlance, the boundary or initial conditions in which timeless laws operate; "Historical science may thus be defined as the determination of configurational sequences, their explanation, and the testing of such sequences and explanation," (p. 25; 1963).

Thus far Simpson's philosophy seems to be akin to Davis's own interpretation of structure, process and stage where the main component is change through time. However, Simpson (p. 45; 1963) goes on to elaborate his scheme in a way that makes it fundamentally different to that proposed by Davis, and despite the interest in time, it is contended that Simpson's historical science would have had no appeal to Davis. Simpson argued that physical science used predictive relationships, where effect was a *necessary* result of a cause.[17] Historical Science including geology/geomorphology conversely uses postdiction, where an effect leads to reasoning about causes, but these causes, Simpson argued, did not always have a *necessary* connection to the effect (Simpson's ideas of the logical non-equivalence of prediction and postdiction are primarily derived from the work of Scriven; 1959) - that is several causes may have produced an effect and there is no logical connection to a particular one. Simpson thus implied that physical science and geological/historical science were not logically equivalent and therefore were separate areas of knowledge.

[17] See **Section 3. 6.** for the importance of prediction in science.

This is fundamentally different to the work of Davis and Gilbert. As already mentioned, Davis's Cycle is a temporal law or a "... deterministic sequence [in which] landforms evolve through a predictable sequence of landform assemblages ... and provide information on its past and future configuration," (Sack, p. 253; 1992: p. 31; 1991). That is, Davis's temporal law provides logically equivalent prediction in the future and postdiction in the past. Similarly the detective analogy used to explain Gilbert's work, illustrates that the consequences of the laws he formulated applied to prediction or postdiction.

Simpson (p. 39; 1963) actually proposes two things. Primarily, and as mentioned above, Simpson argues that the specification of the configurational elements that produced a given effect, is independent of laws, because postdiction involves no necessary connection between effect, law and configuration; secondly, Simpson infers that predictive laws cannot apply in complex environments, for the same reason - that the details of the configurations they act on serve to obscure predictive abilities. Simpson's emphasis is thus to diminish the role of laws or physical science in geology/geomorphology and focus on unique events or configurations: "The primary data of the historical scientist consist of partial descriptions of configurations near the level of plain story [or] a straight description of what there is and what occurs," (Simpson, p. 40 and p. 26; 1963).

Simpson (pp. 40 - 43; 1963) goes on to discuss relationships that can be formed between these plain stories or unique events; he writes of the identification of broadly similar antecedent and consequent entities and their place in a sequence. The analysis which historical science uses examines the antecedent or consequent entities for similar or different configurational variables that are deemed pertinent to the relationship between the two forms of entities. A problem with this description of historical science is that Simpson provides no rational basis for deciding which entities are antecedent or consequent, nor does he provide any rational basis for deciding which configurational variables are pertinent to a relationship between the two.

Furthermore, Simpson (p. 45; 1963) states that in historical science where cause and effect and effect and cause are not necessarily related, "... postdiction involves the self - testing essential to a true science .." though Simpson also admits that this aspect of his philosophy has not been fully expounded. Simpson (p. 45; 1963) writes that the most conclusive test in geological science is the "... confrontation of theoretical explanation with historical evidence ..," but he gives no indication of what constitutes a rigourous theoretical explanation of configurational elements.

Simpson's philosophy, which seems to denigrate the role of law and promotes the inequivalence between physical science and geology/geomorphology, whilst retaining the term 'science', is labelled here the Simpsonian view. A brief review of proponents of Kittonian and Simpsonian philosophies in geomorphology is presented below.

2. 4. Kittonian and Simpsonian Proponents

2. 4. 1.- Kittonian Proponents

Gilbert and Davis have already been described as subscribing to the philosophy elucidated by Kitts (1963), that geomorphology is equivalent to physical science and in particular makes use of laws. Before Kitts formulated his philosophy, Strahler (see above) produced several pioneering papers on process laws, and the scientific philosophy behind these papers is expressed in Strahler (1999). Strahler (1999) has given a detailed exposition of his views on science, and he explicitly states that the philosophy of geomorphology (a science that includes the effects of time) is the same as the philosophy of physical science. The unity of science which Strahler advocates is directly related to the logical equivalence he ascribes to prediction and postdiction, which is the converse view to that of Simpson (1963). Strahler (p. 25) states that differences in temporal prediction (prediction or postdiction) are not logical differences - the predictive relationship should hold logically, reasoning from cause to effect, or from effect to cause. This leads to the statement that:

> *"I have read statements to the effect that different branches of science use basically different means to formulate hypotheses and accumulate knowledge. I would like to think that such statements are generally unwarranted. Special ground rules for special groups is not something with which I can be comfortable ... all empirical science must play by the same rules."*
>
> Strahler (p. 26; 1999)

Simpson (1963), as mentioned above, seems to hold that laws do not apply to prediction in complex situations or to postdiction, where several causal alternatives may be feasible for any given effect. Strahler deals with this problem by effectively stating that even the most certain laws in physical science contain degrees of uncertainty and can only be expressed in terms of the likelihood or probability of them being true. Thus whilst a law in physics has a very high probability of being true, it is never certain. The choice of an appropriate law and the specification of relevant boundary conditions for a given event, relative to the pure law of physics, has a low probability of being the complete explanation, but it can be judged as being more likely than its counterparts. Returning to the detective analogy used for Gilbert's work, the idea of proof beyond reasonable doubt is in operation.

Because Strahler states that the evidence for laws is never 100 percent conclusive in the laboratory or in the field, in prediction or in postdiction, Strahler achieves the result that the methods and facets of physical science and geomorphology are the same and expresses the same view proposed by Kitts (1963).

Watson (1966) was perhaps the first to comment on the philosophy that Kitts (1963) had outlined. Writing in response to the papers presented in the 'Fabric of Geology,' Watson (p. 179; 1966) rejected Simpson's division between physical science (concerned predominantly with laws) and historical geological/geomorphic science (concerned primarily with uniqueness); "All scientific explanation is by subsumption to law-like generalisations, and if these are easier to come by and are more sure and useful in chemistry than in geology, this is no reason for saying that there is a logical difference between the two sciences,". Furthermore Watson (p. 181; 1966) specifically adopted the viewpoint of Kitts; "Kitts' clear and logically cogent presentation goes far towards dispelling the fog spread by Simpson's turgid ambiguities ... this careful paper deserves serious study by geologists."

Haines-Young and Petch (1980: 1983: 1984: 1986) in a series of papers, adopted a strict critical rationalist philosophy of science based upon Sir Karl Popper's concept of falsification. Haines-Young and Petch present a clear view that scientific explanation, whether in geomorphology or in physical science, is defined by a set of criteria that makes information scientific. Haines-Young and Petch (1984) comment on Simpson (1963) and note that although the observation that geomorphologists must investigate changing initial conditions, whilst physicists do not, is correct, it implies no logical difference in the fields of enquiry - science is unified.

"Simpson's distinction between historical and non historical science is significant but not fundamental. The physicist can ignore what might be regarded as the historical circumstances of a particular event because they are irrelevant to the problem in hand. Nevertheless, reference to some set of initial conditions is required if the event is to be explained .. It is evident therefore, that the form of the explanation is similar to that employed by the geologist or geomorphologist. Thus we do not regard explanation in geomorphology, or in physical geography more generally, as in any way different from that employed elsewhere in science."

Haines-Young and Petch (p. 368; 1984)

Because of the clear but rigid nature of Haines-Young and Petch's philosophy of science, many complex problems in geomorphology lie outside the sphere of scientific enquiry. Haines-Young and Petch do not label such investigations as historical science, but simply non-scientific.[18]

[18] This complication to the philosophy of Haines-Young and Petch is discussed on **p. 29 this thesis.**

Richards (1990: 1994: 1996) has developed the ideas proposed by Kitts (1963) that geology/geomorphology should focus on 'normic' laws and the specification of boundary conditions. To that end, Richards has suggested that geomorphology should make use of the 'realist' philosophy of science. The main aspect of realism that Richards advocated was concerned with the failure of experimentally generated laws or the failure of the critical rationalist method and falsification in the real environment, as proposed by Haines-Young and Petch above. Richards (p. 196; 1990) argued that testing of hypotheses and falsification was not likely to be possible in complex systems. This is because "Multiple causes operate together in 'open' systems in the natural environment, and their effects may be self cancelling in certain contexts, so that no observable events occur, or so that events relate to causal processes inconsistently," (p. 174; 1996). Richards' solution was that the 'meticulous description of local boundary conditions is now as critical to successful experimentation and interpretation as the development of fundamental theory, and that increased attention must be focused on innovative means of describing and measuring these boundary conditions." (p. 184; 1996).

Rhoads (p. 269; 1994), in response to Richards (1990), developed the concept of realism and suggested that it provided a '... unifying philosophical framework for geomorphology:' "... Viewed more broadly, the realist perspective encompasses most, if not all, of the types of research that geomorphologists undertake, including experimental, holistic and geohistorical approaches," (p. 271; 1994). Taking the primary meaning of realism as defined by Richards, that it was concerned with the operation of timeless laws in changing boundary/initial conditions, Rhoads suggested that this method provided a unified epistemology for geomorphology, including the geohistorical approach. This view has been expressed elsewhere by Rhoads and Thorn (p. 51; 1996b); "... any geohistorical event can be viewed as a mix of environmental/ historical contingencies and general processes governed by physical laws."

Bassett (1994) also responded to Richards (1990), and advocated a 'broadly realist approach to science' (Bassett, p. 276; 1994). However, Bassett also pointed out some of the difficulties with the realist approach, particularly on the identification of the relevant causes and boundary conditions pertinent to a real system, the difficulty of knowing when a fundamental cause has been reached, *vis - à -vis* the reductionist debate and the claim that all science is physics, and the lack of laws to account for the combinations of causal laws that may manifest themselves in the natural environment (Bassett, p. 275; 1994).

"Theoretical laws ... describe the actions of underlying causal processes and have real explanatory power. However they are generally only true of objects in the closed systems of idealised models, where all sorts of ceteris paribus conditions hold. They are not strictly 'true' in real systems. This is because they usually refer to distinct causes and their effects. In reality, such causes are only likely to manifest themselves in nature through complex combinations and interactions. Unfortunately, even in so called mature sciences, such as physics, there are few laws to account for these combinations, and their effects are thus varying and context specific."

Bassett (p. 275; 1994)

Rhoads and Thorn (1993), wrote a Kittonian response to a predominantly Simpsonian view proposed by Baker and Twidale (1991)[19] and challenged Baker and Twidale's conception of geomorphology on the two fronts of explanation and creation of new ideas. The core of Rhoads' and Thorn's argument was that theories were at the heart of scientific endeavour, that virtually all scientific epistemologies recognised the central role of theory, and that explanation and creation in science were theory laden (echoing the work of Kuhn (1962)). Such was the importance of theory in science that they wrote; "... a 'scientific' approach to geomorphology that denigrates theory in favour of observation is inconsistent with the prevailing views of modern science and jeopardises the respectability accorded to us [geomorphologists] by other scientists," (Rhoads and Thorn, p. 292; 1993).

Rhoads and Thorn (1993) proposed two different types of scientific argument which made up theoretical work in geomorphology. Both these arguments invoked laws and boundary conditions and proposed that the 'immanent' and 'configurational' elements Simpson (1963) had identified could be dealt with by a unified epistemology *à la* Kitts (1963) and that there was no fundamental dichotomy between historical and non-historical science (see also Thorn, pp. 213 - 215; 1988). Rhoads and Thorn thought that in contemporaneous investigations, deductive reasoning should be used as the scientific argument for deriving an event from known boundary conditions and a given law. In historical investigations, abductive reasoning should be used as the scientific argument for deriving boundary conditions from a known event and a given law. These two methods are logically equivalent and refer to prediction of a consequent or an antecedent respectively.

Rhoads and Thorn commented that the *a priori* laws used in abduction could be outrageous and run contrary to established theory. However they regarded this as an exception and that most abductions conformed to established theoretical constructs. Furthermore, in emphasis of the theory ladenness of science, they noted that the investigator would only know that their hypothesis was outrageous because they knew it did not accord with standard theory.

[19] See **p. 24 this thesis** for a description of Baker and Twidale (1991).

2. 4. 2. Simpsonian

Published in the same volume as Simpson (1963), Bradley (1963) gave a non-philosophical account of geology/geomorphology as a science of the description of historical events:

> *"Geology's spine is the history and constitution of the earth - each term to be understood in its broadest sense. Running through this spine is the thread of time - a thread connecting all terrestrial events. The succession and relationships between these events, together with the dynamic processes that have operated, make up the science of geology."*
>
> Bradley (p. 14; 1963)

Bradley envisioned that geology was carried out by asking the question 'why?' of events, and by detailed descriptions and empirical generalisations. Questions of 'how?' events occurred, which Bradley regarded as more amenable to law-like statements, were the domain of physical science; "One needs only to begin asking 'how?' in geology and the list grows like weeds after a warm rain. We cannot begin to answer these queries without the help of chemistry, physics or biology," (Bradley, p. 20; 1963). The paper gives a candid summary of Bradley's conception of geology and geologists:

> *"Most geologists are happy to explore and describe. Few are unhappy until they are able also to explain what they have found. And very few seem interested in trying to integrate the explanations into more general concepts or principles. Just possibly this derives from the patent fact that it is a lot more fun to do field work than it is to put the pieces of explained phenomena together, especially if the pieces were constructed by someone else,"*
>
> Bradley (p. 21; 1963)

Contrary to Simpson (1963) who held that 'historical Sciences' were the most complex of sciences, Bradley's (1963) conception of the science of geology - the description of events - was that it served as a source of data to the physical sciences, and he envisioned that with a greater maturity, geology may be subsumed by physics and chemistry (Bradley, p. 22; 1963).

Baker and Twidale (1991) present an overall conception of geomorphology that has "... peripheral interest in the theoretical and philosophical baggage that is used to explain and justify geomorphological knowledge" (Baker and Twidale, p. 87; 1991). Similar in ethos to Simpson (1963), Baker and Twidale use the term historical science to describe geomorphology, but distance historical science and geomorphology from the methods and facets of physical science.

Baker and Twidale predominantly attack the misuse of theory in geomorphology and suggest that the introduction of theory into geomorphology has occurred because philosophers of science have assigned a second rate status to historical science (p. 83; 1991: see also Simpson, p. 46; 1963) and geomorphologists, out of low self esteem, have aped the 'hard sciences with all their dogmatic ideologies' (p. 85 and 95; 1991). Baker and Twidale's criticism of the use of theory in geomorphology takes two fronts; firstly the inadequacy of theoretical constructs in explanation of historical features;

"... theory, largely borrowed from other disciplines, can have its negative influences. The predictive extrapolations come at a price. Theoretical deductions come at a price. Theoretical deductions must be verified or falsified. For that the critical data, in essence a completed experiment, is needed. However controlled experiments can rarely be achieved at the required scale of real geomorphological processes. As a result, nearly all extrapolative theory for geomorphological processes is unverified across the broad range of scales in time and space at which globally significant geomorphological systems operate,"

Baker and Twidale (p. 82; 1991)

Baker and Twidale's argument seems to be that theoretical schemes which make use of timeless laws cannot be applied in geomorphology, because the precise effects of timeless laws operating over time cannot be ascertained, except by unwarranted extrapolations.

The second front on which Baker and Twidale attacked theory, was the creation of new ideas in science. They argued that new ideas in historical science were generated independently of theory, and that the widespread adoption of theory in geomorphology had led to a constraint on discovery; "... insidious is the prospect of idealised theory becoming dogma, limiting the scope of geomorphological enquiry to that deemed acceptable within an artificial framework ... Lost is the landscape as inspiration for causative factors independent of initial theoretical constraints. Creativity is inhibited," (p. 84 and 89; 1991).

Baker and Twidale took as their model for scientific discovery, the analogical component of scientific reasoning proposed by Gilbert. Defending Gilbert's method from the potential philosophical criticisms of induction and Baconian science, Baker and Twidale described Gilbert's analogical reasoning as abductive. In abductive reasoning, Baker and Twidale assert that reasoning proceeds from effect to cause, by means of an already identified hypothesis; i.e. the hypothesis is not logically inferred from the data but that the data stimulates an analogy with an *a priori* concept.

This provided a precise method for reasoning from effect to cause in historical science which Simpson (1963) himself had not precisely specified, even although the logical difference between reasoning from effect to cause and from cause to effect was the foundation of his divide between historical and physical science. It is of interest to note that, as discussed above, Baker in Baker and Pyne (1978) highlighted the fact that most of Gilbert's analogies were derived from established laws and theories of physical science and translated into geomorphology.

According to Baker and Twidale, dominating theoretical constructs are minimised in the procedure of abductive reasoning. Indeed the abductions to be preferred are those that conflict with established theory, because they result in new discoveries. Baker and Twidale borrowed Davis's term 'outrageous hypotheses' to describe such abductions. In sum, the paper proposed that where theories were applicable in geomorphology, they should be held tentatively, but that primarily, geomorphology was historical and theories were of little value in this sphere; "Those geomorphologists who denigrate history ignore the richest and most important part of their discipline," (p. 93; 1991).

Baker has consistently expressed his scepticism of theory (Baker; 1993: 1996) and fleshed out the historical view set in Baker and Twidale (1991). In particular, Baker developed the point that hypotheses for abductions should be influenced primarily by nature and reality and the geomorphologist's interaction with it (p. 81; 1996). Baker has suggested that the philosopher Kant, whilst forming a hierarchy of science with theoretical science at the top, preferred experience to theorising. Baker cites Kant as saying, "High towers, and the metaphysically-great men resembling them, round both of which there is commonly much wind, are not for me. My place is in the fruitful bathos, the bottom-land of experience," (p. 23; 1993).

Kennedy (1980) also seems to support the Simpsonian view of the distinction between physical science and historical science, but with some important qualifications. In a critique of the systems approach to geomorphology, Kennedy points out that time is an essential component of geomorphological entities; "... our subject matter almost inevitably has a history and that history will frequently prove very important indeed in determining future developments and non-developments," (p. 551; 1980). After providing an explicit outline of Simpson's views, and in particular focusing on the fact that geology is more concerned with the configurational, than the immanent, Kennedy writes that;

"What has been very clear for a number of years now is that the configurational elements which must be taken into account in attempts to explain the past, present and future behaviour of the world in a geographical sense, are extremely difficult to handle in an explanatory framework that derives too narrowly from physics ... any solution, to be effective, must be quite distinct from that which will apply to any academic area where concern is directed largely or solely towards the immanent properties of nature."

Kennedy (p. 552 and 553; 1980)

Kennedy does not explicitly state what this historical approach that is distinct from physical science involves. The following attempt to identify the central elements of the approach, relies on Kennedy's wider literature and the definitions and assumptions of other authors. Firstly, the historical approach is thought to be scientific, even though it is different to physical science. Kennedy (p. 555 - 556 and 558; 1980) criticises what she sees as the rejection of the scientific method in the works of Chapman (1977) and Bennett and Chorley (1978) and advocates the use of Chamberlin's multiple working hypotheses, which Kennedy implies is a good method for scientific enquiry.

Secondly, Kennedy views historical science as predominantly qualitative and descriptive and the primary activity of geomorphology:

"... there ... seems to remain room for two groups upon the environmental scene or landscape. The technologists or systems analysts may be one, whilst the academics or geographers form the other. By all means let the mathematical modelling of the naughty world continue apace, but let us not confuse those models with reality. Nor let us underestimate, as these views of system analysis [Chapman (1977) and Bennett and Chorley (1980)] would have us do, both the enormous extent and the general reliability of that prior and usually qualitative information about the expected behaviour of the earth's landscape which most of us possess."

Kennedy (p. 558; 1980)

"Given that the description and comparison of geomorphic phenomena is not only what most geomorphologists are actually doing, but what they enjoy doing and what no one else really wishes to do, why worry? It is quite possible that ... laws are not the sine qua non of any satisfying and satisfactory scientific enquiry into matters geomorphological."

Kennedy (p. 155; 1977)

Kennedy views historical science as the predominant contribution and role of geomorphology. However, if we transplant the views of Wilson (1972) and Thorn (1988) that process laws necessarily make use of small scales into the reasoning of Kennedy (1977), she accepts that the work of 'technologists or systems analysts' (which correspond to the formation of process laws) has value and may allow historical science to progress beyond a descriptive stage and form analytical theories. Kennedy (1977) suggests that geomorphology's distinctive theories may emerge from rules linking historical and timeless events and their associated spatial scales:

"...it is possible to argue that, to date, geomorphologists have considered that the difficulties associated with widely varying scales of enquiry constitute a strait - jacket for the subject: in reality, however, these problems may point to the fundamental skeleton of the discipline. If we understood the nature of that skeleton more clearly then we might also understand the rules linking events and forms on different temporal and spatial scales; in other words, we might have established distinctive geomorphological theories."

Kennedy (p. 156; 1977)

To summarise, Kennedy views geomorphology as a primarily historical science, which is different from physical science. However, a possible unique niche for geomorphology may be linkages between small scale, timeless laws and large scale historical events.

2. 4. 3.- A Non-Scientific Approach to Geomorphology

In order to present a more balanced overview of the philosophy of geomorphology, the views of those who hold that geomorphology is not scientific, need to be discussed. Many workers of geology/geography/geomorphology that adopt the non-scientific position, seem to take this stance because they think that law formation is the hallmark of science, and that the natural environment is just too complex to be amenable to a treatment by laws.

Guelke (1971) gave a predominantly critical account of geography as law - applying or law-seeking science, on the principle that diverse and unique boundary conditions reduce universal laws to general statements. Accepting the deductive nomological account of science or the covering law model, Guelke (p. 44; 1971) argued that prediction and explanation (postdiction in the sense of Simpson) were equivalent, but that for either to be possible, the boundary conditions must be specified; "In physical geography, the major problem associated with providing accurate explanations and predictions is not the unavailability of appropriate laws but the difficulties encountered in obtaining data on the relevant determining conditions..."

Guelke (p. 45; 1971) suggested that geographical hypotheses needed to be tested by the Popperian criterion of falsification, but because of the difficulty that boundary conditions present, geographers often protect their models or laws by saying that the discrepancies are "... 'explained' by demonstrating that the test situation was not applicable to it." This procedure of protecting models/theories/ laws by the inadequacy of the environmental test is seen by Guelke to be tantamount to inventing *ad hoc* hypotheses to justify laws in the presence of contrary empirical data and he cites W. M. Davis and the Cycle of Erosion as an example of this practice (Guelke, pp. 47 - 48; 1971).[20]

[20] Problems with the complete specification of a system are thought to be a cause of indeterminate morphology. Scatter on regression charts is explained as the influence of unspecified variables (**Section 5. 1.**) and the concept of equifinality is a theoretical scheme that suggests differences in boundary conditions/relevant controls are responsible for different morphology (**Section 5. 2. 3.**).

The thrust of Guelke's argument is that "The much vaunted theoretical approach is revealed to be, on analysis, a pale shadow of the scientific procedure employed in the physical sciences," (Guelke, p. 50; 1971). This pale shadow results from the perceived dilution of laws as they apply over diverse and complex natural environments, resulting in vague generalisations. Guelke (p. 51; 1971) advocates that geography should "... re-establish a vigourous regional interest in the discipline in order to keep a firm hold on reality and as a necessary counterbalance to uncertain theoretical ventures."

The Popperian critical rationalist philosophy of science adopted by Haines-Young and Petch (1980: 1983: 1984: 1986), which is the same philosophy used by Guelke (1971) leads them to a similar conclusion. Haines-Young and Petch (see above) do believe that geomorphology is equivalent to physical science, but only so far as it meets the strict criterion of falsification.

"Conjectures about the immanent properties of nature must be testable if they are to be regarded as part of science. Similarly, propositions about the configurational changes which initiated the particular geomorphological processes must be potentially falsifiable if the explanation is to be scientifically acceptable ... Thus if human impact is proposed as the configurational change responsible for arroyo formation, then other kinds of change (in the climatic record, say) are forbidden."

Haines-Young and Petch (pp. 368 - 369; 1984)

The above example on arroyo formation, shows that in order for a conjecture to be scientific (as defined by Haines-Young and Petch), it must be set in experimental conditions where the relationship between cause and effect is exclusive and directly observable. Haines-Young and Petch (p. 11; 1986) give an example of an historical problem that does not conform to science.

Why did the River Thames form Terraces?

1. **Glaciation occurred because of reduced net global radiation**
2. **Sea level was lowered in a step - wise fashion as a result of glaciation**
3. **The River Thames formed terraces because of a reduced base level for erosion**

Haines-Young and Petch argued that the effects did not necessarily imply the cause; "If the conclusion does not follow logically from the premises, then it is difficult to see what claim the conclusion has for being a coherent explanation ... ... the explanation consists of a description of events and it does not necessarily follow that terraces will always be formed in the way suggested," (Haines-Young and Petch, p. 12; 1986). The difficulty with this conception of science and geomorphology is that based on a certain level of information, causes can have various effects and effects can have various causes, due to complex interactions of causes/boundary conditions that invalidate an exclusive logical connection.

This is the problem that Simpson (1963) alluded to, but whereas he assumed investigations could be carried out without laws and still retain the term scientific, Haines-Young and Petch imply that the absence of standard scientific procedure means that a study is not scientific. This has the consequence that many complex investigations in geomorphology, which may not fall under the rubric of the deductive syllogism and falsification are, according to Haines-Young and Petch, non-scientific.

Summary

The brief review of the philosophy of geomorphology has shown that a conception of geomorphology as a science is common among the discipline's practitioners - Rhoads and Thorne (p. 288; 1993) have stated that the scientific view is the majority view (see p. 10 this thesis). However a more detailed analysis of the literature suggests there are principally two different conceptions of geomorphology; the first suggests that geomorphology is equivalent to physical science and proceeds in the same way as physical science, by the use of laws and theories, to understand phenomena - this is the Kittonian view. The second conception of geomorphology is that it is logically different to physical science and that it does not make use of laws and theories to understand phenomena - this is the Simpsonian view.

The author agrees with the Kittonian view, that geomorphology and physical science are equivalent. It has been stated above by Strahler and others, that science is unified and this is the view taken here. Science is unified because science has some form of definition, even if this definition is hard to state; any discipline that does not correspond to a given definition of science is not a different form of science, it is not science at all. The definition of science used is open to question, but the author's own definition is that scientific explanation is explanation by law. The Simpsonian school of thought appears to attempt geomorphic explanation without the use of laws, and therefore makes geomorphology not historical science, but a non-scientific discipline.

Gilbert and Davis both appear to have had the view that geomorphology is equivalent to physical science and that it makes use of laws, even though Gilbert was concerned with timeless laws, and Davis was concerned with temporal laws.[21] With process studies, it has been shown that temporal laws, such as that used by Davis, are probably not feasible because of the complexity and changeability of process action. This means that process studies have to be incorporated into a temporal framework (e.g. see Schumm and Lichty: 1965: Kennedy; 1977: Brunsden and Thornes; 1979: Thornes: 1983) in a manner similar to the way Gilbert envisaged - that laws and their boundary conditions can be judged by the consequences they make and these consequences can be identified either in the past or the future, providing that a level of information that is detailed enough is given (the detective analogy).

The Simpsonian school may have developed for two reasons; firstly, whereas the complexity of the environment and the inability to form laws has led some to deny the scientific nature of geomorphology (i.e. Guelke; 1971), the Simpsonian school has attempted to take a middle road and retain the term science but without having to use laws. Secondly, perhaps a reason for the development of the Simpsonian school has been the strict concepts of science adopted by some geomorphologists, and rather than adopt the stance of being a subjective discipline, some workers have attempted to form a compromise; not conforming to the rigidity of imposed scientific concepts, but also not forming an arbitrary, individual field of study.

The Simpsonian school appears to have some difficulties; it concentrates on unique sequences of events, but the framework for decision making in the Simpsonian conception seems rather vague; there is no rigourous conception of what constitutes a sensible hypothesis, no philosophy as to how knowledge of what the important variables are is arrived at, no precise rules about the relationships between variables and no detailed method is described for testing hypotheses. These concepts of decision making are difficult within the framework of the philosophy of physical science, where the rules and objectives are generally agreed upon. The Simpsonian school lacks this framework and thus concepts of decision making are much more problematic.

[21] See **p. 14 this thesis** where Davis argues for constant rational forms that are linked by an invariant temporal progression from one to the next, similar to the systematic developments involved in the theory of biological evolution (see Casti; 1991: Strahler; 1999). As **Chapter Three** discusses, it is the invariant relationship that constitutes a law and scientific understanding.

This chapter has examined two distinct conceptions of what form of knowledge characterises geomorphology. The acceptance of the Kittonian view of geomorphology is important for assessing current theoretical approaches to indeterminacy in fluvial channels and for proposing new solutions to the indeterminacy problem. Setting geomorphology in the framework of physical science (as the Kittonian view does) suggests that if physical science can be defined by several key components and rules, then this sets the rubric for investigation in geomorphology and the standards by which those investigations can be judged.

The next chapter addresses the second conclusion that is responsible for the formation of the three test hypotheses in Chapter One. This second conclusion is that suggested in the previous paragraph; that science proceeds according to a set of defined components and rules. A discussion of scientific method answers why laws are important to science and therefore why the Kittonian viewpoint is adopted. Furthermore, the diversity of laws available for explanation in science, and the considerable complexity of the philosophy of science (see Rhoads and Thorn; 1994) may respectively provide some answers to the two problems that founded the Simpsonian School.

Chapter 3

The Nature of Science

Outline

This Chapter explores the second tentative conclusion used to form the overarching question and the resulting three hypotheses in Chapter One - "science is defined by, and works by, several key components and a set methodology or rules." Based on the conclusion in Chapter Two that geomorphology is a branch of physical science, the aim of this chapter is to examine what characterises scientific knowledge and therefore what should characterise geomorphology's study of indeterminacy in alluvial channels.

The chapter begins by giving an introduction to the scientific method and the difficulties that are faced in selecting and defining its principal components (3. 1.). From a review of the literature, the principal components of science are outlined (3. 2) and the remainder of the chapter is given over to the debate on these components. Hypotheses are defined as the starting point of science. It is suggested that hypotheses must be phrased in such a way as to make the contribution of a law possible (3. 3.). A detailed discussion on the nature of laws follows. Because explanation in science is usually held to be explanation by cause and effect, the discussion is focused around answering two questions; 1) Are causal laws the fundamental way in which nature works? 2) Are causal laws fundamental to human cognition? (3. 4.). A review of causal laws and causal thought is given, including epistemic probability (3. 4. 1.). A review of statistical laws and acausal thought is given (3. 4. 2.). An attempt is made to answer the two questions from the reviews of scientific thought presented (3. 4. 3.). Theory is defined as consisting of complexes of laws and the understanding that they generate. Theory is also recognised as the best explanation of the phenomena it purports to describe (3. 5.). It is argued that the essential nature of scientific knowledge is its predictive ability. A table is presented of scientists' and philosophers' views on prediction. It is also contended that most philosophies of science involve prediction, and that the difficulties involved in predictive relationships are the primary reason why different philosophies of science exist (3. 6.). The chapter closes with a conception of science that explanation in science is by law and that causal, statistical and chaos laws all apply to nature, but laws by definition are causal/deterministic in cognition *at some scale*. Prediction is given a central place in science, but no explicit philosophy of prediction is adopted. The fact that geomorphologists view geomorphology as science (Chapter Two) and that science is explanation by laws, accounts for the overarching question and the resulting hypotheses for indeterminacy given in Chapter One (summary).

3. 1. Introducing the Scientific Method

"We are bound to run into trouble if we seek rational justifications of every principle we use, for we cannot provide a rational argument for rational argument itself without assuming what we are arguing for."

Chalmers (p. 53; 1999)

"Philosophy of science does not exist and function on a level above and independent of the substantive content of scientific beliefs; it is integrally and inseparably linked to that content , and its methods and conclusions must rest on the results of the very science with which it is concerned."

Shapere (p. 24; 1987)

"...the vast majority of working scientists remain blissfully unaware of the many perceptive writings by philosophers of science on scientific method and the scientific enterprise. Nevertheless, they continue to produce excellent science..."

Jeeves and Berry (p. 51; 1998)

The procedure of science, sometimes termed the 'scientific method' has been described as an extremely successful mode of thought (Pitt; 1988: Laudan; 1984). Other writers have resorted to more elaborate phrases to convey the effect science has had, describing it as '...the harvest of human endeavour...' (Strahler, p. 9; 1999) or "... among the greatest of civilisation's intellectual achievements" (Shamos, p. 46; 1995).

Having placed geomorphology within the sphere of this special form of thought in the previous chapter, this chapter seeks to highlight the essential qualities of science. However, perhaps as a reflection of the profundity ascribed to it in the quotes above, what defines science is open to much debate and controversy. Philosophers contend over what elements of science are essential; are hypotheses and imagination the defining features or are laws and theories the distinguishing mark of scientific knowledge? Furthermore, philosophers disagree over the definition of each of the contributing elements of science; are hypotheses and theories one and the same or are they different?

In tackling the issue of 'what is science?', these uncertainties must be addressed. This chapter firstly attempts to present what the important components of science are. It then progresses by defining these components in the order in which they are perceived to occur in scientific procedure; language and its meaning are interpreted in different ways in the philosophy of science and every term or component of science (such as hypothesis, law, theory etc.) requires strict definition if a conception of science is to be formed (see Cole and King, p. 522; 1968 - they present four alternative definitions of theory and six different uses of the word law). Wilson (p. 31; 1972) describes the problem and the remedy in the following terms;

"Unfortunately, it is difficult to establish universal conventions on the use of certain words such as system, theory, model and so on. It is important, therefore, that I say how I am going to use such words. I am not saying that this is the only way in which they can be used - there can be no 'correct' way in the circumstances - but if definitions are clearly stated, then we can try to ensure that we are talking about the same underlying concepts."

Wilson (p. 31; 1972)

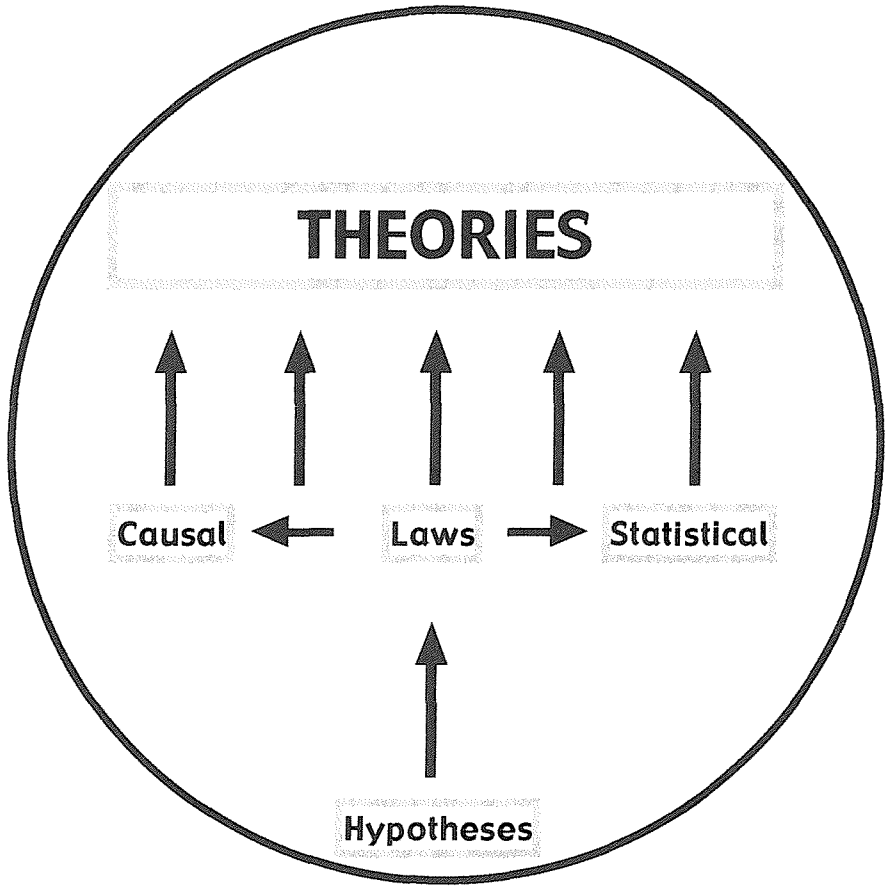
In addition to the lack of a rigid terminology, matters are complicated further due to the holistic nature of the philosophy of science; to define any single term depends on the definition given to other terms elsewhere in science. Thus in defining a single term, one must hold all other terms and their meanings in mind and indeed must have resolved, to his own satisfaction, the whole tone and scope of the scientific enterprise. This feature of science is evident in this section. In defining the individual components of science, the author already takes for granted what subsequent terms will mean.

The heading quotes for this part of the thesis, set an important caveat on the discussion. Chalmers (1999) writes that there comes a point when it is not possible to rationalise the rational; there is a point where rationality stops and intuition takes over. Shapere (1987) and Jeeves and Berry (1998) reinforce this point by stating respectively that the philosophy of science is not independent of the practice of science and that many scientists can perform good science by intuition without a rigourous epistemology of science. Whilst this section attempts to form a conception of science that can be used to assess fluvial theory, it is worth remembering that there is a part of science that defies definition and is beyond the reach of analysis.

3. 2. The Components of Science

The physicist Max Born wrote that, "... there is no philosophical highroad in science with epistemological signposts. No, we are in a jungle and find our way by trial and error, building our road behind us as we proceed. We do not find signposts at crossroads, but our own scouts erect them, to help the rest," (Born, p. 44; 1934). The remainder of this chapter is devoted to discussing some of the signposts left by scouts and the debate that surrounds them. Figure 3. 2. 1. displays those signposts or components of the scientific method, which seem to occur persistently in the literature.

Figure 3. 2. 1. - The Components of Science



○ = Prediction

3. 3. Hypotheses

" Il lume naturale (the light of nature) "

Galileo Galilei

The term hypothesis, is in **Figure 3. 2. 1.**, placed at the beginning of scientific enterprise and knowledge. Whilst the literature seems to agree that hypotheses are essential to scientific thought, there is some disagreement as to what the term hypothesis should mean. This dialectical problem is not new, but has been present since the inception of modern science. In a study on the works of Newton, Koyré (p. 32; 1965) writes "...hypothesis is never a term with univocal meaning ... it covers a whole gamut of meanings which ... slide very easily into one another." The modern problem of the phrase 'hypothesis', is primarily concerned with the tension between two definitions; in the first, an hypothesis is a proven and established form of knowledge, in the second it is a speculative conjecture that requires assessment.[22] This thesis adopts the second definition, partly because the first definition is reserved for theory[23], but also because the use of the first definition for the phrase hypothesis, provides no obvious starting point for science.

The definition of hypotheses as speculative conjectures leads to two further points that need clarification; how do these speculative conjectures come about and what form do they take? The answer to the first question is directly related to the caveat that was set on this debate at the start; "We are bound to run into trouble if we seek rational justifications of every principle we use, for we cannot provide a rational argument for rational argument itself without assuming what we are arguing for." Chalmers (p. 53; 1999). Several philosophers and scientists have argued that the initial insights that lead to developments in science cannot be reasoned. Galileo spoke of *il lume naturale* or the light of nature, which has been interpreted by Baker (p. 57; 1996) as the "... instinctive propensity in science to guess right ... because the human mind is instinctively attuned to certain aspects of nature."

[22] Supporting evidence for the existence of this debate is presented in **Table 3. 5. 1.** in the section on theory which forms a symmetrical discussion to the present one.

[23] This is a result of the holistic nature of science mentioned in **Section 3. 1.** The definition of one term means that it has a reciprocal effect on other terms, and in the writing of this section it is necessary to preempt some definitions of terms in order to define the current term.

The outcome of this view is that there does not exist a set procedure or methodology which results in the output of speculative conjectures, but rather the generation of hypotheses depends on the individual's mind and its ability to perceive aspects of nature. This stance finds support from various philosophers. Kuhn (p. 119; 1962) writing on the works of Galileo, posited that the shift in world view which Galileo advanced, came about "Through Galileo's individual genius." Other writers have exalted the place of "inventful ingenuity ... insightful guessing" (Hempel, p. 17; 1966) and "imagination" (Shamos, p. 57; 1995) in science and Bunge (p. 264; 1979) states that "...it would be foolishness to deny that some 'insight' is needed in order to disentangle orderliness, to solve problems and to invent fact-fitting hypotheses..." Johnson (p. 468; 1933) summarises: "The precise nature of the inventive process is a debatable question ... no one has ever given an explanation of how the hypotheses arise in the mind."

Whilst ascribing a rational basis for scientific insight seems difficult, the form in which speculative conjectures must be stated, is subject to reason and methodology. Whichever definition of an hypothesis is taken (be it a proven and established form of knowledge, or a speculative conjecture that requires assessment) some key criteria are common in the literature[24]. Thorn (p. 22; 1988) provides a good summary of what constitutes an hypothesis:

"Implicit in the term 'hypothesis' are the notions ... postulation of a substantive question; potential for making a law like contribution; falsifiable (i.e. measurable). Phrased rather more colloquially, everything depends on the quality of the question. A good question ... creates the possibility of a precise and meaningful answer."

Thorn (p. 22; 1988)

An hypothesis should generate a form of information whose content can be critically assessed or tested (Chalmers, p. 62; 1999: Woolhouse, pp. 40 and 135; 1988: Dorn, p. 206; 1996) - "... an hypothesis is ... an assumption or conjecture that one examines in its consequences and implications, which should either 'verify' or 'falsify' it," (Koyré, p. 32; 1965).

For a moment, let us set aside the complexities of the fluvial environment and imagine that sediment entrainment of a river bank can be simply related to a single variable. The following example adapted from Chalmers (1999) then illustrates the type of statement that conforms to the pattern of an hypothesis:

1. **At a given velocity (m/s) bank sediment entrainment always occurs.**

2. **At a given velocity (m/s) bank sediment entrainment either occurs or doesn't occur.**

Adapted from Chalmers (p. 62; 1999)

[24] That the same criteria apply to both hypotheses and theory may be taken either as an indication that they are one and the same thing or alternatively, that some fundamental properties remain constant in the whole of scientific endeavour. The latter inference is the one drawn by this thesis.

The first statement in the above example counts as an hypothesis. In the statement 'at a given velocity (m/s) bank sediment entrainment always occurs,' two inseparable features are present that produce reliable knowledge. The first is that the statement generates a level of information deeper than the observations that prompt the hypothesis being made (it is substantive in the words of Thorn). It states that there is a definite connection between velocity and entrainment and it is really this connection that is of interest, rather than the isolated observations of velocity and entrainment. Secondly, because the statement is substantive, it leads to the possibility that it can be tested and its veracity or falsity be established (falsifiable in the words of Thorn) - it is possible to stand in a river, measure a velocity, and see if entrainment occurs. This is because it is the connection and not the events themselves that come under scrutiny. The knowledge is in the connection, not the observables. The second statement 'at a given velocity (m/s), bank sediment entrainment either occurs or doesn't occur', gives no substantive information; it is a description of observable events and thus cannot be proved or falsified because it makes no claim; there is no connection or level of information deeper than the events that can be tested.[25]

Hypotheses must have a form where they generate connections between observables, or relationships between entities, that are more fundamental than the events themselves. Relationships between entities that have survived critical analysis are termed 'laws', and are the defining contribution of hypotheses. It is the potential for making a law-like contribution (in the words of Thorn), that leads to hypotheses producing substantive and testable knowledge. The flash of intuition that leads to an hypothesis being created, seems to be constrained to producing a rational form of statement called 'law' if knowledge is to be produced. The next section examines different categories of law that can be formed.

[25] The possibility that indeterminacy may force fluvial geomorphology to be a discipline that describes uniqueness was discussed on **p. 6 this thesis.**

3. 4. Laws - Causal and Statistical

In the presentation of science given thus far, an hypothesis is the starting point of science. Though the creative processes that lead to an hypothesis being formed are unclear, an hypothesis must be phrased in such a manner that it has the potential to uncover 'relationships between entities,' so that it produces substantive and testable knowledge. If it is taken that hypotheses are speculative conjectures, then it seems to be a guiding principle that they produce knowledge in the form of relationships between entities which, having survived testing, are termed laws. However, whilst it seems acceptable that '... scientific explanation is explanation by law...," (Casti; p. 40; 1991) there is considerable debate on the status and structure of laws. Two main forms of scientific law have been proposed; causal and statistical. A central question is which form of law, causal or statistical, gives a fundamental understanding of phenomena, and produces substantive and testable knowledge.[26] The contemporary, popular, and sometimes erudite view of western science, has been one in which laws that explain the causes of phenomena are discovered.[27] Casti (p. 36; 1991) describes causal laws as the 'common sense view of science' and many other authors have documented the pre-eminence given to causal relationships (Bunge; 1979: Popper, p. 47; 1989: Casti; 1991: Brennan; 1997).

Science attempts to explain nature and therefore a discussion of laws refers to how nature works. In addition, the previous discussion of hypotheses showed that humans are constrained to produce rational statements in the form of laws. A discussion about what form (causal or statistical) those laws can take is thus a discussion about human cognition (Goldstein and Goldstein; 1978). In light of the pre-eminence given to causal laws in the common view of science, and setting this in the context of nature and cognition mentioned above, this section aims to answer two questions:

1. **Are causal laws the fundamental way in which nature works?**
2. **Are causal laws fundamental to human cognition?**

An attempt is made to answer these questions by providing a definition of causal and statistical laws and presenting an historical review of causal and statistical concepts in human thought. We begin by reviewing the concept of causality.

[26] The implications of this debate for fluvial geomorphology have already been touched upon. Is Leopold and Langbein's (1963) suggestion that understanding might be derived from cumulative properties of channel behaviour, a poor substitute for determining the individual case? Does the inability to determine the individual case reflect a fundamental weakness in understanding? These issues are addressed in this detailed discussion of scientific laws.

[27] A view which the author held of science and geomorphology at the start of this thesis.

3. 4. 1. Causal Laws and Concepts

Causal relationships take the form "X caused Y just in case Y would not have occurred unless X had occurred" (Boyd, p. 775; 1983). Using the previous example of velocity and entrainment; given a certain velocity (x) there will be entrainment (y)[28]. The causal view of science can be thought of as removing observations that are irrelevant or coincidental for a phenomenon to occur and elucidating those observations that are intimately connected to the phenomena in question. A law can then be formed which allows the scientist to focus on the relevant 'relationship between entities' for a given phenomenon and ignore extraneous information.

Newton's law of gravitation ($F = Gm_1m_2/r^2$)[29] provides an example of a causal law removing extraneous information. If the task is to find why free objects eventually fall to the earth, modern investigators do not need to make measurements of the colour, shape, type or size of the object; it is now known that these variables can be ignored because objects fall to earth irrespective of these factors. In addition to these variables that may have been investigated prior to Newton, the law of gravitation also removed the need for modern scientists to consider metaphysical factors that could not be measured or quantified.

Pre-Newtonian science was influenced heavily by the writings of Aristotelian physics which divided the universe into corruptible (terrestrial) and incorruptible (celestial) realms. Within the corruptible sphere, the elements of earth, water and air arranged themselves in concentric circles, with earth at the centre. These positions were the natural resting place of the elements. An object consisting mainly of the element earth, if placed in air, would fall to the earth because that is its natural resting place,[30] (Clagett, p. 140; 1959: Russell, p. 216; 1961).

[28] This law, which is phenomenological, would properly be termed 'deterministic.' Causality implies some notion of why an unique association occurs. Deterministic laws do not answer why a relationship occurs, but in all other respects, are similar to a causal relationship.

[29] Where F is the force between objects of mass m_1m_2, separated by distance r, and G is the gravitational constant.

[30] Aristotle's scheme of natural places, led to several immeasurable properties being proposed to account for the acceleration of free falling bodies, before the law of gravity was proposed. The 'mail' theory proposed by Arabic scholars suggested that bodies outside of their natural place had 'violent mail' that resided in the object. This was a force that resisted the object's descent and slowed down the free fall of the object. As the object continued to descend, natural mail increased and the object increased in speed. The law of gravitation provides a single, constant and measurable force, which continually applied during free fall, produces acceleration.

The law of gravitation isolates a specific, measurable reason why objects fall to the earth. The planet is so massive, relative to objects, that it causes them to be attracted to its centre, or causes them to fall (see Lines. p. 129; 1994). The size of the earth's gravitational force is the only cause required to explain and predict the fact that an object will fall. Causality thus forms a fundamental relationship between entities, to the exclusion of other factors - a given entity (cause [in this case the gravitational pull of the earth's mass]) produces a given entity (effect [in this case the fall of an object]).[31]

Aristotle was perhaps the first to define the notion of cause (Bunge, p. 31; 1979), but his definition was different to that accepted by the modern era. This was briefly hinted at in Aristotle's approach to the free fall problem mentioned above. Aristotelian philosophy defined four types of cause that gave an exhaustive, all inclusive explanation for a given event. Using the example of what causes objects to fall to earth, as considered above, and including the modern principle of gravity, Aristotle's causes would have been similar to the following:

The material cause that makes the object fall is the matter of which the object is composed.

The formal cause that makes the object fall is the pre-patterned structure of the universe and natural place.

The efficient cause that makes the object fall is the gravitational pull of the earth.

The final cause that makes the object fall is the purpose that a divine architect has in mind[32]

[31] If it is the rate of free fall that is required, then the related factors of shape, type and size become important, because the rate of fall is determined by air resistance (friction). Factors such as windspeed and variations in the earth's gravitational field are also significant. Whilst causal relationships are exclusive and provide understanding in the natural world, it is often a combination of causes that leads to a given behaviour.

[32] See Russell (p. 181; 1961) and Clagett (p. 421; 1959) for other examples, which show how the quality (magnitude in quantitative science) of the four different causal categories can change according to the effect.

Aristotle's metaphysics, in modern parlance, attempted to account for religion and physics and all four causes were required to adequately explain the phenomena in question. However, the causal principle as expressed in modern physics recognises only efficient causes. The material cause is seen as a product of efficient cause; thus different materials behave in a different manner because of the efficient cause of atomic structure, just as different materials all fall to earth because of the efficient cause of gravity. The formal cause has been dismissed, because contrary to what Aristotle taught (his division of the universe into different spheres), science cannot know what the pattern of the universe is *a priori*. This applies to all laws; the pattern or form of an entity is only revealed in an emergent sense by the accumulation of efficient causes. Form is a function of efficient cause. Finally, the modern scientist does not accept the teleological final cause, because it is not subject to testing and is thus beyond the scope of science (See Bunge, p. 32; 1979).[33]

As the primary instigator of the causal conception of science, and at a time when there was little established method in natural philosophy, Aristotle's pioneering of the now populist or commonsense view of science can only be ascribed to his ability; Aristotle was a "Genius ... endowed with the full scientific mentality," (Whitehead; p. 57; 1925). However, after Aristotle, the reasons for the propagation of the causal view of science become more complex. Other theories and other factors have coloured subsequent geniuses and their views of nature.

From Aristotle to the Renaissance, many historians of science focus on the pervasive influence of Christianity and its '...impress on the European mind .." (Whitehead, p. 63; 1925). With its beliefs that "... God himself is the sole causal agent in the universe..." (Popkin and Stroll, p. 129; 1981) resulting in an ordered world where "...every detailed occurrence can be correlated with it antecedents in a perfectly definite manner, exemplifying general principles," (Whitehead p. 63; 1925), Christianity provided a strong motivation for a causal view of nature[34].

Historians argue that naturalists in the middle ages worked within an 'instinctive tone of thought' established by the church and by the accepted causal position of Aristotle, which because it was (at least in part) in accordance with church doctrine, was readily incorporated as the natural philosophy of the Papacy (Russell, p. 206;1961: Jeeves and Berry, p. 22; 1998: Prigogine, p. 12; 1997)[35]

[33] **See p. 38 this thesis** for Thorn's definition of an hypothesis - it must be falsifiable or testable.

[34] "In Asia, the conceptions of God were of a being who was either too arbitrary or too impersonal ... to have much effect on instinctive habits of mind. Any definite occurrence might be due to the fiat of an irrational despot, or might issue from some impersonal, inscrutable origin of things. There was not the same confidence as in the intelligible rationality of a personal being," (Whitehead, p. 63; 1925).

[35] The relationship between Christianity and science is convoluted. Russell (1985) has documented the relationship to science of various different denominations and movements within Christendom.

The enlightenment of the Seventeenth Century can be viewed as beginning with this heritage of Christian thought. Francis Bacon and Thomas Hobbes, concerned with systematic methods of gaining knowledge, placed causal relationships at the centre of their work on science; "... knowledge of causes ... Bacon says, is the aim of the investigation and interpretation of nature...," (Woolhouse, p. 19; 1988) and Hobbes comments on his own philosophy as "... such knowledge of effects or appearances, as we acquire by true ratiocination from the knowledge we first have of their causes or generations," (Ibid, p. 31; 1988). Both these men wrote from the position of a philosophy in tune with the Christian ethos of the day.

A transition in the basis for causality, from religious to rational grounds, began with Sir Isaac Newton. Like Hobbes and Bacon, Newton's science emerged from a religious viewpoint - one that differed from Christian theology, but nonetheless maintained the Judaic God as supreme creator (Strathern, p. 71; 1997: Gjertsen, pp. 20 and 233; 1986). Gjertsen (p. 453; 1986) in a quantitative analysis of a collection of Newton's works, notes that the majority of Newton's papers were based on theology and the chronology of civilisation and also cites Newton's ideas of God derived from nature; "Does it not appear from Phaenomena that there is a Being incorporeal, living, intelligent, omnipresent .." and "We know God only through his works, 'by his most wise and excellent contrivance of things.'" (Gjertsen, p. 301 and 462; 1986)

The approach that Newton took to science in relation to religion, has been suggested by Koyré (p. 38; 1965); "... for Newton the existence of God was a certainty, and a certainty by which the phenomena, all the phenomena, had ultimately to be explained." Brennan (p. 35; 1997) concurs with and elaborates on this view, writing that "Newton constructed his cosmological system on the assumption of God's existence ... God had established the physical world and its laws, and determining what these laws were was both a religious and scientific enterprise. Indeed, Newton saw science as a form of worship." Newton qualified his most important scientific work, the 'Principia' by four rules of reasoning in philosophy (Chandrasekhar, pp. 346 - 347; 1995):

Rule I

"We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances."

Rule II

"Therefore to the same natural effects, we must, as far as possible, assign the same causes."

Rule III

"The qualities of bodies, which admit neither intensification nor remission of degrees, and which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever."

Rule IV

"In experimental philosophy we are to look upon propositions inferred by general induction from phenomena as accurately or very nearly true, notwithstanding any contrary hypotheses that may be imagined, till such time as other phenomena occur, by which they may be made more accurate, or liable to exceptions."

These four rules of reasoning provided Newton's framework for research; that simple explanations are to be preferred to complex ones (a version of Occam's razor - see Russell, p. 462; 1961); that effects are uniquely related to cause; that if the same effect is produced in many different entities, that property may be termed universal and finally the method of induction can be deemed to yield correct results and can only be challenged by new phenomena. These rules of reasoning which pertain to the ordered nature of the universe, may be seen as deriving from Newton's concept of God and His omnipotence over all things. Whatever the precise motivation for the rules of reasoning, their influence on Newton's mode of thought produced the three laws of motion and the inverse square law of gravitation, as set forth in the 'Principia:'[36] [37]

Law I

"Every body continues in its state of rest, or of uniform motion in a right line, unless it is compelled to change that state by forces impressed upon it."

Law II

"The change of motion is proportional to the motive force impressed; and is made in the direction of the right line in which that force is impressed."

Law III

"To every action there is always an opposed and equal reaction; or, the mutual actions of two bodies upon each other are always equal, and directed to contrary parts."

Inverse Square Law of Gravity

"In two spheres gravitating towards each other, if the matter in places on all sides round about and equidistant from the centres is similar, the weight of either sphere towards the other will be inversely as the square of the distance between their centres."

Chandrasekhar (pp. 22 - 23 and 371; 1995)

[36] It is well documented that Newton did not discover all that the Principia contained, but built substantially on the work of Galileo, Kepler and others (e.g. Koyré, p. 11; 1965). However, Newton was the first to systematise, universalise and formalise the concept of inertia and gravity.

[37] Gjertsen (p. 299; 1986) asserts that the concept of 'laws of nature' emerged in the mid seventeenth century, notably with Descartes and Newton.

Newton's laws of motion and the inverse square law of gravity explained the universe (Koyré, p. 18; 1965) from the celestial, to the mundane phenomena of the terrestrial. Newtonian dynamics proposed that the behaviour (effect) of every entity is an unique product of the force (cause) acting or having acted upon it (rule II, law I) and that changes in the magnitude of behaviour (effect) are proportional to changes in the magnitude of force (cause) (Law II) - furthermore, any effect should be possible to explain by combinations of causes acting upon it. Newton's laws explained then contemporary problems and predicted the existence of new phenomena, and were accepted at the time as the method and philosophy by which to understand the universe:

"The system worked and proved its worth ... Thus fifty years after the publication, in 1687, of the Philosophiae naturalis Principia Mathematica - the leading physicists and mathematicians of Europe - diligently began the work of perfecting the structure of the Newtonian world ... leading it from success to success."

Koyré (p. 17; 1965)

"... Newton's theory was true. It had been tested by the most precise measurements, and it had always been right. It had led to the prediction of minute deviations from Kepler's laws, and to new discoveries .."

Popper (p. 93; 1989)

Newton's causal scheme proved so successful in explaining phenomena, that it had an enduring influence:

"The overwhelming success of Newtonian physics made it practically inevitable that its particular features became thought of as essential for the building of science ... all of us, or if not all still most of us, have been ... bred in the Newtonian, or at least semi-Newtonian world, and we have all, or nearly all, accepted the idea of the Newtonian world machine as the expression of the true picture of the universe and the embodiment of scientific truth - this is because for more than two hundred years such has been the common creed, the communis opinio, of modern science and enlightened mankind."

Koyré (pp. 19 and 4; 1965)

"Newtonian mechanics became the fundamental base of the structure upon which all the layers of physical sciences and technology are built ..."

Brennan (p. 34; 1997)

It was considered at the beginning of the discussion about Newton, that his work formed a transition between a religious and a rational basis for causality. Two very prominent thinkers, writing shortly after the death of Newton, are taken to show how the rationalistic view of causality was extended. Kant, the German philosopher, "...is usually regarded as the most important figure in the history of modern Western philosophy," (Kitcher, p. xxv; 1996). An admirer of Newton's Principia (Gower, p. 136; 1997), Kant included Newton's law of gravitation in his philosophy (Kant, p. 244; 1781) and even thought that the dynamics Newton discovered were part of the inbuilt cognitive equipment by which humans interpret the world around them (Popper, p. 48; 1989). Kant wrote that causal relationships were a necessary way for humans to understand and he assembled the whole of scientific endeavour on the basis of causality being an 'a priori' relationship that directs research (Heisenberg, p. 133; 1958: Brown, p. 14; 1996). In Kant's 'Critique of Pure Reason', the view of causal laws as fundamental is fully recognised:

"Therefore experience itself - i.e., empirical cognition of appearances - is possible only inasmuch as we subject the succession of appearances, and hence all change, to the law of causality. Hence appearances themselves, taken as objects of experience, are possible only in accordance with this law."

Kant (p. 261; 1781)

"Thus for Kant the task of the scientist is to find the causes of events and a scientific problem is solved only when a cause for the event ... in question has been found. The synthetic a priori principle that every event has a cause guarantees that the search for causes must always be successful and thus that no event can count as a counter instance to the causal principle no matter how long scientists have failed to find the specific cause. If scientists are unable to find the cause of an event, it is always the scientists and not the principle of causality that is at fault."

Brown (p. 102; 1977)

An important admonition to Kantian philosophy is that it does not refer to the reality of causal relationships in the natural world, only that causality is needed to understand nature; "... experience reaches phenomena, which flow over the immutable kernel of things - in - themselves, to which causality does not apply," (Bunge, p. 209; 1979).

Kant's basis for causality was a departure from religious directives and the explicit inclusion of Newtonian laws is suggestive of the motivation behind his philosophy.[38] Kitcher (p. Iviii; 1996) has noted that it was the scientific revolution that made studies of the extent and capabilities of human rationality a prime concern, and that reciprocally scientific knowledge (causality) was viewed as the necessary result of cognitive processes, rather than a subscription to articles of faith. This humanistic view that emerged from Kant's work is supported by Bunge who writes (p. 283; 1979) that in "... the young Kant [was] the progressive aim of discrediting supernaturalism and unscientific speculation about matters that were successfully being handled by science."

Kant's philosophy of causation was a prelude to the most extreme, and probably, the most cited exposition of the causal view. Pierre Simon Laplace, proposed the "Principle of Predictability in Principle," (Swartz, p. 32; 1985):

"Given for one instant an intelligence which could comprehend all the forces by which nature is animated and the respective situation of the beings that compose it - an intelligence sufficiently vast to submit these data to analysis - it would embrace in the same formula the movements of the greatest bodies of the universe and those of the lightest atom; for it, nothing would be uncertain and the future, as the past, would be present to its eyes."

Laplace (p. 4; 1814)

Laplace thought that the fundamental relationships that governed the world were causal in the presence or absence of human observers. In other words causal relationships were real, not just a cognitive organising process as Kant had advocated. Laplace writes, "... in the final analysis there is a regularity underlying the very things that seem to us purely to pertain to chance, and [there are] hidden but constant causes on which that regularity depends," (Gillispie, p. 225; 1997). Laplace (p. 3; 1814) also states "Present events are connected with preceding ones by a tie based on the evident principle that a thing cannot occur without a cause which produces it." For Laplace, knowledge was an awareness of causal relationships between entities (laws).

Historians of science document Laplace as continuing and refining the mechanistic, self- governing view of nature that Newton's laws had implied (Jeeves and Berry, p. 24; 1998). Laplace is described as "... the original of the Newtonian idolater" and that "Nearly all of Laplace's work in physics ... was characterised by an interest in what he saw as the outstanding problems of the Newtonian tradition," (Gillispie, p. 67 and 203 respectively; 1997). Russell (p. 523; 1961) cites Laplace as, effectively, the divide between the religious and Newtonian basis of science and of the search for cause that both inspired.

[38] Though having some conception of a divine architect, Kant did not subscribe to the Christian deity or formal religion, and the basis for his causation lay in science. (Jeeves and Berry, p. 95; 1998: Want and Klimowski, pp. 148 - 153; 1996).

There is considerable evidence that Laplace's concern with cause was, like Kant before him, founded on the success of Newtonian science. The religious impress (that historians suggest promoted the search for causal laws), was to Laplace, a measure of infancy and corresponding benightedness of the human condition, and had been replaced by a rational basis of causality that had emerged from the success of deterministic laws:

"Formerly ... a comet having in train a very long tail, the eclipses, the aurora borealis, and in general all the unusual phenomena were regarded as so many signs of celestial wrath. Heaven was invoked in order to avert their baneful influence ... Thus the long tail of the comet of 1456 spread terror through Europe ... This [comet] after four revolutions has excited amongst us a very different interest. The knowledge of the laws of the system of the world acquired in the interval had dissipated the fears begotten by the ignorance of the true relationship of man to the universe .."

Laplace (p. 5; 1814)

Laplace was not reticent in expressing this view that scientific endeavour *per se* had replaced the initiating religious motivation for causal laws. Popkin and Stroll (p. 197; 1986) cite an incident concerning Laplace; "... when he was explaining to Napoleon his theory of how the astronomical universe came into existence, [he] was asked where God fitted into his scheme. He replied 'I have no need of such an hypothesis.'"

It was mentioned that Newton formed a transition from the religious to a rational motivation for cause. Newton did not inspire a complete break from religious directives, because of his own religious fervour. The influence of Newton was particularly felt amongst English academics in the immediate generations following his death, and whilst European scholars such as Kant and Laplace began to disregard the impress of religion, their English counterparts were still concerned with, in Aristotle's terminology, final causes (Hacking, p. 171; 1975: Koyré, p. 21; 1965). Pearson writes:

"Newton's idea of an omnipresent deity, who maintains mean statistical values, formed the foundation of statistical development ... De Moivre expanded the Newtonian theology and directed statistics into the new channel down which it flowed for nearly a century. The causes which led de Moivre to his 'Approximatio' or Bayes to his theorem were more theological and sociological than purely mathematical, and until one recognises that the post-Newtonian English mathematicians were more influenced by Newton's theology than by his mathematics, the history of science in the 18th century ... must remain obscure,"

Pearson (p. 552 ;1926)

The above quote (Pearson; 1926) that establishes the mixed motives for scientific research post-Newton, also introduces a timely complication to this historical review of causality. The quote mentions the development of statistics, a branch of investigation that is assumed to be antithetical to the causal/deterministic approach. However, the introduction of statistics at this juncture is entirely appropriate, for whilst Laplace is regarded as the chief expositor of the causal principle,[39] he is also recognised as perhaps the most influential figure in the history of statistics and probability. Hacking (p. 1; 1975) writes that "...after Laplace, probability was so well understood that a page by page account of published work on the subject became almost impossible," and Gillispie (p. 238; 1997) states that Laplace's "... technical writings have come to have the same sort of relation to the later development of probability that, for example, Newton's Principia Mathematica had to the later science of mechanics."

How can Laplace be foundational to two opposite conceptual views of nature, yet speak with the absolute conviction that the world obeys causal laws? The answer is that probability has two facets; "...probability ... is Janus-Faced. On the one side it is statistical [aleatory], concerning itself with stochastic laws of chance processes. On the other hand it is epistemological, dedicated to assessing reasonable degrees of belief in propositions," (Hacking, p. 12; 1975).[40]

The first face of probability, concerned with laws of chance processes, is the side which is antithetical to the causal view; the alternative face, concerned with degrees of belief, is a natural outworking of the causal principle. The fundamental philosophy behind epistemological probability (statistical inference) is that there exists some definite causal/deterministic relationship between variables that are obscured by statistical variation or in modern terminology, noise (see Bunge, p. 316; 1967). Epistemic probability seeks to infer the true cause from the noise.

Elements of epistemological probability or statistical inference were present in embryonic form prior to Laplace. Hald (p. 33; 1998) notes that Galileo (1632) on discussing observations of a new star commented that the distance to the star was described by one number only - the true distance. However, observational errors and errors in the instruments meant that measurements varied symmetrically about the true value, with small errors being more frequent than large errors. Hald (p. 34; 1998) then goes on to state how the mean value of observations became an estimate of the true value, a theoretical foundation for this practice being provided by Simpson (1756).

[39] See **p. 48 this thesis.**

[40] The distinction between these two forms of probability is essential for understanding fluvial theory. Current understanding of fluvial morphology makes use of both epistemic and aleatory concepts. Hacking (p. 13, 1975) cites various authors who have alluded to the dual nature of probability (i.e. epistemic and aleatory), such as Poisson (1837), Cournot (1843), Carnap (1952), and Russell (1948).

Following the work of Fermat, Pascal and Huygens in establishing a probability calculus, the theoretical underpinnings of statistical inference were made by Leibniz and James Bernoulli. Hacking (p. 57; 1975) writes that Leibniz, "... made no serious formal contribution to probability theory [but] was the first philosopher of probability [and] was the first to insist that probability theory can serve in a branch of logic comparable to the theory of deduction." Hacking (p. 135; 1975) contends that Leibniz anticipates the inductive logic of Carnap (1950: 1952) in respect of there being a true relationship between variables that can be inferred from data and that the nature of this relationship is an objective fact.

James Bernoulli's 'Ars Conjectandi' (1713) provided essential concepts for statistical inference and the ability to identify true causal relationships from noisy data. In the 'Ars Conjectandi' James Bernoulli showed the first limit theorem in probability (Bernoulli, pp. 60 - 66; 1713). Imagine you had a random experimental set-up, and you wanted to calculate the probability of a given event which is not known *a priori*: if for example, you make only one test of the experiment and no more, it is unlikely that the true probability of that event will be established. However if you make a certain number of trials, and in particular a calculable minimum number of trials, then the probability of achieving the true probability of the event becomes almost certain. In other words the probability distribution of the probability of the event becomes fixed. James Bernoulli succeeded in providing a formula for calculating the number of trials required for the probability distribution of the probability of the event to become fixed. This result is called the weak law of large numbers.

The importance of the weak law of large numbers is twofold; firstly it suggests the minimum number of data for experimental tests. Secondly, because the probability of getting a probability becomes fixed, it is possible to know for any statistical test the confidence that can be placed in the result. This is the basis of hypotheses testing and statistical inference in modern statistics.

Abraham de Moivre published the Doctrine of Chances[41] (1718), upon which the '... mathematics of probability was recognised as an independent discipline in its own right,' (Hacking, p. 166; 1975). However it is not until the work of Laplace that mathematical statistics and probability flourish; "It is true that adepts of the subject were much given to celebrating its [probability] applicability, but prior to Laplace what they were praising was more prospect than actual accomplishment," (Gillispie, p. 13; 1997). Laplace consolidated previous work in statistical inference, developed new techniques and applications and provided a causal conceptual framework. Laplace added to knowledge on the calculation of the arithmetic mean and error theory, built on the work of Gauss and provided linear estimation models that can be regarded as forerunners of the Chi Squared statistic, devised the central limit theorem, provided a statement on inverse probability (reasoning from effects to causes) that was an independent formulation of Bayes theorem, built on the work of Bernoulli and provided the foundation for the modern theory of hypothesis testing. Laplace also applied statistical inference to natural philosophy, the moral sciences, court rulings and life expectancy, amongst many other contributions. In short, Laplace provided '...the most impressive contribution as yet made by one man,'(Bell, p. III; 1951) to mathematical statistics (For details of Laplace's contributions see; Laplace (1814): Hacking; (1975): Gillispie; (1997): Hald; (1998)).

Laplace and many of his predecessors advocated the epistemic branch of probability to remove noisy data from true data. This noise arises from two sources; firstly errors in observations. Statistical distributions in epistemic probability have one true value. The remaining values that make up the distribution have no intrinsic scientific significance - they are terms that arise because of imperfection in observations and external environmental noise and obscure the real data.[42]

Secondly, epistemic probability was proposed by Laplace in particular, to remove noise that arises from our lack of knowledge, because humans are not the supreme intelligence (see above) that can make perfect causal connections. In the absence of certainty, scientists can express causal relationships between entities in probabilistic terms; scientists can be almost certain of cause. Probability for Laplace is ".. a branch of knowledge required by the limitation of the human intelligence and serving to repair its deficiencies in part," (Gillispie; p. 227; 1997). Whilst Laplace and others developed probabilistic tools, the conceptual framework to which they were applied was very much a causal one.

[41] Moivre's view of science was influenced by the theological writings of Newton. Moivre opined that nature worked according to causal laws that served teleological purposes: "... it is thus demonstrable that there are, in the constitution of things, certain Laws according to which Events happen ... those laws serve to wise, useful and beneficent purposes .." (Moivre, p. 252; 1718).

[42] The concept of noise or error is cornerstone to current fluvial theory, which assumes that regression relationships derive true relationships between cause and effect, and departures from this trend are due to error (see **Chapter 5** and in particular **Sections 5. 2. 2. - 5. 2. 4**).

"... ignorance of the different causes involved in the production of events, as well as their complexity, taken together with the imperfection of analysis, prevents our reaching the same certainty about the vast majority of phenomena. Thus there are things that are uncertain for us, things more or less probable, and we seek to compensate for the impossibility of knowing them by determining their different degrees of likelihood. So it is that we owe to the weakness of the human mind one of the most delicate and ingenious of mathematical theories, the science of chance or probability,"

Laplace (pp. 144 - 145; 1776)

Though the methods and principles of Newtonianism and the causal interpretation of nature it advocated still influence current scientific research, it is not within the scope of this dissertation to give a complete account of the promotion of causality from Laplace to the present. Rather, attention is turned to the current era in which Newtonian causal science and the populist view of science as identifying exclusive, causal relationships between entities have been circumscribed (the reasons why will follow shortly).

Einstein was responsible for a major upheaval of scientific theory which began with Newton, and he placed Newton's work within the more general framework of relativity. However, despite the changes Einstein made to Newton's theory, he retained the view established by Newton, that a causal relationship between entities gave the only true understanding of phenomena (see Polkinghorne, p. 85; 1991).

"[if forced to abandon strict causality] I would rather be a cobbler or an employee in a gaming house than a physicist."

Einstein (in Born, p. 82; 1971)

"...I would be very unhappy to renounce complete causality"

Einstein (in Born, p. 23; 1971)

"It is hard to sneak a look at God's cards. But that he would choose to play dice with the world ... is something that I cannot believe for a single moment."

Einstein 1942 (see Calaprice, p. 176; 1996)

Einstein upheld Laplace's view that causal relationships were actually part of reality, in contrast to Kant who thought that causal relationships were essential only as a mental method of processing information (see Calaprice, p. 151; 1996). However, Einstein resorted to the same proviso that forced Laplace to use epistemic probability; "... as far as the propositions of mathematics relate to reality they are not certain and as far as they are certain they do not relate to reality" (Einstein, p. 233; 1954). Mathematics is both the language and structure of science and causal connections are expressed in mathematical terms[43]. Einstein's statement can then be taken as a limitation on the causal relationship between entities. Whilst definite and real relationships may exist, in the natural world, other things interfere with the purity and uniqueness of the relationships identified in mathematical or laboratory derived axioms. Hempel (1965) summarises this position:

"When this kind of causal locution is used, there usually is some understanding of what 'proper' or 'standard' background conditions are presupposed in the given context. But to the extent that those conditions remain indeterminate, a general statement of causal connection amounts at best to the vague claim that there are certain further unspecified background conditions whose explicit mention in the given statement would yield a truly general law connecting the 'cause' and the 'effect' in question."

Hempel (p. 348; 1965)

3. 4. 2. Statistical Laws and Acausality

"...probability ... is Janus - Faced. On the one side it is statistical [aleatory], concerning itself with stochastic laws of chance processes. On the other hand it is epistemological, dedicated to assessing reasonable degrees of belief in propositions,"

Hacking (p. 12; 1975)

Having presented a brief and partial review of the history of causal laws and the populist conception of science, we return to Hacking's definition of probability, and focus attention on the aleatory branch of probability which is concerned with the stability of statistical frequencies, and is the alternative view to exclusive causal relationships between entities[44] . In the aleatory concept of laws, the distribution of results does not consist of errors and one true value, but rather all values are possibilities and are of scientific significance; "Statements of statistical law apply to situations in which there are several alternatives, and exceptions are nothing but the least frequent alternatives," (Bunge, p. 23; 1979). Defining statistical relationships in the algebraic form used for causal laws above yields; for W, the states X, or Y are possible, followed by the relative probabilities of each state. In the example of velocity and entrainment given earlier;[45] given a certain velocity (W) entrainment may occur (X - 90% probability) or may not occur (Y - 9% probability).

[43] See **p. 77 this thesis** for the significance of mathematics in science.

[44] See **footnote 40 (p. 50 this thesis)** for citation of evidence concerning the dual nature of probability.

[45] See **p. 38 this thesis.**

At the beginning of the 20th Century, the atomic structure of atoms was described by Ernest Rutherford in terms of negatively charged electrons orbiting a positively charged nucleus in the same manner as the planets orbited the sun; in effect it was the application of causal Newtonian mechanics to the smallest scales (See Brennan, Chapter Four; 1997: Feynman, p. 5; 1985). Niels Bohr (1913) altered this model slightly by including the quantum results of Planck and Einstein, to give a partial explanation as to why electrons remained in their levels around the nucleus. However, Bohr retained the classical view of a determined spatio-temporal location of the electron and its orbit.

In 1926 a new theory began that radically altered both the conception of atomic structure and the place of causal laws in scientific endeavour. Following the work of Prince Louis de Broglie, who demonstrated that every piece of matter could have wave like properties, Erwin Schrödinger (1926) produced his version of the central equation of quantum mechanics theory, that removed the orbits of Bohr and replaced them with a standing wave description.[46] A year later Werner Heisenberg produced his paper "On the Perceptual Content of Quantum Theoretical Kinematics and Mechanics," (1927). As a result of these two papers, the causal principle which states that every effect should have an unique cause, was shown to be of limited application in what some hold to be "... the most fundamental and important area of scientific study," (Gribbin, p. xv; 1984)

Schrödinger's wave equation and Heisenberg's uncertainty principle meant that atomic structure was either probabilistic in reality or could only ever be understood in probabilistic terms. (See Zumdahl, p. 525; 1995: Brennan, p. 104 - 106; 1997). Quantum mechanics was a weakening of Newtonian determinism, where x could produce y uniquely, to the exclusion of other results. Schrödinger's wave function described a cloud of possibilities for the position of an electron. In a resolution of the wave/particle duality that atomic phenomena possessed, the physicist Max Born, incorporated both features into his explanation of why the wave function in Schrödinger's equation produced the right experimental result. Particles were real, but they were guided by a non-localised wave; the greater the strength of the wave in a location, the higher the probability of finding the electron in that location. However, it is not a definite relationship, only a probable one; "The strangest thing about this idea is that it means the electron might be anywhere at all, it is just that it is extremely likely to be in some locations and very unlikely to be in others," (Gribbin, p. 119; 1984).

[46] Heisenberg produced matrix mechanics as an explanation of the wave-particle duality problem, whilst Dirac explained in terms of quantum algebra. Dirac's version of quantum mechanics is regarded by some to be the most complete description, because it includes both Schrödinger's wave mechanics and Heisenberg's matrix mechanics as special cases.

Heisenberg's uncertainty principle built upon the wave / particle duality that the Schrödinger equation aimed to explain. This duality was shown in experiments, but never at the same time. Some experiments would show electrons behaving as waves, others as particles, but never wave and particles at the same time. What the uncertainty principle stated, was that it was not possible to specify both the momentum (a wave feature) and the position (a particle feature) of an electron (Gribbin, p. 120; 1984). If an experimenter measured the position, the wave function would collapse to give the particle description of the electron, but would reveal nothing about the momentum of the electron and vice versa. "Co-ordinates in space - time represent position; causality depends on knowing precisely where things are going, essentially on knowing their momentum. Classical theories assume that you can know both at once; quantum mechanics shows us that precision in space - time co-ordinates has to be paid for in terms of uncertainty of momentum, and therefore of causality," (Gribbin, p. 175; 1991).

Furthermore, the uncertainty principle states that the very act of measurement changes the course of particles and thus means that the future is inherently uncertain. This is because knowledge is restricted to the instant of measurement - the measurement is incomplete - and the measurement changes the part of the feature that we are unaware of, so that the course of the particle cannot be definitely forecast.

In contrast to the causal position, which has been described as 'the most intuitive' form of law, "One had to lose one's common sense in order to perceive what was happening at the atomic level," (Feynman, p. 5; 1985). It was this counter intuitive theory with its irreducible probabilistic nature of atomic phenomena that led to Einstein's comments on causality (see above). Einstein wanted to identify a cause behind the behaviour of atomic phenomena. However, whilst no causal connection has been found, quantum mechanics has been one of the most successful of scientific theories (Feynman, p. 5; 1985) and its apparent circumscription of causality has marked a significant change in the basis of scientific knowledge (Tauber, p. 9; 1997).

Quantum mechanics is a more general theory than Newton's laws, because it subsumes classical mechanics (Newton's laws) as special cases of its own laws (Gribbin, p. 119; 1984). At the scale of everyday reality, Planck's constant is effectively set to zero and matter can be regarded as continuous (not quantised), because the quantum effects are so small, they become negligible for practical purposes. Whilst the quantum revolution changed the philosophy of science and challenged the central place held by the causal relationship between entities, science in the macro environment was still dominated by the axioms of Newton's laws.

Although quantum theory applies over all scales, its axioms and laws apply mainly to atomic phenomena. In the middle of the nineteenth century, the existence of atoms was not universally accepted, and a central preoccupation of Victorian physicists was the study of heat (Baeyer, p. xii; 1999). Following the early work of Count Rumford, Robert Mayer, James Joule and Sadi Carnot, Rudolf Clausius formulated two laws for the behaviour of heat;

1. The energy of the universe is constant[47]
2. The entropy of the universe tends toward a maximum

Baeyer (p. 62; 1999)

Clausius had defined entropy as the ratio of heat over temperature, and that entropy was an increase in the ratio of heat to that of absolute temperature.[48] For example " If, say, a calorie of heat from a cup at a temperature of 330 Kelvin meanders into a room at 300 Kelvin, the ratio of heat to temperature increases from $^{1}/_{330}$ to $^{1}/_{300}$, or by about 10 percent," (Baeyer, p. 60; 1999). However, whilst Clausius had identified this transition, he had no mechanism to explain it and no reason as to why entropy always increased. An advancement in the understanding of entropy or the second law of thermodynamics, only began when James Clark Maxwell ventured an hypothesis of temperature, based on the then uncertain notion of atoms and molecules.

Maxwell used the kinetic theory of gases that had been suggested by Daniel Bernoulli in his work Hydronamica (1738). Commonly referred to as the 'Billiard Ball Model' (Baeyer, pp. 67 - 75; 1999: Goldstein and Goldstein, p. 69; 1995;) the kinetic theory assumes that a gas comprises of a " ... large number of identical particles which undergo elastic collisions and obey Newtonian mechanics, (Crystal, p. 663; 1990)[49]. The novelty of Maxwell's approach to temperature was that he considered the behaviour of gas molecules that obey Newton's laws in terms of aleatory probability or the analysis of stable statistical frequencies.

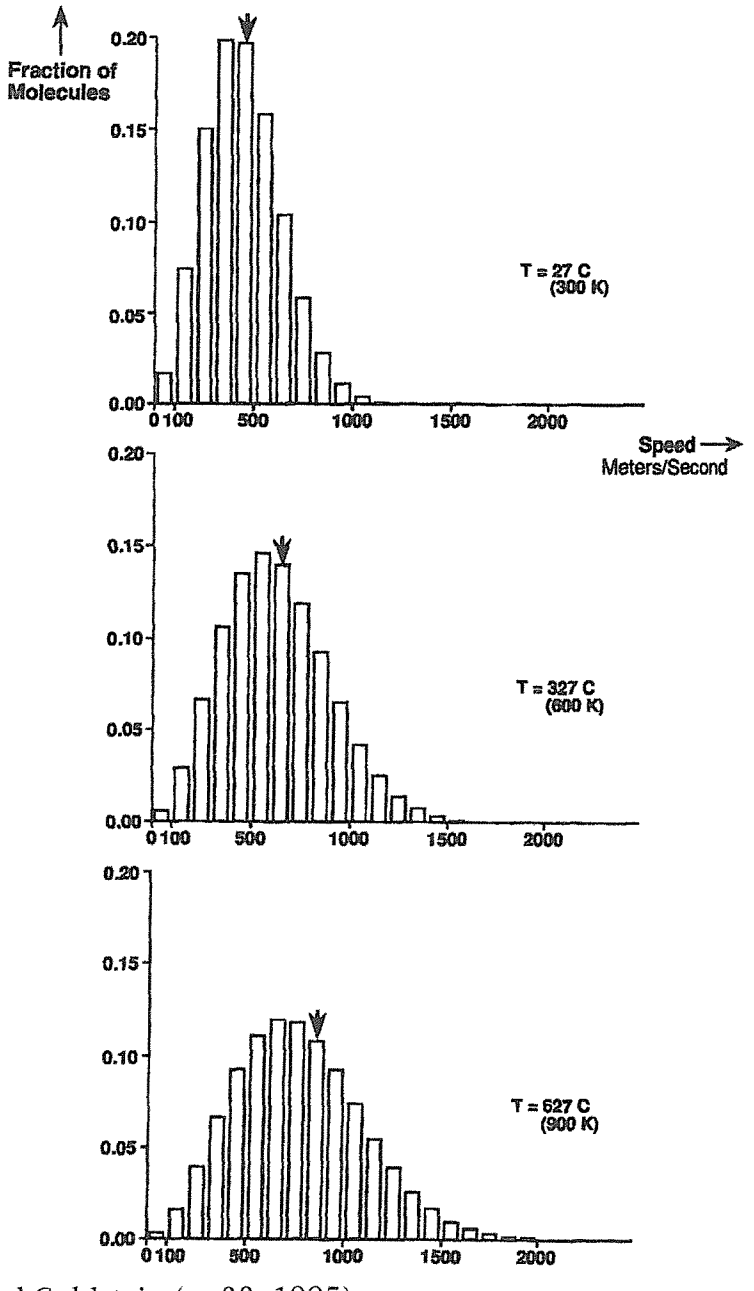
[47] The equivalence of energy and heat as motion had been made by Clausius' predecessors such as Count Rumford etc. (see Baeyer; 1999).

[48] "Heat is a measure of the quantity of energy and temperature a measure of its intensity," (Baeyer, p. 59; 1999).

[49] **See p. 55 this thesis** for a discussion of the application on Newton's laws to atomic phenomena.

Maxwell assumed that the particles in a gas had an equal probability of having a given velocity or that '... all ways of distributing a fixed quantity of total energy among the molecules are equally likely,' (Goldstein and Goldstein; 1995). This assumption led to the formation of a normally distributed curve, the precise properties of which depended on the temperature of the gas (see figure 3. 4. 2. 1). At lower temperatures the distribution is narrow and peaked, but as the "... temperature is increased, the curve maximum, which reflects the average velocity, moves towards higher values and the range of values becomes much larger," (Zumdahl, p. 158; 1995). The frequency distributions of particle speeds for any temperature were stable and constant when a thermal system was in equilibrium - that is when no changes in macroscopic properties such as temperature, pressure and volume occur.

Figure 3. 4 .2. 1 - Frequency Distributions of Particle Speed with Different Temperatures



From Goldstein and Goldstein (p. 88; 1995)

The statistical law derived by Maxwell (and the Austrian physicist Ludwig Boltzmann), entitled the Maxwell - Boltzmann distribution, effectively states that at equilibrium "... the identity of the molecules that are travelling at, say, twice the average speed is constantly changing, but the number travelling at that speed remains about the same," (Goldstein and Goldstein, p. 73; 1995). Because the distribution of molecular speeds is stable at equilibrium, the following formula could be proposed;

Equation 3. 4. 2. 1.

$$\varepsilon = \frac{3}{2} kT$$

Where:

ε	=	Average kinetic energy per particle (joules)
k	=	Boltzmann constant (joules per kelvin)
T	=	Temperature (kelvin)

And:

$$k = \frac{R}{Na}$$

Where:

R	=	Gas Constant (joules per kelvin)
Na	=	Avogadro's constant

The gas constant in the above equation relates the total change in kinetic energy for a molar volume of gas, with the change in temperature - that is for every change in temperature, the kinetic energy of the mole of gas is raised by a constant amount. Maxwell divided this quantity by the number of molecules in a molar volume of gas (Avogadro's constant) to yield the average kinetic energy per particle. This procedure was only possible because the velocities of the particles were normally distributed and because the distribution of particle speeds was stable at equilibrium. Maxwell's equation resulted in the concept of temperature being equivalent to the average kinetic energy of molecules. Thus temperature was defined by an aleatory statistical law.

Ludwig Boltzmann used the Boltzmann constant (k in Maxwell's equation) which is the constant energy per particle per kelvin at equilibrium to derive a quantitative definition of entropy and a theoretical mechanism for its increase. Boltzmann's equation was the following:

Equation 3. 4. 2. 2.

$$s = k \log W$$

Where:

s	=	entropy function
k	=	Boltzmann constant (joules per kelvin per particle)
W	=	the number of microstates that correspond to a given macrostate

Boltzmann's equation provided an answer to Clausius's problem of why hot bodies go cold (or why the heat to temperature ratio always seems to increase) in terms of the particle description of temperature. The Boltzmann constant, which depends on the normal distribution of particle speeds, only applies at thermal equilibrium. However, Boltzmann's equation implicitly states that maximum entropy is a condition of equilibrium (Gull, p. 172; 1991), because it assumes that all the microstates are utilised - the concept of entropy thus gives a theoretical reason why equilibrium temperatures correspond to a normal distribution of particle kinetic energies. In any given system at equilibrium, 10 % of the particles do not have 90 % of the kinetic energy - rather there is a tendency to produce a uniform distribution of energy - this tendency is due to entropy and is explained by Baeyer (1999) in the following terms:

"Probability [microstates] is a measure of the variety of ways in which a system can be rearranged without changing the external, macroscopic properties of the entire system. When the number of ways is high, the arrangement is typical and common - when it is low, the arrangement is exceptional and rare. My teenage daughter Madelynn's room illustrates the connection. Order is defined as the state in which all her books rest on their shelves in the same upright position. Madelynn prefers them scattered over the floor in arbitrary orientations; because there are many more ways to achieve such an arrangement than there are ways to put the books on the shelves, the helter - skelter state is more probable than the 'orderly' one. In other words, if you were to toss the books into her room at random, they would be far more likely to land on the floor than on the shelves."

Baeyer (p. 108; 1999)

Boltzmann's equation states that particles arrange themselves in the way which is most common - in terms of temperature, the kinetic energy of the particles is distributed normally across all the particles because this represents the maximum number of ways that the particles can be arranged (analogous to the books on the floor). At equilibrium the kinetic energy is not confined to a certain portion of the gas, because this represents a state of high order and is improbable (analogous to books on the shelf). Two things emerge from this definition of entropy. Firstly, entropy is what gives the mean value of kinetic energy per particle (temperature) physical meaning for an ideal gas. If the kinetic energy of the particles was not normally distributed (maximum entropy) the mean value of the kinetic energy per particle would be physically meaningless, because a certain portion of the gas could have all the kinetic energy whilst the rest could have none, though the mean temperature for the volume would be the same.

Secondly, systems always tend to maximum entropy (normal distribution) because it is much more probable that they will assume this state than stay ordered. This definition of entropy answers the question why hot objects cool; for example a hot cup in a room is likely to cool because it is more probable that the particles with high kinetic energy (from the cup) will interact with molecules of lower kinetic energy and fit into a normal distribution of speeds. Thus whilst the temperature of the room will increase slightly, this increase is likely to be taken up by all the molecules in the room, rather than stay concentrated in or around the cup (i.e. in Clausius terminology, the ratio of heat to temperature increases). This tendency for more probable or more disordered states gives direction to processes; when all systems are taken as a whole, systems tend towards more probable, more disordered states or maximum entropy - this direction defines time (Coveny; 1992).

Thermodynamics' main statement in terms of the philosophy of science, is that concepts such as temperature and time can only be understood in terms of the aleatory statistical law of averages or the normal distribution (Baeyer, p. 90; 1999). Causal laws cannot explain the phenomena of heat and temperature, but the analysis of stable statistical frequencies and concepts of probability can.[50]

[50] The idea that understanding of fluvial morphology may only come about using distributions of data and that this may be the appropriate method of investigation, is discussed on **p. 269 this thesis.**

The foregoing probabilistic laws of Quantum Mechanics and Thermodynamics, had their origin in the enlightenment. As has already been discussed, probability developed from Galileo and culminated in Laplace, emerging contemporaneously to Newton's causal paradigm. Hacking (p. 35; 1975), in a study of the history of probability, has suggested that statistical concepts emerged from the 'low' sciences such as medicine, alchemy and geology, where phenomena were more complex and less amenable to deterministic laws than those of the 'high' sciences such as physics and astronomy. In particular, medicine relied on probabilistic relationships; Hacking (p. 28; 1975) cites Fracastoro (1546), a doctor who wrote just prior to Galileo, to demonstrate the crude probabilistic reasoning in vogue;

"Contagions have their own peculiar signs of which some announce beforehand contagions to come ... one ought not to consider ... all [signs] as prognostications, but only as signs of probability."

Fracastoro (bk. I, ch. xiii; 1546)

Hacking suggests that the practitioners of the low sciences began to establish the concept of 'evidence' or a different form of proof to that accepted by the high sciences. Whereas the high sciences wanted demonstration or certainty for relationships between entities, the low sciences were beginning to express evidence in terms of 'nearly always', or 'usually follows.' This different idea of evidence eventually filtered through to the mathematicians and philosophers such as Pascal and Leibniz, so that they worked in the conceptual space delineated by the low sciences (Hacking, p. 45; 1975).

Because of the dualistic nature of probability (i.e. epistemic and aleatory) the intellectual lineage covered in the review of epistemic probability applies to aleatory probability. Fermat, Pascal, Huygens, Leibniz, Bernoulli, Moivre and Laplace all contributed to the development of aleatory probability, because its dual concepts are inseparable. This intertwined nature of probability perhaps provides the most illuminating testimony to the multifaceted nature of scientific laws - causal and aleatory laws are intimately related.

It was mentioned, that James Bernoulli provided a limit theorem in the 'Ars Conjectandi' and that this limit theorem formed the basis of statistical inference, because the probability distribution of the probability of an event becomes constant as the number of trials increases, and from this distribution it is possible to assess confidence limits in the results of experiments. What follows from this example is that any epistemic version of probability is superimposed, and relies on, an aleatory distribution. In other words, if scientists have a statistical distribution of an effect and want to ascertain the probability that it is the result of a certain cause, they must rely on a stable frequency distribution that is the result of the weak law of large numbers. This statistical law of large numbers has no other cause or explanation apart from the structure of mathematics. The result is that if the 'high' sciences want to remove error from their experimental data, they can only assess the effectiveness of the procedure by an appeal to an irreducible and fundamental frequency distribution that had its origin in the 'low' sciences.

Thus far in our discussion of statistical laws, a mirror image of developments in causal thinking has been presented; Einstein upheld the causal position, and was complemented by the development of quantum mechanics; the development of Newton's laws and its causal philosophy was balanced with the emergence of thermodynamics and aleatory probability. This pattern now continues with an examination of probabilistic thought during the classical period and its comparison with Aristotelian causation.

Most historians of science agree that a calculus of probability did not emerge until the enlightenment, with the contributions of Galileo and the other workers mentioned above (Kendall; 1956: Sambursky; 1956: David; 1962). However, a clear concept of nature behaving in a probabilistic way was in place in the classical period, even if, like Aristotelian causation, it had little or no mathematical formulation.

Prigogine (1996) has identified Epicurus as a philosopher who, although not concerned with laws of probability, had a probabilistic interpretation of nature. The founder of the Epicurean school of thought, Epicurus was born in 342 B. C. and Aristotle (born in 384 B. C.) was his contemporary (Russell, p. 250; 1961). As an atomist, Epicurus followed the doctrine of Leucippus and Democritus who held that ' ... everything is composed of atoms which are physically ... indivisible; that between the atoms there is empty space ...,' (Russell, p. 83; 1961). According to the atomists, the atoms fell through the void in a regular and determined manner. As a result of this motion, the atomists concluded that the world was strictly determined by mechanical principles and that nothing could happen by chance.

Epicurus however, was disturbed at the thought of a mechanistic world that is perfectly determined once set in motion, and in particular, was disturbed at the removal of free will which this teaching implied. Thus Epicurus attempted to provide a mechanism for free will within the atomists' world view. A strange phenomenon called the 'swerve' or 'Clinamen' was proposed (See Bailey, p. 316; 1928); "While the first bodies [atoms] are being carried down through the void, at times quite uncertain and at uncertain places, they deviate from their course, just enough to be defined as having changed direction," (Bailey, p. 837 ; 1947). Epicurus' Clinamen was a scheme that introduced the notion that random happenings could disturb otherwise determined events and provide novelty and chance in an otherwise ordered world. Epicurus provided no explanation for the Clinamen, and it appears to be a somewhat ad hoc scheme. However, it illustrates the fact that during a period when Aristotle was forming a philosophy of nature dominated by cause, there was an intellectual movement in classical Greece that attempted to admit a random/probabilistic interpretation of nature.

Sambursky (1956) has carried out a survey of probability concepts in ancient Greece and identifies Socrates (circa 470 B. C.) using ideas of statistical uncertainty in the law courts (Sambursky, p. 36; 1956). Tracing the direct intellectual lineage of Socrates, Plato and Aristotle, Sambursky shows that the concept of statistical knowledge was applied to a philosophy of science by Plato and Aristotle;

> *"The awareness of the fact that empirical truth does not rest on the same foundations as does mathematical truth independent of the time factor, led to the coining of a very striking term for the natural sciences; Simplicius tells us: Aptly did Plato call natural science the science of the probable; Aristotle was of the same opinion..."*
>
> Sambursky (p. 37; 1956)

Sambursky (p. 39; 1956) contends that Aristotle, the first formulator of the causal principle, was no absolute determinist. Rather, his philosophy of chance was directed by his doctrine of the universe. As was discussed on p. 41 (this thesis) Aristotelian physics divided the universe into corruptible (terrestrial) and incorruptible (celestial) realms. Sambursky (1956) has argued that Aristotle viewed causality as primarily belonging to the celestial sphere, and looked for regularities and relationships in the incorruptible heavens. The earth which was corruptible was subject to chance, and Sambursky argues, this overarching conception prevented the art of experimentation and the discovery of regularities or relationships that applied to the earth[51]. Aristotle did not recognise aleatory or probabilistic laws, but nonetheless thought that a part of nature worked according to some random principle.

[51] The history of science seems to support this view. Workers such as Ptolemy, Copernicus and initially Galileo, were concerned with relationships amongst heavenly entities.

In chronological terms, the final person in this review of the history of laws is considerably out of joint - in conceptual terms his place is correct. Hume was a sceptical philosopher who proposed the view that no relationships between entities, or laws, causal or statistical, could be discovered. His work implies that scientific knowledge as it is defined in this thesis (explanation by laws) is not possible.

A contemporary of Kant, Hume produced his major work, a Treatise on Human Nature (1739 - 1740), in the climate of causality and determinism that Newton's Principia had fostered. The most comprehensive of his works, 'The Treatise on Human Nature,' aimed to discover a scientific basis of philosophy based on methods of experiment and observation used so successfully by Bacon and Newton (Hume, p. 4; 1739: Norton; p. I15; 2000: Popkin and Stroll, p. 263; 1993). Thus the Treatise could claim its intellectual heritage directly from Newton and the seemingly '...real, certain indubitable and demonstrable knowledge,' (Popper, p. 93; 1989) that Newton had found.

Whilst Hume began with Newton, his Treatise finished with a view of human understanding that was far removed from what Newton had advocated and from what other philosophers who were also influenced by Newton (Kant and Laplace) had proposed. When Hume applied experimental and observational methods to philosophy, what he proposed was that humans cannot know anything with certainty beyond their observations; in particular there is no such thing as a causal relationship between observations: there is not even a rational basis for any relationship between entities, be it causal or statistical.

Hume arrived at this conclusion by stating that humans have two perceptions of things; firstly 'impressions' which are strong, exact, mental images derived from experience and observation; secondly 'ideas' which are derived from impressions and which are capable of the whole spectrum of human thought. Ideas are able to interpolate between and combine impressions into new forms (Hume, p. 7; 1739): Russell, (p. 636; 1961) demonstrates this by use of a winged horse: though clearly such a creature has not been experienced through the senses, the idea of the winged horse is composed from our experience of wings and horses and a synthesis of the two. The important point that Hume proposes, is that all human thought is derived from experience.

If this criteria for human thought is accepted, it appears that causal relationships between observables, or indeed any relationship between observables, is rationally untenable because the connection cannot be observed; we are unable to experience the connection in the same way as we experience an object. How then do humans seem to form causal relationships? How do we see things in terms of exclusive cause and effect?

Hume argues this point, again by appealing to the view that all human thought is derived from experience. Hume's argument is that causality is nothing more than a force of habit: "Thus we remember to have seen that species of object we call flame, and to have felt that species we call heat. We likewise call to mind their constant conjunction in all past instances. Without any further ceremony, we call the one cause and the other effect, and infer the existence of one from that of the other," (Hume, p. 61; 1739). It is this constant conjunction that is observed and gives an idea that there is a relationship between two observables. The insistence of Hume on experience and constant conjunction producing notions of cause, brings itself into direct conflict with those who think that cause is an intuitive mental faculty (which is precisely what Hume intended, p. 56; 1739). If we had never experienced flame, would we know that it was conjoined with heat? The real question is, 'can we have knowledge that is devoid of content and purely intuitive?' That is, when we see flame for the first time can we know *a priori* that it will produce heat, or is our knowledge of flame and heat based on experience and thus observable, constant conjunctions?

Hume clearly espouses the latter view, and the result of his argument is one of complete scepticism. Because we have no criteria for causes independent of experience, cause requires implicit faith in induction; in essence, 'all general ideas are nothing but particular ones,' (Hume, p. 17; 1739). All laws of science are based on isolated observations and there is no reason why these observations cannot change in the future. There is no rational criteria for determining that when I see a flame it will feel hot or cold - only previous experience.

Despite advocating a sceptical view of knowledge as a philosopher, Hume conceded that causal ideas were an inevitable way for humans to think[52] and that although there is no rational basis for his decision, he was just as likely as the next person to reach the conclusion by constant conjunction, that flames produce heat, not cold; "Thus the sceptic continues to reason and believe, even tho' he asserts, that he cannot defend his reason by reason," (Hume, p. 125; 1739).

"Shou'd it here be ask'd me, whether I sincerely assent to this argument ... and whether I really be one of those sceptics, who hold that all is uncertain ...; I shou'd reply, that this question is entirely superfluous, and that neither I, nor any other person was ever sincerely and constantly of that opinion. Nature, by an absolute and uncontrollable necessity has determin'd us to judge as well as to breathe and feel; nor can we any more forbear viewing certain objects in a stronger and fuller light, upon account of their customary connexion with a present impression, than we can hinder ourselves from thinking as long as we are awake. Whoever has taken the pains to refute the cavils of this total scepticism, has really disputed without an antagonist, and endeavour'd by arguments to establish a faculty, which nature has antecedently implanted in the mind, and render'd unavoidable."

Hume (p. 123; 1739)

[52] Russell has noted that the synthesis of human understanding Hume presented, required causal relations in psychology for it to work. "The frequently observed conjunction of A and B causes the impression of A to cause the idea of B," (Russell, p. 640; 1961).

"My intention ... is only to make the reader sensible of the truth of my hypothesis, that all our reasoning concerning causes and effects are deriv'd from nothing but custom; and that belief is more properly an act of the sensitive, than of the cogitative part of our natures."

Hume (p. 123; 1739)

Hume, as a pragmatist and not a philosopher, actually proposes something similar to Kant, who, as discussed above, proposed that cause was an *a priori* principle. Hume writes that causal connections have been 'antecedently implanted in the mind' and are 'unavoidable.' Thus, even perhaps the most acausal view of knowledge proposed, still returns to the fact that humans will inevitably form exclusive relationships between entities.

It is interesting to note that Hume was also sceptical about religious matters (Norton, p. I12; 2000; Russell, p. 634; 1961) and published several works aimed at discrediting faith in religious matters. This relationship between Hume's atheism and the acausal philosophy he proposed, lends some support to the view that causal relationships were supported by the Christian world view.

Having completed this brief historical review of causal and statistical thought and sketched the opinions of some of the most prominent personages of science and philosophy, an attempt is made to answer the two questions posed at the beginning of this section. The answers to these questions are essential for interpreting previous work in fluvial geomorphology and for proposing new hypotheses for indeterminacy in alluvial river channels.

3. 4. 3. Causal Laws - Fundamental to Nature or to Cognition.

3. 4. 3. 1. Are causal laws the fundamental way in which nature works?

From the historical review, it seems that nature has been thought to work in two ways - causal and statistical: A dualistic conception of nature has long been present. Aristotle proposed a view of nature where the heavens worked according to cause and effect, and the incorruptible part of the universe obeyed four types of cause. At the same time, Epicurus thought nature worked by aleatory means and explained free will in terms of random fluxes. 'High science,' Newton's causal laws and the associated epistemological probability that arose from them, gave strong evidence that the universe worked according to causal laws, and Kant and Laplace expressed philosophies based upon a causal universe. At the same time, the 'low sciences' were developing aleatory probability, Hume expressed the view that no relationships between entities (laws) were possible, and thermodynamics developed. Finally, the quantum era emerged and nature was thought to be fundamentally probabilistic; at the same time, Einstein insisted that the universe worked by causal laws.

Whilst this dualistic conception of nature appears long to have been present, the view that nature fundamentally obeys causal laws has been predominant for several reasons. Aristotle's causal conception of nature was the one that survived the classical period. In the middle ages, causal relationships became almost exclusively dominant (Bunge, p. 32; 1979). Historians of science have explained this dominance as resulting from the pervasive influence of Aristotle's causal teaching, and the doctrines of the church which favoured a causal interpretation of the world [the religious positions of the foundational philosophers and scientists considered, seem roughly to correspond to this connection between causality and religion (Bacon, Hobbes, Newton) and acausality and atheism (Epicurus, Hume)].

Out of this historical lineage of Aristotle and Christianity and the causal position it advocated, emerged Newton. The causal laws that Newton formulated explained most of the outstanding problems of the day (such as astronomy) and accounted for so much of the phenomena humans observed. The Principia, and the science it contained, formed an era similar to that of the religious one, where it promoted a new directive for research. In addition, Newton's theology had a profound impact on the scientific workers of his day, and science, in the words of Pearson (1926) was channelled into the mould Newton had, perhaps inadvertently, formed for it. Modern science and Western thought still feel the effects of the Newtonian era and struggle to come to terms with statistical interpretations of nature (Kendall, p. 12; 1956). However, causal and statistical concepts have persistently been present, and the dominance of causality seems to be due to historical and sociological influences rather than nature working in a fundamentally causal way.

Modern science is beginning to emerge from the Newtonian era and the perception that causality is the most fundamental form of law (Koyré; 1965: Bunge; 1979: Casti; 1994: Cohen and Stewart; 1994). Bunge (p. 333 and 335; 1979) writes that 'Causality [is] neither Myth nor Panacea' and that "The causal principle is one of the various valuable guides of scientific research and, like most of them, it enjoys an approximate validity in limited ranges." Bunge like other scientists, is a strong advocate of both causal, exclusive relationships and aleatory statistical relationships with non-exclusive, probable outcomes, each with their own particular use (see also Casti, pp. 38 - 40; 1991).

Science is in the place of attempting to construct a conciliatory position, which recognises the legitimacy of both causal and probabilistic laws and their application to different spheres of understanding. Whereas philosophers and scientists such as Kant, Laplace and Hume could fall either side of a divide between causality and probability, modern science as a whole cannot. Many of science's most successful laws, i.e. thermodynamics and quantum mechanical relations are statistical, but Newton's causal deterministic laws still apply at the macroscale; science makes use of both statistical and causal laws.

Modern science recognises three forms of law; causal, statistical and a hybrid of the two termed Chaos laws. The formulations of these and their place in science are discussed below.

Causal Laws

Causal laws are recognised by modern science to exist in nature. In algebraic form, causal terms are expressed as:

Equation 3. 4. 3. 1. 1.- Causal Laws

$$X_1 \Rightarrow Y_1$$

$$X_2 \Rightarrow Y_2$$

That is, for a given value of X a unique value of Y results.

Sometimes causal relationships are obscured, either due to poor observation/noise and other errors, or due to ignorance of other effects/boundary conditions that have an influence on the cause. These errors or other causes produce a statistical result in the effect of a given cause. A quote from Hempel cited in the above discussion illustrates this principle;

"When ... causal locution is used, there usually is some understanding of what 'proper' or 'standard' background conditions are presupposed in the given context ... there are certain further unspecified background conditions whose explicit mention in the given statement would yield a truly general law connecting the 'cause' and the 'effect' in question."

Hempel (p. 348; 1965)

When a statistical relationship emerges in data, the problem becomes one for the epistemological probability founded by Laplace and others. Epistemological probability deals with both sources of variation (errors or ignorance) from the pure causal relationship. Firstly, it provides a detailed and consistent mathematical framework for assessing error limits and noise, and for providing a good approximation of the true value, thus forming an exclusive relationship between the cause and the true value.

Secondly, epistemological probability in connection with experiments can assess the likelihood of the contribution of any boundary condition or additional cause in producing a given effect, in a real situation. A systematic set of experiments that isolate the given variable, provide the *a priori* probability set for that variable, or stated differently, assesses its influence on the cause. In a real situation these probability sets in combination with data on the magnitude of the given variables, can remove noise and provide several direct and exclusive causal relationships. A simple example of the procedure is provided by the 'velocity and bank entrainment' idea.[53]

A geomorphologist finds a statistical relationship that with a given velocity (cause) bank entrainment occurs 50% of the time and does not occur the remaining 50% of the time.

A geomorphologist finds a statistical relationship that with a given velocity (cause) with a given sediment type bank entrainment occurs 70% of the time and does not occur the remaining 30% of the time.

A geomorphologist finds a statistical relationship that with a given velocity (cause) with a given sediment type and in the absence of vegetation bank sediment entrainment always occurs.

By specifying the precise conditions in which an entity will result exclusively in another entity, statistical relations are reduced to causal ones. This is a legitimate and indeed necessary approach in science, but it can result in a philosophy of science that views genuinely statistical relationships only as starting points of knowledge or approximation to causal laws. Hempel (1966) and Brown (1977) provide two summaries of this philosophy and Watson's (1966) statement is directly aimed at geomorphologists:

"A scientist who holds that scientific explanations must be deductive [causal] *will take a case of a non-deductive* [statistical] *explanation ... as an occasion for further research, while one who holds that statistical explanations are sufficient may well conclude that there is no further work to be done here. This is, of course, essentially the kind of situation that is involved in the refusal of some contemporary physicists to accept the statistical physics provided by quantum theory as complete."*

Brown (p. 175; 1977)

[53] See **p. 38 this thesis.**

"Thus we may distinguish deductive-nomological[54] from probabilistic explanations by saying that the former effect a deductive subsumption under laws of universal form, the latter an inductive subsumption under laws of probabilistic form ... the important, steadily expanding role that probabilistic laws and theories play in science and its applications, makes it preferable to view accounts based on such principles as affording explanations ... though of a less stringent kind than those of deductive - nomological form."

Hempel (p. 68; 1966)

"... indeterminacy can, in principle, be reduced to the determinate. [It is] only the result of limitations of human knowledge and/or comprehension. We resort to stochastic generalisations ... because of human interests and limitations, not because there is an inherent variability in nature which exhibits itself by the presentation of events according to no reason but 'chance.' 'Inherent variability' ... is the introduction of a supernatural element which saps the universe of intelligibility at its foundations."

Watson (pp. 183 -184; 1966)

This is essentially the view of causal and statistical laws that Laplace and Einstein held. Statistical relationships may be admitted into science, but only because scientists suspect error or ignorance on their part. This philosophy believes that as science progresses, statistical laws will become causal, because the universe works according to causal laws. This conception of science as being fundamentally causal corresponds with the populist conception of science. It is important that geomorphologists are clear on this issue. Is the use of mean values inferior to a causal understanding or does understanding, particularly in indeterminate river channels, *only* emerge through cumulative behaviour?

Statistical Laws

"We understand that a change of 18/35 entails certain statistical stability, but we do not understand what, in the fine structure of the universe, creates the statistical stability summed up by the number 18/35"

Hacking (p. 174; 1975)

Statistical laws are recognised by modern science to exist in nature. Statistical laws refer to aleatory probability and the stability of frequencies in data. Some phenomena have proven to be irreducibly statistical (i.e. quantum mechanics and thermodynamics) and knowing the individual causal laws and their actions does not help. Most statistical laws refer to group behaviour where mathematical rules act as mediators between variables. In algebraic form:

[54] Deductive-Nomological explanations include causal laws.

Equation 3. 4. 3. 1. 2.- Statistical Laws

$$X_1 \Rightarrow (p)_{Y1}\ (p)_{Y3}\ (p)_{Y6} \ldots$$

$$X_2 \Rightarrow (p)_{Y1}\ (p)_{Y4}\ (p)_{Y8} \ldots$$

That is, for a given value of X different values of Y are possible, each with a given probability (p).

Statistical laws function at two different scales; firstly the scale of the individual and secondly the scale of the ensemble or the collective of individuals. If any individual test of a phenomenon (y) that operates according to a statistical relationship is related to a variable (x) then the outcome is not exclusive. Although the outcome (expressed in y) is related to a variable (x), it is related randomly; there is no precise mathematical relationship between x and an individual y, because y is a function of statistical mathematical relationships. An example may clarify the point:

Discharge	**Velocity of individual particle in dune assemblage**	
$3\ m^3/s$ (X_1)	0.1 m/s	(Y_1)
$3\ m^3/s$ (X_1)	0.5 m/s	(Y_3)
$3\ m^3/s$ (X_1)	1.0 m/s	(Y_4)
$3\ m^3/s$ (X_1)	1.2 m/s	(Y_5)
$3\ m^3/s$ (X_1)	0.5 m/s	(Y_3)
$3\ m^3/s$ (X_1)	0.2 m/s	(Y_2)

In any individual test, we have no mathematical basis for deciding which velocity will occur for a fixed discharge; there is no one to one relationship between the variable (discharge) and the phenomenon of individual particle velocity, because discharge influences the individual particle velocity through statistical rules. After measuring many different particle speeds, a distribution of individual particle speeds is produced, for a given variable (discharge). If a statistical law is present, this distribution does not change.

This results in two things. Firstly at the individual particle level, though we cannot *determine* the velocity for any given particle, we can state the *probability* of any individual particle having a given velocity. Secondly, from the control variable (discharge), the characteristics of the ensemble of individual particle speeds can be determined, or, stated differently; from the control variable (discharge) the velocity of an individual dune may be uniquely determined.

Though a deterministic relationship exists between, in this case the discharge and the velocity of a single dune, if another scale is incorporated so that there is an ensemble of dunes, the deterministic relationship may become subject to statistical behaviour in a new assemblage. Thus particles in a dune assemblage may follow an aleatory statistical law and dunes in a sediment assemblage may lead to an aleatory statistical law.[55]

An important point emerges from this conception of statistical laws. There is a difference between statistics and statistical law (aleatory probability). Statistics are not laws but descriptions. Using the example of discharge and particle velocities explained above, the difference between statistics and statistical law is illustrated in the following statements.

1. **A given discharge produces various distributions of particle speed.**
2. **A given discharge produces an invariant distribution of particle speed.**

The first statement is an example of statistics. In this situation, the statistics describe what happens, and they are subject to change. Statistics by themselves cannot provide understanding in science. This was shown earlier in the discussion of hypotheses; scientific knowledge is by laws, which are defined as relationships between entities that give an understanding deeper than the phenomena themselves. Statistics by themselves have no law to interpret them, and thus as they change and flux, so does our understanding. Statement one is like the example given in Section 3. 3. about velocity- 'Sediment entrainment either occurs or doesn't occur.'

Expounding the nature of statistics further, it is found that statistics nearly always relate to something; either a causal mechanism or an associated observation. If statistics and probabilities for numbers of heads and tails are given, one immediately thinks of a coin, perhaps even what type of coin. Similarly if statistics of individual particle speeds in a single dune are given, the conditions under which these occur need to be specified; was the dune dry, was it on a slope, what was the discharge flowing over the dune etc? The point is that it is very difficult, if not impossible, mentally to construct statistics that stand alone - statistics of a phenomenon are invariably related to something.

[55] This expanding of scales at which determinism becomes possible, is found to be a feature of the flume data (see **Section 7. 4.**).

If then statistics constantly vary with a fixed association (variable), it suggests three things. Firstly, that causal laws control the phenomenon; the presence of a statistical relationship is due to errors or boundary conditions/causes that require specification. Secondly, the associated variable that is randomly related to the statistical phenomena is not particularly relevant and a new variable should be considered; as an extreme example, a distribution for particle speeds in a dune is likely to have varying statistical properties if it is associated with the tile pattern on the laboratory floor, whilst other relevant variables go unnoticed. Thirdly, a persistently varying statistical pattern, after the previous two options have been considered, may mean that neither a statistical, nor a causal law is applicable. This brings us on to the final category of laws that modern science recognises.

Chaos Laws[56]

Modern science recognises that chaos laws exist in nature.[57] Because of their complexity, analysis of these laws and their mathematical structure has depended on advances in computing. Chaos laws are essentially neither strictly causal nor strictly statistical but contain components of each; "Chaos seems to bridge the gap between the deterministic laws of physics and the laws of chance," (Davies, p. 213; 1992). For any individual case, the mathematical relationship between any two phenomena is determined; that is the relationship is not random - only one phenomenon can follow another. However, the equations on which these laws hang, appear to behave randomly because they are sensitively dependent on initial conditions and have the ability to expand and contract infinitesimally small differences in starting conditions, given enough time (See Chapter Four for a detailed description of chaos theory). In algebraic terms:

Equation 3. 4. 3. 3.- Chaos Laws

$$X_1 \Rightarrow Y_1$$

$$X_1 \Rightarrow Y_8$$

In a specific example, these values of the variables may read;

$$3.000000000020 \Rightarrow 32$$
$$3.000000000001 \Rightarrow 75$$

[56] Chaos laws offer the potential for a new hypothesis in fluvial theory. See **Section 5. 3. 2.**

[57] In his work 'Chaos: Making a New Science', Gleick (1997) devotes an entire chapter to discussing the idea of chaos as a Kuhnian revolution in science and gathering and citing evidence from scientists on the effect that the concept of chaos has had on the scientific community.

The equations are deterministic at every reiteration. The problem is that scientists do not know what the value of the effect will be after, say, 30 iterations, unless they perform the calculation or follow the equation. These causal or deterministic equations force scientists to describe their behaviour, at an individual level. In terms of the definition of hypotheses, there is no understanding deeper than the phenomena. This is a perplexing problem for scientists, because although they know the correct relationship between entities, it doesn't interpret the data for them. An analogous example would be understanding the gravitational constant that Newton proposed, knowing that objects suspended in mid air will fall to the ground. However, if the gravitational constant was a chaos equation, the scientist would have to observe every incident of objects suspended in mid air to check on their behaviour, because whilst the correct equation is known, the outcome is not specified.

At an individual level, chaos laws are descriptions of data, and we have no understanding that is deeper than the phenomena themselves. However, at an aggregate or statistical level, the mathematics of chaotic non-linear equations produce stable and consistent forms known as strange attractors. For a given variable, a given strange attractor is produced. The statistical laws that define attractors are different from those that define stable linear frequency distributions. By focusing attention on the mathematics of populations or total behaviour, an understanding deeper than the phenomena themselves is possible.

Thus the historical and current philosophy of science does not correspond to the populist conception of science as solely identifying causal, exclusive laws. In particular, modern science views nature neither in a strictly reductionist nor holistic manner, but rather applies these two complementary approaches to different phenomena and to different scales of investigation. Based on the concept of explanation by law in physical science expounded here, geomorphology (which is thought to be a branch of physical science by its practitioners), may similarly apply these different types of law and their scales of investigation to indeterminate fluvial morphology.

3. 4. 3. 2. Are causal laws fundamental to human cognition?

Two premises discussed so far are relevant to this section. Firstly, it was suggested in Section 3. 2. that the human mind could only produce scientific knowledge in statements of law. Secondly, it has been stated that the populist conception of science is that the laws science form are fundamentally causal. If these two premises are correct, then the conclusion is that causal laws are fundamental to human cognition, as it pertains to science. This section examines this conclusion.

The historical review of science has shown that from the enlightenment onwards, scientists formed both causal laws - as evinced by Newton and the epistemological view of probability, and statistical laws - as evinced by aleatory probability, thermodynamics and quantum mechanics. It would appear from this summary of science that causal laws and statistical laws are equally valid forms of cognition.

Evidence that is contrary to causal laws and statistical laws being equally valid in terms of cognition, are the supposedly opposing philosophies of Kant and Hume. Kant contended that causal relationships were given *a priori* and were an essential part of rational thought. Hume wrote that causal relationships, or indeed any relationship between entities, had no rational basis and that a critical philosopher should not admit any cause into thinking. However, as was shown, as a pragmatist, or as an ordinary human rather than a sceptic, Hume concluded that it was impossible to think without cause. The surprising result is that both Kant and Hume concluded that cognition required cause, even if their basis for this conclusion was different.

The equivocal evidence that is encountered in assessing the question, 'are causal laws fundamental to cognition?' is due to the misleading nature of the question. The real point is that laws by definition are causal or deterministic. The relationship between entities that allows an understanding of the data, is fixed and exclusive. Both causal and statistical laws, as they apply to nature, find meaning in the human mind because they are framed in causal terms; the human mind does not appear to function statistically.

Causal relationships in nature obviously need no explanation in this scheme; they are both causal in nature and causal in the human mind. The problem is 'how do statistical relationships become causal in cognition?' The answer has already been touched upon in the discussion on statistical laws. At some scale, the level of the ensemble, the relationship between a variable and its effect, is stable and exclusive. The example of the dune was used to illustrate this;[58] at the level of an individual particle within the dune, we can only describe its behaviour. However, if we take the dune as a whole, there is an exclusive relationship between the discharge and the dune velocity. If there is not a stable relationship, we assume one of the options mentioned previously: either a causal relationship is obscured by errors or other causes, the variable we have chosen is the wrong one, or a different type of statistical relationship exists (i.e. a chaos system).[59]

[58] **See pp. 72 and 73 this thesis.**

[59] This observation is intimately related to the previous discussion of statistics being different to statistical law (**p. 73 this thesis**) and also the concept of error (**p. 52 this thesis**). The use of mean values in fluvial geomorphology must be justified by a deterministic distribution (not varying statistics) when sources of error have been removed - hence the attempt to fit laws in an experimental situation (**Chapter Seven**).

An understanding or relationship between entities that is deeper than the phenomena observed, can only result in a stable deterministic relationship between entities at *some appropriate scale.* All laws, causal, statistical or chaos, produce stable relationships and therefore a deeper understanding of phenomena *at some scale.* In the cognitive sense, the populist conception is right; science does seek causal or deterministic, exclusive relationships between entities, because this is the definition of a law. Maybe this is why both Kant and Hume concluded that causality was a necessary part of human cognition?

As a final comment on laws, it is perhaps necessary to answer Hume's sceptical argument that no relationships between entities are possible. The answer to Hume's scepticism might provide an adequate justification for the equivalence of causal and statistical laws in nature and give a deeper understanding of the necessity of statistical relationships and how they emerge.

As was shown above, Hume wrote that most human thought was based upon impressions and observations and what could not be observed, could not exist. This statement led Hume to disregard the rational basis of causality, but from the point of science, it forced him to disregard all relationships between entities, because humans never observed the connection between them. Hume's assertion means that science or any rudimentary knowledge of nature should not be possible, yet scientific knowledge seems to work (see Section 3. 1.).

To answer Hume, we need to postulate what the connection between entities is, and how it can produce causal and statistical forms of laws. Ironically Hume provides an answer in the Treatise. Hume (p. 50 - 51; 1739) proposed an idea that algebra and arithmetic provided certain knowledge - because they are products of rational thought and are not derived from experience, 'algebra and arithmetic [are] the only sciences in which we can carry on a chain of reasoning to any degree of intricacy, and yet preserve a perfect exactness and certainty,' (Hume; p. 51; 1739). In other words, it doesn't matter what objects or entities are used in mathematics, the relationships remain constant.

Mathematics has two qualities that make it a candidate for the connection between entities. Firstly it is immutable in rational thought: different experiences, objects or perceptions can be applied but the relationship remains the same; "Mathematics is, I believe, the chief source of the belief in eternal and exact truth ...for mathematical objects, such as numbers ... are eternal," (Russell, p. 55 and 56; 1961). Secondly, mathematical relationships appear to be more than just mental ideas, "mathematical laws ... really do exist; they are real, not just artefacts of human imagination ... Only a mathematical universe can develop brains that do mathematics," (Cohen and Stewart, p. 27; 1994). The reality of mathematics may be what makes scientific knowledge work.

Because of their existence in nature, mathematical relationships are given *a priori*. This is not to say that in cognitive terms they are given *a priori*. The history of science shows that though nascent ideas of causality and probability were present, they depended on the construction of the relevant calculuses, which were not developed until the enlightenment. Furthermore, infants still require training in mathematics; they are not born with a full knowledge of mathematics. Perhaps the best way to summarise the *a priori* status of mathematics is to state that the cognitive equipment for mathematics is given *a priori,* mathematical relationships exist in nature *a priori*, and once they are discovered, they become *a priori,* and as far as we can tell, immutable guides.

The idea emerges that different forms of laws, causal, statistical and chaos are different forms of mathematical operators, and the empirical observations which hang on these mathematical frameworks. Statistical and chaos relationships are caused by the structure of mathematics and are as equally valid as causal relationships. Recent advances in mathematics, if anything, show that statistical operators and chaos type mathematics are more likely to be the norm rather than those mathematics that govern causal or deterministic systems.

Chaitin (1992: 1988) has discussed a mathematical result, which suggests that "... the very structure of arithmetic itself is random," (Casti, p. 390; 1991). In brief, Chaitin attempted to specify laws for the generation of arithmetic numbers on computer programs. If the law or programme was not shorter than the desired number or result, the number was termed random. When this occurs, the relationship between entities is not deeper than the observables, or put differently, there is no shortcut to understanding the system - the system is its own code. Chaitin's work showed that nearly all numbers are random, and that our knowledge in mathematics is perhaps confined to the limits of our experimentation:

> *"I have found an extreme form of randomness, of irreducibility, in pure mathematics ... randomness and unpredictability are fundamental ... at the very heart of pure mathematics."*
>
> Chaitin (p. 202; 1992)

The axioms used in mathematics are not deeper than the observables they describe. In other words, laws *sensu stricto* do not exist in mathematics - rather they consist of empirical regularities which are constant and certain because no boundary conditions interfere with their relationships. That laws do not exist in mathematics is to be expected, because it is our deepest rationality; it is, with reference to Chalmers (1999) above and to the nature of science, the '... rational which cannot be rationalised'. This leads to the paradox that ultimate rationality is an empirical activity.

Where the empirical system of mathematics, for want of a better phrase, is 'well behaved', axioms that lead to causal/deterministic laws result. Aleatory statistical operators are an intrinsic part of the fabric of mathematics and allow a deterministic understanding *at some scale* of otherwise, indeterminate phenomena. Chaos mathematics clearly reveals the random/empirical nature of mathematics and presents difficulties for law-making in those physical systems that follow these mathematical axioms.

Mathematics may be an answer to Hume's scepticism about scientific laws. Mathematics includes immutable axioms that result in both causal, statistical and chaos laws. That these laws are all possible may not be a result of frailties in human knowledge, but part of the fabric of mathematics - the ultimate rationality.

3. 5. Theory as an Established Framework of Scientific Knowledge

> *"The concept of theory is certainly a fuzzy one within the philosophy of science, within science in general, and within geomorphology in particular."*
>
> Rhoads and Thorne (p. 126; 1996c)

At the start of this discussion on the scientific method, an opening definition of an hypothesis was given as a speculative conjecture. An alternative definition for an hypothesis was an established body of knowledge. Just as both terms have been applied to the term 'hypothesis,' so reciprocally they both have been applied to the term 'theory'. Philosophers and scientists have used the word 'theory' in the conjectural sense of hypotheses as demonstrated in the phrase 'Oh! I have a theory about that.' On the other hand, a much more established tone is given to 'theory' by viewing this component of science as a complex framework for scientific enquiry, '... an explanatory scheme into which phenomena of interest can be fitted, (Cartwright; p. 380; 1981). Table 3. 5. 1. lists some uses of the word 'theory' in both science and philosophy literature to illustrate the different use of the term.

Table 3. 5. 1. shows that the majority of references cited adopt the more complex use of the word 'theory', and this inclination seems to be reflected in the wider literature.[60] This usage suggests several aspects of theory: it uses frameworks of laws to provide new knowledge and explanations of various phenomena; it makes use of established laws; and it represents the best understanding of phenomena currently available (Cartwright; 1981: Brown; 1977). This thesis adopts the complex definition of theory, because it allows for a starting point in science (hypotheses) and for science to have a product of established knowledge (theory), even if that product turns out to be incomplete, in error and in need of revision.

Table 3. 5. 1.- Opposing Definitions of Theory

Author		Definition of 'theory'
Johnson	(p. 180; 1933)	"The discovery of facts that are wholly unanticipated, but which nonetheless, find full explanation in an hypothesis under investigation, has exceptionally high value as confirmation of the hypothesis. If many investigators discover many facts, all fully accounted for by the hypothesis, and this is repeated over a long period of time, the confirmation becomes of such high quality that the hypothesis is transformed into a well attested theory."
Harvey	(p. 88; 1969)	"Theory is a system of statements."
Guelke	(p. 45; 1971)	"A scientific theory is designed to provide a deeper understanding of observed regularities, especially laws"
Popper	(p. 115; 1989)	"... scientific theories ... are genuine conjectures - highly informative guesses about the world."
Casti	(p. 27; 1991)	"A theory explains a set of laws"
Shamos	(p. 90 & 138; 1995)	[Major conceptual schemes or big ideas]
Zumdahl	(p. 8; 1995)	"A theory is actually an educated guess."
Strahler	(p.10; 1999)	[Hypotheses and theory are interchangeable]
Collins English Dictionary	(p. 1587; 1998)	"A set of arguments related by logical or mathematical arguments."

[60] The use of the word 'theory' in this manner, may be seen as one of the most successful aspects of Thomas Kuhn's (1996) treatise on Scientific Revolutions where the view of theories as frameworks or paradigms was promoted [see Brown; (1977) who promotes the New Image of Science from a Kuhnian perspective and also Lakatos (1970) whose developed research programmes were theoretical structures] . Popper (1989) was a proponent of the other extreme where theories are speculative claims. The different usage of the word theory reflects the whole underlying ethos of the respective philosophies of science. All these philosophical constructions of science are discussed in **Section 3. 6.**

The definition of theory as established scientific knowledge stresses that theory is composed of several tested laws and the collective understanding that is generated between them - theories in this sense are composed of 'relationships between relationships among entities.' Chalmers (p. 131; 1999) and Brown (p. 95; 1977) both point to the core of Newtonian theory as being the three laws of motion and the inverse square principle of gravitation. However, a clear example of the complex nature of theory is given by the kinetic theory of gases mentioned in Section 3. 4. 2.

In order to produce a kinetic theory of gases and an understanding of their behaviour, a multitude of scientific concepts and established laws is required. Firstly the units of pressure for a gas depend on Newton's laws of motion. Pressure is defined as the force of one Newton acting on an area of one square metre. A Newton is defined as the force which causes an acceleration of one metre per second per second for an object of mass one kilogram (Newton's second law of motion). Secondly, the pressure acts on a given volume (measured in metres cubed) to yield units of kinetic energy. Kinetic energy is measured by the unit Joule which is defined as the work done by a force of one Newton applied over a distance of one metre in the direction of force.

The gas constant for ideal gases, which is the macroscopic version of the Boltzmann constant, uses the concepts of pressure and volume to produce a relationship between temperature and energy. This gas constant is possible because ideal gases have fixed relationships between pressure, volume and temperature described by two empirical laws. Boyle's law states that the product of the pressure and volume of a gas at equal temperature forms a constant. Charles' law states that the change in volume of a gas with temperature at equal pressure is a constant. These two laws combine to provide the molar gas constant or the equation of state. The formation of a probabilistic interpretation of gas behaviour depended on the concept of atoms and the laws of probability. The quantitative calculation of the Boltzmann constant depended on Avogadro's law, which stated that equal volumes of gases contain equal numbers of molecules, when at the same temperature and pressure.

The kinetic theory of gases depended on various laws which had been found to be successful (such as Newton's law, Boyle's law, Charles' law) and several concepts such as energy and atoms. All these laws and concepts were taken together to produce a collective understanding and to provide an explanation of new phenomena. The kinetic theory is also illustrative that whilst theory makes use of established laws and represents the best available explanation of phenomena, theory is subject to review. The kinetic theory was eventually modified substantially by Quantum Mechanics (Goldstein and Goldstein, pp. 339 -341; 1995).

3. 6. Prediction

A key question arising from the above discussion of science is how do hypotheses become laws, and how do laws become theory or an established body of knowledge?[61] For many scientists, the ability to predict is the test hypotheses must pass to become laws, and laws must pass to become scientific theory; all stages of the scientific endeavour are scrutinised according to predictive ability. Table 3. 6. 1.[62] documents some statements of philosophers and scientists on the value of prediction.

From Table 3. 6. 1. , it can be seen that prediction is valued as a test of scientific knowledge. The importance placed on prediction in scientific investigation, suggests that the aim of science is to produce knowledge that is predictive. Why should this be the case? The concept behind the answer is simple but the practice of this concept is complex.

First the concept: scientific knowledge is held to be different to other forms of knowledge, because it seeks objective confirmation or falsification of the ideas it proposes. Rather than accept hypotheses, science attempts to evaluate whether they are true, by seeing if they work. Invariably this involves the construction of an experiment, where the hypothesis under investigation can be tested thoroughly. If the results of experiment agree with the predictive statement made on the basis of the hypothesis, then the hypothesis gains some standing and is subjected to further tests. If it continually passes predictive tests, then it may gain the status of a law and eventually become incorporated into a theory. Below is the example seen in Section 3. 2. subjected to predictive tests.

1.	**Hypothesis:**	**With a given velocity bank sediment entrainment always occurs.**
2.	**Predictive Statement:**	**Given the critical velocity bank sediment entrainment will occur.**
3.	**Test:**	**Record bank particle movements at critical velocity.**
4.	**Result:**	**Bank particle entrainment occurred at the critical velocity.**
5.	**Law formation:**	**At a given velocity, bank sediment entrainment will occur .**

[61] The answer to this question has important implications for testing current fluvial theory and the validity of new hypotheses for indeterminacy in fluvial systems.

[62] Because philosophies of science differ, some usage of the words in **Table 3. 6. 1.** will differ from those proposed in this thesis.

Table 3. 6. 1.- The Value of Prediction in Science

Author	Statement
Gasper (p. 291; 1991)	"We value theories that make accurate predictions: ... prediction is important evidence in favour of a theory."
Quine (p. 167; 1969)	"prediction which [is] the very life of science"
Goldstein and Goldstein (p. 5; 1978)	"..laws of great generality and accurate predictive power are taken as essential to what we define as science."
Hempel (p. 333; 1965)	"The formulation of laws and theories that permit prediction of future occurrences are among the proudest achievements of empirical science."
Gieryn (p. 298; 1995)	"...the institutional goal of science is the extension of certified knowledge which is empirically confirmed and logically consistent statements of regularities (which are in effect predictions)."
Shamos (p. 48; 1995)	"Since most useful statements in science are essentially predictive."
Swartz (1985)	"The scientific enterprise consists in observing, experimenting on and of course hypothesising about the world in order to explain, predict and control..."
Richards (p. 226; 1987)	"Predictive extension is ... the mark of a scientifically authentic research program. If a program accounts for the empirical content of rivals but also generates further predictions, then the program is theoretically progressive and thus scientific."
Tauber (p. 3; 1997)	"...when referring to the maturity of a science, we are concerned with the differing sophistication of the various disciplines to command prediction."
Laudan (p. 149; 1984)	"Consider one specific part of the success of science, its predictive ability ... it is quite clear that science puts us in a position to anticipate what the world will do next with a rather higher ability than many systems of belief commonly regarded as nonscientific."
Chalmers (p. 233; 1999)	"..it seems plausible to evaluate theories solely in terms of their ability to order and predict observable phenomena."
Trovimov and Phillips (p. 203; 1992)	"The ultimate goal of geomorphology, and any science is prediction."

The reason behind using predictive tests, is that if a prediction based on an untested knowledge statement (hypothesis) turns out to be false, then it suggests that something is wrong with the knowledge of the phenomenon under investigation. If the prediction is right however, it lends support to our understanding of the phenomenon. Knowledge is supposed to work; "Whatever knowledge may be, its hallmark is the ability to do something with it ... if it can't be successfully used in some such fashion then it doesn't qualify as knowledge," (Pitt, p. 7; 1988).

The above is a description of the pure conception of prediction. Most philosophies of science have some version of this pure conception of prediction as a central issue in their schemes, but invariably it is the weakness of how prediction is interpreted in these philosophies, and the complex history of predictive science, that is the undoing of the absolute acceptance of any one philosophy of science (see Chalmers; 1999). The remainder of this section examines how several of the major philosophies of science employ the tool of prediction and the attendant benefits and problems of each approach.

In the example of 'velocity and entrainment' given above, the empiricist, logical positivist account of scientific knowledge, would argue that if the predictive statement 'given a critical velocity bank sediment entrainment will occur' was shown to be true a large number of times, then confidence can be put in the causal law; given velocity (x) there will be entrainment (y). Detractors from this viewpoint highlight the absence of logic in the inductive use of predictive statements. Chalmers (p. 44; 1999) cites the following macabre illustration:

> *"A turkey ... noted on his first morning at the turkey farm that he was fed at 9 am. After this experience had been repeated daily for several weeks the turkey felt safe in drawing the conclusion "I am always fed at 9 am." Alas , this conclusion was shown to be false in no uncertain manner when, on Christmas Eve, instead of being fed, the turkey's throat was cut."*
>
> Chalmers (p. 44; 1999)

The argument against the inductive use of predictive evidence, points to the fact that the prediction only applies to those cases that have been measured, so that the prediction is really a retrodictive statement. Because there is no logical connection between the hypothesis and the predictive result (the prediction is not a necessary conclusion to the premises) in the empiricist conception of science, the results of the tests between velocity and bank entrainment cannot be extrapolated to future velocities with confidence. Thus the predictive statements for the velocities measured are event specific and effectively take the role of a statement summarising what occurred (a retrodictive statement).

Proponents of the empiricist viewpoint would argue that although there is no logical basis for extrapolation of predictive statements, the fact that predictions are confirmed for a large number of events, suggests that the probability of a prediction being confirmed is high.[63] The fact that bank sediment entrainment has been observed on 2000 tests at the critical velocity lends considerable support to the law that, 'given a velocity (x) there will be entrainment (y)'. However, opponents of this position counter with several important points. From a conceptual basis, it is argued that the data set corresponds to a finite number of observations, whilst there are an infinite number of observations for which the predictive statement must apply. In terms of probability "... the law in light of the evidence is thus a finite number divided by infinity, which remains zero by whatever factor the finite amount of evidence is increased," (Chalmers, p. 52; 1999); thus all scientific theories have zero probabilities.

Ignoring the above concept of zero probabilities, from a practical stance it is questioned how many observations are required for any given predictive statement. Given an hypothesis and its associated prediction, how many times must it be tested in order to be confirmed or verified and is this the same for all hypotheses or does it vary?

A related point to the inductivist use of prediction, which uses instances of correct prediction to bolster its theories, is that often a prediction may be right but the explanation of that phenomenon may be wrong. It has been repeatedly stated above, that scientific laws (the basis of scientific theory and knowledge) must give an understanding deeper than the data itself. In this current section, it has been supposed that a correct understanding gives predictions and that the accuracy of scientific knowledge is tested by the predictions it makes. It has already been seen that prediction's place in the inductive method has substantial problems, but to add to this, the history of science shows that some incorrect explanations have yielded correct predictions.

[63] The most advanced attempt to provide probabilities for theories, is the Bayesian viewpoint - so called because it utilises the conditional probability postulated by Thomas Bayes in 1763. However, the theory is essentially the same as inverse probability that Laplace proposed (Laplace was a contemporary of Bayes - see Gillispie, p. 16; 1997). Laplace attempted to discover true theories by ascertaining their probability and this is an approach which has gained considerable adherence in recent years (Chalmers, p. 174; 1999)

A scientific theory that falls into this category is Copernicus's heliocentric universe. Copernicus placed the sun at the centre of the universe with the planets orbiting this central star in a circular motion. Because it emphasised circular motion, the Copernican theory required the addition of epicycles to explain planetary movements.[64] The Copernican theory made many predictive statements and had them confirmed. Among them were: the prediction of eclipses (Bunge, p. 256; 1979) and the explanation, calculation and prediction of the behaviour of retrograde motion in the planets' paths (Hoskin and Ginerich, p. 97; 1997). All these predictions were confirmed, despite the knowledge now that planets move in ellipses, not in circular motion. Thus, though a substantial part of Copernican theory was wrong, predictions matched observation.

The important point of the above discussion is that, whilst prediction is a fundamental part of scientific knowledge, its relationship to knowledge is subject to profound difficulties and is not clearly defined. The next section shows that the falsificationist philosophy, one of the main alternatives to empiricist philosophy, fares no better in delineating predictive knowledge. Falsification requires prediction, but its relationship to prediction suffers from the opposite criticisms that were levelled against inductive empiricism.

As proposed by the philosopher of science, Karl Popper (1972), Falsificationism depends on placing observational statements within the framework of deductive logic. The investigator then relies on logical relations between premises (an hypothesis and a test) and the conclusion (prediction) derived from them. Here is the now familiar example of 'velocity and entrainment' set in deductive form.

Major Premise (hypothesis)	**Given a critical velocity, bank sediment entrainment always occurs.**
Minor Premise (specific test)	**The test uses the critical velocity.**
Conclusion (predictive statement)	**Entrainment will occur.**

In the Falsification scheme, the success of the prediction (i.e. entrainment takes place) is of little value in itself. If the prediction is confirmed, the deductive relationship is of no more value than the inductive inference. This is because Falsificationists are philosophers who advocate the zero probability of hypotheses (as written above), where a finite number of observations is divided by an infinite number of possible variations. In a purely logical form, if the conclusion is true, then the premises are taken to be true, but these premises are based on inductive arguments (all the velocity experiments observed) and so the fulfilment of prediction does not transcend the inductive limitations of the premises.

[64] Brown (p. 114; 1977) has noted that Copernicus '...even retained the notion that the celestial bodies are carried by crystalline spheres..."

Furthermore, as seen in the case of the Copernican revolution, true conclusions can be derived from premises that are later found to be false. It is only the logical validity of the argument that must connect them. Brown (p. 18; 1996) provides the following illustration: "all dogs are guppies, all guppies are mammals, therefore all dogs are mammals." Clearly though the premises are false, a true conclusion has been derived.

Popper's view of science appeals to the one certainty of deductive logic; if a false conclusion (prediction) is drawn from logical relations among premises, then at least one of the premises is wrong. Thus whilst confirmation does not support an hypothesis, falsification means that the hypothesis is wrong and science needs to readjust its knowledge, because it has not been able to predict events that should be logically certain if the premises are true. The falsified "velocity and entrainment' example is shown below.

Major Premise (hypothesis)	**At a critical velocity entrainment always occurs**
Minor Premise (specific test)	**The test uses the critical velocity**
Conclusion (predictive statement)	**Entrainment will be observed**

After testing, the deductive syllogism appears as following:

Major Premise (hypothesis)	**At a critical velocity entrainment always occurs**
Minor Premise (specific test)	**The test uses the critical velocity**
Conclusion (result)	**Entrainment was not observed**

The appearance of a false conclusion from a logical point of view, suggests that one of the premises is wrong. It cannot logically follow that if both the above premises are true, entrainment is not observed. This is the strength of Popper's scheme. Scientific progress is based on the discovery of failed predictions. Until an hypothesis fails a prediction, it is tentatively accepted and is the best understanding that science has of the phenomena in question. Once it does fail predictive testing, it opens up the possibility of advancements in theory and the acquiescence of new hypotheses and tentative knowledge.

Popper's conception has important implications for the nature of the test or experiment. The experiment should be set up with the intent to disprove the hypothesis; to make it fail a predictive test. Thus in the case of 'velocity/entrainment' hypothesis, the experimenter tests the critical velocity using different slope/discharge combinations, different sediment types and shapes etc. It has already been stated that confirmation of predictive test does not lend support to a specific hypothesis and that is true from a purely logical point and in stringent probability theory. However, confirmation does enter into Popper's Falsificationism for pragmatic purposes. The more Falsification experiments an hypothesis passes, and the greater generality and scope it has, the more valuable the hypothesis, and the more likely it is to become established as a law and form the foundation of scientific theory.[65] If, for example, the 'velocity/entrainment' hypothesis passed all the experimental attempts to falsify it, it would become a law of considerable power. If it only passed one of the tests (i.e. bank entrainment occurs at a given velocity, only in the presence of a given slope) it would be of less scope and value.[66]

The presentation of Falsification thus far, suggests that it is a more reliable form of evaluating knowledge by prediction: if a prediction fails, it is a logical certainty that something is wrong with the hypothesis. However, falsification has had some severe criticism levelled against it both in conceptual and historical terms. Falsification is subject to two forms of error: the first of these is internal and is related to the relationship between the hypothesis premise and test premise; the second is external and is related to the relationship between the test premise and the conclusion.

Internal source of error is due to unknown limitations and effects on the hypothesis which may alter its application. In this case, the hypothesis is correct under certain *conditions* or in a certain *context*. The 'velocity/entrainment' hypothesis provides a basic example of the two cases of condition and context. 'Given a critical velocity, bank sediment entrainment will occur' is tested in the presence of vegetation and it doesn't apply (i.e. entrainment does not occur at the critical velocity). The hypothesis is tested using the critical velocity at a given slope value and it does not apply. The same hypothesis is tested in the absence of vegetation and at a different slope value and it does apply. This is the *condition* which is required for the hypothesis to yield a successful prediction. *Context* can be illustrated by the example that 'given the critical velocity bank sediment entrainment will occur' applies in every situation, except when the previous flow velocity has been particularly low. If there has been a low velocity flow prior to the critical velocity, the predictive relationship between the hypothesis and the test may be disrupted, because antecedent conditions have an effect that the hypothesis has not considered.

[65] Note that Popper subscribes to the version of theory which is tentative and speculative, i.e. an hypothesis in this thesis (see **Table 3. 5. 1.**).

[66] This principle is applied in the experimental analysis (see **Chapter Seven**).

The important point of condition and context (internal sources of error) lies in the logical certainty of falsification and the emphasis this is given by philosophers of its school. In a perfect logical world, one false predictive statement yields the result that the premises are false. The implication of the condition influence for experimenters is that success clearly depends in which order they carry out their experiments. If experimenters test the 'velocity/entrainment' hypothesis in the absence of vegetation and at an appropriate slope value, then they have evidence that the hypothesis applies in a given situation. If further results indicate that the test is falsified in other environments, experimenters can take the hypothesis as one of low value and subject it to further testing with the potential for falsification in the absence of vegetation and at an appropriate slope value. If however, the experimenters try the predictive capabilities of the 'velocity/entrainment' hypothesis in the presence of vegetation, they are immediately faced with a false conclusion and must reject the hypothesis on logical grounds. To persist in testing the hypothesis would be to deny the logical connection between the premise (hypothesis) and the conclusion (prediction). Context presents a similar problem in that it can logically disable a whole hypothesis by providing one special case where the hypothesis does not apply.

The history of science shows that strict falsificationism is not the manner in which science has been carried out, nor is it pragmatic for science to adopt the falsificationist position. Science appears to accept certain theories and hypotheses as correct and defends them, despite predictions that should logically falsify them; science requires faith in its theories (Brown, p. 96; 1977). Newton's law of gravity provides an example where the hypothesis premise was affected by other conditions and altered its predictive relationship to the test premise:

"Early in the nineteenth century astronomers recognised that none of the orbits for Uranus that had been computed on the basis of Newtonian mechanics fitted all the observed locations .... within the framework of Newtonian mechanics there is one clearly acceptable factor which could affect the orbit of a planet; the existence of an as yet unknown planet exerting a gravitational force ... The planet Neptune was eventually discovered and Newtonian mechanics had achieved one of its greatest triumphs."

Brown (p. 97; 1977)

Newtonian theory was supported by the scientific community, despite the falsification of prediction in the trajectory of a major planet. A condition for gravity to apply was calculated and corroborated, leading to new discoveries in the natural world. The law of gravity not only failed to predict the trajectory of Uranus, but did not match observations for the moon; Newton's predicted motion of the moon's perigee remained only half of that observed, but scientists persisted with his gravitational law until in 1750, sixty years after it had been proposed, the conditions needed for gravity to apply were identified, (Kuhn, p. 81; 1996).

Though gravity suffered these predictive blunders, scientists did not falsify the law, but persisted with it, in spite of these failures. Gravity remains a fundamental force in modern physics (Feynman, p. 4; 1985).

The above discussion has focused on internal difficulties with falsification and predictive knowledge, where the relationship between the hypothesis and test is disrupted because of a lack of awareness of other factors (condition and context) that can change the prediction (although the basic thrust of the premise is correct given the right initial conditions). The following section examines the external difficulties with falsification and predictive knowledge.

External problems in deductive knowledge correspond to a breakdown between the test premise and the conclusion (prediction). This is not due to any logical omissions in the argument such as condition or context and hence is termed external. In this case, both the hypothesis and the test premises, as stated within the deductive argument, are correct, but the ability to perform the test is limited and contingent on available equipment.[67] The situation here is the opposite to that considered with inductive knowledge[68] - the correct explanation yields the wrong predictions.

Copernican theory is once again used to illustrate the involution of science. Whilst Copernicus was wrong about the planets' orbits being circular, his hypothesis that the Earth moved round the sun was correct, but it was not easy to demonstrate this principle. The Copernican theory, by necessity, made several predictions. If the Earth went around the sun, the observed position of a star should shift with changes in the Earth's orbit; there would be what astronomers term, an 'annual parallax'. Because the annual parallax was not observable with the naked eye, it implied two things: either Copernicus was wrong, or it meant that the Copernican theory would have to make another prediction, and state that "... the stars were so far away that their apparent movements were too small to be detected," (Hoskin, p. 111; 1997). Also, because the Earth rotated the sun with the other planets, it implied that it was similar in character to them; this was against the accepted teaching of the day which was based on Aristotle's cosmology[69] and ran contrary to observation with the naked eye (because topographic features on the moon, which would have supported the conjecture that the Earth was similar to heavenly bodies, could not be observed).

[67] This is particularly relevant for the experimental work in this thesis - it would always be desirable to have more control over the experimental variables and to be sure of the conclusions given in **Chapter Eight** it is recommended there that more accurate experimental tests are performed.

[68] **See p. 84 this thesis.**

[69] **See p. 41 this thesis.**

The heliocentric theory proposed by Copernicus did not gain general acceptance after its publication, because astronomers' observations did not accord with those predicted by the Copernican scheme (see above)[70] (Chalmers, p. 96; 1999: Hoskin and Ginerich, p. 97; 1997). In Popper's terms, the theory was logically falsified, and a new hypothesis should have been developed whose predictions were fulfilled. However, the history of science shows that the heliocentric theory appealed to the physical intuition of several men of learning (Hoskin, p. 116; 1997) despite its significant failings.

Galileo Galilei was perhaps the most influential of such men, as he provided evidence for the Copernican theory, not by changing its hypothesis, but by refining the technology with which he carried out the observational tests. By designing and using telescopes, Galileo found "... stars that had remained hidden since the Creation ...," (Hoskin, p. 125; 1997) that were evidence for a large universe as required by Copernicus. He also discovered that Jupiter had moons like the Earth, and that the Earth's moon exhibited similar topography to the Earth. Both of these observations were taken to be strong indicators that the Earth functioned and behaved like the celestial objects. The annual parallax was not directly observed, although the discovery of other stars made the size of the universe larger and hence the possibility that the parallax could be very small. Galileo proposed a method for estimating this parallax and in 1838 this technique combined with specialised instruments, made the observation of a star's parallax possible, giving direct confirmation of Copernican theory, almost 300 years after it was proposed (Hoskin, p. 219; 1997b).

Similar situations have occurred in modern science. One pertinent example is Quantum Mechanics[71], where hypotheses have postulated new particles, but have had to wait on technological advances before predictive requirements can be fulfilled. The broken symmetry theory that aims to explain the weak nuclear force present in atoms consisted of such hypotheses. Developed in a primitive form in 1961, the hypotheses postulated the existence of a new particle (the neutral z boson), "...to the embarrassment of physicists, who had no experimental evidence for its existence," (Gribbin, p. 264; 1984). The hypotheses persisted, in the absence of this predictive fulfilment (i.e. no experimental evidence of the particle), and after several mathematical reformulations, its developers were awarded the Nobel Prize in Physics (1979). In 1983 the particles were observed using the particle accelerator at CERN, fulfilling Popper's predictive requirements, but the theory already had the confidence of scientists[72].

[70] Other factors that delayed acceptance of the Heliocentric theory were the nature of motion and acceleration, which Galileo attempted to answer.

[71] **See pp. 55 and 56 this thesis.**

[72] See Gribbin (pp. 262 - 267; 1984).

The history of science reveals that correct explanations can yield seemingly wrong predictions (Copernican revolution) or cannot fulfil predictive requirements at the time the theory is proposed (Quantum Mechanics), because of inadequacies in the testing method. Strict falsificationism demands that in the first instance (Copernican revolution), the hypothesis is rejected and in the second (Quantum Mechanics), no progress can be made by using the unconfirmed hypothesis as a basis for other advances, because it has not had the opportunity to be falsified. If science treated prediction in the manner that falsificationism suggests, then its progress would be considerably impeded.

In the discussion on the deductive approach to predictive knowledge, it has been explained that errors can arise through internal and external inconsistencies. However, the problems of falsification are compounded by the fact that deductive logic cannot distinguish whether it is the relationship between the hypothesis and the test (internal error), or the relationship between the test and the prediction (external error) that is at fault; "When we encounter an incorrect prediction, logic tells us that at least one member of our total set of assumptions is mistaken, but logic does not provide a clue as to which should be challenged," (Brown, p. 12; 1996).

The difficulty facing falsificationism is that good hypotheses which offer explanations may yield incorrect predictions, but practising scientists are reluctant to label these hypotheses as false. Rather, scientists are prepared to live with wrong predictions and make fine adjustments to hypotheses and experiments until a correct prediction results from the basic hypothesis in question. The non-specific nature of deductive knowledge means that many experiments may have to be carried out before the fault is identified; numerous apparent falsifications of the hypothesis must be endured before a correct prediction emerges.

Inductivist and falsificationist philosophies of science both feature prediction as a tool for assessing whether hypotheses are correct. However, as has been shown, neither of them have established a precise relationship between prediction and knowledge; it has been shown that predictions may be right, but the understanding is wrong or that the predictions may be wrong, but the understanding is right. Nonetheless, prediction remains a feature of these schemes. Before summarising the results and implications of this section, two other philosophies of science that were born out of the failures of inductivism and falsificationism are considered.

Imre Lakatos (1970) attempted to reformulate the Popperian approach by making the fundamental tenet of an hypothesis less vulnerable to falsification. Lakatos termed the theoretical structures that science develops 'Research Programmes.' At the centre of these structures was a hard core hypothesis that remained unchanged, but surrounding it was a 'protective belt' consisting of initial conditions, context and experimental design which could be reformulated. However the central problem remains; how do scientists know when it is the conditions (protective belt) or the hypothesis (hard core) that need altering? The central quest for philosophers of science is to provide a rational, infallible link between prediction and knowledge. Lakatos, like inductivists and falsificationists, is unable to establish what this rational basis is. Lakatos wrote of this inability to form a link between prediction and knowledge that, '... there is no instant rationality in science,' (Chalmers, p. 144; 1999). In other words, when an hypothesis is proposed, there is no contemporary, logical means by which a scientist can judge whether it will be successful or not.

Though there is no instant test by which a scientist can determine between bodies of knowledge, science must eventually progress and choose between one hypothesis or another. Lakatos (1970) attempted to provide an objective/ independent criteria by which scientists eventually regarded one hypothesis as superior to another. Initially, the 'Research Programme' which becomes established theory was that which provided predictions of new phenomena; the successful research programme has greater extent than the theory it replaces. However, the history[73] of science often shows that new theories do not result in predictions of new phenomena but offer better explanations of existing phenomena. Copernican theory, though less simple in its practice, offered a more compact explanation of planetary motion.

[73] Because of the absence of an 'instant rational basis' of science, Lakatos' scheme was formulated not as a guide for scientists, but as advice for historians of science (Chalmers, p. 143; 1999). It is therefore essential that Lakatos' conception matches with the history of science. In the case of novel predictions there are discrepancies (as above), but more importantly it is difficult to identify the hardcore and protective belts in some theories, because of the non specific nature of deductive knowledge. In the case of Copernicus, he attempted to make the moon orbit the earth rather than the sun, because in Popperian logic he could not identify whether it was external or internal consistencies that led to wrong predictions (Chalmers, p. 145; 1999). Thus he altered the hardcore hypothesis that is supposed to remain unchanged.

In light of these historical inconsistencies, Lakatos' (1978) changed the criteria by which scientists choose between tested hypotheses. The hypothesis that is to be favoured and seen as a progression in knowledge, is the one which makes 'natural predictions'; natural in the sense that hypotheses are not created to fit the data and describe the phenomena. Thus the earth orbiting around the sun in Copernicanism gives a more natural prediction, than the multitude of fitted epicycles in Ptolemaic astronomy. Similarly, Newton's elliptical orbits give a more natural explanation of planetary motion and remove the epicycles required by Copernicus in order for his theory to work. However, judging whether a prediction is more natural or not is in practice difficult. The probabilistic, counterintuitive, cockeyed (Feynman, p. 5; 1985) nature of quantum theory does not make 'natural' predictions, but makes predictions which have been forced upon it by the duality of the particle/wave experiments; that is, the theory has been devised to fit the data.

Lakatos cannot provide a consistent rational basis for the truth of an individual hypothesis or the acceptance of a given hypothesis by the scientific community, because of the difficult relationship between prediction and knowledge. Prediction features heavily in Lakatos' scheme, but once again, critics are able to level charges against his conception of how prediction works; scientists often do not know whether it is the hypothesis or the assumptions that lead to faulty predictions: new theories do not always result in new predictions of phenomena; and emerging hypotheses often result in predictions which are not 'natural' but explain the data in question.

The absence of a consistent rational basis for deciding between hypotheses, led Kuhn (1962) to formulate a new conception of science, its decision making progress, and what makes knowledge successful. In what could be described as an inductive manner, Kuhn derived the workings of the scientific community 'that can emerge from historical record of the research activity itself,' (Kuhn, p. 1; 1996). Differing from the inductivists, falsificationists and Lakatos, who had attempted to show what the ideal scheme was for determining successful predictive scientific knowledge, Kuhn's evaluation was a gritty venture into how science actually functioned and he was prepared to admit factors that other philosophers would not entertain.

Kuhn proposed that there was a phase in scientific development that could be regarded as 'normal science'. During such periods, the scientific community holds to an established theoretical base (paradigm). The theoretical base (paradigm) colours the scientists' interpretation of the world, as it is regarded as the correct understanding of the world. This belief in the current paradigm is an essential feature of Kuhn's conception of science, that aids 'in exploiting [the paradigms] scope and precision,' (Kuhn, p. 5; 1996). A failed prediction of a theoretical structure, does not invalidate it, as Popper suggested, but rather the scientific community rallies behind the current understanding so that the scientific community's "... resistance guarantees that scientists will not be lightly distracted and that anomalies ... will penetrate existing knowledge to the core," (Kuhn, p. 62; 1996).

For Kuhn, science advances when normal science reaches some point where the failed predictions of a theoretical structure overwhelm the faith that has been placed in its ability to solve them. This phase of scientific development is termed a crisis, because the current theory cannot adequately solve the problems encountered, and the whole body of knowledge that science is based on, is set to change. Importantly, it is usually only a few individuals who are working on the difficult problems that the current theory has run into, that recognise the need for alteration. Attempts to solve the areas where predictions fail, lead to a new conceptual understanding of the difficult phenomena and the possible perception of novel aspects of nature (Kuhn, p. 57; 1996).

In the time of crisis, the general scientific community must choose between two bodies of predictive knowledge; one is the new hypothesis that has led to the solution of some difficult problems and is espoused by a few scientists; the other is the established theory that has been successful in making some predictions, but has failed in some important areas. Kuhn gives some factors that may help in this decision: "Among the most useful would be: accuracy of prediction, particularly of quantitative prediction; the balance between esoteric and everyday subject matter; and the number of different problems solved ... less useful ... would be such values as simplicity, scope and compatibility," (Kuhn, p. 206; 1996).

Some of these factors have played a role in the other accounts of predictive knowledge considered in this section, but the nature of their inclusion in Kuhn's scheme differs markedly. In the other conceptions of science, these factors played rational roles; in Kuhn (1996) they are only suggestive of what may be the best hypothesis to choose. From historical case studies, Kuhn attempts to show that "... the competition between paradigms is not the sort of battle that can be resolved by proofs," nor can a choice between established theories and new hypotheses be "... forced by logic and neutral experience," (Kuhn, pp. 148 and 150; 1962). The actual decision scientists make between hypotheses is affected by "An apparently arbitrary element, compounded of personal and historical accident" and the "sociology or social psychology of scientists," (Kuhn, pp. 4 and 8; 1962).

What feature of science is it that has forced Kuhn into proffering a subjective account of its history? The answer is a now familiar one:- though scientists need to test the knowledge they have, and they fulfil this test by prediction, the relationship between 'truth' and prediction is very difficult (see Kuhn, p. 147; 1962). In terms of the revolution that occurs when one theory is replaced by an emerging hypothesis, the difficulties manifest themselves as follows: The old theoretical structure of science may be the right explanation, but it has persistently failed to solve problems due to some omission of context or conditions needed for it to fulfil its predictive requirements or because of inadequate experimental conditions. If the theory is pursued further, these difficulties will be resolved. Scientists, as discussed, have faith in the current description of nature. Alternatively, the new hypothesis may give a solution to the problems faced by the old theory, but for wrong reasons; it may, by coincidence, predict the anomalous phenomena, but have little explanatory value or application to other features of nature when subject to further testing. Kuhn describes the process of subscribing to an emerging hypothesis in the following terms.

"The man who embraces a new paradigm at an early stage must often do so in defiance of the evidence provided by problem solving. He must, that is, have faith that the new paradigm will succeed with the many large problems that confront it, knowing only that the older paradigm has failed with a few. A decision of that kind can only be made on faith."

Kuhn (p. 158; 1962)

Kuhn replaces rationalism with faith. Though rational arguments can provide pointers, the value of any given hypothesis can only be explored by establishing it as scientific knowledge and having faith that it will produce results. Only after much testing will a clearer picture begin to emerge about the worth of the theoretical structure. However, no amount of testing in Kuhn's scheme *proves* that the theory is true; it is accepted by faith as true by the practitioners of normal science. Scientific revolutions occur when apostates to the theory[74] begin to doubt and start a new theory of their own, which is also a product of faith.[75]

Critics of Kuhn's view such as Shapere (1987) and Chalmers (1999), take issue with the subjective nature that he has given science. In all the other views of science (inductivism, falsificationism, research programmes) commented on in this section, an attempt has been made to provide some explanation of predictive knowledge that is deeper than the events on which they are based, and which is testable. In other words, most philosophers of science want a theory of knowledge (epistemology) that behaves much like a scientific theory; they want a theory that is deeper than science and explains it.

Because of the problems associated with the consistency and reliability of the relationship between prediction and 'truth', Kuhn has opted out of providing a strict logical basis for deciding between different sets of knowledge and has allowed an unqualified, personal, subjective element, to enter the decision making process. The fundamental problems remain though; how do Kuhn and practising scientists determine what is good knowledge and what isn't at any given time, in light of the uncertain relationship between prediction and knowledge?

[74] "... they are men so young or so new to the crisis ridden field that practice has committed them less deeply than most of their contemporaries to the world view and rules determined by the old paradigm." (Kuhn, p. 144; 1996)

[75] Faith in a theory is the reason why Kuhn writes of competing paradigms as being incommensurable and that there is no neutral system of language or concepts in science. Scientists must commit themselves wholeheartedly to a theory if progress is to be made, and view the world through the lenses of established knowledge (see Kuhn, pp. 146 and 150; 1996).

In this section, it has been argued that science is concerned with generating predictive knowledge. Prediction, or lack of it, is the key criterion by which hypotheses, laws and theories in science and consequently, in geomorphology, are assessed. The importance of prediction has been argued in two ways. Firstly, in table 3. 2, a compilation of quotes from scientists and philosophers on the pre-eminent place of prediction in science was assembled. Secondly, in discussing the precise nature of the relationship between prediction and knowledge, most of the major philosophies of science have been discussed - prediction is central to these philosophies, which attempt to define what the process of science is. Prediction is unifying in its strengths and divisive in its weaknesses; many philosophies of science contain prediction as the tool for scientific advancement, but equally the different philosophies have arisen because of the problematic nature of the relationship between prediction and knowledge. Any theory of science that attempts to define in strict terms what this relationship should be like, is open to criticism. Similarly, any theory of science which does not define the relationship in strict terms is open to criticism.

Having reviewed the philosophies of science and keeping in mind that any concept of the relationship between prediction and knowledge is open to criticism, an opinion on this relationship is formulated here in order to provide a working definition or expectation of science and geomorphology.

The proposed view is an elementary one. An hypothesis or law is required to make predictions in order to provide information that is 'deeper than the events themselves'[76] and to be testable. Any hypothesis or law which does not make predictions is not subject to review and the scientist has little basis for subscribing to it. In Kuhn's terms, an hypothesis or law which makes prediction, requires the scientist's faith, because at some point it may be shown to be in error, either in part or in whole. An hypothesis or law which does not make predictions is akin to blind faith, where no evidence is available whatsoever and any speculation can be entertained. Predictive knowledge is the defining feature of science.

Though the relationship between prediction and knowledge is subject to profound difficulties, it can be used constructively, provided the scientist is aware of the limitations. When prediction fails, something is wrong, whether it be the hypotheses, condition, context or experiments. Hypotheses should not be discounted because they fail tests, as strict Falsificationism advocates, because the reasons for failure may be diverse. Rather, a systematic trial of the hypothesis and variations of its theme, along with analysis of experimental conditions should result in the correct premises and their consequent conclusions. This seems to be much like what science does in practice, by refining hypotheses and setting the boundary conditions for their application.

[76]See **p. 39 this thesis.**

Conversely, though correct predictions do not guarantee that everything is correct, they imply that certain parts of our understanding are. The more tests a theory stands and the more diverse the nature of the tests, the probability that our understanding is correct increases. When anomalous conditions occur, they provide occasion to revise understanding and should not be fitted ad hoc into the existing theoretical structure. The weakness of inductivist verification should mean that anomalies are particularly important and require the rigourous testing mentioned above.

In short, both inductivist verification and falsification provide important information for science. Neither is infallible and both require perseverance on behalf of the scientist. As long as this perseverance is grounded in critical analysis of the phenomena and a thorough review of all the components of science (hypothesis, context, condition, experiment), this perseverance is not based on faith, but in the objective (though complex) relationship that exists between accurate knowledge and prediction. The concept of the relationship between prediction and knowledge presented above, is used to underpin the experimental analysis in Chapter Seven.

Summary

Whilst admitting that opinion on scientific terminology and concepts is diverse, an attempt has been made to identify and qualify some components of scientific enquiry. This has been undertaken, because Chapter Two suggested that geomorphology is a science. In order to evaluate current approaches in fluvial geomorphology, some expectations of scientific knowledge must be outlined.

It was stated in this chapter, that hypotheses are the starting point of the scientific method, and though it appears that the origin of hypotheses is undecided, the form which they must take is constrained. In order to produce scientific knowledge, hypotheses generate a level of information, or a relationship, deeper than the events they describe and this relationship can be critically assessed by testing its claims. Such relationships are termed laws, and it is asserted that scientific knowledge is in the form of laws - "scientific explanation is explanation by laws." Whilst laws and their ability to be tested are the *sine qua non* or the defining feature of science, three different types of law exist to describe nature 1) Causal laws 2) Aleatory statistical laws 3) Chaos laws. All laws are causal in human cognition *at some scale* because laws by definition are invariant, known relationships between entities. Prediction is the method by which laws are tested and is essential to scientific enquiry. The relationship between prediction and knowledge is problematic, but use of the benefits of all the major philosophies of science should help in establishing whether a given hypothesis or law is the correct understanding of any given phenomenon.

The definition of science given above, which states that laws are the defining feature of scientific knowledge, is the reason why the Kittonian approach was adopted in Chapter Two. If fluvial geomorphology is to be considered as science, in terms of the definition of science given herein, it must form laws to account for the indeterminacy of alluvial river channels. Hence this thesis investigates which laws, causal, aleatory statistical or chaos, best explain alluvial channel indeterminacy and ascertains their applicability by an experimental test of the predictions they make (Chapter Six).

The formulation of causal and aleatory statistical laws was dealt with in some depth in this chapter. The next chapter considers the development of chaos laws and the central features of their behaviour.

Chapter 4

The Development of Chaos Theory

Outline

Chapter Three discussed the development of causal laws and statistical laws and suggested that both were applicable to nature. It was briefly mentioned that laws which use chaotic mathematics also applied and that these were an intermediate form of law having characteristics of both causal and statistical laws. The development of chaos mathematics and the resulting chaos laws are the subject of this present chapter. They are treated in the same detail as causal and statistical laws were in Chapter Three, because a justification of their existence and a knowledge of their development and potential implications, sets an agenda for research in fluvial geomorphology.

The chapter begins by giving a very brief historical background to the development of chaos (4. 1.). A review of the fundamental characteristics and tools of chaos follows. Firstly, sensitive dependence on initial conditions is discussed and it is suggested that this is the defining feature of chaos (4. 2.). Secondly, the concept of phase space is explored and is illustrated with reference to the three variables of continuity, sediment transport and resistance for alluvial river channels. It is noted that total energy of a system can define the maximum amount of phase space available (4. 3.). The concepts of sensitive dependence and phase space taken together explain the behaviour of chaos, termed 'strange attractors.' The most basic forms of attractors in phase space are discussed and then it is shown how equations which are sensitively dependent on initial conditions stretch and fold in limited phase space to produce complex, strange attractors. It is shown that though the location on the strange attractor of a system is unpredictable, the existence of the strange attractor is determined, or that chaotic laws exist at *some scale*. (4. 4.). The final section focuses on the author's supposition that chaotic laws could be applied to alluvial river channels through an understanding of turbulence. To illustrate the complexity of turbulence, a brief review of causal and statistical approaches to turbulence is provided (4. 5. 1. and 4. 5. 2). It is then shown how chaos has been applied to the turbulence phenomena and that turbulence is the archetypal strange attractor. Two approaches to turbulence are discussed; the Ruelle/Takens method and Feigenbaum's universality constant. Experimental evidence for these schemes is discussed as are their potential problems (4. 5. 3 and 4. 5. 4.). The chapter closes with a summary of the conceptual framework for geomorphology outlined in this thesis and the consequences of this framework for an investigation of indeterminacy in alluvial river channel cross-section and river channel planform.

4. 1. Introduction

The development of chaos laws, like the development of causal and statistical laws has been an intricate process involving many contributors, and as with causal and statistical laws, a discussion of the history of chaos laws is severely limited in its ability to do justice to all the significant milestones in the formation of this field of study. Several publications on chaos (Lorenz: 1993: Gleick; 1997: Stewart 1997) are available and the review presented here attempts to draw on common consensus from these works.

Systems with chaotic properties are generally thought to have been discovered by the French mathematician Henri Poincaré in 1890 (Percival, p. 17; 1992: Lorenz, p. 117; 1993: Stewart, p. 57; 1997). Poincaré examined the three body problem in astronomy, whereby three masses interact according to Newton's laws of motion and the inverse square law of gravity. Poincaré studied a scaled down version of this problem, in which two bodies have significant gravity effects and the third has no attraction of its own; its movements depend only on the other two large bodies. Poincaré examined qualitative solutions to these difficult equations for the motion of the small body, and in a mathematical *tour de force* that is a mainstay of present chaos analysis, took a section of the results. What this Poincaré section showed was that simple sets of deterministic equations could lead to very complicated results.

The exploration of the properties of chaos lay dormant after Poincaré's discovery, even through the quantum revolution, as the formulation of causal laws with well defined equations and solutions continued apace in non-atomic phenomena. The study of chaos was revived and perhaps had its defining discovery in 1963. Edward Lorenz, a statistical weather forecaster, produced non-periodic, complex behaviour similar to that described by Poincaré, from a deterministic system containing only three equations; one less than the Hill's reduced three body problem used by Poincaré and a system not associated with complexity (Lorenz; 1963: Lorenz; 1993). Previously, Lorenz had produced similar behaviour using a twelve variable model (Lorenz; 1962). Common to both these results was what Lorenz termed 'sensitive dependence on initial conditions' and the study of chaos and its associated laws began in earnest.

4. 2. Sensitive Dependence on Initial Conditions

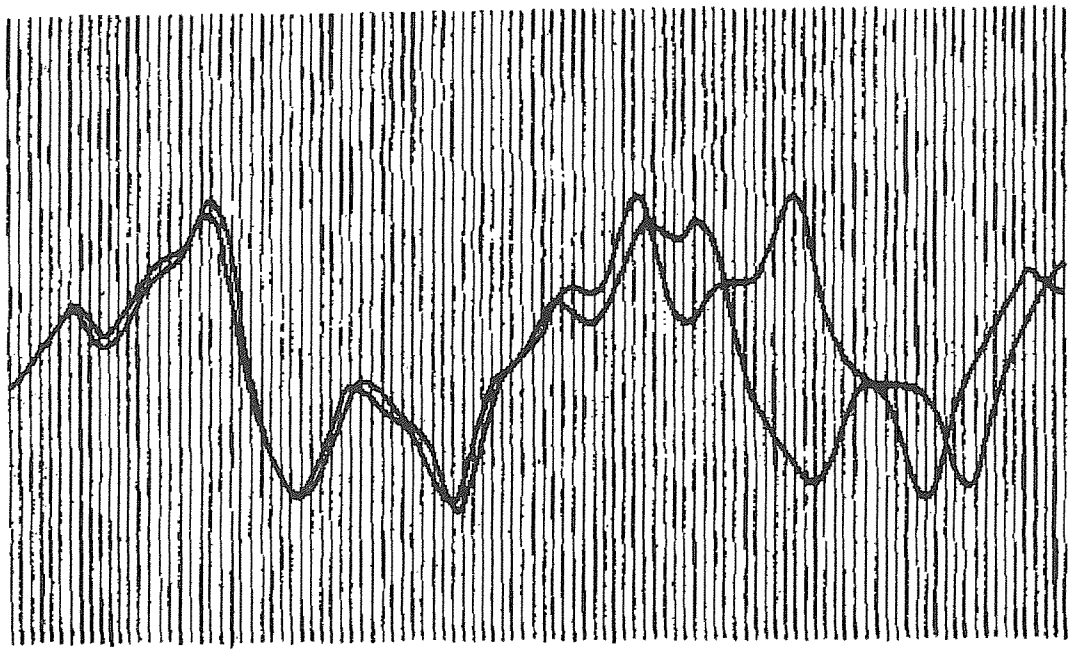
Lorenz (p. 8: 1993) writes that "... sensitive dependence can serve as an acceptable definition of chaos..." The concept of sensitive dependence is an easily understood veneer for the essential mathematical processes that occur in chaotic systems.

The discovery of sensitive dependence by Lorenz is well documented in a number of works (Lorenz: 1993: Gleick; 1997: Stewart; 1997) and only the essential elements are covered here. Lorenz was constructing numerical weather on a computer. When inputting constant data, a non-periodic time series would be produced. As long as this data was exactly the same, the time series would be the same; non-periodic - i.e. little pattern, but the same lack of pattern. Then, in a now almost fabled accident, Lorenz entered rounded off numbers from a printout, to examine a section of simulated weather more closely (0.506) when the computer used six decimal places to calculate the atmospheric changes (0.506127). After returning from a coffee break (Gleick, p. 16; 1997) Lorenz noted that the two time series differed in the manner of **figure 4. 2. 1.**

Lorenz has formalised the circumstances under which this behaviour arises, or can be detected, and his introduction to the subject gives lucid examples where sensitive dependence arises as a natural consequence of the structure of the system. The following illustration is taken from Lorenz's exposition of sensitive dependence on initial conditions (Lorenz; 1993).

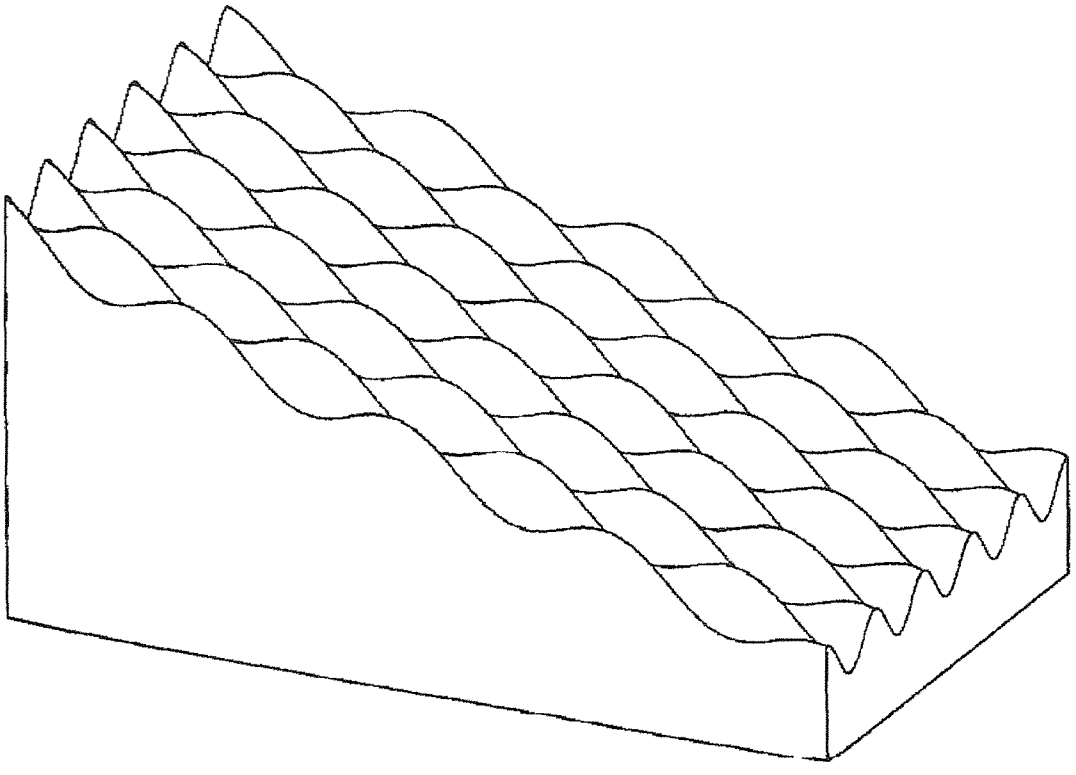
Using a set of deterministic equations, Lorenz models a mogul ski slope such as that shown in **figure 4. 2. 2.** The virtual constants of the system must remain the same, or stated alternatively, the input variables must be constant; sensitive dependence is due to interior changes within a system. The constants for the mogul ski model are: the angle of slope, the height and spacing of moguls (bumps), gravity and a selected coefficient of friction and starting velocity.

Figure 4. 2. 1. Lorenz's Weather Simulation



From Gleick (p. 17; 1997)

Figure 4. 2. 2. Lorenz's Mogul Slope



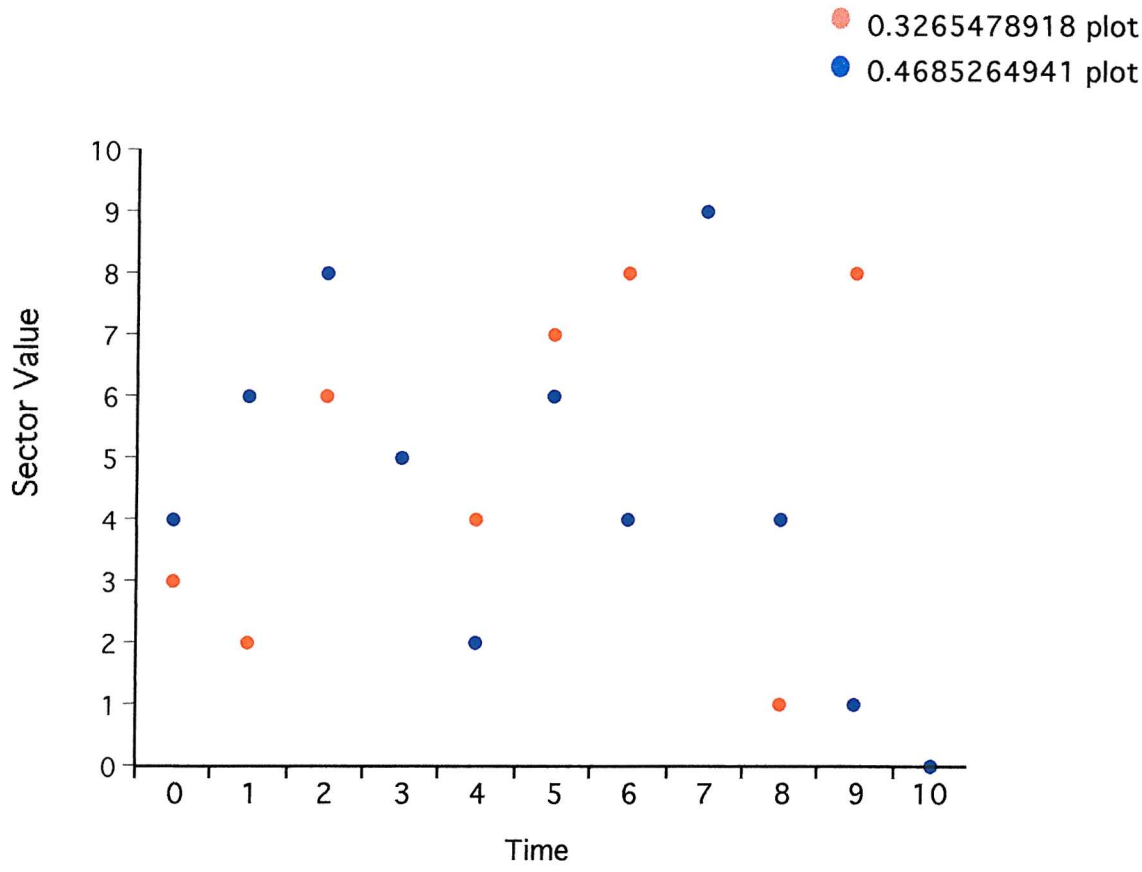
From Lorenz (p. 27; 1993)

The aim of the mogul experiment is to test whether boards that are initially close in space, deviate en route to the bottom of the slope; "... an essential property of chaotic behaviour is that nearby states will eventually diverge no matter how small the initial difference may be..." (Lorenz, p. 32; 1993). This divergence is the result of a fundamental property of the equations involved - they must possess a degree of stretching. Both Stewart (1997) and Casti (1991) use a simple model to illustrate this point. A circle with a fixed circumference of unit 1 is given, and divided into ten sectors. These sectors range from 0 to 0.0999 and 0.1 to 0.1999 etc. An important point to grasp, is that the circumference can be no more than 1 and no less than 0, so that a result must lie within one of these ten sectors. An initial starting point x is chosen and thereafter subsequent points correspond to the formula $x \rightarrow 10x$. Because the starting point is stretched by ten times with each iteration, points infinitely close together will drift apart after several repeats of the equation and end up in different segments of the circle. A table of results (table 4. 2. 1) from two different starting points is presented here, with an accompanying scatter plot (figure 4. 2. 3.) The important figure in each case is the sector value.

Table 4. 2. 1. Results for Stewart and Casti's Circle Experiment

Time	Number	Point on circle	Sector	Time	Number	Point on circle	Sector
0	0.3265478918		3	0	0.4685264941		4
1	3.2654789180	0.2654789180	2	1	4.6852649410	0.6852649410	6
2	2.6547891800	0.6547891800	6	2	6.8526494100	0.8526494100	8
3	6.5478918000	0.5478918000	5	3	8.5264941000	0.5264941000	5
4	5.4789180000	0.4789180000	4	4	5.2649410000	0.2649410000	2
5	4.7891800000	0.7891800000	7	5	2.6494100000	0.6494100000	6
6	7.8918000000	0.8918000000	8	6	6.4941000000	0.4941000000	4
7	8.9180000000	0.9180000000	9	7	4.9410000000	0.9410000000	9
8	9.1800000000	0.1800000000	1	8	9.4100000000	0.4100000000	4
9	1.8000000000	0.8000000000	8	9	4.1000000000	0.1000000000	1
10	8.0000000000	0.0000000000	0	10	1.0000000000	0.0000000000	0

Figure 4. 2. 3 Scatter Plot for Table 4. 2. 1.

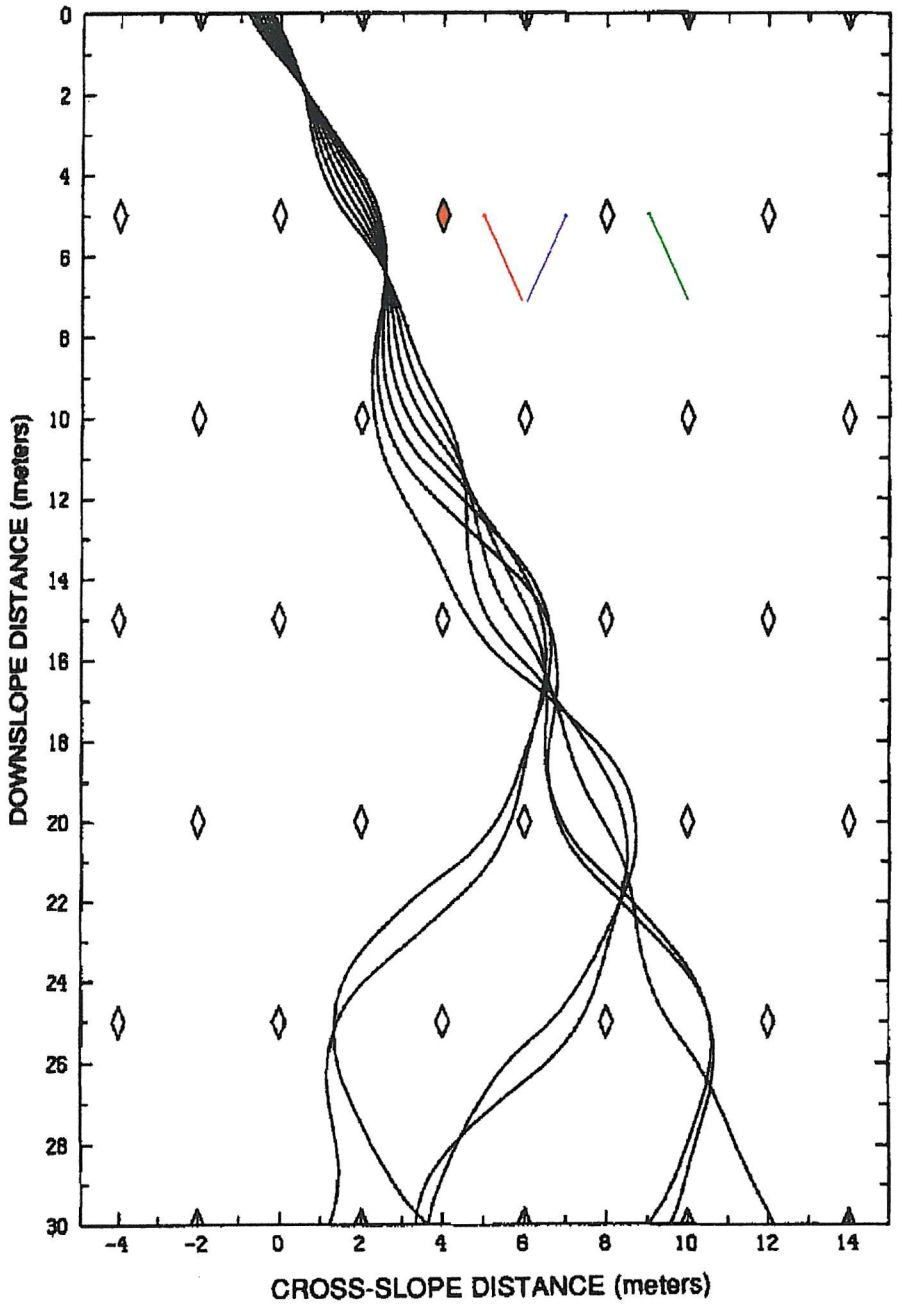


In the example above, initial values that differ by one decimal place experience transformations that lead resulting states to diverge by as much as seven units over the same time lapse. Lorenz demonstrated this stretching in the ski slope model, (figure 4. 2. 1.) and used it to illustrate several other conditions that were required to produce sensitive dependence.

A prerequisite for deriving sensitive dependence is the non-linearity of equations. In a defence which argued that non-linearity and chaos were not synonymous, Lorenz demonstrated qualitatively how the ski slope model was non - linear and gave rise to sensitive dependence (see figure 4. 2. 4.). Linearity means that an effect is directly proportional to a cause. If then a ski board is placed one metre east of a reference mogul (red diamond in diagram) it will progress south-eastward along the line of steepest descent (red line). If another board is placed five metres east of the first board, it again will be one metre east of a mogul and will follow the same steepest path south-eastward (green line).

And now Lorenz's point. If the system was linear, a board placed in the middle of the previous two would follow a trajectory that was halfway between the red and green line. Because the system is non-linear though, the middle board is one metre west of a mogul and will follow the line of steepest descent, which in this case is south-westward (purple line).

Figure 4. 2. 4. The Non-linearity of the Mogul Ski Slope



Adapted from Lorenz (p. 33; 1993)

Further consideration of sensitive dependence and the ski model illustrate why linear equations are not able to produce chaotic dynamics in real world systems. If in the ski model, a small difference was to have a large effect and the system was linear, it is clear that prohibited values will soon be met. For example, say that a difference of one centimetre in initial position led to a difference of one metre in the final state, this is sensitive dependence in a magnitude of 100 and this ratio must be preserved if linearity is present. Therefore a difference of one metre in starting conditions will lead to a final difference of 100 metres; this is outside the bounds of the ski slope. Non-linearity is needed in order for large differences to occur from small displacements in initial conditions, within limited space. In real systems, boundaries and constraints which do limit the 'phase space' are present and hence non-linearity is the only means by which sensitive dependence can occur.

4. 3. Phase Space

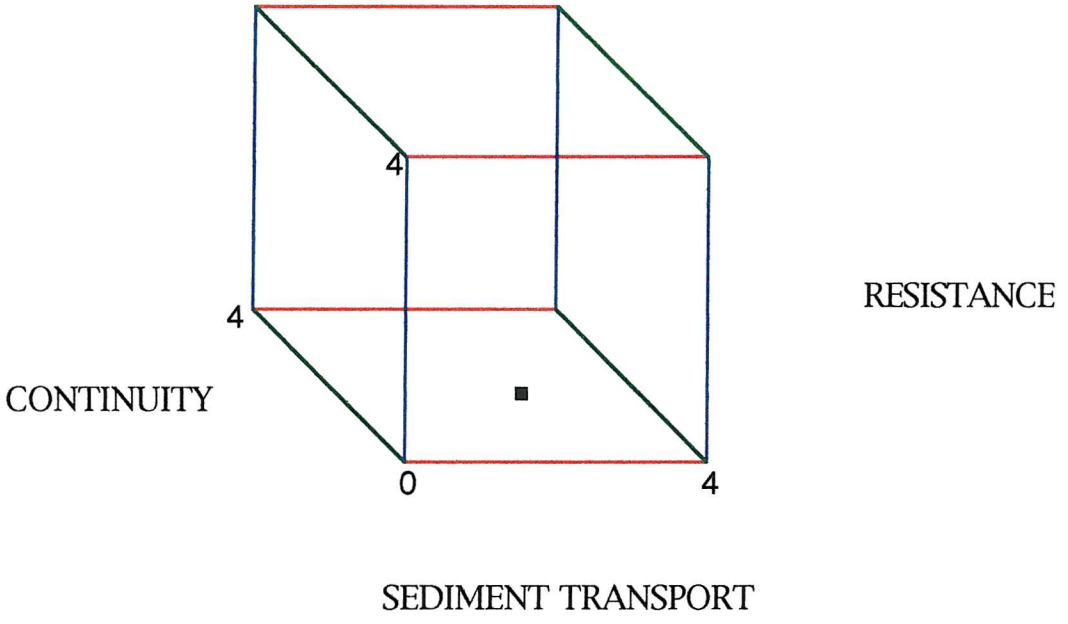
Conventional space is described by three dimensions, (height, length and width) but mathematicians have devised a way of thinking about systems that uses artificial 'spaces' of many dimensions - this mathematicians' space is called 'phase space.' Phase space is best thought of as a way of describing all the possible behaviour of a system. If we want a complete description of the ski slope model above for example, we would need to state the ski board's spatial position (height, length and width), its place in time and its velocity. If we have values for these five variables, a description of the system is complete. These variables are called the system's 'degrees of freedom' and for each degree of freedom that there is, a new dimension is added to the mathematicians' phase space.

Phase space is determined by three things 1) The number of degrees of freedom 2) The total energy of the physical system in question and 3) The constraints placed on a degree of freedom. These are dealt with in turn below.

1) The number of degrees of freedom depend on the system under investigation. Many systems will contain the conventional Cartesian spatial dimensions and a temporal degree of freedom for a complete specification of the system. The other degrees of freedom, like velocity in the ski slope model, often come from the number of ways a system can use the energy supplied to it. In Hey's (1997) model of alluvial channel systems, there are nine potential ways that the system can utilise the energy supplied to it, and so a complete description of a river channel at any given time requires nine dimensional phase space. The number of degrees of freedom depend on the scale at which the system in question is investigated; if Hey (1997) had considered the dynamics of individual sand grains then his model would have had millions of degrees of freedom.

Figure 4. 3. 1. shows the alluvial channel river system reduced to its three fundamental components (visualising nine dimensional phase space is difficult).

Figure 4. 3. 1.- Alluvial River System Represented in Three Dimensional Phase Space



The black dot describes the current state of the system (app. continuity 2/sediment transport 2/ resistance 0). In this description of the fluvial system, the river channel is supplied with a total power of 4 watts and this energy may be distributed in any combination between the three variables (e.g. continuity 4, sediment transport 0, resistance 0). Phase space is infinite, because for any given virtual constant or set of virtual constants (i.e. in this case the virtual constant is power, or 4 watts), there are infinite numerical combinations of data. Any number can be specified to any degree of precision and hence phase space is infinite.

2) The total energy of a system is important for two reasons. Firstly from a physical point of view, the total energy of the system explains the scale or magnitude in which a chaotic system operates. An alluvial river that is supplied with a constant power of 8 watts will most likely have a greater physical impact on its surroundings than an alluvial channel supplied with a constant power of 4 watts. If the dynamics (the equations that govern behaviour) of the system remain unchanged, then total energy will affect the scale at which these equations operate; in other words, total energy sets maximums for phase space (i.e. 4 in figure 4. 3. 1.) which are physically important but may be of little relevance in defining the dynamics or the workings of the system.

Secondly, energy is important because the same physical system may behave differently with different virtual constants or different magnitudes of input. That is, energy may not just increase or decrease the scale at which constant dynamics operate, but may change the nature of the dynamics themselves. Scott (p. 113; 1992) provides an example of changing dynamics with different virtual constants from chemistry. The Belousov-Zhabotinskii reaction can show different behaviour under different flow rates of reactants when using a continuous flow reactor; at low flow rates and high flow rates, the reactions have periodicity that only varies in frequency and amplitude (or the scale of the dynamics changes). In between these flow rates however, the Belousov-Zhabotinskii reaction exhibits chaotic, not periodic, dynamics. Thus the energy of a physical system may determine what kind of dynamics occur within phase space.

Energy is also important in physical systems because how it is used determines the overall behaviour of the system and its associated phase space. Some systems, termed Hamiltonian, conserve energy - that is, if a given quantity of energy is imparted to the system it remains constant and the volume of phase space is preserved. Frictionless systems (such as celestial mechanics) are Hamiltonian. The majority of physical systems consume useable energy through friction, such that if a given quantity of energy is imparted to the system, work or power (useable energy) always tend to zero - these systems are termed dissipative and the volume of phase space contracts with time, if no new inputs of energy are imparted to the system. This contraction of phase space and a tendency towards zero is an intrinsic part of the dynamics of the dissipative system.

3) Constraints placed on a system, other than virtual constants, can change the degrees of freedom, and the dynamics of a system. For example, in an alluvial channel that encounters bedrock at its bed and banks, it is possible that Hey's (1997) nine degrees of freedom model may be reduced to a five degrees of freedom dynamical model because mean depth, channel slope, width, and maximum flow depth may become constant. In this situation, five dimensional phase space would be required to describe the evolution of the system.

4. 4. Strange Attractors

"I could be bounded in a nutshell and count myself a King of infinite space"

Hamlet; Act II Scene II

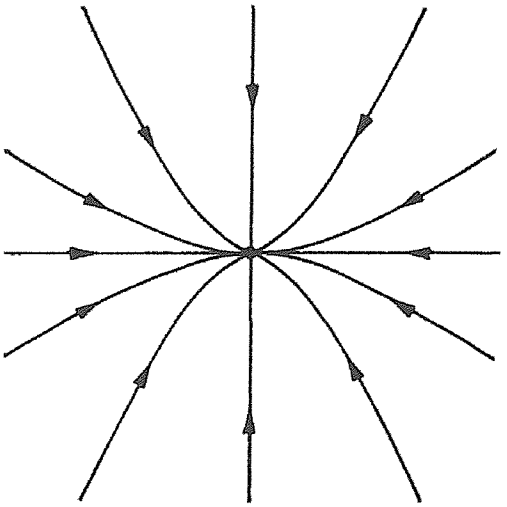
The review of the concepts of sensitive dependence and phase space presented in Sections 4. 2. and 4. 3. respectively has outlined the components of chaotic systems necessary to understanding their fundamental behaviour. This behaviour is summarised by the term *strange attractor.* It has been shown in the discussion of sensitive dependence that causal or deterministic equations produce apparently random results. However, as suggested in Chapter One, chaos laws at *some scale* are deterministic by the definition of law. As (Gleick, p. 22: 1997) has phrased it "Had he stopped with ... an image of predictability giving way to pure randomness, then Lorenz would have produced no more than a piece of very bad news." This deterministic behaviour (at some scale) of chaos systems is described by strange attractors.

Before looking at strange attractors, a review of basic attractors would be beneficial. The history of mathematics is as convoluted as the geometry it creates. Much of the inspiration for chaos, both in concept and technique, stemmed from Poincaré. His study of Hill's reduced problem, and the surprising results it generated have been covered in Section 4. 1. It was mentioned there that Poincaré's discovery was sidelined in pursuit of more manageable equations. What was not revealed was that Poincaré himself was horrified at the complicated solution to the equations, and focused attention on solvable aspects of mathematics. The disturbing complexity he observed was in a four dimensional phase space. Leaving that alone, he generated a theorem that specified the nature of attractors in phase space of two dimensions (See Stewart, pp. 49 - 63; 1997); "Poincaré and a Swedish mathematician named Ivar Bendixson, proved a theorem to the effect that 'typically' only .. four types of behaviour occur in a system of differential equations in the plane [two dimensional phase space]." (Stewart, p. 95; 1997).

These four typical forms of behaviour emerge in flows in phase space, which are defined as the collection of all trajectories and their simultaneous evolution over time. Two forms of behaviour are attractors, the other two are not. The most basic form of attractor is a sink (figure 4. 4. 1.), where the flow tends toward a single point. The sink is a steady state - a system starting off from the central point of a sink will remain there. A common analogy is that if a marble is placed anywhere in a bowl, the marble will roll to the middle of the bowl because it is the most stable place - it will not roll up the bowl by itself once in the middle. For any flow, it is possible that several sinks may exist.

UNIVERSITY OF SOUTHAMPTON LIBRARY

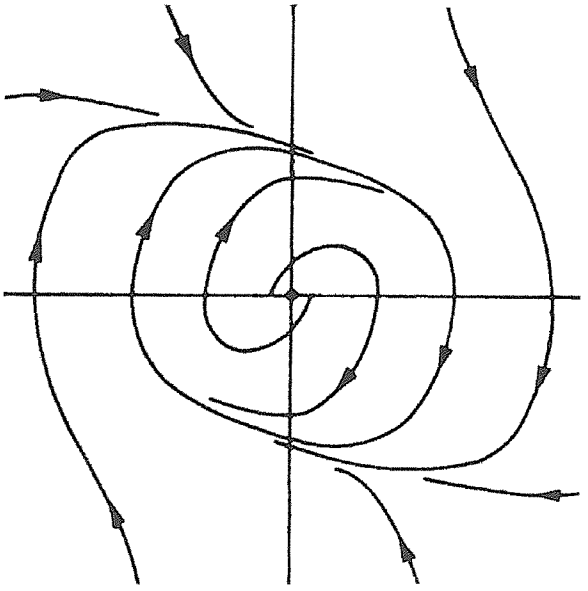
Figure 4. 4. 1. A Sink



Taken from Stewart (p. 88; 1997)

The second form of attractor is a limit cycle (figure 4. 4. 2.). A limit cycle shows periodic behaviour with the trajectories always passing through the same values of phase space with a fixed orbit. Its stability attracts other nearby trajectories towards it. Periodic behaviour is more difficult to identify, because it comprises a whole trajectory, not just a point. In real world systems the length of time needed before a loop is composed, may be the limiting factor in identifying such behaviour.[77]

Figure 4 .4. 2. A Limit Cycle.

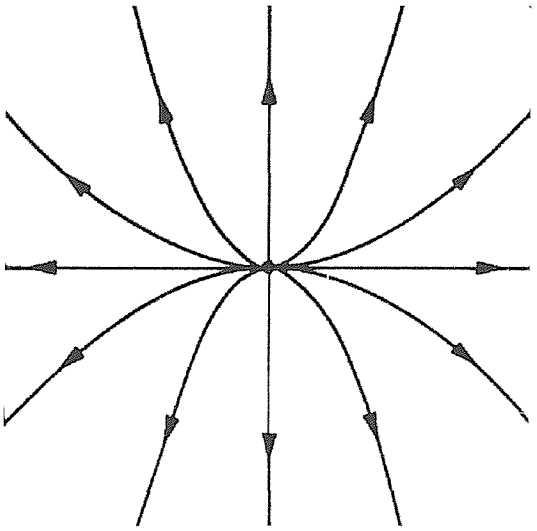


Taken from Stewart (p. 92; 1997)

[77] Thornes (1983) has considered the application of steady states and limit cycles to physical geography.

The third typical mode of behaviour in two dimensional phase space, a source (figure 4 .4. 3), is the opposite of a sink and rather than attract trajectories, it is a point in phase space where trajectories diverge. Sources are unstable states, and the same physical analogy used to describe sinks may be applied in reverse; if a marble is placed on top of an inverted bowl, it is unstable and small perturbations will set the marble in motion until it reaches a stable state somewhere at the bottom of the bowl. The bottom of the inverted bowl has a wide circumference and in terms of phase space, the variables describing the system could have many different final values.

Figure 4. 4. 3. A Source



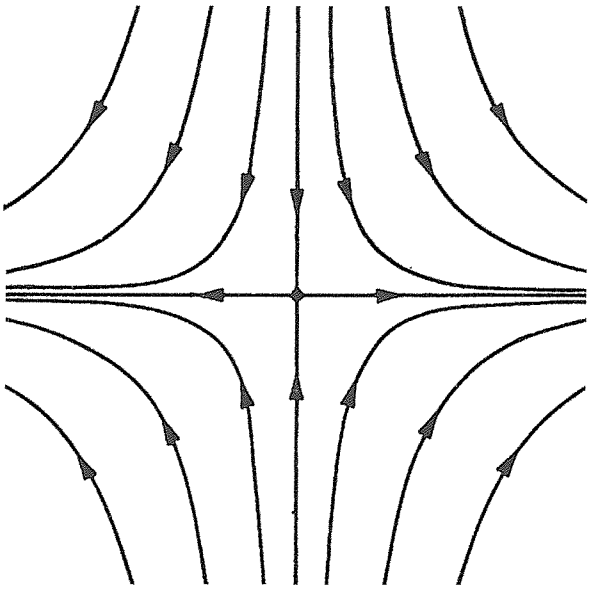
Taken from Stewart (p. 89; 1997)

The fourth state identified by Poincaré-Bendixson is termed a saddle (figure 4. 4. 4.). This is an unstable state, but with both stable and unstable directions. Stewart (1997) describes a saddle in the following terms:

"Imagine a rider sitting on a horse, on a saddle that has just been greased. If the rider moves forward or backward in the saddle, he just slides back to his central position. But if he starts to slip sideways, he topples off. His position is stable with regard to forward or backward displacements; unstable with regard to sideways ones."

Stewart (p. 90; 1997)

Figure 4. 4. 4. A Saddle



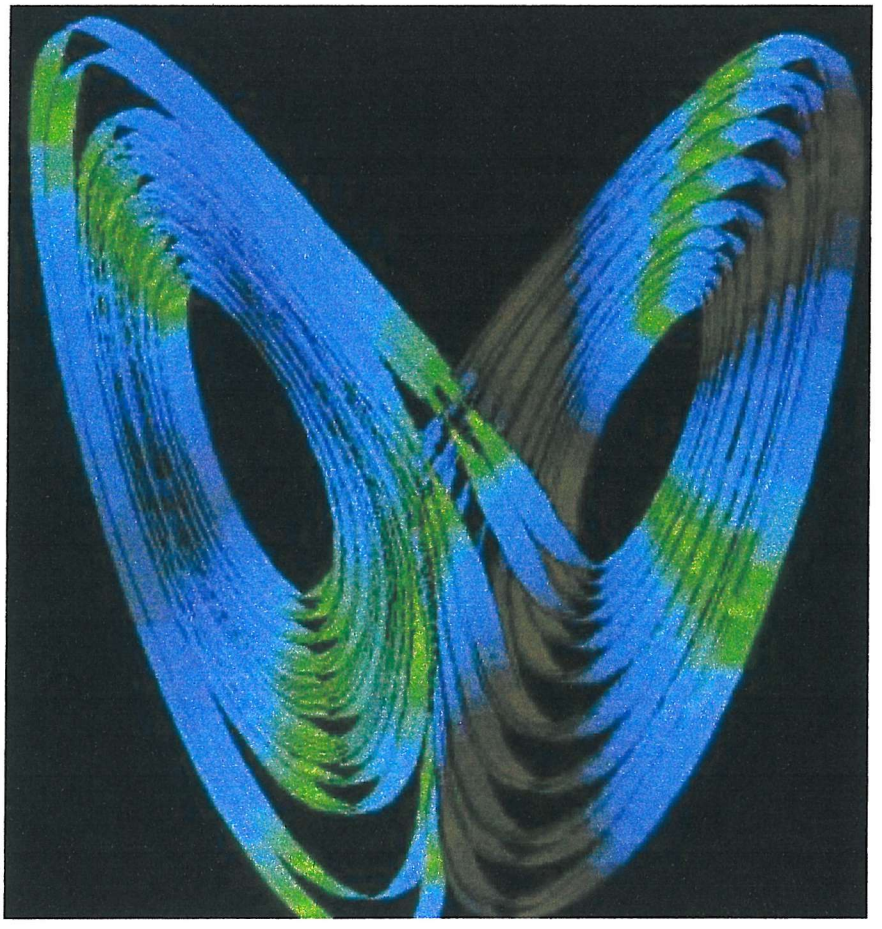
From Stewart (p. 90; 1997)

Before the discovery of chaos, behaviour in phase space and the concordant real world environment were typically constricted to these four modes. Anything else was regarded as noisy, complex and beyond analytical treatment. In three dimensional phase space, new categories appeared within this classification - two types of saddle and three types of limit cycle were possible, a total of seven possible states (Stewart, p. 96; 1997). No other forms of trajectory occurred with enough regularity to make them a bona fide species. This situation persisted despite the knowledge that the majority of nature's components cannot be described adequately by thousands of degrees of freedom (i.e. global weather), hence much more convoluted phase space. If different possible attractors existed, they were too difficult to find.

Lorenz changed the capabilities of phase space with his discovery of sensitive dependence, and discovered a new form of attractor - the so called 'strange' variety. Figure 4. 4. 5. is a graphical depiction of the three equations that Lorenz modelled and is labelled the 'butterfly attractor' because of its shape. Strange attractors follow as a natural consequence of the properties of the equations used in their construction. These equations are deterministic, non-linear, exhibit sensitive dependence on initial conditions (due to stretching) and must fit within a delineated volume of phase space (folding).

The defining property of strange attractors is the manipulation of space; they are capable of filling finite space to an infinite degree (numbers can be specified to infinite degrees of precision), by constantly folding over again and again within a boundary (maximum values are set by the virtual constants). The 'butterfly' for example has infinitely many layers, all similar but on different scales, (known as fractal)[78]. The shape does not change with size and at every scale imaginable there is a reproduction of the general form, having the same confounding abilities as the large scale manifestation. A frequently used similitude is that of Russian dolls possessing infinite lineage. Strange attractors can be described as infinite space within a nutshell. Strange attractors never intersect themselves - because of folding in phase space, the trajectory of a system of variables may pass infinitely close to a given point but not intersect it. If an intersection did occur the attractor would become periodic

Figure 4. 4. 5. The Butterfly Attractor of Lorenz



From Hall (plate three; 1992)

[78] The fractal dimension of the Lorenz attractor is 2.06 (Palmer; 1992).

The behaviour of strange attractors has an important implication. Because the attractor never intersects itself and the values of its variables are determined by equations that are sensitively dependent, the accuracy of a measurement in a chaotic system needs to be made with infinite precision if the location of the system in phase space is to be determined. This level of precision is a clear impossibility, particularly given the philosophical implications of Heisenberg's Uncertainty Principle, whereby the act of measuring changes the measurement.[79]

Importantly though, strange attractors behave as the more simple forms of attractor (sinks and limit cycles) mentioned above and do what their name suggests - they 'attract' orbits. This is why Lorenz's discovery is more than just 'a piece of very bad news;' "The chaos Lorenz discovered, with all its unpredictability, was as stable as a marble in a bowl. You could add noise to this system, jiggle it, stir it up, interfere with its motion, and then when everything settled down, the transients would return to the same peculiar pattern of irregularity as before. It was locally unpredictable, globally stable." (Gleick, p. 48; 1997). The heart of a chaotic system is surprisingly regulated. For the case of a single attractor, if a trajectory commences far away, it will eventually be drawn into the structure of the attractor. This bestows on the system what Gleick refers to as global stability. For appropriate levels of the inputs, the attractor remains unperturbed and it can be predicted with certainty that the state of the system will be found somewhere on it. As mentioned above, the limitation that sensitive dependence places on our knowledge is that we do not know where on the attractor the system will be found after several iterations of an equation.

The 'butterfly' is a model of atmospheric processes which exemplify this scenario. The weather three weeks from now may lie anywhere on the attractor, but the climate (the attractor itself) is stable. Therefore, the temperature in England won't reach a hundred degrees Celsius; the attractor's bounds do not allow for that. However, within the permissible range of variables, there can be considerable scope for difference, and if those differences are significant then problems arise. Eighty mile an hour winds have important consequences.

[79] See **p. 56 this thesis.**

Partly in deference to historical correctness, but also to reinforce the stability of attractors, the work of Smale must be considered. It was Smale (1967) who decided to prove the Poincaré-Bendixson in n-dimensional phase space, an enterprise that failed, because he came across strange attractors. The criteria by which Smale judged an alternative attractor as typical, i.e. occurring enough to make it a class of attractor and a recognised mode of behaviour, was that it was structurally stable. This meant, in effect, that the general system of equations was not affected by small changes in the initial variables (in chaos the attractor is not sensitive to initial conditions but the place of the phase point on the attractor is). The stability of these attractors is thus a fundamental property of their existence. Smale produced such a stable form by a method of topological stretching, squeezing and folding known as the Horseshoe (Smale; 1967). His proof of stability therefore involved the constituents of the other aforementioned strange attractors.

One final characteristic of strange attractors is worth noting. Sensitive dependence involves the stretching of variables within phase space. If stretching is occurring, it means that one variable is expanding at the expense of another (if the input to the system remains fixed, the values of the variables must sum to this input). In dissipative systems, the volume of phase space contracts with time as a given variable stretches and other variables contract (see Section 4. 3.). "The structure of the strange attractor is the result of exponential divergence in one or more dimensions, that for stability is necessarily dominated by contraction on at least one dimension .... As the volume in three dimensions tends to zero, the length in two dimensions may become infinite" (Culling, p. 61; 1987b). It is this infinite length which must fit into finite phase space and thus strange attractors involve folding and stretching.

4. 5. Turbulence

"Turbulence is probably the most important and yet least understood problem in classical physics."

(Mullin, p. 59; 1992)

"It is striking to note that twenty years ago little more was known about [turbulence] than at the beginning of the nineteenth century when Navier was setting down the equations governing the flow of a fluid."

(Bergé *et al.*; 1984)

It is the author's supposition that chaos laws may have a place in fluvial geomorphology's explanation of alluvial river channel cross-section and alluvial river channel planform through turbulence,[80] hence the detailed treatment given to it in this section. It is arguable that significant advances in our understanding of turbulence have only emerged through the study of chaos and this is why turbulence is considered in this section. However, to do justice to the complexity of turbulence, and to give a more thorough understanding of how chaos theory has provided a breakthrough in this difficult area of work, this review begins with the application of causal and aleatory statistical ideas on turbulence.

4. 5. 1. Reynolds' Number and Navier-Stokes

Turbulence is described as being characterised by irregular velocity and pressure fluctuations (Scheidegger; 1970: Crystal; 1990) in a fluid flow.[81] A more graphic observation is that it is, "... a mess of disorder at all scales, small eddies within large ones. It is unstable. It is highly dissipative, meaning that turbulence drains energy and creates drag. It is motion turned random," (Gleick, p. 122; 1997).

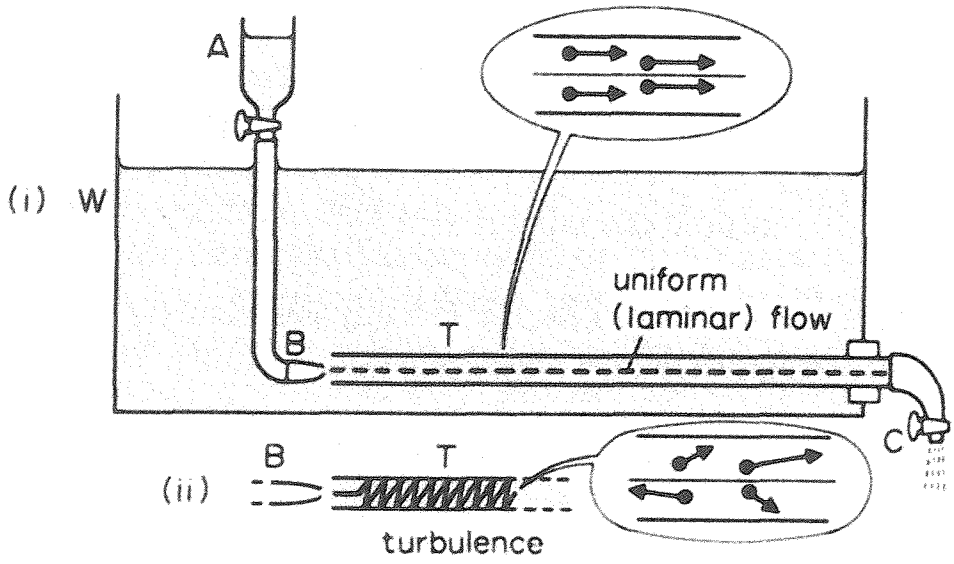
Turbulence occurs most easily in fluids which are Newtonian - substances where "...the rate of internal fluid shear is found to be proportional to the shearing force" (Williams, p. 2; 1996). A consequence of this behaviour is that stationary water will undergo deformation with the slightest disturbance. Given this property of water flows, it can be seen that orderly, coherent flow should be a rare condition, and indeed, this is the case. Water moves in a coherent body only when the viscous forces are stronger than those inertial forces causing shear in the flow. As the viscous forces are weak and any stress produces commensurate strain, only at low stress will the viscous forces be able to overcome this tendency to pull apart the flow.

[80] See **Section 5. 3. 2.** for an outline of a new hypothesis in fluvial geomorphology that is based on the chaotic nature of turbulence.

[81] Tennekes and Lumley (1972) are at pains to point out that turbulence is a feature of flows. Given a high enough Reynolds number, turbulence is not dependent on the individual properties of the substance, but on the dynamics of any moving liquid. This may have some bearing on the subject of universality discussed later.

The quantitative expression of this behaviour was given by Osborne Reynolds (1883). In a series of experiments (See figure 4. 5. 1. 1.), Reynolds observed the change from a laminar condition, (where the viscous forces organise the velocity components so that they are aligned in the direction of flow), to turbulence, (where deformations due to stress are great enough to prevail over the viscous resistance, causing parcels of water to move in directions orthogonal to the direction of flow). Stress on the system is summarised in terms of velocity - the greater the velocity, the larger the stress.

Figure 4. 5. 1. 1. Design of Reynolds Turbulence Experiment



From Nelkon and Parker (p. 146; 1977)

The equation describing this competition of forces is given as:

Equation 4. 5. 1. 1. Dimensionless Reynolds Number

$$Re = \frac{p\,v\,d}{\eta}$$

Where:

Re	=	Dimensionless Reynolds Number
ρ	=	Density
v	=	Velocity
d	=	Characteristic diameter of flow system (i.e. hydraulic radius)
η	=	Viscosity

All of the parameters are expressed in equivalent units so that the result is a pure dimensionless number (See Hooke; 1968). The cited value of Reynolds number at which turbulence ensues varies. Simons (1969) gives figures of $\leq$ 500 for laminar flows and $\geq$ 750 for turbulent behaviour, whereas Scheidegger (1970) quotes 2200 as being the region where turbulence sets in. Some understanding of these discrepancies is given by Scheidegger when he writes "...this criterion is strictly applicable to straight tubes only; in other types of flow systems the critical Reynolds number may be different" (Scheidegger, p. 57; ibid). The number differs depending on the boundary conditions where the flow is occurring; Culling, (p. 60; 1987b) reported that "It was known to Reynolds that laminar flow can be persuaded to persist up to Reynolds numbers of well into four figures."

The Reynolds equation gives a good description of the competing forces in fluid flows and the relative magnitude of those forces when turbulence begins. The Reynolds number can give an estimation of whether a flow will be turbulent or not, but it cannot say what is occurring in the turbulence. For a complete description of the turbulent state, an analytical treatment of the velocity fluctuations in all directions is required. A first principle approach to the analysis of fluid flows was established by Claude Navier and George Stokes. The set of differential equations they developed, appropriately called the Navier-Stokes equations, connect four relationships; a viscous condition, continuity, an equation of motion and an equation of state (Scheidegger, p. 56; 1970).

The viscous condition has already been considered in the context of Reynolds number; continuity is the condition that mass, however arranged, is conserved and the equation of motion consists of the velocity terms that describe the strain on the system. This leaves the equation of state to be defined. The Navier- Stokes equations assume a large control body of water, with an infinitesimal fixed volume residing within it. The equation of state refers to the starting state of the control body of water, given in terms of Cartesian co-ordinate static pressure (x, y and z) (See Williams, p. 6; 1996).

The concept of Navier-Stokes is based on simple principles. The control volume is fixed for a time *t*. The equations then relate the changes of outward mass and momentum through the faces of the infinitesimal volume; this is the velocity component expressed in Cartesian co-ordinates (u, v and w). The full set of equations with the acceleration considered is given below:

Equation 4. 5. 1. 2. Navier-Stokes Equations

$$\rho\left(\frac{\partial u}{\partial t}+u\frac{\partial u}{\partial x}+v\frac{\partial u}{\partial y}+w\frac{\partial u}{\partial z}\right)=X-\frac{\partial p}{\partial x}+\mu\left(\frac{\partial^2 u}{\partial x^2}+\frac{\partial^2 u}{\partial y^2}+\frac{\partial^2 u}{\partial z^2}\right)$$

$$\rho\left(\frac{\partial v}{\partial t}+u\frac{\partial v}{\partial x}+v\frac{\partial v}{\partial y}+w\frac{\partial v}{\partial z}\right)=Y-\frac{\partial p}{\partial y}+\mu\left(\frac{\partial^2 v}{\partial x^2}+\frac{\partial^2 v}{\partial y^2}+\frac{\partial^2 v}{\partial z^2}\right)$$

$$\rho\left(\frac{\partial w}{\partial t}+u\frac{\partial w}{\partial x}+v\frac{\partial w}{\partial y}+w\frac{\partial w}{\partial z}\right)=Z-\frac{\partial p}{\partial z}+\mu\left(\frac{\partial^2 w}{\partial x^2}+\frac{\partial^2 w}{\partial y^2}+\frac{\partial^2 w}{\partial z^2}\right)$$

Where:

ρ	=	Density
p	=	Pressure
X, Y and Z	=	Static Pressure
u, v and w	=	Velocity components

For the initial conditions, the static pressure is given in each of the three Cartesian directions. For this situation there exist six unknowns; the three velocity components (u, v and w) and the resulting pressure fluctuation in X, Y and Z. For every iteration of the equations, a new set of static pressures would be generated, as the velocity components change their values.

Though following simple principles, the Navier-Stokes differential equations can only be experimentally tested - there is no general analytical solution given the starting static pressures. The reasons for this are manifold; the equations are non-linear; though stress is proportional to strain in Newtonian fluids, the Cartesian distribution of stress is nonlinear and change can occur in any of the directions.[82] Furthermore, a solution for one co-ordinate depends on the values of the others, which in turn are dependent on the variable they are supposed to be determining. The similarities with the indeterminacy problem (Chapter One) are apparent.

[82] A fluid in a turbulent state becomes non-Newtonian and suffers memory effects (Tennekes and Lumley; 1972).

In fully turbulent flow, the idea of a control volume is seriously deficient. A prominent feature of turbulence is the lack of spatial uniformity in the flow, and in a volume of water, some areas may be experiencing velocity fluctuations, whilst others remain primarily aligned in the direction of flow; "...at the rough surface of a river ... the eddies always mix with regions of smooth flow. The vorticity is localised. Energy actually dissipates only in part of the space," (Gleick; p. 123; 1997). To define the static pressures for such a case requires that the velocity fluctuations are solved for any volume. As seen in the last paragraph this in itself is a formidable task, but added to this is the effective removal of the starting conditions. If the velocity fluctuations need to be solved to define static pressure, what is the starting static pressure on which the fluctuations are based? The problem appears to be analytically intractable without arbitrary measures or gross approximations.

The Navier-Stokes equations also suffer from the same limitations as the Reynolds number; Beven (p. 291; 1996) writes "These equations [Navier Stokes] are, however, very difficult to solve ... in specific applications, because of the poor knowledge of boundary conditions, both locally and for the flow domain as a whole". Turbulence, in addition to the fluctuations that occur within the flow, reacts to any boundary condition whether rough or smooth, and the phenomena of flow and boundary conditions combining, confounds the problem for would-be theorists of turbulence. Grass et al. (1991) noted that bursting phenomena occurred both in smooth and rough wall situations, but that "...the spanwise scale of the near-wall vortex structures, is directly proportional to the size of the boundary roughness elements" (p. 35; ibid), though with constant conditions, the growth of these structures was uncertain. In heterogeneous sediment situations, the possible complexities are obvious. The Grain Reynolds number is further evidence of the boundary layer altering turbulence structure. Used by Shields (1936), the criteria expresses the type of flow to be expected for a given velocity and grain size. It is given by the Equation 4. 5. 1. 3:

Equation 4. 5. 1. 3.

$$Re^{*} = \frac{U_{*}\, d_{g}}{v}$$

Where:

Re^{*} = Grain Reynolds Number

$U*$ = Shear Velocity (shearing force/density)$^{0.5}$

d_{g} = Representative Measure of Grain Dimension

v = Kinematic Viscosity (dynamic viscosity/density)

Williams (1996) wrote that at Re* numbers greater than 3.5, grains shed eddies and contribute directly to turbulence generation whilst for values below 3. 5 flow is smooth (p. 9; ibid). In combination with the contribution of Williams above, Kirchner et al. (1990) in a paper on sediment transport highlight the problem between turbulent flow and its interaction with boundaries. Whilst particles can generate turbulence, the shear velocity that acts on the grains is itself not uniform; "Because turbulent flow produces stochastic stress fluctuations at the bed, the probability of grain motion has no definite cut off, sampling variation makes the dependence of transport rate on stress vague in low stress regimes, and the determination of transport threshold shear stresses is thus inherently subjective" (Kirchner, p. 647; 1990) - as is the determination of how much turbulence a given grain size will produce, given that the turbulent shear velocity acting on the bed is not known.

The Navier-Stokes equations, whilst clear in principle, are hard to apply in practice. There is considerable controversy as to whether they really reach the nub of the turbulence problem. Stewart (p. 159; 1997) wrote that microscopic variations in the highly idealised equations could propagate and make the equations irregular and indeterminable. Alligood et al. (p. 539; 1996) wrote "The best differential equation approximation for a general fluid flow is afforded by the Navier-Stokes equations, whose solutions are known only approximately. In fact, at present it has not been proved that solutions exist for all time for Navier-Stokes." Contrary to these views is the report "That a microscopic system of interacting particles with oversimplified dynamics could result in the physically correct macroscopic Navier-Stokes equations [because] the Navier-Stokes equations are the consequence of appropriate conservation laws and are quite insensitive to microscopic details," (Lam, p. 35; 1998).

4. 5. 2. Statistical Turbulence

"Despite many attempts turbulence is not to be rendered measurable by averaging or other established statistical techniques, not because of the technical difficulties, great though these may be, but because the frame of reference is inadequate."

Culling (p. 346; 1987)

Whatever the merits or otherwise of the Navier-Stokes equations, it is inevitable that difficulties should be expected in the application of Newtonian deterministic methods to turbulence. Even if the assumption of an initial static volume of water is employed, the time evolution of the infinitesimal volume and the control volume (given constant stress) is too complex. The full Navier-Stokes would have to be solved for every instant.

This complexity is borne out by empirical studies. Using hydrogen bubble tracer grids and filming the movement of the bubbles in a turbulent flow, Grass *et al.* (1991) reported that "with the 132 node points and 20 Hz sampling rate ... it took in the region of 15 - 20 hours of film analysis to generate three dimensional data for 1 second of flow space," (p. 51; ibid). The stirring of a cup of coffee produces ~ 10^{12} bits of information per second (Shaw, p. 106; 1981 in Culling; 1987). Even for laminar flow without Reynolds' stresses, the full Navier-Stokes test the limits of supercomputers (Mullin, p. 60; 1992).

The difficulties in the detailed Newtonian analysis have led to the use of various statistical techniques that reduce the information content required for a solution. Most of these stem directly from the Navier-Stokes equations when time-averaged values around a mean value replace the instantaneous velocity components and fluctuations in velocity. These equations are referred to as the Reynolds' equations as they give rise to the Reynolds' stress tensor ($-\rho u'w'$ or density, mean fluctuation in Cartesian u and mean fluctuation in Cartesian w) (Williams; 1996). In order to close the equations, an expression for the Reynolds' stress tensor must be found. It is here that various models of turbulence have been proposed (Hervouet and Van Haren; 1996).

One method develops explicit evolution equations for the Reynolds' stress tensor. Within this theory, there is a choice of equations that can be used to derive the stress tensor. A problem with this model is that for every solution obtained, a new set of unknowns is generated at a higher order; thus the solution of second generation statistics gives rise to triple order statistics *ad infinitum*, much like the physically observed scaling nature of turbulence. Younis (1996) favoured this method of turbulence modelling and regarded that the approach offered "The genuine prediction of open channel flows to acceptable engineering accuracy." (p. 328; ibid).

Boussinesq's assumption is used to derive the Reynolds stress tensor by assigning them a linear proportionality to the local mean rate of strain (Younis; 1996). The stress tensor is dependent on the turbulent viscosity or eddy viscosity of the water which itself can have different definitions according to which model is used. According to the Constant Eddy Viscosity Model, for depth averaged calculations, the turbulent viscosity equals the bottom friction velocity multiplied by the water depth. For a time and space average of turbulence, this is substituted into the Reynolds' equations.

An alternative method of finding the turbulent viscosity is by Prandtl's mixing length. The concept assumes that there is a certain average distance that can be travelled before a parcel of water loses its identity by mixing. This distance is defined experimentally for a range of conditions. The last considered method of identifying the turbulence viscosity is the so called $k-\varepsilon$ model, which specifies the value in terms of turbulent kinetic energy and its dissipation rate. However it retains the need for empirically defined coefficients.

Problems with statistical turbulence are evident. Firstly, the quantity of information in turbulent flows presents problems for averaging. Although statistical methods coarsen the amount of data required, the very necessity of having to use these techniques means that much information is lost - a significant amount of error is probable, due to cumulative effects below the level of temporal and spatial resolution.

Scale causes difficulties in statistical concepts of turbulence. When sampling at a point, it is unclear what level of turbulence is being sampled - a velocity reading could be composed of first order Reynolds stress, and second or fifth order decay cycles. The mean flow value is both a cause and a result of these interchanging cycles and is not independent of them, as is the eddy viscosity. Defining the average mixing length for a single streamline would appear practically fraught with difficulties and would require boundary conditions so confined that they would invalidate the result. In a river, mixing length must be altered by conflicting mixing tendencies from other eddies, resulting in a wide range of values for eddy persistence and very general Prandtl mixing length values.

The use of linear relationships for the "... non-linearity of the convective terms of the Navier-Stokes equations" (Hervouet and Van Haren; p. 191; 1996) seems problematic. The Reynolds tensor terms (average velocity fluctuations) are these convective terms averaged over time and the process of averaging does not remove this non-linearity. Turbulence is quintessentially anisotropic (Younis; 1996) and apart from simplicity, the use of 'arbitrary linearisation' (Church; 1996) has little justification. As a final note, the fact that boundary conditions do not just modify a well defined theory, but rather are an intrinsic part of the problem, has meant that no general theory of turbulence has been derived.

This section opened with Culling's (1987) statement that the statistical frame of reference is inadequate for turbulence modelling. The import behind that statement has partially been expounded above. Current analytical models rely on the idea that there is a normal flow line from which Reynolds stresses are generated in directions contrary to this mean flow direction and the use of averages can filter out this behaviour. However the flow lines themselves are an intrinsic part of turbulence - "Turbulence *is* the confused hierarchy of eddies. In detail the flow pattern is unmanageably complex and refuses categorically to sit in a regular straight jacket" (Culling; p. 350; 1987). Aleatory statistical approaches based upon linear deviations, can produce ranges of values for the onset of turbulence. However, aleatory statistical laws, based upon linear deviations appear inadequate to explain turbulence where the non-linear convective terms of Navier-Stokes are the behaviour of the system. Chaos has provided an explicitly non-linear formulation of turbulence and attention is now turned to this development.

4. 5. 3. *The* Strange Attractor.

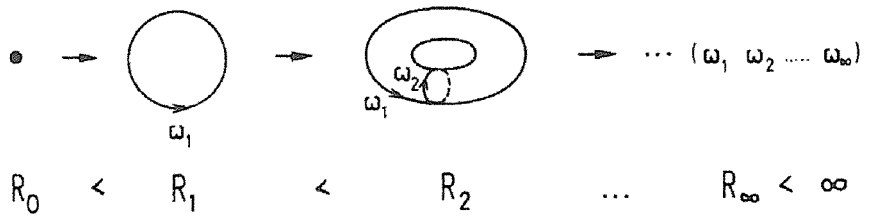
...if there were no such thing as a critical Reynolds number and water and air flows over the surface of the globe remained in a laminar rather than their normal turbulent state ... a modest scale river such as the Thames ... would be transformed into a shallow, swiftly moving sheet flow a fraction of a metre deep and with surface velocity commensurate with that of a motor car ... paradoxically therefore, turbulence actually promotes tranquillity."

Grass *et al.* (p. 35; 1991)

In 1971 two mathematicians brought the study of chaos to bear on the problem of turbulence and the strange attractor was thus called for the first time. David Ruelle and Floris Takens (1971) proposed a topological analogy of turbulence which suggested that complex motion of a fluid could be produced in a system with just three degrees of freedom; the high information content of turbulence could be reproduced from simple deterministic equations.

The established theoretical understanding of how turbulence arose in liquids with changes in Reynolds' number, formed the foundation for the statistical treatments shown above. In the Landau-Hopf theory, turbulence is viewed as the successive disturbance of an underlying laminar flow. A steady state condition of constant velocity is transformed to a periodic velocity, by means of a Hopf bifurcation. As the Reynolds number is increased further, the new periodic velocity regime loses its stability and another single period of different time occurs. This period is incommensurate with the previous one. With increases in the Reynolds number, new periods of velocity enter faster and faster and in accordance with the dissipation of energy, the new periods and the flows they represent become progressively smaller (Landau and Lifshiftz, p. 106; 1959). The process occurs until, at the limit, infinite periods of different time are present. Figure 4. 5. 3. 1. shows what is happening in this situation.

Figure 4. 5. 3. 1. Formation of Different Period Velocity with Increases in Reynolds Number.

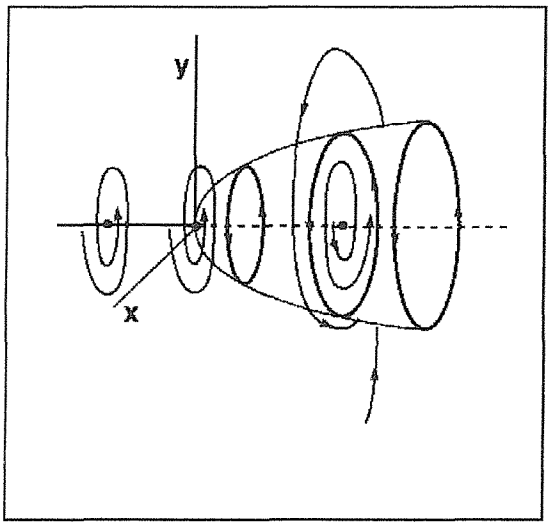


From Schuster (p. 147; 1995)

As the Reynolds number increases in the above diagram, the velocity changes from a steady state, to a periodic state and finally to the quasi - periodic torus shape. At fully turbulent flow, this torus would be a mass of periods. As each new period corresponds to another degree of freedom in phase space, fully turbulent flow in the Landau-Hopf scheme is the product of infinite degrees of freedom. True to the Poincaré-Bendixson theorem discussed above, Landau and Hopf limited their scheme of turbulence to those class of attractors that were known at the time to be stable and typical. In summary then, the theory proposed that whilst the periods themselves may be easily comprehended, the sheer number of them precluded any analysis of fully developed turbulence.

As stated above, Hopf bifurcations always have as their reference the R_0 condition or the steady state, which in this case, is the laminar state "In a Hopf bifurcation, a family of periodic orbits bifurcate from a path of equilibria," (Alligood, p. 483; 1996). This is shown diagrammatically in figure 4. 5. 3. 2. The Hopf bifurcations are independent of each other and are relative to the steady state. The use of Hopf bifurcations to give a theoretical explanation of turbulence, naturally supported statistical views of turbulence which treat pressure and velocity fluctuations as distributions centred around a laminar flow; these fluctuations arise from perturbations of the laminar flow.

Figure 4. 5. 3. 2. Deviation of Landau - Hopf Bifurcations from Path of Equilibria

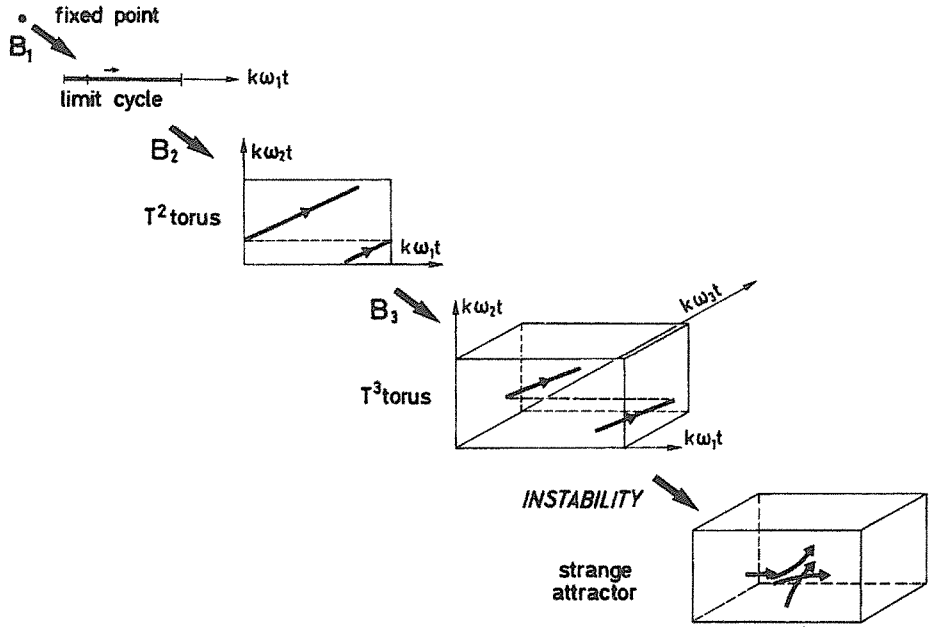


The limit cycles generated by the Hopf bifurcations continue to orbit the steady state (the dashed line). The steady state corresponds to laminar flow and it is supposed that regular behaviour underlies the perturbations.

From Alligood *et al.* (p. 484; 1996)

Ruelle and Takens (1971) used the Landau-Hopf theory as a starting point for their concept of the strange attractor. Critical to the development of a chaotic view of turbulence was a dissatisfaction with the stability of quasi - periodic tori (Ruelle; 1980). The new theory concurred with Landau to the extent that two Hopf bifurcations were feasible, but with the addition of the third period, it was possible to destroy the quasi - periodicity and replace it with a completely different form of attractor. This final stage, where the strange attractor emerges, changes the mathematical nature of the flow itself. No longer is the periodicity superimposed on top of a steady state or laminar flow, but the attractor is the flow itself; i.e. in agreement with Culling (1987), the seemingly orderless velocity fluctuations are the steady state. Figure 4. 5. 3. 3. shows the transition of the flow in terms of phase space.

Figure 4. 5. 3. 3. Formation of a Strange Attractor Using Three Dimensions



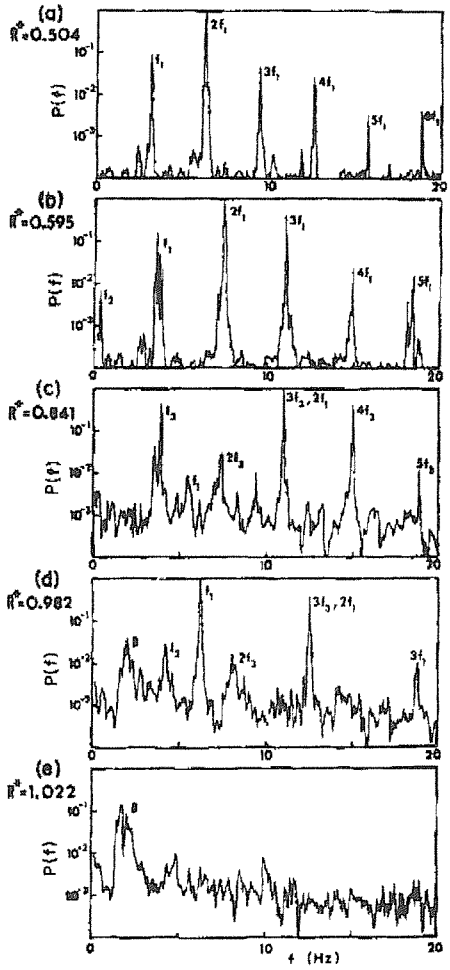
From Bergé *et al.* (p. 164; 1984)

With each new degree of freedom that a different period imparts, a disparate type of behaviour becomes possible. The final state, which was named the strange attractor is the condition of interest here. In topological terms, Ruelle and Takens (1971) demonstrated that turbulence exhibits all the properties commensurate with the Lorenz attractor; being sensitively dependent on the initial conditions and exhibiting structural stability. All such attractors that possess this form of behaviour have adopted the name 'Strange' after this paper, making turbulent flow the archetypal 'Strange Attractor'.

In a similar vein to Lorenz, Ruelle and Taken's model was abstract and was not related directly to the physical equations that govern turbulence - consequently viewing a strange attractor in physical experimental conditions was unlikely. Two techniques have persuaded the scientific community of the existence of the Strange Attractors in physical turbulent flow. The first was a series of experiments that disproved the Landau concept of turbulence. Distinct limit cycles of given period should correspond to a clear frequency in a power spectra analysis of turbulence data. If the Landau theory is correct, multitudes of these frequency signals should pile up as the Reynolds number is increased.

An experiment performed by Gollub and Swinney (1975) using new optical measuring techniques on the Taylor- Couette system, revealed that the expected frequency changes of Landau only occurred up to the addition of the third period. Subsequently, no new period was produced and broad band power spectra were observed (figure 4. 5. 3. 4.). Though this behaviour was expected of turbulence, even in the Landau scenario, it was thought that it would be composed of many distinct frequencies merging together and its appearance after three periods did not fit the Landau scheme at all.

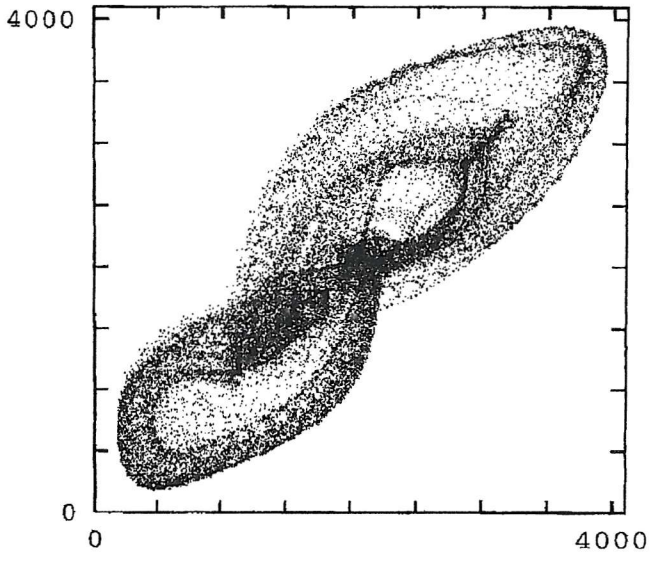
Figure 4. 5. 3. 4. Power Spectra of Gollub and Swinney's Turbulence Experiment.



From Gollub and Swinney (p. 144; 1975)

The period succession follows the Landau scheme until the power spectrum shown in (d), where the three period sequence becomes unstable and forms the broad band power spectrum associated with fully developed turbulence. Though "... perhaps consistent with the proposals of Ruelle and Takens" (Gollub and Swinney, p. 927; 1975), the experiments did not conclusively prove the existence of the strange attractor concept. A more powerful proof would be to generate a strange attractor from the physically appropriate equations - the Navier-Stokes - but this remains difficult. Instead of first principle evidence, Crutchfield *et al.* (p. 54; 1986), introduced a mathematical method that would reveal an attractor's strangeness or otherwise, that was independent of the variables used. If in the case of the Taylor Couette system only one variable was measured (i.e. velocity), the chaotic behaviour of the system could not be known, as it requires three variable phase space. To counter this, it was proposed that if the time series of velocity measurements were delayed and then run in sequence, the structure of the attractor would appear. The second measurement in the time series would become the first measurement for the first 'fake' time series and the third measurement in the time series would become the first measurement for the second 'fake' series (Stewart; p. 172; 1997). The technique is independent of which variable is used and in this manner, it was shown that Taylor Couette flow possesses a strange attractor (**Figure 4. 5. 3. 5.**). Turbulence appears to have the attendant properties of chaos aforementioned.

Figure 4. 5. 3. 5. The Taylor-Couette Time Delayed Strange Attractor



From Alligood *et al.* (p. 189; 1996)

The surprise about turbulence is that it can be created from a few degrees of freedom and simple deterministic equations. Chaos seems an appropriate tool for the study of the phenomena, as it combines the unpredictability and infinite variety of behaviour on the attractor, whilst retaining a definite level of order in the attractor itself. This order may correspond to the frequently cited forms such as eddies and bursting phenomena that are reproducible under experimental conditions. Furthermore, sensitive dependence has as its basis the heart of the turbulent environment; non-linearity. Chaos does not view Reynolds stresses as linear perturbations of a true flow, but rather an integral part of what turbulence fundamentally consists of. This frame of reference has to be preferred.

4. 5. 4. Universality

"Universal numbers $\delta = 4.6692016...$ and $\alpha = 2.502907875...$ determine quantitatively the transition from smooth to turbulent or erratic behaviour for a large class of non-linear systems."

Feigenbaum (p. 4; 1980)

The relationship between turbulence and chaos is a mutual and a fruitful one. Ruelle and Takens studied turbulence and provided a result which showed how systems that obeyed chaos laws could be formed from simple equations and they showed that turbulence was in itself a chaotic system - turbulence explained elements of chaos and chaos explained elements of turbulence. However Ruelle and Takens are not the only scientists to have exploited this mutual relationship with turbulence. Mitchell Feigenbaum studied turbulence and derived a qualitative solution for a whole class of non-linear equations. Feigenbaum's results are important because they reveal a different aspect of turbulence and chaos and they are considered below.

In the 1970's, Feigenbaum was considering turbulence and the non-linearity of the Navier-Stokes' equations, (Feigenbaum, p. 68 ; 1980: Stewart, p. 196; 1997: Gleick, p. 158; 1997). Rather than attempting to study the full complexity of the Navier-Stokes Equations, Feigenbaum attempted to understand the general behaviour of non-linear equations and began examining the logistic equation, one of the simplest non-linear equations. An iterative form of the equation which uses values of k between zero and four is given in equation 4. 5. 4. 1.

Equation 4. 5. 4 .1 - The Logistic Difference Equation

$$x_{t+1} = kx_t(1 - x_t)$$

Where:

X_{t+1} = value of variable x at time t.

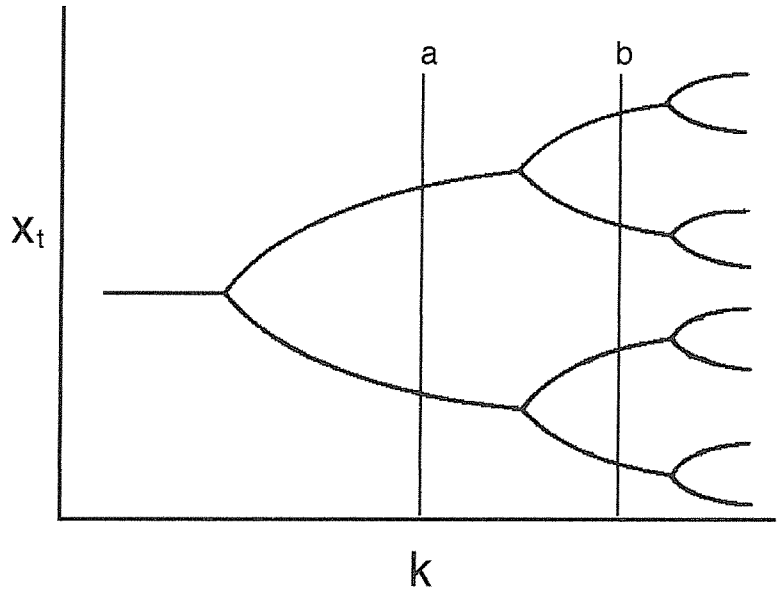
k = constant between 0 and 4.

For different values of the constant k, the logistic difference equation behaves differently when it is iterated. For values of k below 3 the equation is attracted to a steady state; when K = 3. 2 - 3. 5 the equation generates two values of x_t such that the equation gives the same value after every two iterations. This can be expressed by a pattern such as RLRLRL. This pattern is termed a period two cycle because the system comes back to the same state after two iterations. At K = 3. 5 the period two pattern becomes unstable and splits into a pattern where the system returns to the same state after every four iterations; i.e. RLFBRLFBRLFB - this is a period four cycle. At k = 3. 56 a period eight pattern appears such that the system only returns to the same state after every eight iterations.

The alteration the system undergoes with changes of k is termed a period doubling cascade because the interval needed before the system returns to the same state progressively doubles. The period doubling cascade is shown by the graph in figure 4. 5. 4. 1. Any vertical section of this graph shows the number of values of $x_{t\,1..n}$ that are possible for a given value of k. The more values there are, the higher the period, or the longer it takes before the logistic difference equation reaches the same state. For example, at intersect 'a' on figure 4. 5. 4. 1. there are two values for x_t and at intersect 'b' there are four values for x_t.

At k = 3. 58 the period required before the system returns to its original state has doubled infinitely often; in other words, at k = 3. 58 the system will take infinitely long to reach the same state. This infinite period is similar to a hallmark of strange attractors, mentioned above; points may pass infinitely close to previous ones without intersecting them.

Figure 4. 5. 4. 1. - Period Doubling for the Logistic Difference Equation.



Feigenbaum had begun looking at turbulence with a clear aim in mind; he wanted to apply the ideas of renormalization in physics to the problem of turbulence. Renormalisation, a technique that had been used to study non-linear phase transitions or changes between the state of matter such as liquid to gas, was 'based upon the idea of self-similarity, the tendency of identical mathematical structure to recur on many levels,' (Stewart, p. 184; 1997). Gleick (p. 162; 1997) surmises Feigenbaum's reasoning:

"[Renormalisation] worked ... enough to inspire some physicists, Feigenbaum included, to try it on the problem of turbulence. After all self-similarity seemed to be the signature of turbulence, fluctuations upon fluctuations, whorls upon whorls,"

Gleick (p. 162; 1997)

When experimenting with the general behaviour of non-linear equations using the logistic difference equation, Feigenbaum discovered that self similarity was indeed present in this equation. The story of Feigenbaum's discovery is well documented in (Gleick; 1997 and Stewart; 1997), but in essence Feigenbaum found that the individual branches of the graph in Figure 4 .5. 4. 1. obeyed scaling ratios, that asymptotically approached precise values. In other words, the values of k at which period doubling occurred, and the values of x_t for those values of k, could be predicted at higher periods. The scaling ratio in K was 4. 66920166 and the scaling ratio in x_t was 2.502908. Each bifurcation branch was self similar, so that if the smallest end of the chain was magnified by the above constants, it would be a replica of the previous bifurcation.

Percival (p. 43; 1992b) summarises the implication of these 'Feigenbaum numbers':

> *"When the periods are sufficiently high the magnified structure for one orbit, period 2048 for example, is indistinguishable from the structure for the previous period, in this case 1024, provided that you carry out the magnification to a precise specification; the magnification in [k] should be 4. 66920166 and the magnification in [x_t] should be 2.502908."*
>
> Percival (p. 43; 1992b)

Feigenbaum tried other non-linear equations to see whether a scaling ratio was present (Gleick, p. 173; 1997: Stewart, p. 187; 1997) and to his surprise, the same constants (4.66920166 and 2.502908) appeared in all his investigations; these constants were not just properties of the logistic difference equation but were universal constants for a whole class of non-linear systems; "Feigenbaum found that the same numbers and the same structure appear for all sufficiently smooth functions $f(x)$, whose graph has only one maximum. So these numbers like π, are universal," (Percival, p. 43; 1992b).

Based on this general behaviour of non-linear systems, Feigenbaum proposed a new route to turbulence from laminar flow, which went through period doubling to chaos, and had universal scaling constants. Feigenbaum's route to turbulence fundamentally differs from Ruelle and Takens, because unlike Hopf bifurcations it involves instabilities in periods that give rise to new bifurcations emanating from the unstable, established period source, not an equilibrium value. Put succinctly, the periods that arise in period doubling are dependent on the periods before them, whereas in Hopf bifurcations they are independent.

Feigenbaum's conjecture that this period doubling behaviour may be present in the transition to turbulent flow, needed experimental evidence which showed that period doubling occurred in line with the universal constants discovered in the logistic equation.

Experimental support for the period doubling route to turbulence was provided by Libchaber and Maurer (1982). They performed convective experiments on Helium in a small box, which generated turbulence in a stationary liquid. They concluded that "The qualitative picture proposed by Feigenbaum seems to be correct. Quantitatively there are discrepancies," (ibid p. 134). The experiment generated Feigenbaum's constant δ at 3.5 ± 1.5, a figure partly due to bad experimental precision and the fact that only the first bifurcation was observed (Feigenbaum's constants are asymptotic).

In addition, other forms of bifurcations occurred such as three and five divisions - a scheme that Feigenbaum's constant does not allow for. The results of the experiment did indicate constant scaling and led to further empirical studies that improved on the accuracy of the measurement. In a damped system of a mercury cell that favoured period doubling, Libchaber *et al.* (1982) derived a figure of 4.4 ± 0.1 for Feigenbaum's constant. Succeeding these, various experiments have obtained a greater approximation of the constant in various media (Swinney; 1983: Cvitanovic, p. 29; 1984).

Feigenbaum's discovery of universality of turbulence strengthens the argument that the emerging laws of chaos are providing breakthroughs in this difficult area of study. However, there have been challenges to the validity of universality and these are discussed below.

Firstly, the extent of universality has been seen as applying to the phase transition from laminar to turbulent flow, and not dealing with the fully developed turbulent condition (Eckmann; 1981). The difficulty with the condition of fully developed turbulence is that it appears to take the form of a strange attractor, where the sheer number of period doublings involved results in broad band noise and difficulties in ascertaining a universal constant. It is not yet known whether attractors continue the period doubling that may be experienced *en route* to turbulence. Franceschini and Tebaldi (1981) writing on their inability to find the Reynolds' number at which period doubling exhausts itself, state that; "The existence of more infinite sequences of stable periodic orbits in very small ranges of r or with very long periods then cannot be rejected," (Franceschini and Tebaldi, p. 394; 1981).

Part of the problem of strange attractors is that they may contain many degrees of freedom, and another criticism that could be levelled against the Universality theorem is that it applies to only one dimensional systems. Two related ideas may be used to counter this claim and provide support for scale universality of turbulence. The first is numerical evidence which suggests that Feigenbaum's constant persists in multidimensional scenarios. Using a truncated version of the Navier-Stokes equations, Franceschini and Tebaldi (p. 391; 1979) noted that the scaling parameter of Feigenbaum was present in their data; "Our results make possible the hypothesis that universal properties of one dimensional mappings also hold in dynamical systems with infinite sequences of bifurcations. The fact that complicated n-dimensional phenomena possess in some sense one dimensional behaviour appears significant and very suggestive."

Secondly, much work has pointed to the contraction of multidimensional phase space to effectively one dimension. This concept has already been expounded in the consideration of strange attractors for dissipative systems (Section 4. 4.). It was noted that for the attractor to become infinitely long, one variable had to expand at the expense of another, such that the other variables contracted as energy was drained from the system. This reduces the system down to one dimension, so that even if universality cannot be identified in multidimensional systems, it makes its absence less relevant; the system effectively behaves in a one dimensional manner - Collett Eckmann and Koch (1981) have given a detailed mathematical treatment of higher dimensional systems becoming contracted to one variable.

Finally, the difference between Ruelle and Takens' approach to turbulence and that of Feigenbaum needs to be discussed. If turbulence can be generated in various ways, doesn't this limit the universality that Feigenbaum found and indeed limits, or at least complicates, the application of chaos to turbulence? The fact that turbulence can be generated by different means, is in itself disappointing because it forces the question of which route to turbulence applies in any given real world situation?

Only a partial answer to this problem can be given at present. Work has shown that Ruelle and Takens' method may have similarities with Feigenbaum's method and perhaps points to a more fundamental law at work. In particular, Ruelle and Takens' route to turbulence (reduced to two dimensions by a Poincaré mapping) may also have universality or self similarity, but the constants are different to that of Feigenbaum, because they apply to a different family of mappings (Shenker, p. 410; 1982). Cvitanovic (p. 34; 1984) has highlighted the difficulties of validating this number experimentally.

Summary

A brief review of chaos laws has been given. The fundamental concepts of sensitive dependence and phase space have been outlined. The intermediate position of chaos laws between causal and statistical laws has been shown; chaos laws are determinate, but their behaviour is unpredictable due to sensitive dependence on initial conditions - the behaviour of chaos laws only becomes regulated and predictable at the scale of the strange attractor, which is a stable system, but which is much more complicated than other forms of attractor such as sinks or limit cycles.

It has been highlighted that breakthroughs in the study of turbulence have been made by the study of chaotic systems. The development of turbulence has been found by Ruelle and Takens to be described by the interaction of three variables and Feigenbaum has suggested a method for producing turbulence from just one variable. Both these methods suggest that fully developed turbulence is described by a strange attractor and that the laws of chaos can offer some explanation of this complex phenomena. There is considerable experimental data that supports both these schemes for turbulence. Chapter Five proposes the hypothesis that because fluid flows in nature are nearly always turbulent and turbulence has been shown to be a chaos phenomenon, chaos laws may provide valuable insight into the indeterminacy of river channels.

Having considered that geomorphology is science (Chapter Two), that science forms laws and that three types of law are possible, namely causal, statistical and chaos, and having presented a review of the development and concepts of these laws (Chapters Three and Four), a review of the application of these laws in fluvial geomorphology to alluvial river channel cross-section and river alluvial river channel planform is the subject of Chapter Five.

Chapter 5

The Application of Laws to Alluvial River Cross-Section and River Planform

Outline

Chapters Two to Four have argued that geomorphology is a science and that scientific knowledge is defined by laws. There are three types of law - causal, aleatory statistical and chaos - and the fundamentals of these laws have been outlined. The solution to indeterminacy in river channels must be in the form of an appropriate fundamental law and the specification of the boundary conditions required in order for that law to apply (so that potential sources of error or variance may be eliminated). In line with the Kittonian view expressed in Chapter Two, this chapter examines the application of process laws to alluvial river channel cross-section and planform morphology. The chapter begins by considering causal laws that have been applied by the school of 'characteristic form.' Specific laws such as hydraulic geometry and power relations for channel planform are considered (5. 1.). It is suggested that in order for causal laws to be applied to indeterminate alluvial channel form, four assumptions are required (5. 2.). Of particular note is the use of mean values for the controls and effects, which imply that causal laws in geomorphology are really epistemic relations between aleatory entities[83]. The chapter goes on to examine the application of chaos laws to alluvial river channels. It is suggested that several precursors to chaos laws exist in the fluvial literature, and recent approaches are discussed. It is proposed that most recent studies of chaos have focused on the general identification of chaos in topographic forms, and have not considered the specific processes or laws by which chaos influences geomorphology (5. 3.). The chapter considers the idea of Lorenz and Culling, that chaos is present in systems by their association with phenomena which are governed by chaos laws, and based on this principle, a new hypothesis for chaos laws in alluvial channel systems is proposed. Because nearly all fluid flows in nature are turbulent, it is proposed that chaos laws could apply to river morphology because turbulence has been shown to be a chaos phenomenon (5. 4). The chapter closes with a review of the argument presented in this thesis; having considered that geomorphology is a science, that science explains by laws, that three types of laws exist in nature and presenting a review of their previous application in geomorphology and suggesting a new hypothesis for the application of chaos laws, the remainder of the thesis is written to test these laws for alluvial river channel cross-section and river channel planform in the manner required by science - i.e. by the predictions they make (summary).

[83] See **Section 3. 4. 3. 2.** on causal laws as fundamental to cognition.

5. 1. The Application of Causal Laws in Fluvial Theory

"It is convenient to consider river flows as causes and river forms and other river phenomena as their effects."

Yevjevich (p. 2-3; 1969)

"... physical geographers are much readier to adopt a deterministic approach, arguing that ultimately there is a series of physical and chemical laws which can be invoked to explain the environmental interactions and relationships that are of interest."

Unwin (p. 206; 1977)

"... the fundamental conceptual requirement that, if form is to shed light on process, there must be a unique, one to one correspondence between a process and the ensuing form."

Thorn (p. 91; 1988)

Attempts to explain channel cross-sectional form and channel planform, have made extensive use of exclusive causal laws.[84] In particular, the causal laws of Newtonian Mechanics (the gravitational constant and the three laws of motion)[85] have been applied to channels and have resulted in a conception of river channel behaviour which views channel form as a product of the ratio between force and resistance (Chorley, p. 283; 1966: Richards, p. 65; 1977).

The application of causal laws in fluvial theory is summarised by the concept of 'characteristic form'; Brunsden and Thornes (1979) and Summerfield (1991) provide the following definition of 'characteristic form' in geomorphology, whilst Rosgen (1994) illustrates the use of the concept in fluvial geomorphology:

'For any given set of environmental conditions, through the operation of a constant set of processes, there will be a tendency over time to produce a set of characteristic landforms."

Brunsden and Thornes (p. 464; 1979)

"A core concept in geomorphology is that of characteristic form; distinct landform environments exist which are determined by a set of external variables ... reflecting a strong interdependence between process and form which persists for as long as the controlling conditions remain constant."

Summerfield (p. 465; 1991)

"The morphology of the present day channel is governed by the laws of physics through observable stream channel features and related fluvial processes."

Rosgen (p. 171; 1994)

[84] See **Sections 3. 4. 1.** and **3. 4. 3. 2.** for a discussion of causal laws and concepts.

[85] See **pp. 44** to **46 this thesis** for a discussion of Newtonian Mechanics.

The remainder of this section is devoted to considering the details of the research papers that have been responsible for the formation of the 'characteristic form' concept or those research papers that have utilised 'characteristic form' in their explanations of channel cross-section and channel-planform. The research papers considered are illustrative of much of the process-form theory that has been essential, and has underpinned, recent modelling approaches to geomorphology. Fundamentally they demonstrate the idea that the relationship between process and form is understood and that departures from this relationship are due to error (e.g. see review of Carlston (1965) p. 150 this thesis).

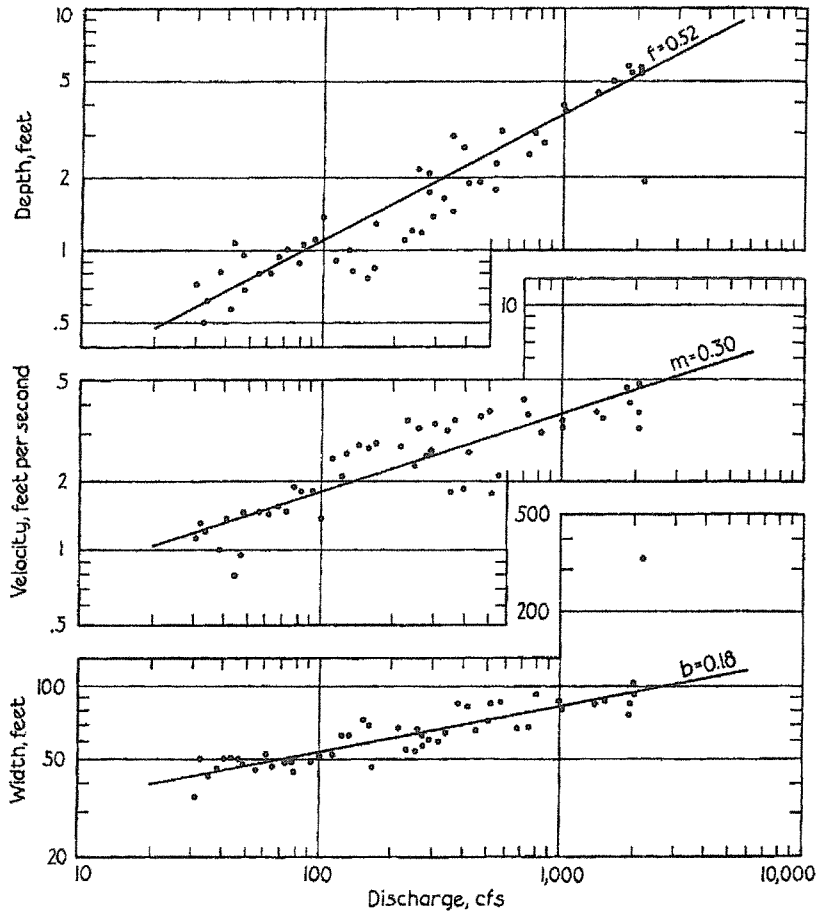
It may be argued that '... the foundation of descriptions of river form and process ...' (Clifford, p. 86; 1996) and the foundation of characteristic form in fluvial geomorphology, were laid by Leopold and Maddock (1953). In a 'preliminary' paper that sought to provide '.... generalised relations of interest to the geomorphologist,' Leopold and Maddock (p. 2; 1953) used data from 70 years of streamflow gauging in the U. S. A. to derive logarithmic regression relationships or power functions between discharge (cause) and velocity, width and depth, (effects).

The power functions were developed for two distinct problems in fluvial geomorphology. The first problem was how the descriptors of channel morphology such as velocity, width and depth changed at any given channel cross-section, as different flows passed through them. Because "Natural alluvial channels are noted for their morphological diversity which *both* reflects and influences hydraulic conditions," (Hey, p. 223; 1997)[86] the change in velocity, width and depth with different flows is not just a passive description, but the formulation of how the morphological dimensions of river channels may develop at any point in space and time. The power functions developed for any given cross-section were termed at-a-station curves (Leopold, Wolman and Miller, p. 216; 1964). Graphs for river changes in width, depth and velocity with increasing discharge at-a-station, are shown in figure 5. 1. 1.

Leopold, Wolman and Miller (p. 217; 1964) commented that the exponents of the equations summed to unity, such that an increase in one exponent necessitates a decrease in one or both of the remaining exponents, and that the value of the exponents in any field situation is predominantly determined by the type of sediment that composes the bed and banks. In particular, rivers which flow in channels where the banks are composed of non-cohesive sediment have a large exponent for width, a smaller exponent for velocity and the smallest exponent for depth. The converse applies to rivers which flow in channels where the banks are composed of cohesive sediment; the largest exponents are depth and velocity and width is the smallest exponent.

[86] See **Chapter One** for a description of indeterminacy.

Figure 5. 1. 1.- At-a-Station Hydraulic Geometry



From Leopold, Wolman and Miller (p. 216; 1964)

Changes in width, depth and velocity with increasing discharge are the effects of fundamental processes occurring in the channel. Recognising this, Leopold, Wolman and Miller (p. 220 - 241; 1964) discussed the effects of bed configuration and scour and fill on river channels and proposed a theoretical derivation of sediment transport based on the ratio of the rate of increase of velocity with discharge (m) divided by the rate of increase of depth with discharge (f). However, this ratio was not used, but rather an empirical equation in the form of a regression power function was derived by using data from sediment traps collecting suspended load. The values for suspended sediment load differed by up to two orders of magnitude for some values of discharge and Leopold, Wolman and Miller subsequently comment on the difficulties of measuring sediment load:

"... all correlations of measured load of natural streams with other factors is influenced by the limitations of ... measurements ... Measurements of sediment load are available in quantity only on that portion of the load caught ... Where total load and the load caught in a modern sampler have been compared, it has been shown that the device catches on the order of two-thirds of the total load in streams carrying principally sand and silt."

Leopold, Wolman and Miller (p. 220; 1964)

The second problem that hydraulic geometry addressed was how river channels change along their course, or in a downstream direction. "Proceeding downstream in a given river, the discharge tends to increase because of the progressively increasing drainage area ... It can be seen that for all the rivers plotted, the depth, width and velocity tend to increase progressively as power functions of discharge" (Leopold and Maddock, p. 11; 1953). These power functions derived for the relationship between increasing discharge with drainage area (cause) and width, depth and velocity (effects) were termed 'downstream hydraulic geometry.'

Leopold and Maddock (pp. 2 - 4; 1953) went to considerable length to show that downstream hydraulic geometry uses discharges of equal frequency, not arbitrary measures of discharge. The use of a frequency measure of discharge provides a consistent and a relevant datum for river channel sections, because the channel experiences a given discharge frequency (whatever its absolute magnitude) the same amount of time each year (regardless of its location in the basin) and thus makes comparisons between different scales (absolute magnitude) possible.

Downstream hydraulic geometry used the same methods of calculation and presentation as at-a-station hydraulic geometry (i.e. epistemic probability)[87] , but with different values for the coefficients and exponents. Whilst noting a "... considerable scatter of points in the graphs," (p. 11; 1953) Leopold and Maddock concluded that:

"... in a given river basin where all cross-sections are experiencing the same frequency of discharge, the corresponding values of depth, width and velocity at different cross-sections having the same discharge tend to be similar, regardless of where in the watershed or on what tributary the cross-sections may be."

Leopold and Maddock (p. 11; 1953)

Because the regression relationships are derived from data for several rivers, the downstream hydraulic geometry relationships also suggest that the relationship between a measure of discharge frequency, and depth/width/ velocity is consistent for any drainage basin;

"As a rough generalisation, it may be stated that in a downstream direction the rates of increase in width, depth and velocity relative to discharge are of the same order of magnitude for rivers of different sized drainage basins and of widely different physiographic settings."

Leopold and Maddock (p. 11; 1953)

[87] See **Section 3. 4. 1** for an outline of epistemic concepts.

A further implication of downstream hydraulic geometry, somewhat paradoxically, is that when comparing rivers *between* drainage basins, the form of the channels in question will be the same for any given magnitude of a frequency discharge measure, regardless of the locations of the particular sites in the drainage basins. That is, *between* drainage basins, downstream hydraulic geometry does not predict changes downstream, but predicts constant form for a given value of discharge. For example, if a particularly small drainage basin has a mean annual discharge of 3 cumecs in its lower reaches, it should, according to the empirical laws of downstream hydraulic geometry, have the same form as the larger basin's headwater with the same mean annual discharge of 3 cumecs. The location in the basin (upstream or downstream) is unimportant *between* basins because discharge is the causal factor. However *within* a basin, discharge varies with distance from source and therefore the the hydraulic geometry does change downstream.

Downstream hydraulic geometry relations can be viewed as discharge exclusively causing channel form, hence the equivalence mentioned above between the lower reaches and the headwaters of channels between basins of different sizes. At-a-station hydraulic geometry on the other hand, can be viewed as discharge causing channel form, but heavily constrained by the boundary conditions of sediment. Both forms of hydraulic geometry have difficulties, some of which were acknowledged by Leopold and Maddock (pp. 2 and 11; 1953). As already mentioned, there is considerable scatter on the charts of downstream hydraulic geometry relationships, suggesting that a causal relationship may not be present. Concerning this scatter, Leopold and Maddock (p. 11; 1953) suggest that it is due to the limitations of the data - "... the points on the graphs represent values of depth, width, and velocity at stream gauging stations situated at positions not uniformly representative of the average river cross-section," and that "... possible variations ... might be introduced by these considerations."

However, given that at-a-station hydraulic geometry relationships are heavily dependent on sediment properties and vary considerably between different sediment types (see table 5. 1. 8), it seems that a universal, quantitative, causal law that relates mean annual discharge to depth, width and velocity for widely different physiographic settings, should not, in principle, be possible.[88]

Leopold, Wolman and Miller (p. 248; 1964) touch on this; writing about downstream hydraulic geometry they state that , "It must be recognised that these are mean values .. at constant slope and discharge ... the channel will vary markedly with character of the bank material .. Such variations produce scatter,"

[88] The experimental analysis in **Chapter Seven** shows that, with constant discharge, morphology is contingent on its location within a homogeneous sediment system, solely because of queuing effects in sediment transport.

(see also Huang and Warner; 1995).[89]

The scatter in downstream hydraulic relationships due to at-a-station relationships is compounded further by the scatter that is present in at-a-station charts, where within a given cross-section with relatively constant sediment conditions, there is considerable deviation from the trend line that relates discharge to depth, width and velocity for that cross-section (see **figure 5. 1. 1.**). Rhodes (p. 195; 1978) thought that the most important idea to emerge from at-a-station hydraulic geometry was that '... rivers adapt to variable ... sediment load and flow regime by nearly every possible combination of the hydraulic geometry factors,' and that there "... is not a tendency for streams to accomplish their basic tasks in one way, but rather that rivers may perform their primary functions in such a large variety of ways." Clifford (p.84; 1996) discussed some of the physical reasons behind the variability in at-a-station relationships; "Cross-section adjustment may be a non-linear process, and at-a-station hydraulic geometry is not fixed in time either. Cross-section form is inherited, and sequences of variable discharge help determine cross-section form."[90] The non-linearity of hydraulic geometry relationships, and its relationship to channel roughness has also been discussed by Richards (1973).

To conclude this discussion on hydraulic geometry, three views are presented by different authors. These comments take account of the extended development of hydraulic geometry since it was proposed by Leopold and Maddock (1953),[91] and reflect both the significance of the 'all pervading unity of river channels,' that Leopold and Maddock (1953) uncovered, and the limitations to the hydraulic geometry approach.

"Hydraulic geometry is a useful means of summarising the complicated interactions of morphologic and dynamic variables occurring in natural rivers. As a tool, it will become even more useful when we are able to understand the sources of the observed variation of the parameters ... in essence more interest has been expressed in how streams are alike than why they are different in terms of their hydraulic geometry ... If hydraulic geometry is to become a practical analytic and predictive technique it is essential that the causes and characteristics of the highly variable responses to changing discharge be understood."

Rhodes (p. 92; 1977: pp. 195 - 196; 1978)

[89] The idea of statistics being different to aleatory law is of relevance here. The real question is not whether individual measurements vary and are different, but whether the properties of a given distribution are invariant. If an aleatory law is present then at the scale of the distribution, or the mean, the values should be the same, except for any different boundary conditions that may lead to error (see **p. 73 this thesis**).

[90] Current fluvial theory has difficulties choosing a representative measure for controls when there is considerable variation in these variables (see **Section 5. 2. 1.**). Experimental runs 9 - 12 attempted to assess the effect of flow sequences (see **Appendix**) on morphology and whether representative discharge measures were valid.

[91] Ferguson (1986) provides a comprehensive review of the development of hydraulic geometry.

"It is difficult to overestimate the influence of this classic work ... For generations of high-school and university students, it continues to be the foundation of descriptions of river form and process ... It was in essence a practical approach to geomorphology, founded upon (but ultimately limited by) the availability of simple environmental data. It posed significant questions,and offered some of the answers, even if, scientifically, these were correct only to the first order ... perhaps because of its simplicity and effectiveness as a means of representation, one of the unfortunate aspects of the paper is that geomorphologists have tended to overlook the complexities of its physical underpinning,"

Clifford (p. 86; 1996)

"These are the power laws in which the seductive quality of the trend may disguise order-of-magnitude local variability."

Lane and Richards (p. 250; 1997)

Following from earlier work on hydraulic geometry (Chorley, p. 280; 1966), Leopold and Wolman (1957) and Leopold *et al.* (1964) produced several observations on river form and process, using both field and experimental data. Leopold and Wolman prefaced their research with the following statement:

"From the consistency with which rivers of all sizes increase in size downstream, it can be inferred that the physical laws governing the formation of the channel of a great river are the same as those operating in a small one. One step toward understanding the mechanisms by which these laws operate in a river is to describe many rivers of various kinds."

Leopold and Wolman (p. 45; 1957)

The most notable contribution of Leopold and Wolman (1957) was the construction of a discriminate function, or empirical law for channel planform. Leopold and Wolman (p. 59; 1957) began by plotting average channel slope (effect) as a function of the bankfull discharge (cause) as shown in Figure 5. 1. 2. The next stage was to fit a trend line to the plot, not between channel slope and bankfull discharge but a line that discriminated between the channel patterns of braided and meandering channel planform. The equation for this line was expressed in terms of the dependent variable discharge (cause) and the independent variable channel slope (effect), such that the discriminant function was expressed in terms of channel slope.

Three things appear to have contributed to the expression of the discriminant function for channel pattern as it appears in Leopold and Wolman (1957). Firstly, they hold the view that channel slope is caused by discharge. Secondly, they hold the view that channel planform is caused by channel slope. Thirdly, they lack a quantitative measure for channel planform (sinuosity does not account for degrees of braiding and it is difficult to provide a continuous quantitative measure of channel planform that progresses from straight to meandering to braided).

Figure 5. 1. 2.- Slope, Discharge and Channel Planforms

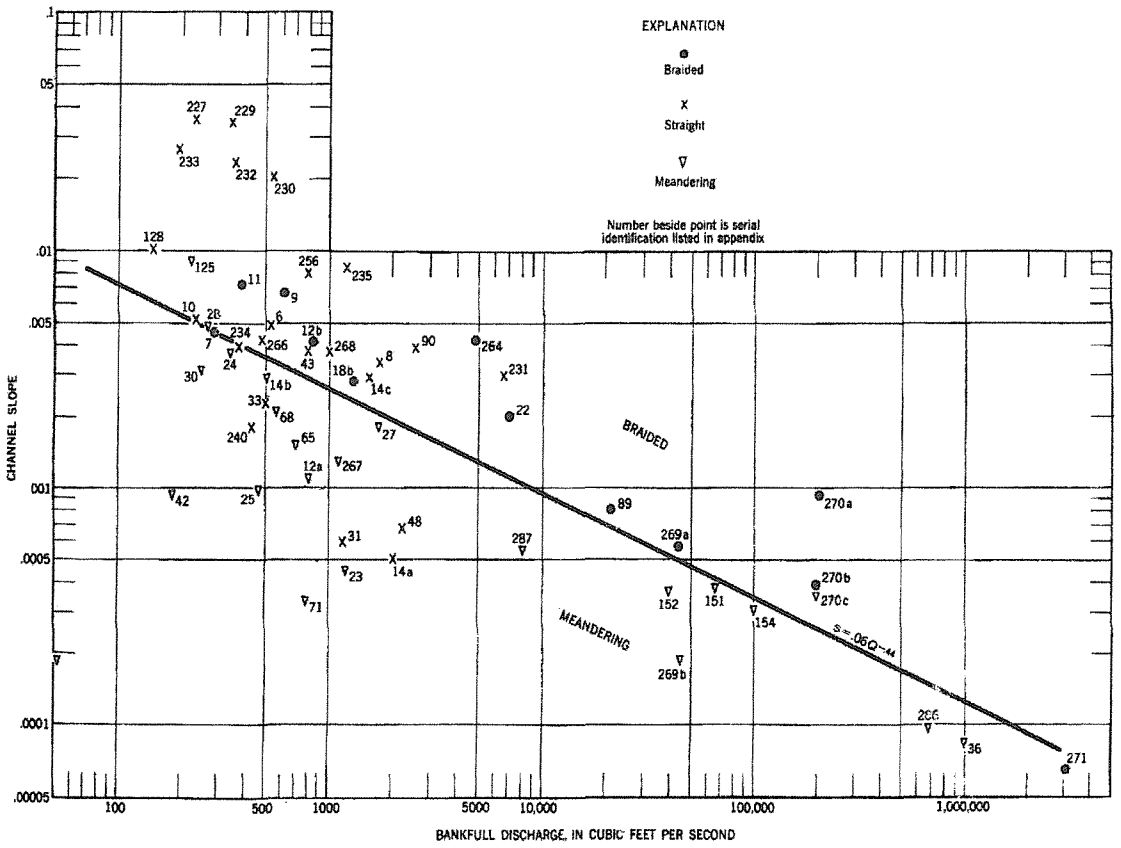


FIGURE 46.—Values of slope and bankfull discharge for various natural channels and a line defining critical values which distinguish braided from meandering channels.

From Leopold and Wolman (p. 59; 1957)

Several issues are raised by these three factors in Leopold and Wolman's paper. The discriminant function in written form states that; 'for a given bankfull discharge, there exists a critical channel slope. If a real river channel plots above this critical slope on a chart, it should have a braided channel. If a real river plots below this critical slope on a chart, it should have a meandering channel.' Problems emerge when trying to relate this statement that the discriminant function makes, with the premise that channel slope is caused by discharge. How can the same discharge produce a range of channel slopes, such that some rivers fall above the critical value (braid) and some rivers fall below the critical value (meander)? Or, how can a single cause produce multiple effects?

Secondly, Leopold and Wolman hold that channel pattern is caused by channel slope. The discriminant function could have been reversed, such that in written form it would have stated; 'for a given channel slope, there exists a critical bankfull discharge. If a real river channel plots above this critical bankfull discharge on a chart, it should have a braided channel. If a real river plots below this critical bankfull discharge on a chart, it should have a meandering channel.' That the discriminant function is not expressed in terms of discharge is due to the causal connection between bankfull discharge and channel slope.

Leopold and Wolman (p. 60; 1957) touch on the discrepancy which exists between the statement the discriminant function makes, and the lack of an exclusive causal relationship between bankfull discharge and channel slopes; "For a given discharge, meanders occur on the smaller slopes. At the same slope a braided channel will have a higher discharge than a meandering one." Leopold and Wolman then go on to explain that the variation in channel slope for constant bankfull discharge is a product of variability in the controls, particularly in sediment:

"It is important to keep in mind that these data describe certain natural channels. In natural channels specific variables often occur in association. For example, steep slopes are associated with coarse material. In a system which contains ... a minimum of seven variables, a diagram ... which treats only two of these cannot be expected to describe the mechanism of adjustment or all theoretically possible conditions."[92]

Leopold and Wolman ((p. 60; 1957)

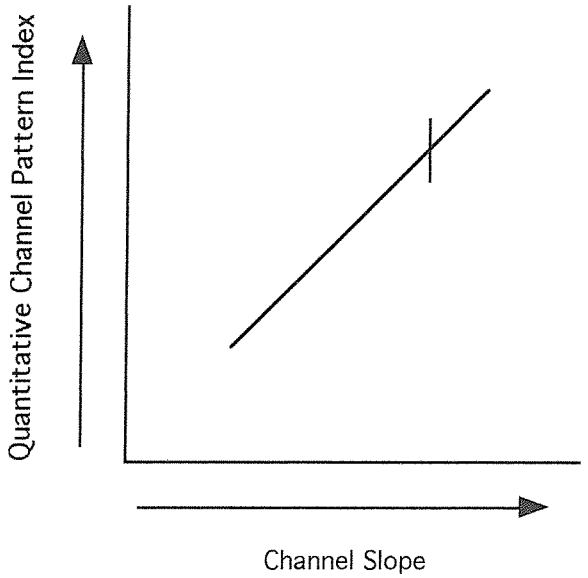
The concept of a discriminant function implies that the relationship between channel-slope and channel-pattern does not function as a causal law, whereby increments in the cause lead to increments in the effect; rather it appears that channel-slope can increase up to just below the discriminant function channel-slope, with no discernible difference in effect (the channel remains meandering), and then, at the point of the discriminant function channel-slope, the channel-pattern suddenly becomes braided. This does not appear to be the concept that Leopold and Wolman had in mind; "Channel patterns do not, however, fall into well defined categories ... there is a gradual merging of one pattern into another ... If we assume that the pattern of a stream is controlled by the mutual interaction of a number of variables, and the range of these variables in nature is continuous, then we should expect to find a complete range of channel patterns," (pp. 45 and 59; 1957). The apparent lack of a causal law is due to the absence of a quantitative method of assessing river pattern, and thus specifying different degrees of pattern.[93]

Taking these three interpretations of Leopold and Wolman (1957) together, the statement the paper made, can be expressed in Figure 5. 1. 3:

[92] This statement indicates the concept of error discussed in **Chapter Three** and is an example of equifinality - a theoretical tool which suggests that diversity in morphology is due to an incomplete specification of the system components (see **Section 5. 2. 3.**).

[93] For an attempt at a quantitative index of braiding see Brice (1960: p. d27; 1964)

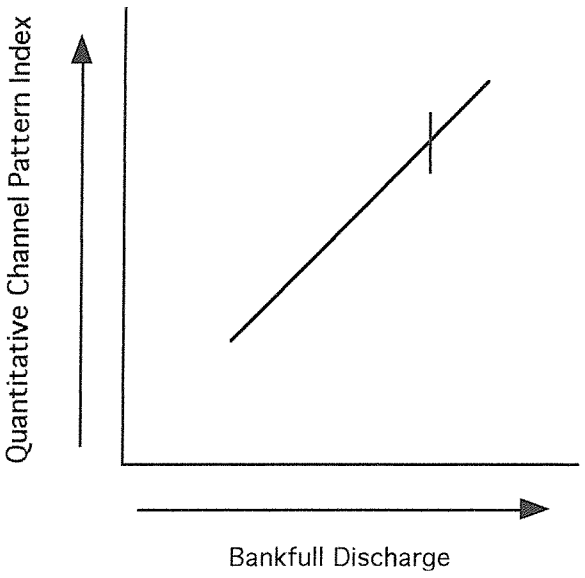
Figure 5. 1. 3.- Change in Quantitative Channel Pattern Index with Slope Given Constant Discharge



| = Point of discriminant function where braiding is designated to start

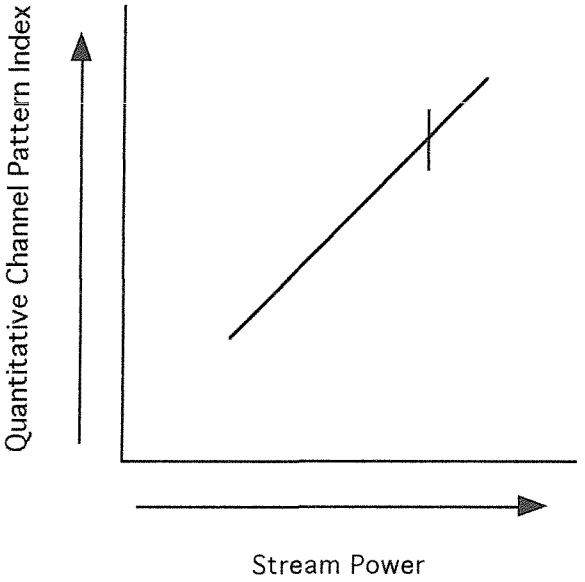
Because of the lack of a distinct causal relationship between bankfull discharge and channel-slope the relationship could equally have been drawn as Figure 5. 1. 4.

Figure 5. 1. 4.- Change in Quantitative Channel Pattern Index with Bankfull Discharge Given Constant Slope



If no unique relationship exists between bankfull discharge and channel-slope, the most concise way of expressing the relationship that Leopold and Wolman suggested, would be to form a product of bankfull discharge and channel-slope or a stream power variable (Figure 5. 1. 5.).

Figure 5. 1. 5.- Change in Quantitative Channel Pattern Index with Stream Power



Leopold and Wolman (p. 215; 1957) also used techniques of epistemic probability[94] (regression) to define log-linear power functions between channel bankfull discharge (cause) and meander wavelength (effect) and between channel bankfull width (cause) and meander wavelength (effect). The equation of the lines in these charts provides the mathematical relationship between cause and effect, so that if the magnitude of the cause is known, the magnitude of the effect is predicted from this relationship. The relationship between channel bankfull width and meander wavelength was judged to be the stronger relationship.

Finally, Leopold and Wolman (pp. 222 - 224; 1957) suggested a relationship between sediment size (cause) and flow resistance (effect), an important component in the analytical solution of a flow.[95] Discussing the difficulties in choosing a representative sediment size in channels with a 'wide range of particle size,' Leopold and Wolman elected for a grain size parameter that included 84% of the material - that is 84% of the material is finer than the specified sediment size.

As in the case of bankfull width and meander wavelength (see above), epistemic probability (regression) was used to provide an equation between the grain size or relative smoothness and the resistance to flow. Commenting on the scatter present on the plot of grain size and resistance, Leopold and Wolman (p. 224; 1957) wrote that "The computed resistance is probably not solely a function of the size of the bed material ... the resistance in some of them [channels] may be affected by vegetation, channel alignment or other factors not attributable to grain roughness."

[94] See **p. 50 to 53 this thesis** for definition of epistemic ideas and concepts - particularly the idea of error.

[95] See **p. 2 this thesis.**

Charles Carlston (p. 864; 1965) aimed to " ... determine as accurately as possible the relation of free meander dimensions to discharge," a free meander being defined as " ... one developed in alluvium and free to erode laterally, unrestrained by valley walls, and with waveforms not materially distorted by inhomogenities in the alluvium."

Carlston's paper began with an analysis of previous equations developed for river form and discharge, particularly in the work of Jefferson (1902) and Inglis (1941: 1949). Carlston (p. 866; 1965) noted that the work of Inglis had been based on data from Jefferson (1902) and that the results of Inglis (1941: 1949) had been accepted by workers such as Leopold, Wolman, Miller and Dury. The predominant result of Inglis (1941: 1949) was that river variables such as the width of the meander belt, the meander length, the width of the river, the wetted perimeter, all varied as the half power or the square root of discharge (Carlston, p. 869; 1965). In a discussion of Inglis' relationship between the square root of discharge and river form, Carlston (p. 885; 1965) argued that the relationship was in error for three reasons; 1) The relationship was based on the sparse data set by Jefferson (1902) (three data points) and that the half power exponent was used even when values differed by up to two significant figures 2) The half power law was assumed to apply over all discharges including mean annual, bankfull and maximum discharge 3) Inglis made a mistake with Jefferson's data, using half the meander wavelength measure instead of the full meander wavelength measure, resulting in low coefficient values for Inglis' regression plots (see Carlston; p. 870; 1965).

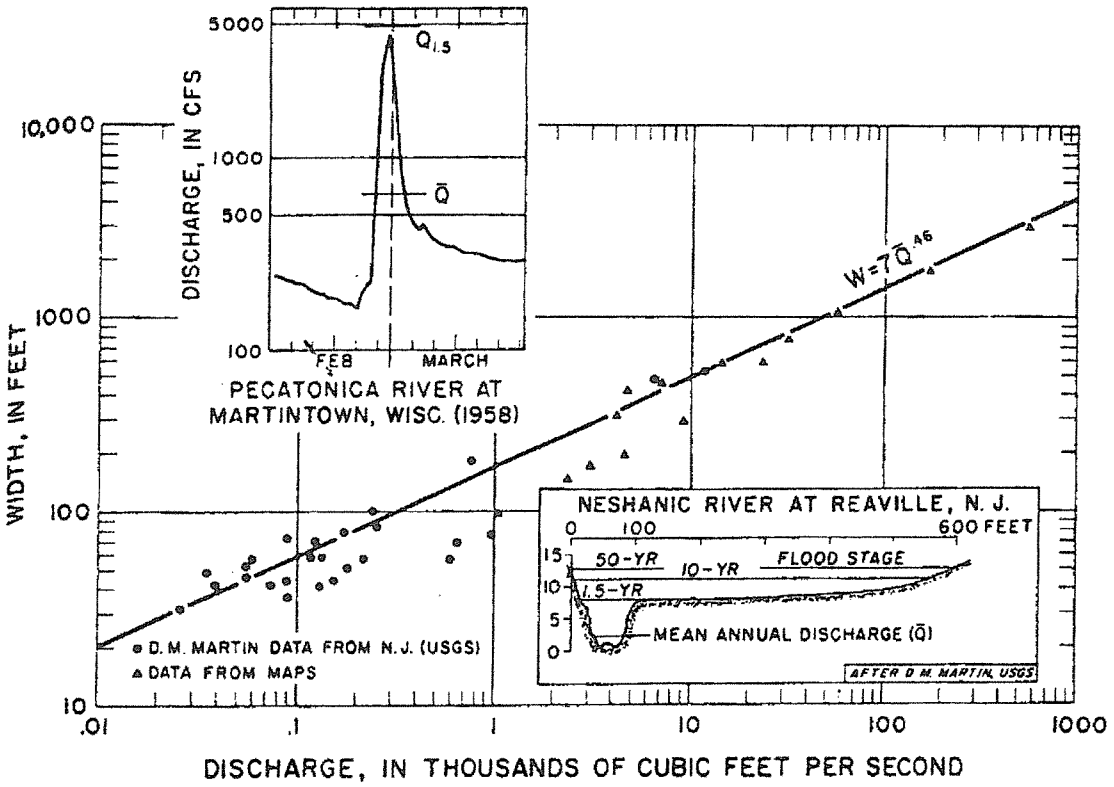
Having discussed the above problems that some of the previous literature had, Carlston (1965) went on to outline his derivation of the relationship between discharge (cause) and channel form (effect). The first relationship to be derived was that between discharge and meander wavelengths. Carlston (pp. 873 - 874; 1965) writes that 'The precise measurement of meander wavelengths is difficult and subjective,' and that to eliminate this subjectivity, the mean dimensions of six to eight local meanders was used to define the dimensions of a single representative meander for a given stream or river. The best regression relationship was found between mean annual discharge and mean meander wavelength.[96] The regression relationship between the mean discharge of the month of maximum discharge and mean meander wavelength was also good, and led Carlston (p. 885; 1965) to the conclusion that the "... dominant discharge which controls the wavelength of free meanders is a range of discharges ... between mean discharge of the month of maximum discharge and mean annual discharge."[97]

[96] The use of mean values for controls and effects, imply that laws in geomorphology are causal relationships between deterministic aleatory distributions. A justification for the use of aleatory statistical laws and their equivalence to causal laws has been given in **Sections 3. 4. 3. 1.** and **3. 4. 3. 2.**

[97] Mean meander wavelength had a very poor regression with bankfull discharge (bankfull discharge was used by Leopold and Wolman (1957))

The second relationship derived by Carlston (1965) was between discharge and channel width. Carlston (p. 878; 1965) provided a caveat to his relationship between discharge and channel width; "The high variability of width of channel along the course of a stream makes the establishment of an exact equation for stream width and discharge very difficult." An attempt was made to reduce the variability of width by providing an average measure of channel width. This average measure of width was then correlated against mean annual discharge with a considerable degree of scatter along the regression line (see figure 5. 1. 6).

Figure 5. 1. 6.- Carlston's (1965) Plot of Mean Annual Discharge Against Width



From Carlston (p. 878; 1965)

Carlston (p. 883; 1965) also examined the relationship between channel width and meander wavelength[98] but the 'very great scatter' between the two variables meant that the relationship was rejected by Carlston. A regression between discharge and meander width was also found to be poor and Carlston (p. 878; 1965) concurred with Leopold *et al.* (p. 297; 1964) that " ... amplitude of meander loops is determined more by erosion characteristics of stream banks and other local factors than by any hydrodynamic principle."

[98] See **p. 150 this thesis** - Leopold and Wolman (1957) thought that meander wavelength was a direct effect of channel width.

To summarise, Carlston (p. 882; 1965) noted that "The hydrodynamics law that causes regular waveform of free meanders of fully mature streams is yet to be discovered," but the purpose of Carlston's paper was to provide more accurate empirical laws of the relationship between discharge and average river form, and to improve on what he saw as the errors of previous approaches.

Ackers (1964) examined the application of empirical formulas for stream geometry. A set of experiments was undertaken that generated alluvial channels with completely free boundary conditions, i.e. the channel could erode both bed and banks. The alluvial channels were developed from straight trapezoidal sections initially cut into uniform sand (though a proportion of silt and clay was present) and were run for between 10 and 20 days. The developed channels of primary interest were those channels that eroded their bed and banks but retained a straight planform.

Ackers' (pp. 5 and 1; 1964) aim was to test "The regime concept [which] implies a unique solution for a stable channel at a given steady discharge, conveying a known concentration of solids in alluvium of given character." This test of regime theory and 'uniquely determined geometry' was achieved by examining the goodness of fit of regression relationships and by a comparison with analytical equations. The analytical equations did not make *a priori* predictions of channel geometry, but rather used data from the experiments to generate results; "Although the initial physical assumptions in the analyses should lead to dynamically sound formulas, their successful application is nonetheless dependent on experimentally determined coefficients ... These 'rational' approaches ultimately depend on empiricism for the evaluation of the relationship between the parameters ,"[99] (Ackers, pp. 3 and 16; 1964).

The experimental data thus formed the basis of both the 'regime approach' and the 'analytical approach' and from the results of the experiments, Ackers (pp. 9 and 33; 1964) concluded that "... the regime dictum that there is a single - valued solution to the stable channel problem is confirmed," and that "The empirical equations correlating geometry with discharge, as deduced from this experimental research, are consistent with an algebraic combination of three fundamental physical relationships for channel resistance, sediment transport, and cross-sectional shape .."

A point that may require clarification in Ackers (1964), is the lack of quantitative data on the value of the bed gradient for the initial straight, trapezoidal channel. Ackers (p. 3; 1964) writes that the small streams were "... left free to adjust their sectional areas ... their alignments and their sediment concentrations and gradients, until stability was reached." Commenting on the practicalities of the experiments, Ackers notes that;

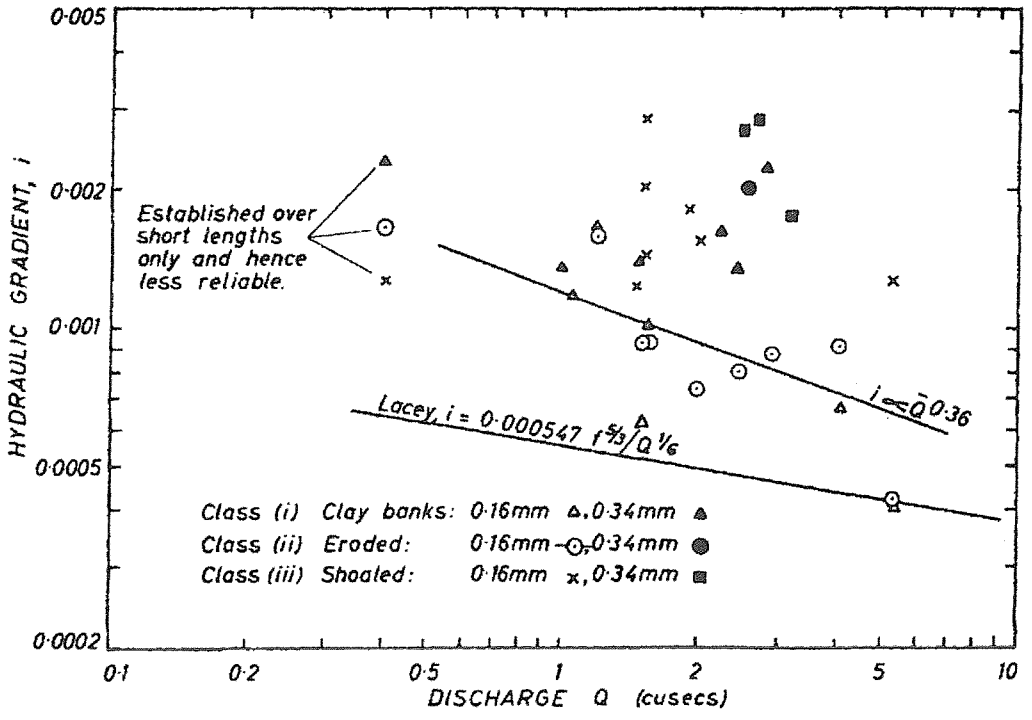
[99] The need for empiricism is a result of indeterminacy (see **Chapter One**).

"... the solids concentration was largely dependent on the initial bed gradient ... In some cases, the gradient 'as dug' was too great for stability: The high transport rate, reinjected at the head of the stream, would then cause shoaling and perhaps meandering. When this occurred, the run was regarded as of limited value, and would be repeated at the same discharge but with progressively slacker initial gradients, until a straight stream was produced."

Ackers (p. 5; 1964)

Ackers (p. 33; 1964) notes the important role that initial bed gradient has within the straight channel pattern (the pattern of primary interest), concluding that "[channel] slope did not show good correlation with discharge, being much influenced by sediment concentration." Sediment concentration, as noted in the paragraph above is directly related to initial bed gradient and thus the initial slope affects final slope within any given class of channels (i.e. straight channel pattern). Ackers' (p. 11; 1964) charts show that within the class of straight channels, there is no clear relationship between discharge and hydraulic gradient or channel slope. This discrepancy increases for the other classes of channel pattern - the shoaled pattern (x) in Figure 5. 1. 7. may have four different slope values for a discharge of approximately 1.5 cusecs.

Figure 5. 1. 7.- Ackers (1964) Discharge/ Slope Relationship



From Ackers (p. 10; 1964)

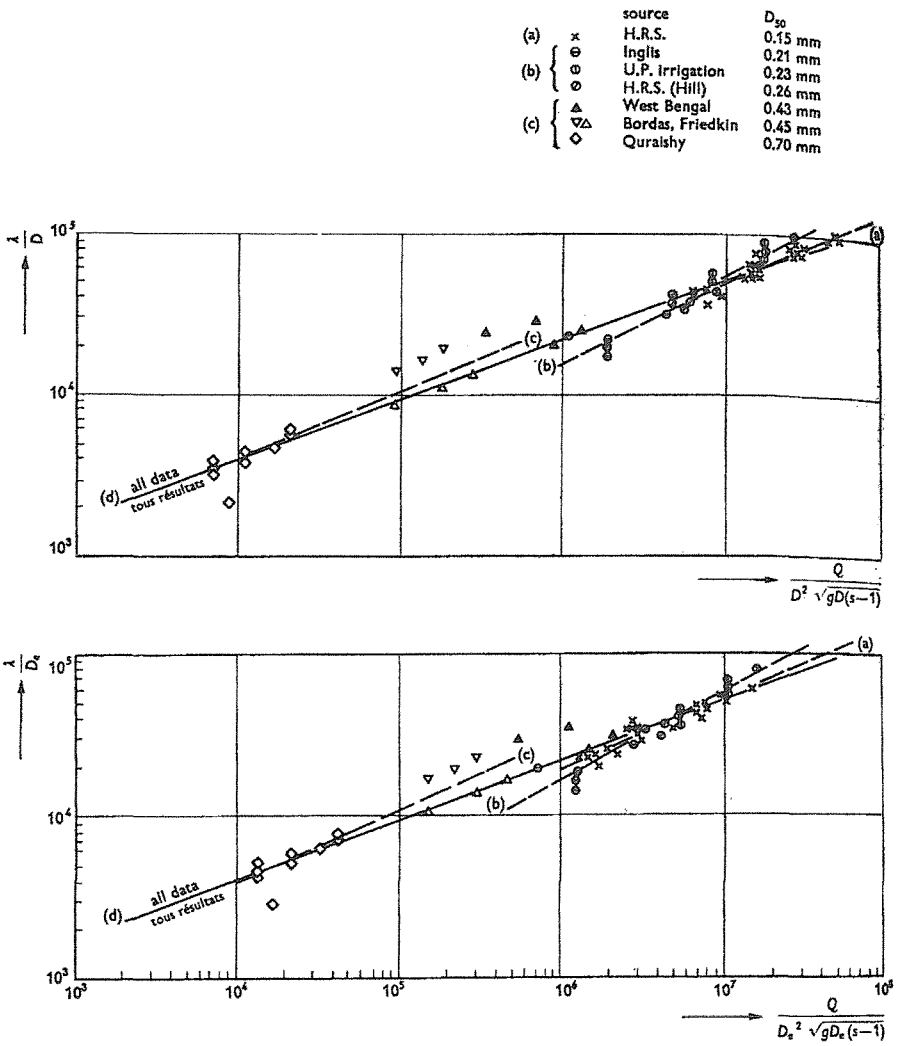
Given the importance of initial slope within any given class, it appears unusual that a quantitative calibration measure of initial slope does not appear in the analysis. Furthermore, given the apparent sensitivity of channel slope to initial bed gradient (i.e. there is large variation in final channel slope within a class of channel pattern which is meant to have similar initial bed gradients), it appears feasible that different patterns of river (shoaled/meandering or straight) may have been produced under fairly similar conditions. Quantitative data on the initial bed gradients appears essential to determine the effects of initial conditions and the possibility that similar initial conditions produced dissimilar forms or that 'the regime dictum that there is a single - valued solution to the stable channel problem is falsified.'[100]

Ackers and Charlton (p. 290; 1970), reporting on the same set of flume experiments as Ackers (1964), began their paper with the premise that "Simple empirical analyses of the relationships of the geometrical properties of an alluvial stream to the independent quantities which determine its form can yield valuable information."

Ackers and Charlton focused mainly on the dimensionless empirical relationship between discharge, sediment diameter, and meander wavelength. Two measures of sediment diameter were used; the D_{50} measure and the critical shear stress criteria for any D_{50} or D_e. A plot of the dimensionless sediment measures/discharge against dimensionless wavelength is shown in figure 5. 1. 8. (p. 156).

[100] Qualitatively the behaviour represented in the chart bears similarities to the concept of sensitive dependence on initial conditions discussed in **Section 4. 2.**

Figure 5. 1. 8- Ackers and Charlton's (1970) Plot of Dimensionless Sediment Measures/Discharge Against Dimensionless Wavelength



From Ackers and Charlton (p. 300; 1970)

Ackers and Charlton (p. 301; 1970) noted that the exponents for the dimensionless sediment diameter/discharge functions were markedly different for individual sediment classes (pink section table 5. 1. 1.) than for all the data taken together (orange section table 5. 1. 1). This result led Ackers and Charlton to suggest that whilst sediment size was important in controlling meander geometry, a plot of a sediment diameter measure/discharge versus meander wavelength was not an adequate measure;

"... a reasonable average line at a slope of about 0.375 [can] be drawn through all the data, but inspection shows that the slope of the average line does not accord with the trend of data for any single sediment size ... Presumably the influence of sediment size on meander length, though very real, is only secondary, and therefore the use of sediment diameter as a basic scaling function is unsound ... how easy it is to oversimplify the correlation, either by looking so broadly at the collection of data that the departures of individual groups from the overall trend are obscured or minimised, or by assuming that experiments with only one size of sediment will provide an exponential equation that is generally applicable and also extrapolatable."

Ackers and Charlton (pp. 299 and 307; 1970)

Table 5. 1. 1- Ackers and Charlton's (1970) Different Equations for the Relationship Between Dimensionless Sediment/ Discharge and Meander Wavelength

Sediment size	
(a) 0.15 mm	$\frac{\lambda}{D} = 26.9\left(\frac{Q}{D^2\sqrt{gD(s-1)}}\right)^{0.467}$
	$\frac{\lambda}{D_e} = 41.5\left(\frac{Q}{D_e^2\sqrt{gD_e(s-1)}}\right)^{0.445}$
(b) 0.21–0.26 mm	$\frac{\lambda}{D} = 7.58\left(\frac{Q}{D^2\sqrt{gD(s-1)}}\right)^{0.552}$
	$\frac{\lambda}{D_e} = 7.53\left(\frac{Q}{D_e^2\sqrt{gD_e(s-1)}}\right)^{0.557}$
(c) 0.45–0.70 mm	$\frac{\lambda}{D} = 59.7\left(\frac{Q}{D^2\sqrt{gD(s-1)}}\right)^{0.453}$
	$\frac{\lambda}{D_e} = 56.8\left(\frac{Q}{D_e^2\sqrt{gD_e(s-1)}}\right)^{0.454}$
(d) all together	$\frac{\lambda}{D} = 123\left(\frac{Q}{D^2\sqrt{gD(s-1)}}\right)^{0.376}$
	$\frac{\lambda}{D_e} = 130\left(\frac{Q}{D_e^2\sqrt{gD_e(s-1)}}\right)^{0.371}$

Adapted from Ackers and Charlton (p. 301; 1970)

Ackers and Charlton (p. 313; 1970) discussed a four parameter dimensionless solution to the problem of meander wavelength, relating a discharge/froudian wavelength function to dimensionless meander wavelength. This relationship was mitigated by a dimensionless critical shear stress criteria for median grain diameter, producing a complex functional relationship, that failed to produce a universal exponent for all the data.

In an influential series of papers, Schumm (1969: 1971a: 1971b: 1972) expounded the role of water and sediment discharge in fluvial geomorphology. The ethos underpinning these papers is summarised by Schumm (p. 4-5; 1971a); "In order to continue a discussion of river morphology beyond the purely descriptive stage, it is necessary to inquire more closely into cause and effect relations. That is, just what are the independent variables that in a given reach influence river morphology and behaviour?"[101]

[101] See **Chapters One** and **Two**, for a discussion of the uniqueness that indeterminacy presents and for the consistent scientific knowledge that geomorphology wants to produce. Schumm's statement expresses a desire to derive relationship between entities for fluvial morphology.

Schumm (1960: 1963) had provided a classification of alluvial river channels based upon the percentage of silt - clay found in the channel bed and banks and concomitantly the mode of sediment transport. This association between the silt - clay index and the mode of sediment transport formed the basis for Schumm's evaluation of river morphology. Schumm (p. 256; 1969) wrote of the connection between the perimeter materials and the mode of sediment transport that "... consideration of the nature of the bed and bank materials in stable, alluvial channels that are transporting primarily clay, silt, and sand, leads to the obvious conclusion that the sediment forming the perimeter of a stable alluvial channel reflects the type of sediment being moved through the channel."

Schumm (p. 257; 1969) used this connection between perimeter material and sediment transport to develop causal relationships between the type of sediment load (suspended, bedload or mixed) and representative measures of discharge (mean annual discharge/mean annual flood) (causes) and morphological descriptors (width, depth, width/depth ratio, meander wavelength) (effects). Because the " ... type of sediment load moved through ... channels is not readily measured...," Schumm (p. 257; 1969), the percentage of silt-clay found in the channel perimeter is used as a surrogate for direct measurements of the type of sediment load. Accordingly, Schumm (pp. 270 - 271; 1969) used channel perimeter silt-clay percentages and discharge values for 36 Australian rivers to derive multiple regression relationships between these causes and the aforementioned morphological effects (table 5. 1. 2).

From the regression relationships, Schumm first attempted to view the type of sediment load and the representative measures of discharge as controls on river morphology that act in isolation, or independently of each other. From this perspective Schumm concluded that:

"The shape (width-depth ratio) and sinuosity of an alluvial channel are primarily determined by the type of sediment load or the ratio of bed-material load to total load moved through the channels, whereas channel width, depth, gradient, wavelength, and amplitude are significantly influenced by both discharge and type of sediment load."[102]

Schumm (p. 259; 1969)

[102] Schumm bases this statement on the result of his regression equations which revealed that both depth and width increase as discharge increases. Because the direction of change in width and depth is the same for the two variables, Schumm assumes that the ratio between the two will remain unchanged with changes in discharge and therefore the width-depth ratio is independent of discharge. With type of sediment transport, the regression relationships revealed that depth increases with an increase in M and width decreases with an increase in M. Because there is a progressive change in the ratio of width to depth as M changes, Schumm concludes that the width - depth ratio of rivers is dependent on the type of sediment transport. The validity of this conclusion depends on the changes in width and depth with discharge being quantitatively consistent - that is width and depth always change by quantitatively the same amount.

Table 5. 1. 2.- Schumm's (1969) Regression Equations

$$w = 2.3 \frac{Qma^{0.58}}{M^{0.37}}$$
$r = 0.94$
$Se = 0.13$ log unit

$$w = 37 \frac{Qm^{0.38}}{M^{0.39}}$$
$r = 0.93$
$Se = 0.14$ log unit

$$d = 0.09\ M^{0.35}\ Qma^{0.42}$$
$r = 0.86$
$Se = 0.13$ log unit

$$d = 0.6\ M^{0.34}\ Qm^{0.29}$$
$r = 0.89$
$Se = 0.13$ log unit

$$F = 21.4 \frac{Qma^{0.18}}{M^{0.74}}$$
$r = 0.93$
$Se = 0.15$ log unit

$$F = 56 \frac{Qm^{0.10}}{M^{0.74}}$$
$r = 0.93$
$Se = 0.15$ log unit

$$L = 234 \frac{Qma^{0.48}}{M^{0.74}}$$
$r = 0.93$
$Se = 0.19$ log unit

$$L = 1890 \frac{Qm^{0.34}}{M^{0.74}}$$
$r = 0.96$
$Se = 0.16$ log unit

From Schumm (pp. 257 - 258; 1969)

Schumm persisted with viewing the type of sediment load and the discharge in isolation, to produce equations which related direction of change in effects with positive or negative changes in type of sediment load or discharge (table 5. 1. 3.).[103] These directions of change were inferred from the direct or inverse proportionality of the variables in the regression equations (see table 5. 1. 2). Schumm made a statement that:

"An increase or decrease of discharge changes the dimensions of the channel and its gradient, but an increase or decrease of bed-material load at constant mean annual discharge changes not only channel dimensions but also the shape of the channel (width-depth ratio) and its sinuosity."

Schumm (p. 260; 1969)

[103] Qs in **table 5. 1. 3.** refers to the amount of bedload in the sediment load - it is the inverse of M (silt-clay percentage). As M decreases the amount of sediment transported as bedload is deemed to increase. The consequence of this change in sediment criteria is that those relationships which were directly proportional to M in the regression equations are now inversely proportional and vice versa.

Table 5. 1. 3.- Schumm's (1969) Qualitative Univariate Equations

$$Qw^{+} \simeq \frac{w^{+}\, d^{+}\, L^{+}}{S^{-}}$$

$$Qw^{-} \simeq \frac{w^{-}\, d^{-}\, L^{-}}{S^{+}}$$

$$Qs^{+} \simeq \frac{w^{+}\, L^{+}\, S^{+}}{d^{-}\, P^{-}}$$

$$Qs^{-} \simeq \frac{w^{-}\, L^{-}\, S^{-}}{d^{+}\, P^{+}}$$

From Schumm (p. 260; 1969)

The thrust of the two conclusions that Schumm makes from treating discharge and type of sediment transport independently, is that discharge affects the magnitude of alluvial channel dimensions, whilst sediment type affects the form of alluvial channels. This idea was present in nascent form, when Schumm (1963) proposed his classification of river channels. Commenting on the reasoning behind that classification Schumm states:

"Discharge is an independent variable that largely determines the size of the stream channels and the amplitude and wavelength of meanders. Discharge however, is not a valid basis for classification unless size is considered most important...."

Schumm (p. 4-22; 1971a)

Having treated the type of sediment load and discharge as independent variables Schumm (p. 261; 1969) noted that "... it would be rare that a change in discharge or sediment load could occur alone. Generally any change in discharge will be accompanied by a change in the type of sediment load and vice versa." Schumm thus produced combinatory equations based on the individual results of the type of sediment load and discharge (table 5. 1. 4.). These equations explicitly incorporate elements of indeterminacy where specific effect variables are affected by both type of sediment load and discharge in different ways. Thus in the case of slope for increasing bedload and increasing discharge, increasing bedload promotes increasing channel slope, whilst increasing discharge promotes decreasing slopes. It is thus not clear whether channel slope will increase, decrease or remain the same.

Table 5. 1. 4.- Schumm's (1969) Bivariate Qualitative Equations

$$Qw^{+}\ Qt^{+} \simeq \frac{w^{+}\ L^{+}\ F^{+}}{P^{-}}\ S^{\pm}\ d^{\pm}$$

$$Qw^{-}\ Qt^{-} \simeq \frac{w^{-}\ L^{-}\ F^{-}}{P^{+}}\ S^{\pm}\ d^{\pm}$$

$$Qw^{+}\ Qt^{-} \simeq \frac{d^{+}\ P^{+}}{S^{-}\ F^{-}}\ w^{\pm}\ L^{\pm}$$

$$Qw^{-}\ Qt^{+} \simeq \frac{d^{-}\ P^{-}}{S^{+}\ F^{+}}\ w^{\pm}\ L^{\pm}$$

From Schumm (p. 261; 1969)

The equations derived by Schumm (1969) may face several theoretical problems; foremost among them is that discharge and the type of sediment transport are not independent. That is for a given particle size, different modes of transport may be possible depending on the discharge (Simons and Richardson, p. 9-20; 1971). The forms produced by discharge and type of sediment transport are a result of the mutual interaction of discharge and sediment with all the inherent uncertainty produced by indeterminacy and the possible variations in river form.[104] Although it is necessary to separate discharge and the type of sediment transport, in order to ascertain their effects, the results obtained by isolating causal mechanisms in fluvial mechanics should only have gross applicability.

Secondly, the relationship between the *in situ* percentage of silt-clay found in the channel bed and banks and the type of sediment transport may not be deterministic. The *in situ* bed and bank material may be inherited and not reflect current conditions (particularly as silt and clay are generally removed from within the channel during current flows - see Shen, p. 11-3; 1971a). Also, the relationship between particle size and sediment transport is complex; even with a given sediment size and discharge, different modes of transport are possible (Abbott and Francis; 1977) and sometimes suspended load can actually promote increases in bedload transport through increased viscosity, and increased tractive force (Shen, p. 16-4; 1971b: Knighton, p. 70: 1984). These difficulties aside, Schumm's papers provided a valuable insight into a complex system, attempted to provide some direction of change in river channels, and gave explicit recognition to the indeterminacy of some elements of river channel behaviour.

[104] See **p. 5 this thesis.**

A paper that summarised developments in fluvial theory was presented by Richards (1977). Downstream hydraulic geometry was the first area of fluvial theory to be reviewed. Richards began by considering the bivariate approach which relates an independent variable (cause) to a dependent variable (effect). Hydraulic geometry uses discharge as the independent variable and all other variables which characterise the channel geometry such as width, depth, velocity, slope roughness and sediment load are dependent upon discharge (Richards, p. 69; 1977). Presenting a table of hydraulic geometry exponents derived by various authors,[105] Richards (p. 73; 1977) noted that there was a variability of exponents from basin to basin, but in addition, Richards stated that "The variety of exponents describing downstream trends *within catchments* reflects the influence of variables other than discharge on the channel morphology," (italics added).

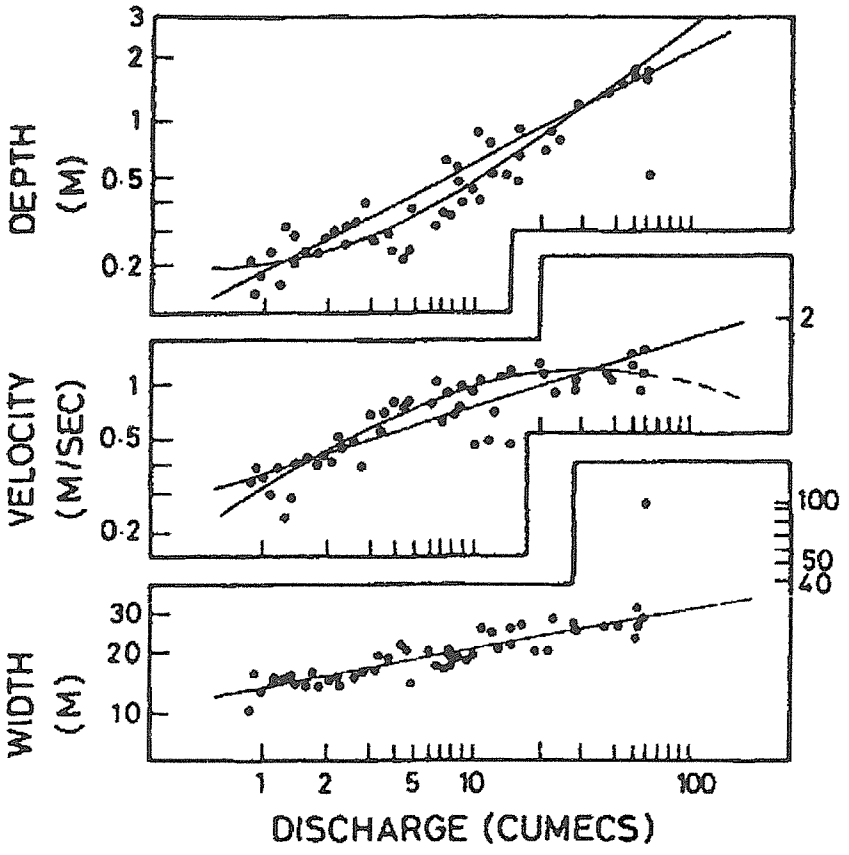
Following this statement that variables other than discharge affect morphology (width, depth, velocity, etc.), Richards (1977) proceeded to outline multivariate approaches that relate several independent variables (causes/boundary conditions) to a dependent variable (effect). Richards (pp. 73 - 78; 1977) discussed quantitative multivariate regression equations of; the percentage of channel bank and bed material composed of silt-clay (cause) against the width - depth ratio (effect) (Schumm; 1960); discharge and the percentage of channel bank material composed of silt-clay (cause/boundary condition) against the channel width (effect) (Ferguson; 1973); sediment particle size (D_{50}) (boundary condition) and discharge (cause) against channel width (effect) (Henderson; 1961); and sediment particle size (D_{50}) (boundary condition) and discharge (cause) against depth (effect) (Kellerhals; 1967). A summary of what Richards (p. 75; 1977) termed unquantifiable variables, such as bank vegetation and non - fluvial bank erosion were also discussed.

Richards (pp. 77 - 78; 1977) concluded his section on downstream hydraulic geometry with the conclusion that, ".... a multivariate approach is necessary. Scatter in univariate downstream hydraulic geometry curves must be rationalised by reference to additional independent variables."

[105] A similar table is presented by Shields (1996) and is shown on **p. 177 this thesis.**

After reviewing both bivariate and multivariate approaches to downstream hydraulic geometry, Richards (1977) focused on at-a-station hydraulic geometry or changes in flow geometry with different discharges. Following previous research on hydraulic geometry (Richards; 1973), Richards' emphasis was on the complexity of at-a-station relationships. The suggestion was made that depth and velocity may depend on second order polynomial functions of discharge, whilst width may retain its conventional power function relationship to discharge (figure 5. 1. 9.). An alternative arrangement of second order polynomials (width/depth) and power function (velocity) may be found in Richards (1973).

Figure 5. 1. 9.- Richards (1977) Second Order Polynomial Function for Hydraulic Geometry



From Richards (p. 83; 1977)

Richards (pp. 82 - 85; 1977) discussed four possible explanations for the complex behaviour of at-a-station hydraulic geometry 1) In sand-bed streams, the river controls its own roughness and bed configuration as discharge increases and because channel roughness (through bedforms) varies in a non-linear manner, curvilinear trends provide a better fit for the depth and velocity data 2) Scouring of sand-bed channels affects depth, cross-sectional area and velocity and is responsible for scatter in hydraulic geometry relations 3) Relative roughness (determined by grain size and depth of flow) reduces the increase in velocity with discharge 4) Width-discharge relations are complex, because low flows channelise within channels and force a lack of continuity in hydraulic geometry curves.

To conclude the review of at-a-station hydraulic geometry, Richards (p. 88; 1977) writes that, "It is not in the nature of river channels to behave in a simple manner, and it seems that ... polynomial relationships ... provide a more flexible approach to at-a-station hydraulic geometry, particularly because power functions are simply a special case."

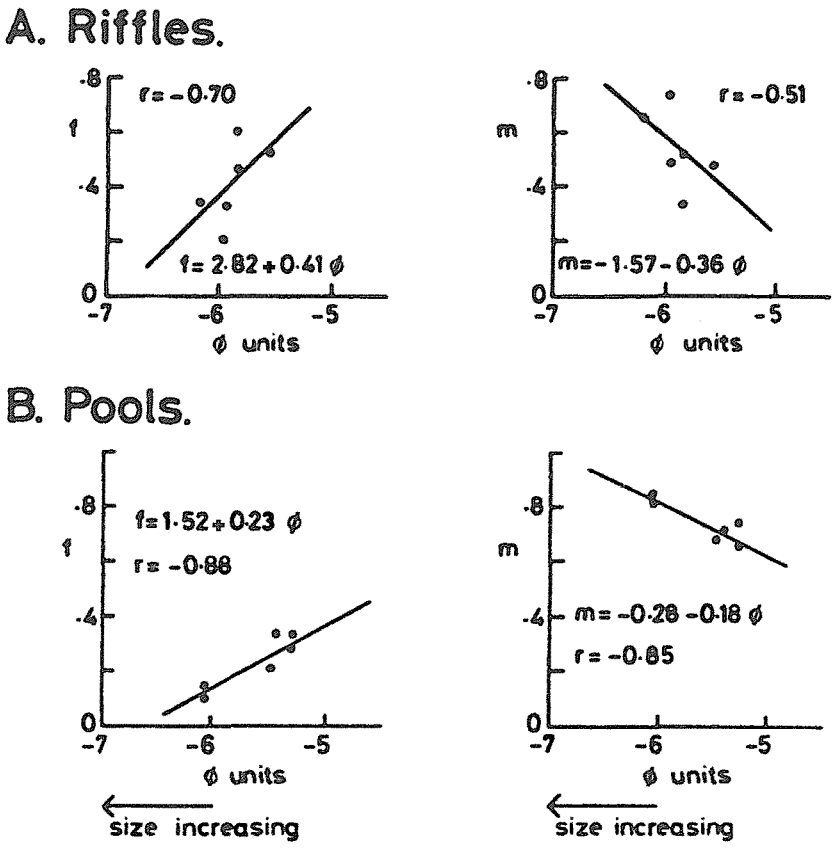
The final section of Richards (1977) was introduced with the statement that " ... local variability in channel form [is] a major contributor to scatter in downstream hydraulic relations, and this source of variation must be filtered out." In addition to considering spatial variations in bed and bank material (variables that were mentioned previously in the multivariate treatment of downstream hydraulic geometry), Richards (p. 90; 1977) suggested that hydraulic exponents differed according to their location in the riffle and pool sequence that occurs in natural channels, and presented figure 5. 1. 10. (see p. 164) which shows different magnitudes of relationship between sediment size and depth and velocity exponents in riffles and pools.

From figure 5. 1. 10. it does seem that the exponents f and m are more responsive to changes in sediment size at riffle locations than at pool locations. However, what may be more interesting is the scatter on the charts. The investigation of riffles and pools signifies two things: 1) A reduction in the spatial scale of investigation and 2) A further standardisation of boundary conditions in which processes operate - that is investigating trends in relationships in very similar environments. Without knowing the auxiliary boundary conditions that may affect the relationship between bed material size and the f and m exponents (such as bank material, channel width etc.) there is considerable variation in exponent values for relatively constant conditions. For example, in diagram A (figure 5. 1. 10.), at bed material value -6 ϕ units, four different values of the exponent f are possible, ranging from approximately 0.2 - 0.6.

Richards (p. 95; 1977) ends his paper with the statement that, "Understanding processes of channel change awaits multivariate process response models of channel geometry...," a view which provides a concise summary of the theme of the paper and is entirely consistent with the realist approach that Richards advocates.[106]

[106] See **p. 22 this thesis** for a discussion of Richards' approach to geomorphology.

Figure 5. 1. 10.- Richards (1977) f and m Exponents for Riffles and Pools



From Richards (p. 90; 1977)

Osterkamp (p. 1254; 1978) stated that "The primary purpose of the present study is the identification of typical relations of gradient to mean discharge for relatively natural alluvial stream channels with generally perennial discharge." The relationships between gradient and discharge took the form of power functions, developed using the epistemic probability technique of structural analysis.[107] This method was applied first to the gradient-discharge relationships of Kansas streams, which had been classified into ranges of sinuosity. This led to the section of table 5. 1. 5. marked in pink.

[107] A technique similar to regression analysis which measures error in the independent variable, structural analysis takes account of error in both the independent and the dependent variable.

Table 5. 1. 5.- Osterkamp's (1978) Gradient-Discharge Relationships

Sinuosity or sediment character*	Data Sets	Range of $\bar{Q}(m^3/s)$			Geometric Mean value of $\bar{Q}$ (m^3/s)	Geometric Mean value of G (m/m)	Equation, Unimposed Exponent (G=)	Average Standard Error** (percent)	Equation, imposed exponent of -0.25 (G=)	Average standard error of estimate (percent)
		Low	Hi	Log Units						
$s_i \geq 2.0$	17	0.0343	16.40	2.7	1.79	0.00055	$0.0007\bar{Q}^{-0.31}$	37	$0.0007\bar{Q}^{-0.25}$	50
$s_i = 1.7 - 1.9$	14	0.1540	47.30	2.5	3.27	0.00051	$0.0007\bar{Q}^{-0.31}$	25	$0.0007\bar{Q}^{-0.25}$	27
$s_i = 1.4 - 1.6$	17	0.0748	29.50	2.6	2.53	0.00052	$0.0007\bar{Q}^{-0.18}$	48	$0.0008\bar{Q}^{-0.25}$	49
$s_i = 1.1 - 1.3$	18	0.0986	22.40	2.4	1.08	0.00110	$0.0011\bar{Q}^{-0.24}$	33	$0.0011\bar{Q}^{-0.25}$	33
Braided	10	1.2500	200.00	2.2	9.84	0.00095	$0.0019\bar{Q}^{-0.31}$	20	$0.0017\bar{Q}^{-0.25}$	22
All Data	76	0.0343	200.00	3.7	2.41	0.00071	$0.0010\bar{Q}^{-0.24}$	49	$0.0009\bar{Q}^{-0.25}$	49
$d_{50} < 0.1mm$	17	0.1910	16.40	1.9	2.42	0.00041	$0.0005\bar{Q}^{-0.24}$	35	$0.0005\bar{Q}^{-0.25}$	35
$d_{50} < 0.1$-2 mm SC < 50%	13	0.0748	7.99	2.0	1.55	0.00074	$0.0008\bar{Q}^{-0.32}$	31	$0.0008\bar{Q}^{-0.25}$	33
$d_{50} < 0.1$-2 mm $s \geq 3.0$	23	0.0343	200.00	3.7	2.01	0.00080	$0.0010\bar{Q}^{-0.26}$	40	$0.0009\bar{Q}^{-0.25}$	40
$d_{50} < 0.1$-2 mm $s < 3.0$	23	0.0986	51.50	2.7	3.71	0.00090	$0.0012\bar{Q}^{-0.23}$	36	$0.0013\bar{Q}^{-0.25}$	36

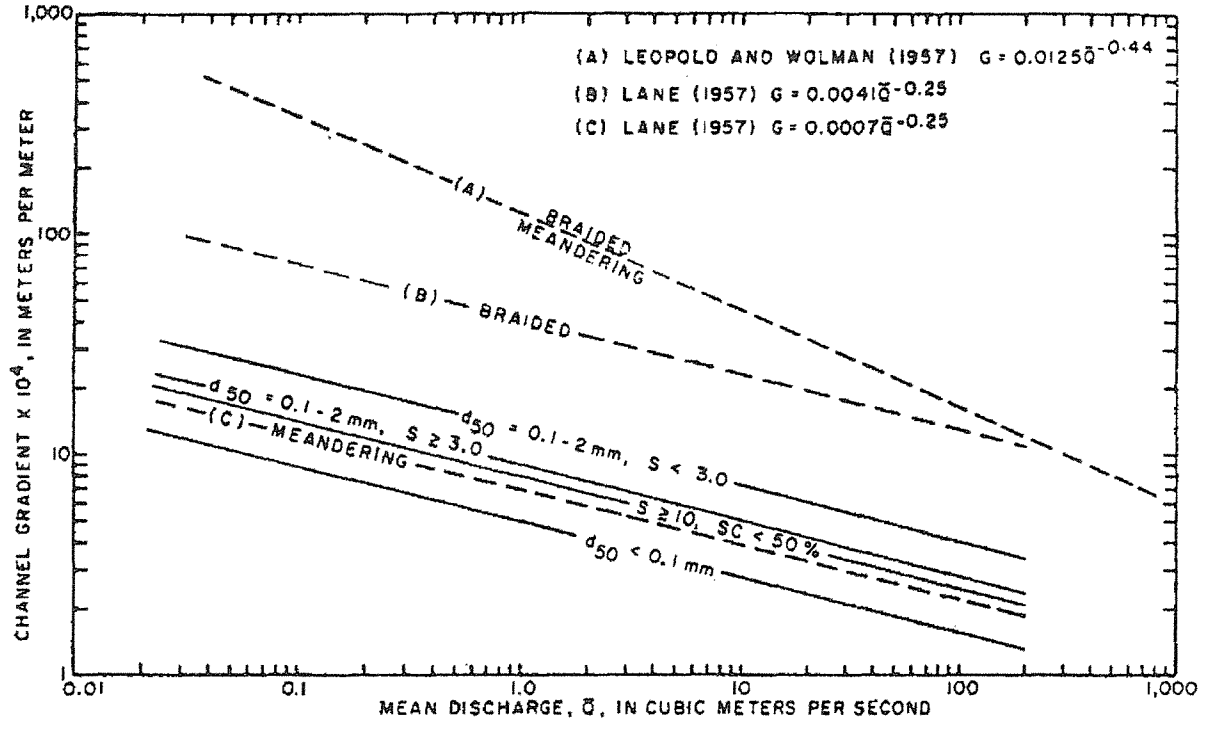
Adapted from Osterkamp (1978)

Osterkamp applied the best-fit structural analysis to the data, but obtained a poor fit (up to 48 % standard error) for individual relationships between sinuosity and channel gradient. Furthermore, there was a lack of trend or systematic variation in the coefficients and exponents between sinuosity and channel gradient. That is to say that there was no constant change, or direction of change, in the relationship between sinuosity and channel gradient as sinuosity was increased. This lack of relationship between sinuosity and gradient prompted Osterkamp (p. 1258; 1978) to lump all the channels together and provide a single measure of the relationship between sinuosity and channel gradient (the orange section of table 5. 1. 5.), similar to the approach in Leopold and Wolman (1957) and Lane (1957). The result for the exponent (-0.24) was close to that achieved by Lane (- 0.25) (1957) and Osterkamp returned to the original classification of data by sinuosity and imposed the standard exponent (-0.25) on the scatter. This led to the yellow section of table 5. 1. 5. and whilst the standard error increased (up to 50 %) there was an orderly progression from least (0.0007) to greatest (0.0017) in the coefficients.

Because the coefficients for meandering were all fairly similar (straight channels begin at S_i = 1.1 - 1.3) and because of the poor relationships between channel pattern (sinuosity) and channel slope, Osterkamp (p. 1260; 1978) concluded that "The relations indicate ... channel pattern or sinuosity is a poor criterion for grouping and statistical analysis of data."

The data for the Kansas streams were reclassified into uniform groups according to median particle size (d_{50}) and a sorting index, and a new best-fit structural analysis was performed (green section of table 5. 1. 5.). The narrower range of the exponents generated (-0.24 to -0.32), compared to the sinuosity classification, gave Osterkamp confidence in the standard exponent value of -0.25, and this value was applied to the sediment size classification (blue section of table 5. 1. 5.). This final structural analysis provided an orderly progression in the coefficients of the relationship between increasing sediment size / better sorting, and increasing slope, with less error in the structural analysis compared to that given by the sinuosity classification (see Figure 5. 1. 11.). To summarise, "The results of this set of structural analyses for sediment-size groups of Kansas streams suggest that, for a given mean discharge, gradient increases in a consistent manner with both median particle size and sorting of bed material, although the increases are not necessarily linear," (statement a) (Osterkamp, p. 1261; 1978).

Figure 5. 1. 11.- Relationship Between Sediment Size/Sorting and Channel Gradient



From Osterkamp (1978)

Having made statement a (see above), on the relationship between sediment size and gradient, Osterkamp (p. 1267; 1978) makes an observation on braided channel patterns and notes that "If braiding does require a minimum gradient, perhaps steeper gradients without change in discharge or bed characteristics can be correlated with the degree of braiding," (statement b). Statement a and statement b appear to be logically incommensurable; statement a in effect says that;

"Given a constant discharge, larger sediment sizes have steeper channel gradients."

Statement b in effect says that;

"For a constant discharge and a constant sediment size, steeper channel gradients will be braided."

The difficulty with these statements is that if channel gradient is an effect of sediment size, given constant discharge (as shown in figure 5. 1. 11.), how can different channel gradients be produced by the same controlling conditions? Osterkamp provides an answer to this logical incommensurability at the beginning of the paper;

"For a channel with known gradient, mean discharge and sediment character, the relations indicate if the gradient is higher or lower than that which generally occurs for streams with similar discharge and bed material sizes ... channel reaches that differ significantly from typical gradient-discharge relations either are influenced by anomalous conditions, which should be definable, or represent a form of instability, which should ultimately result in naturally occurring channel changes."

Osterkamp (pp. 1254 and 1253; 1978)

Braiding, according to Osterkamp, must be caused by anomalous conditions that result in a steeper than usual gradient for constant discharge and sediment size. Osterkamp (p. 1267; 1978) identifies these anomalous conditions as 1) Large amounts of sediment available relative to stream competence 2) A narrow range of particle sizes with silt and clay lacking 3) High discharge variability and 4) Regional or areal slopes sufficiently great that channel gradients can be high, relative to discharge and the sediment particle size supplied to the stream. This approach is fully in accordance with the ethos of epistemic probability which holds that the mean value is the true value of the data, and the scatter is due to error in measurement or deficiencies in knowledge/specification of boundary conditions (see Chapter Three).

In a study based on the hydraulic geometry of Leopold and Maddock (1953) and Hey's (1978) interpretation of hydraulic geometry, Mosley (p. 127; 1981) wrote that, "Although a theory using physical laws to establish a set of equations relating morphology to the controlling variables is the desirable objective, at present a more practicable alternative is to use field data to develop empirical relationships in the form of regression equations."[108] Citing Schumm (p. 256; 1969), Mosley's investigation into the causes of river channel morphology in 72 South Island New Zealand rivers, followed the maxim that "... the dimensions, shape, gradient and pattern of stable alluvial rivers should be controlled by the quantity of water and quantity and type of sediment moved through their channels."

[108] A statement which suggests Mosley subscribes to the Kittonian philosophy of geomorphology - that geomorphology is equivalent to physical science (see **Section 2. 3.**).

Accordingly, Mosley (p. 130; 1981) identified several hydrologic and sediment controls on river form (causes / boundary conditions) - with a particular emphasis on flow variability - and several descriptors of river channel morphology (effect) (table 5. 1. 6.). After some preliminary analysis, this data on hydrologic and sediment controls and river morphology was then analysed using canonical correlation analysis, a technique which is able to analyse relationships between the entire set of variables simultaneously. Mosley (p. 133; 1981) reported on the results of the canonical correlation analysis and concluded that only 53% of the total variation in the morphologic variables could be statistically explained by the sediment and hydrologic variables. The majority of this 53% explanation was related to the good correlation between the morphologic variables of channel cross-sectional area, channel slope, and relative roughness and the hydrologic and sediment variables of the mean annual flood and the mean particle size. Morphologic effects such as sinuosity, braiding index and aspect ratio (a measure of asymmetry - Mosley, p. 134; 1981) could not be explained in terms of hydrologic and sediment causes. In addition, there was no significant correlation between measures of flow variability and channel morphology.

Table 5. 1. 6.- Mosley's (1981) Variables of Cause and Effect

Variable name	Symbol	Units	Range
Flood variables			
Mean annual flood	Q_{maf}	m^3s^{-1}	3·2–4710
Standard deviation of annual floods	Q_{mafsd}	m^3s^{-1}	2·2–1230
Ratio of flood of record Q_{max} to Q_{maf}	Q_{pk}	—	1·1–60
Bankfull discharge	Q_{bf}	m^3s^{-1}	1·7–3110
Stream power	*POW*	Ns^{-1}	270–200 000
Instantaneous flow variables			
Mean discharge	Q_{bar}	m^3s^{-1}	0·091–413
Standard deviation of instantaneous flows	Q_{barsd}	m^3s^{-1}	0·17–473
Median flow	Q_{50}	m^3s^{-1}	0·025–260
Variability index	*VI*	—	0·16–1·8
Range of instantaneous flow	Q_r	—	2·2–21
Ratio of Q_{maf}/Q_{bar}	Q_{mfbr}	—	3·8–136
Sediment variables			
Mean diameter of bed sediment	*d*	m	0·001–0·17
Median diameter of bed sediment	d_{50}	m	0·001–0·19
Representative diameter, surface layer	d_{75}	m	0·004–0·36
Standard deviation of bed sediment	*SIG*	ϕ units	0·83–5·0
Silt–clay percentage, bank sediment	*SILT*	—	0·2–66
Morphologic variables—reach			
Sinuosity	*SN*	—	1·0–1·8
Slope	*SP*	—	0·0006–0·058
Braiding index	*BI*	—	1·0–7·2
Meander wavelength	*WL*	m	85–5540
Morphologic variables—cross-section			
Cross-sectional area	*XS*	m	1·4–1630
Wetted perimeter	*WP*	m	4·2–1750
Hydraulic radius = *XS*/*WP*	*HR*	m	0·33–4·6
Maximum depth	*DX*	m	0·57–6·2
Width–depth ratio = *WP*/*HR*	*WD*	—	10–2100
Aspect ratio = *DX*/*HR*	*AR*	—	1·2–4·0
Relative roughness = (HR/d_{75})	*RN*	—	1·6–618

From Mosley (p. 130; 1981)

In an attempt to formulate the quantitative empirical relationships that were the avowed aim of the paper (see above) Mosley formulated the multivariate regression equations listed in table 5. 1. 7. The multivariate regression equations followed the pattern of results outlined by the correlation analysis, with channel cross-sectional area, channel slope, and relative roughness having reasonable coefficients of determination, and sinuosity, braiding index and aspect ratio having very weak coefficients of determination. See table 5. 1. 6. for a definition of each of Mosley's variables.

Table 5. 1. 7.- Mosley's (1981) Regression Equations for River Form

1. $XS = 0{\cdot}41\ Q_{maf(0{\cdot}040)}^{0{\cdot}86}\ d_{(0{\cdot}083)}^{-0{\cdot}299}\ SILT_{(0{\cdot}048)}^{-0{\cdot}162}$ $(n = 72, R^2 = 0{\cdot}88, see = 0{\cdot}25)$
2. $WD = 0{\cdot}028\ POW_{(0{\cdot}076)}^{0{\cdot}576}\ d_{(0{\cdot}171)}^{-0{\cdot}982}\ VI_{(0{\cdot}253)}^{-0{\cdot}749}\ SIG_{(0{\cdot}340)}^{-0{\cdot}973}\ SILT_{(0{\cdot}080)}^{-0{\cdot}200}$ $(n = 65, R^2 = 0{\cdot}66, see = 0{\cdot}38)$
3. AR = no equations with coefficient significantly different from zero.
4. $SP = 0{\cdot}071\ Q_{maf(0{\cdot}034)}^{-0{\cdot}275}\ d_{50(0{\cdot}089)}^{0{\cdot}550}\ SIG_{(0{\cdot}159)}^{0{\cdot}491}\ Q_{mfbr(0{\cdot}110)}^{0{\cdot}459}\ Q_{sr(0{\cdot}166)}^{-0{\cdot}607}$ $(n = 65, R^2 = 0{\cdot}75, see = 0{\cdot}19)$
5. $SN = 0{\cdot}304\ POW_{(0{\cdot}012)}^{-0{\cdot}06}$ $(n = 61, R^2 = 0{\cdot}30, see = 0{\cdot}06)$
6. $BI = 0{\cdot}198\ POW_{(0{\cdot}036)}^{0{\cdot}159}\ d_{(0{\cdot}061)}^{-0{\cdot}133}\ VI_{(0{\cdot}121)}^{-0{\cdot}245}$ $(n = 65, R^2 = 0{\cdot}34, see = 0{\cdot}18)$
7. $RN = 0{\cdot}28\ d_{50(0{\cdot}081)}^{-0{\cdot}892}\ Q_{maf(0{\cdot}032)}^{0{\cdot}249}\ VI_{(0{\cdot}125)}^{0{\cdot}420}\ SIG_{(0{\cdot}145)}^{-0{\cdot}322}$ $(n = 65, R^2 = 0{\cdot}78, see = 0{\cdot}19)$
8. $WL = 46\ Q_{maf}^{0{\cdot}51}$ $(n = 53, R^2 = 0{\cdot}75)$

Standard errors of regression coefficients in parentheses, beneath equation.
n is sample size; R^2 is coefficient of determination; *see* is standard error of estimate.

From Mosley (p. 134; 1981)

A significant qualification to Mosley's work is the statement by Schumm (1971a) that "Discharge is an independent variable that largely determines the size of the stream channels and the amplitude and wavelength of meanders. Discharge ... is not a valid basis for classification unless size is considered most important." It may be argued that discharge is important for determining other aspects of channel morphology other than size, because it determines both the mode of sediment transport and the amount of sediment transport. However, discharge is a clear control on the size of the channel. Mosley (p. 132; 1981) noted that cross-section was the only dimensional variable included in the analysis; that is, cross-sectional measurements are absolute, not relative, and as such, reflect differences in scale in correlations. It is possible that the good correlation between cross-sectional area, relative roughness (which is dependent on cross-sectional area via hydraulic radius) and discharge (which accounts for most of the 53 % explanation) reflects changes in scale and magnitude, not fundamental differences in process and form relationships.

Mosley (pp. 135 - 137; 1981) provided some possible explanations for why only 53% of the variability in the descriptors of morphology could be explained by the hydrologic and sediment variables. Attention focused on; the non-normal distributions of sinuosity, braiding index and aspect ratio (normality is essential for the regression model), quasi-random controls, the effect of bank vegetation, and the extreme variability in bank material characteristics. Whilst these variabilities contributed to the lack of explanation by the hydrologic and sediment variables, another source of variability was cited.

"Even if all these identifiable factors are considered, and even if they could all be quantified, both the analysis and observations made during the course of the study indicate that they cannot completely determine channel morphology. Channel width and shape (width-depth ratio, asymmetry, etc.) vary greatly along short reaches in which the controlling variables are constant, while planform, (degree of braiding, sinuosity) also can change markedly in short distances. In many places, rivers change irregularly from braided to meandering and back again with commensurate changes in other morphologic variables; difference in sediment load, discharge regime, valley slope, vegetation etc. simply cannot account for variations of this type."[109]

Mosley (p. 136; 1981)

Mosley (p. 136; 1981) concluded that " ... overall, however it appears that a model of channel morphology or hydraulic geometry cannot be completely deterministic, but must include a component to take account of random or quasi-random effects."

Carson (1984) began an investigation into river channel patterns by providing a critique of Leopold and Wolman (1957) and noting two circularities in the arguments they presented for a discharge-slope discriminant function. Firstly, Carson (p. 318; 1984) pointed out that channel slope is partly an effect and not a cause of channel pattern; " ... channel slope is not a fully independent control on channel pattern, being in part dependent on sinuosity, and hence upon whether the stream is braided or meandering." In other words, a meandering pattern will have a lower channel slope because, by definition, a meandering channel requires a longer distance than a straight channel to cover the same fall in gradient.

Secondly, Carson (1984) observed that the use of bankfull discharge was not completely independent in determining channel planform; " ... there are grounds for suspecting that some braided rivers have larger Q_{bf} values simply because of larger 'bankfull' cross-sectional areas, a consequence of the multiple wide channels that are formed by braiding ... bankfull discharge has a far more variable relationship to actual discharge frequency than was originally supposed."

[109] This is the complexity of the variation that scientific laws must address. **Chapter Seven** contains contour plots of flume morphology which must be predicted at some scale in terms of an appropriate law.

Building on these criticisms, the thrust of Carson's paper deals with the statement in Leopold and Wolman (1957) that channel slope is an effect of bankfull discharge. Carson argues that channel slope is an effect of discharge (taken as the two year flood) but that it is heavily dependent on a measure of the sediment particle size for the channel. To define the effect of sediment size on channel slope, Carson first specifies an equation for channel slope given grain diameter and flow depth.

Equation 5. 1. 1

$$s \cong d / 11D$$

S = threshold slope for movement of particle size d
d = d_{50} particle size (mm)
D = flow depth

Carson then proposes an equation from hydraulic geometry, relating the best-fit relationship for change in flow depth with discharge:

Equation 5. 1. 2.

$$D \cong_c Q^f$$

Where:

D = flow depth (m)
c = coefficient of hydraulic geometry
f = exponent of hydraulic geometry
Q = discharge ($m^3 s^{-1}$)

Finally Carson combined the two equations for slope/depth and discharge/depth, producing a log-linear change in slope with changes in discharge for a constant sediment size.

Equation 5. 1. 3.

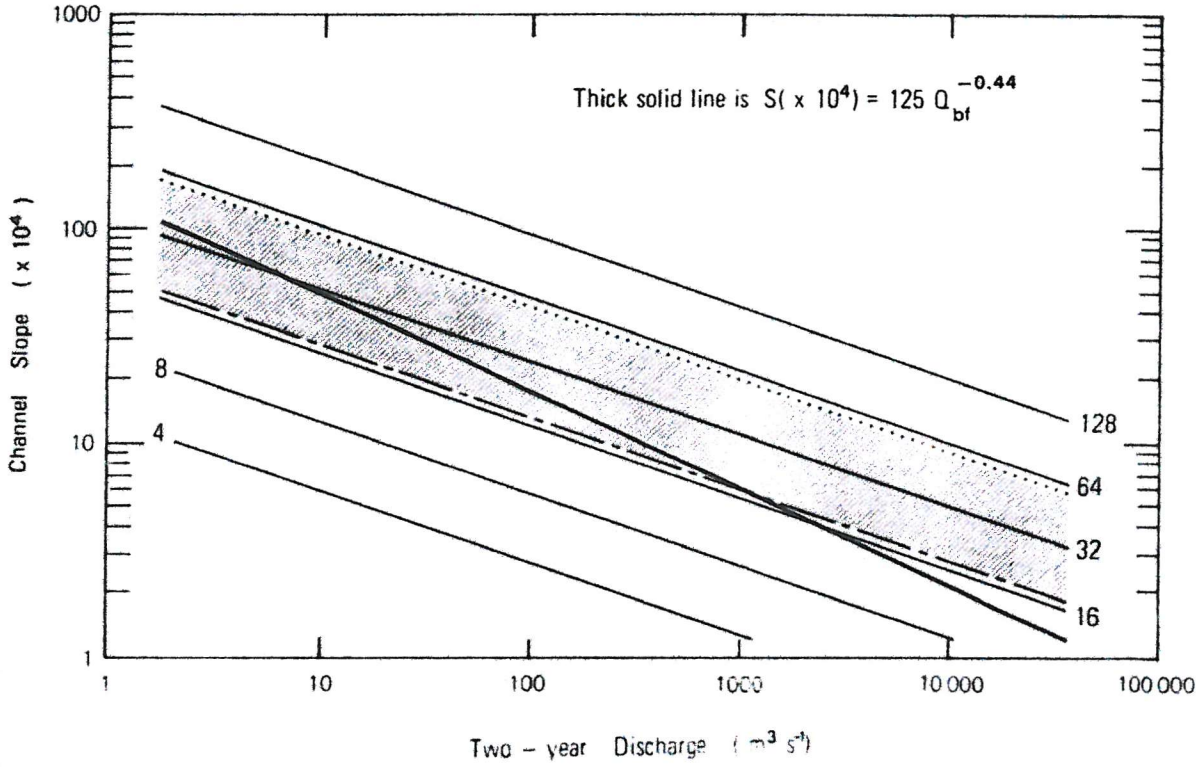
$$s^*(x\,10^4) \cong 0.91(d / c)Q^{-f}$$

Where:

S* = slope
d = particle size (mm)
c = coefficient of hydraulic geometry
Q = discharge ($m^3 s^{-1}$)
f = exponent of hydraulic geometry

Carson's rationale then, is that channel slope is an effect of sediment size and flow depth. Because flow depth is an effect of discharge, channel slope is an effect of sediment size and discharge. This reasoning results in a summary of the relationships expressed as figure 5. 1. 12. Because coarser sediment has a higher threshold slope for sediment movement, for the same discharge, coarser sediment channels will have steeper slopes (this is similar to Osterkamp's (1978) result).

Figure 5. 1. 12.- Carson's (1984) Relationship Between Discharge and Channel Slope for a Given Sediment Size (d_{50}).



From Carson (p. 323; 1984)

In figure 5. 1. 12. the shaded area represents a broad band of threshold movement for gravel of size 32 mm. Because the chart is on a logarithmic scale, this broad band states that for any given discharge (boundary condition), the resulting channel slope (effect) for this particle size (cause) can vary by an order of magnitude (i.e. at Q = 10, channel slope can assume values of 30 or 100). Carson (p. 321; 1984) explains this behaviour; " ... because of variance in the values of *c* and *f* there is no unique threshold line for given bed material, rather a fairly broad band."[110]

In terms of the relationship between channel slope and channel pattern, Carson (p. 315; 1984) states that within a given sediment size there is "... only a weak statistical association between pattern and slope - discharge relationships." Carson (p. 330; 1984) prefers to explain channel braiding in terms of a physical mechanism of "... a shoaled thalweg that locally forces flow out of the single thread pattern .." and which is caused by an "... imposition of bed calibre sediment on the reach."

[110] This statement of varying slope for constant discharge and constant sediment size, reinforces the scatter present in Leopold and Wolman's chart and provides some contrary evidence to their explanation, that the scatter in slope with discharge was caused by different sediment classes (see **p. 148 this thesis**).

This mechanism is attached to sediment size (pp. 329 - 332; 1984); because sandy channels have low gradients, cohesive banks and predominantly transport sediment as suspended load, it is not likely that sandy channels will develop the crucial imposition of bedload; conversely because gravel bed channels have steep gradients, erodible banks and predominantly transport sediment as bedload, they are likely to develop the crucial imposition of bedload. This means that, according to Carson (p. 328; 1984), the discriminant function which Leopold and Wolman (1957) proposed for meandering and braided channels, using rivers with different sediment sizes, is approximately correct because it discriminates between sand and gravel bed channels, and the mechanism associated with braiding is more likely to occur with gravel bed channels.

Schumm, Mosley and Weaver (p. 129; 1987), expounded the motivation for experimental fluvial geomorphology; "A prime reason for resorting to experimental work is that, under closely controlled constraints, there is a definite unique relationship between the dependent (form) and independent (controlling) variables that is not influenced by other factors over which there is no control. In other words, under the same constraints and with the same value of the independent variables, channel form should be reproducible."[111] The ethos behind this statement is that "Rivers have a regularity of form that is both intriguing and holds out the promise that explanation is feasible," and "It seems that, in principle, the form and behaviour of alluvial channels should be explicable in the laws of physics," (pp. 133 and 129; 1987).

Schumm *et al.* (p. 135; 1987) write that the laws that apply to river behaviour often take the form of power functions, and example of power functions from experimental studies are provided. Schumm *et al.* (p. 169; 1984) state that 'valley slope partially controls the erosive capacity of a watercourse' via its affect on shear stress and stream power, and provide graphs relating changes in slope (for given discharges) to changes in width, mean depth and the width depth ratio in experimental conditions. (**Figures 5. 1. 13: 5. 1. 14: 5. 1. 15**).

[111] The experimental analysis in **Chapter Seven** aims to examine at which scale the relationship between control and form becomes deterministic.

Figure 5. 1. 13- Changes in Channel Width with Valley Slope

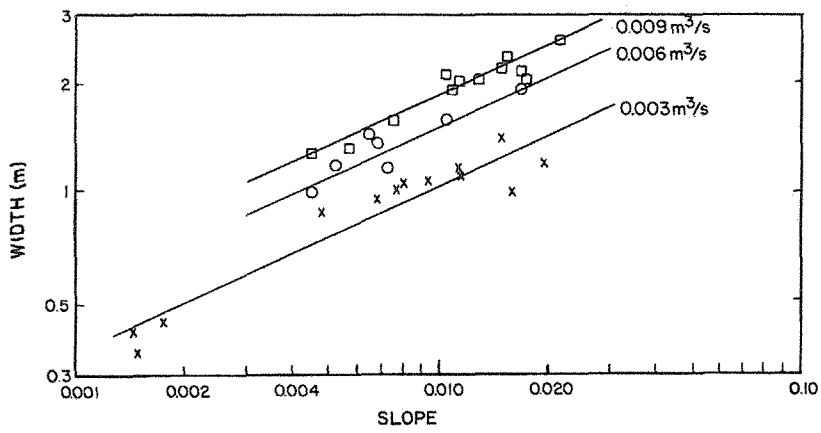


Figure 5. 1. 14- Changes in Mean Channel Depth with Valley Slope

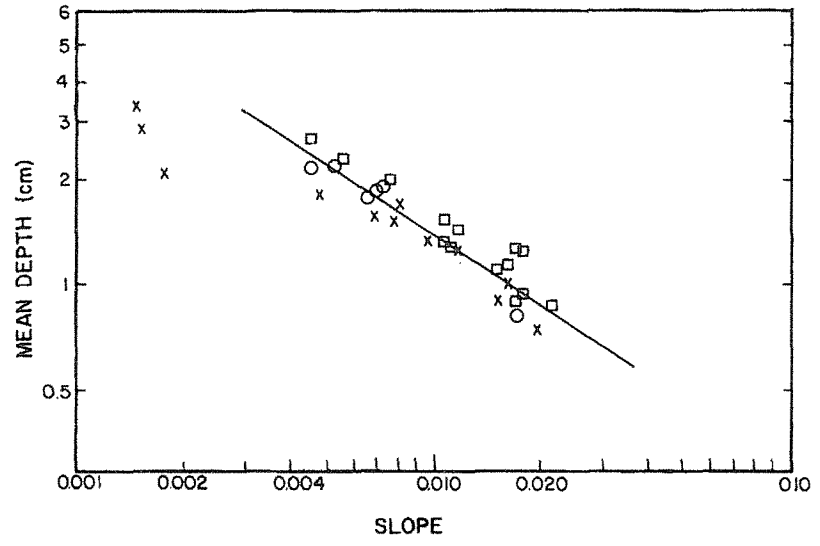
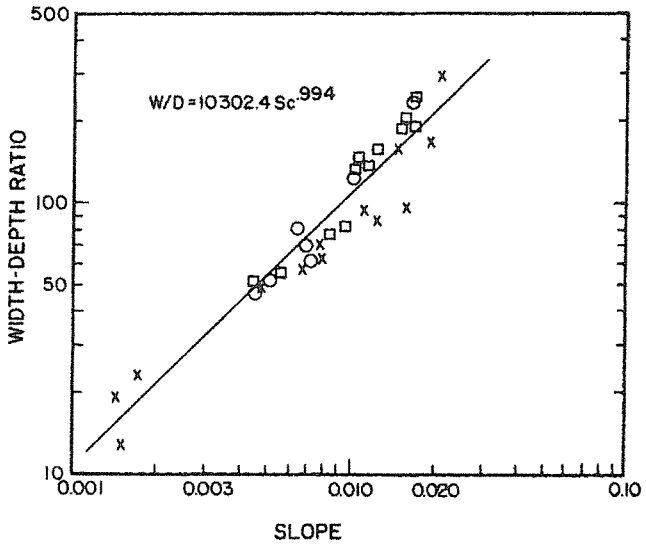


Figure 5. 1. 15.- Changes in Channel Width/Depth Ratio with Valley Slope



From Schumm *et al.* (1987)

The graphs in Figure 5. 13 - 5. 15. contain a scattering of points - this is particularly the case for the 'x' data set in the slope/width and the slope/width-depth charts. The presence of scatter in experimental data sets, does not fit with the reproducibility of form (effect) given constant causes and boundary conditions. However, Schumm *et al.* (pp. 174 and 176; 1987), though not specifically explaining the scatter in the above relationships, generally explain scatter as differences in experimental set up.[112]

Shields Jr. (1996) presented a review of existing knowledge that would assist engineers in making decisions for channel restoration. The aim of channel restoration claims Shields Jr. (p. 30; 1996), is to effectively predict what channel form would have developed in the absence of human intervention, given the prevailing natural controls on river behaviour; "Channel restoration ... implies designing a channel similar to one that would develop under similar watershed conditions."

Shields Jr. (1996) uses the hypothetical example of the 'sorry river,' an engineered river that has been selected for channel restoration. A review is presented of the empirical regime and hydraulic geometry equations that are available to calculate the average values of depth and width. Table 5. 1. 8. lists the coefficients and exponents that predict width, depth and slope values for river channels, given values of discharge and/or particle size.

[112] The concept of error is key to fluvial theory (see **Section 5. 2. 2.**)/

There are considerable differences in the values of the exponents, particularly in the case of exponent k_6 for depth, and exponents k_8 and k_9 for slope.[113] Shields (p. 50; 1996) inserted the 'sorry river' channel forming discharge of 40 $m^3 s^{-1}$ and the particle size measure d_{50} = 10 mm into the different hydraulic geometry equations and obtained the results given in table 5. 1. 9. Finally, using varying discharge, Shields derived the different values of width, depth and slope shown in Figure 5. 1. 16. Shields recognises the problem of indeterminacy in river channel form, but suggests that the primary reason for differences in empirical hydraulic geometry relationships is that the equations are based upon regional data sets (p. 37; 1996) - differences in equations reflect different conditions in nature.

Table 5. 1. 8.- Shields (1996) Review of Hydraulic Geometry Equations.

Coefficients for equations of the form $w = k_1 Q^{k_2} D_{50}^{k_3}$; $d = k_4 Q^{k_5} D_{50}^{k_6}$; $S = k_7 Q^{k_8} D_{50}^{k_9}$, where w is mean bankfull width (m), Q is the bankfull or dominant discharge ($m^3 s^{-1}$), d is mean bankfull depth (m), D_{50} is median bed-material size (mm), and S is slope (m/m)

Reference	Data	Domain (also see Table 2.6)	k_1	k_2	k_3	k_4	k_5	k_6	k_7	k_8	k_9
Nixon (1959)	UK rivers	Gravel bed rivers	2.99	0.5		0.55		0.33	$0.850n^{2b}$	−0.11	
Leopold et al. (1964)	Midwestern US	Ephemeral streams in semiarid US		0.5 0.5		0.4	0.3			−0.49 −0.95	
Kellerhals (1967)	Field (US, Canada and Switzerland) and laboratory	Gravel-bed rivers with paved beds and small bed material concentration	3.26	0.5		0.417	0.4	-0.12^a	0.00015	−0.4	0.92^2
Schumm (1977)	US (Great Plains) and Australia (Riverine Plains of New South Wales)	Sand-bed rivers with properties shown in Table 2.6	$43.69k_1^*$	0.38		$0.514k_4^*$	0.29		$0.0036k_7^*$	−0.32	
Bray (1982)	Canadian rivers	Gravel-bed rivers	6.21	0.53	−0.07	0.31	0.33	−0.03	0.001	−0.33	0.59
Parker (1982)	Single channel Alberta rivers	Gravel-bed rivers, banks with little cohesion	9.15	0.444	−0.11	0.205	0.4	−0.0025	0.000312	−0.394	0.985
Hey and Thorne (1986)	UK rivers	Gravel bed rivers with: grassy banks with no trees or shrubs	4.33	0.5		0.47	0.37	−0.11	$0.00049k_7^{**}$	−0.43	−0.09
		1–5% tree/shrub cover	3.33	0.5		0.47	0.37	−0.11	$0.00049k_7^{**}$	−0.43	−0.09
		greater than 5–50% tree/shrub cover	2.73	0.5		0.47	0.37	−0.11	$0.00049k_7^{**}$	−0.43	−0.09
		greater than 50% shrub cover or incised flood plain	2.34	0.5		0.47	0.37	−0.11	$0.00049k_7^{**}$	−0.43	−0.09

[a]Bed material size in Kellerhals equations is D_{90}
[b]Manning n
$k_1^* = M^{-0.39}$, where M is the percent of bank materials finer than 0.074 mm. The discharge used in this equation is mean annual rather than bankfull.
$k_4^* = M^{0.340}$, where M is the percent of bank materials finer than 0.074 mm. The discharge used in this equation is mean annual rather than bankfull.
$k_7^* = M^{-0.38}$, where M is the percent of bank materials finer than 0.074 mm. The discharge used in this equation is mean annual rather than bankfull.
$k_7^{**} = D_{84}^{0.84} Q_k^{0.10}$
where Q_k = bed material transport rate in kg s^{-1} at water discharge Q, and D_{84} refers to bed material and is in mm.

From Shields (1996)

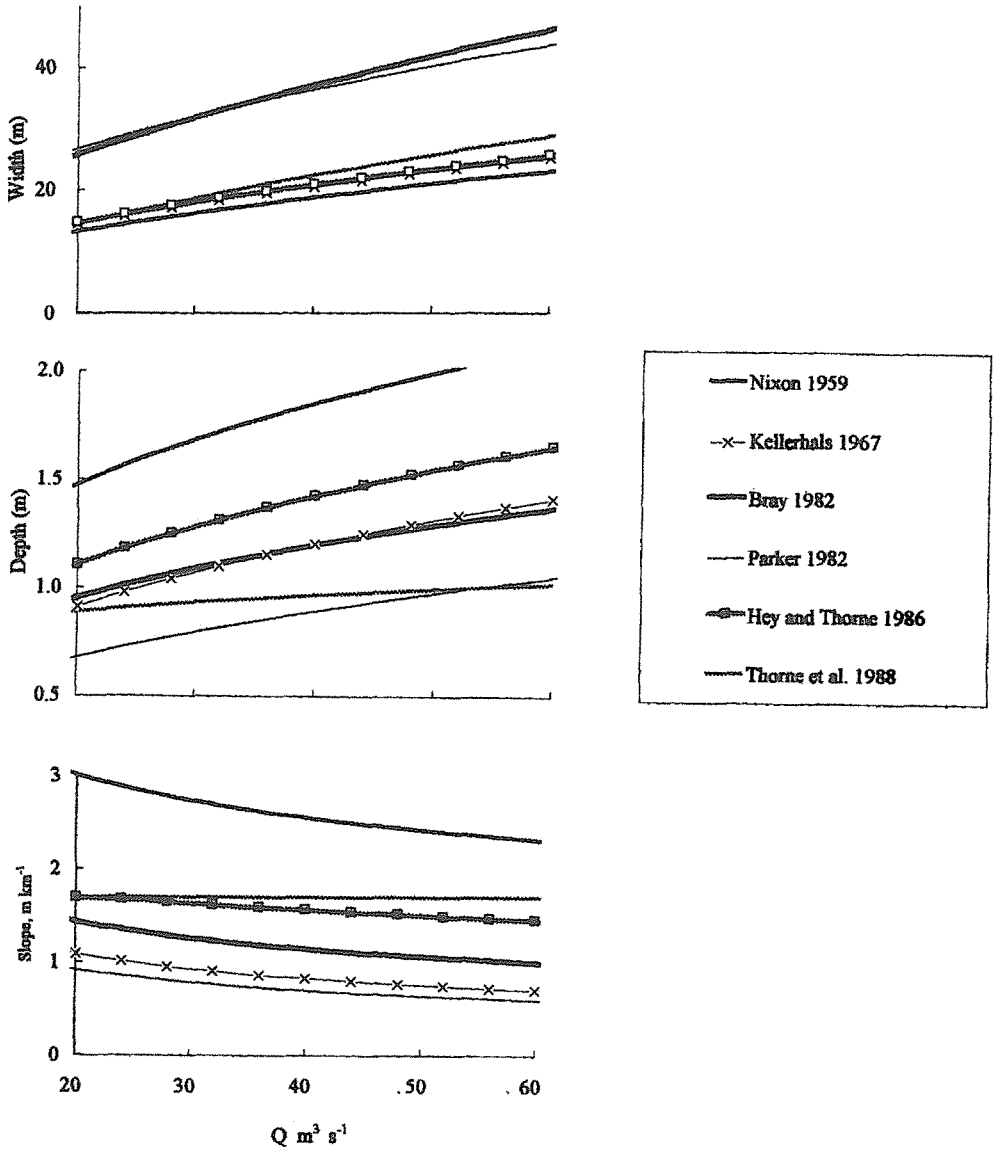
[113] This may be of particular significance between Parker (1982) (k_6 = -0.0025) and Hey and Thorne (1986) (k_6 = -0.11) where the bank conditions appear to be most similar (no cohesion).

Table 5. 1. 9.- Shields (1996) Different Results for Sorry River Using Selected Equations from table 5. 1. 8.

Reference	Top width, m	Depth, m	Slope
Nixon (1959)	19	1.9	0.0026
Bray (1982)	37	1.0	0.0012
Kellerhals (1967)	21	1.2	0.0008
Parker (1982)	37	0.9	0.0007
Hey and Thorne (1986)	21	1.4	0.0016 to 0.0019

From Shields (1996)

Figure 5. 1. 16.- Plot of Shields (1996) Results for Sorry River in table 5. 1. 9 with Varying Discharge.



From Shields (1996)

Alabyan and Chalov (1998) provided a review of what they perceived as an ignored body of Russian literature on river channel patterns, and proposed that Rossinskiy and Kuz'min (1947) and Andreev and Yaroslavtsev (1958) devised a tripartite division of river channel patterns prior to, or contemporaneous to, Leopold and Wolman (1957). Alabyan and Chalov (p. 467; 1998) cite Lohktin (1897) and report that this paper proposed a channel development criterion in terms of the ratio between stream power (energy) and the erodibility of the bed (resistance).

Developing this stream power criterion of Lohktin, Alabyan and Chalov aimed to provide a consistent operational definition of river patterns, suggesting that pattern form could be examined on three structural levels: valley bottom, flood channel and low water channel. The most important product of this scale division of channels was that it attempted to eliminate circularity in the cause and effect of channel planforms. It was seen in Leopold and Wolman's (1957) classification that channel planform was defined in terms of channel slope, when a channel slope is an intrinsic feature of channel planform.[114] In order to secure some independence of variables, stream power (unit weight of water x slope x discharge) in Alabyan and Chalov was calculated with reference to the slope value of the scale above that being investigated. Thus if the pattern of the flood channel is being investigated, the valley bottom slope is used to calculate stream power; if the low water channel is being investigated, the flood channel slope is used to calculate stream power.

Proceeding from this scale division of channel form, Alabyan and Chalov (pp. 471 and 472; 1998) constructed a median annual flood/valley gradient chart for flood channel planform and an effective discharge[115]/flood channel slope chart for low water channel planform (Figure 5. 1. 17. and Figure 5. 1. 18). Channel planform was plotted as a third variable and discriminatory lines of equal stream power delineated between different planform types.

[114] See **p. 171 this thesis.**

[115] "Practically, the effective discharge may be determined using a diagram in which values of the product of sediment discharge and frequency of occurrence are plotted against corresponding water discharge values - *the effective discharge curve.* The effective discharge has the maximum value of this product and corresponds to the maximum of the curve." (Alabyan and Chalov, p. 471; 1998).

Figure 5. 1. 17.- Change in Flood Channel Planform with Valley Slope / Median Annual Flood

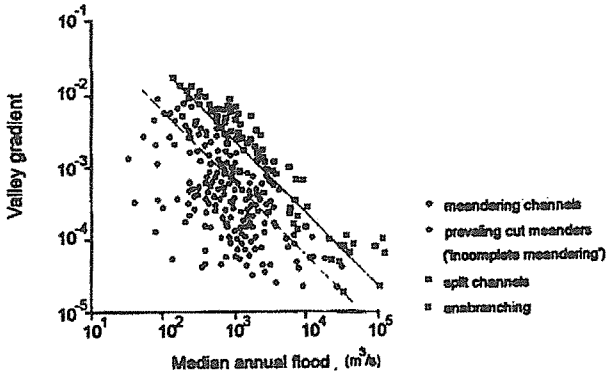
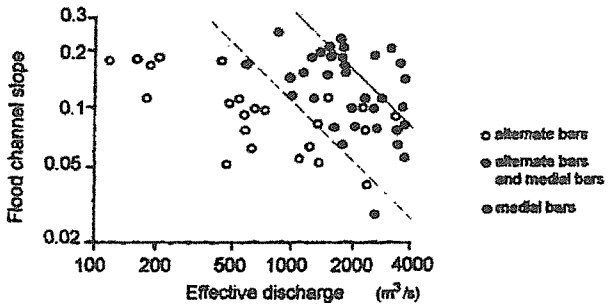


Figure 5. 1. 18.- Change in Low Water Channel Planform with Valley Slope/ Effective Discharge



From Alabyan and Chalov (pp. 471 - 472; 1998)

Unlike Leopold and Wolman (1957), Alabyan and Chalov (1998) do not causally relate slope and discharge. Channel planform (effect) is causally related to the product of slope and discharge, or stream power (cause). Figure 5. 1. 17. expresses the same relationship seen earlier in Figure 5. 1. 5. and it is only the absence of a quantitative measure of river planform that prevents a power/planform chart being constructed. In agreement with Leopold and Wolman (1957), Alabyan and Chalov (p. 467; 1998) are at pains to stress that river channels form a continuum and that '..many intermediate patterns exist, and are the norm rather than the exceptions.' It seems as if they would expect changes in magnitude of stream power to produce proportional changes in effect and it is only the lack of a quantitative measure of river planform that forces a discriminant line to be drawn.

Alabyan and Chalov (p. 470; 1998) do not attempt to explain the scatter of channel planforms on their own charts (i.e. in Figure 5. 1. 18. alternate bars persist at stream powers associated with medial and mixed bar forms), but do suggest an explanation as to why discriminant functions for channel planform have different values depending on the researcher; "... discriminant functions for meandering and braided channels vary across a wide range. One of the main reasons for this lies in differences between operational definitions both of the slope and discharge and meandering."

5. 2. Four Ideas that Support the Application of Causal Laws in Fluvial Theory.

In order for characteristic form or causal laws to be applied to complex fluvial systems, several assumptions need to be made and additional theoretical constructs need to be proposed. Two assumptions behind the characteristic form concept are 1) that representative independent and dependent variables can be formulated 2) that scatter on regression charts is due to error. Two theoretical constructs that have developed from the supposition of error are 1) equifinality and 2) equilibrium. These four ideas are discussed below.

5. 2. 1. Aleatory/Representative Variables.

Causal laws produce an exclusive relationship between a unique cause and a unique effect and exhibit a proportionality in that exclusive relationship, such that if the magnitude of the cause increases, the magnitude of the effect increases.[116] In geomorphological investigations, reasoning usually proceeds from the effects[117] which are *in situ* to the causes which produced them. Due to indeterminacy, fluvial theory is an essentially empirical study and relies on accurate comparisons between data, and the first priority in the application of causal laws to in-channel behaviour is to define an appropriate measure of channel morphology (effect) to which a cause can be associated. This is difficult because the variables that describe channel morphology (effect) such as width, depth, cross-sectional area, channel slope and channel pattern may fluctuate considerably within a location.

[116] **See Sections 3. 4. 1.** and **3. 4. 3. 2.** for a definition of causal laws.

[117] **See p. 23 this thesis** for a discussion on abduction.

Several authors have commented on this variability. As mentioned previously[118] Carlston (p. 878; 1965) noted that there was a, "... high variability of width of channel along the course of a stream." Similarly, Dury (p. 220; 1976) comments that "Width on some channels varies much over short distances," and Mosley (p. 137; 1981) expounds this variability writing that, "Channel width and shape (width-depth ratio, asymmetry etc.) vary greatly along short reaches in which the controlling variables are constant, while planform (degree of braiding, sinuosity) also change markedly in short distances."[119]

An attempt has been made to overcome this variability in the descriptors of channel morphology by using average values of width, depth etc. as shown in Table. 5. 2. 1. 1. (see p. 196 this thesis) Whilst mean values are the most common representative measures of channel morphology, other values have also been used. The River Habitat Survey (N. R. A., p. 2; 1996: N. R. A., p. 9; 1998) for example, uses "Cross-Section measurements of water and bankfull width, bank height and water depth ... made at one representative location," for each reach of river channel surveyed.

Given that the complex variables (width/depth etc.) which describe channel morphology (effect) can be measured by representative mean values, a cause, and the boundary condition that produce the observed channel morphology (effect), need to be defined. This is problematic because river systems are subjected to varying ranges of discharge (cause) and diverse sediment calibre (boundary condition), and it is difficult for investigators to know which magnitude of discharge and which measure of sediment have caused the channel to have the observed morphology.

Some assistance for choosing the discharge that caused the channel was provided by the landmark paper of Wolman and Miller (1960). In this paper, Wolman and Miller argued that landforms were generally caused by those fluid flows that achieved the most work, and that work could be measured by the product of the frequency of the fluid flow and its magnitude (measured in applied stress). The events which accomplished the most work would be those of moderate frequency and moderate magnitude (figure 5. 2. 1. 1. - p. 197 this thesis). Because applied stress is difficult to measure, Wolman and Miller (p. 56; 1960) suggested a pragmatic equation for determining the work done by a given fluid flow, by examining the amount of sediment it carried.

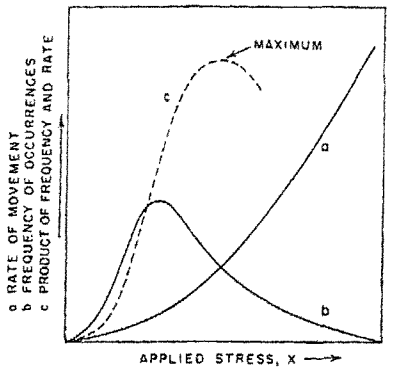
[118] **See p. 152 this thesis.**

[119] Schumm *et al.* (p. 173; 1987) have commented on the difficulty of defining channel width in flume studies.

Equation 5. 2. 1. 1.

$$\frac{\text{(Sediment carried by given flow) x (Frequency of given flow)}}{\text{Total Sediment Transported}} \times 100 = \%\text{ of total sediment carried by flows of different magnitude}$$

Figure 5. 2. 1. 1- Wolman and Miller's (1960) Concept of Frequency and Magnitude



From Wolman and Miller (p. 56; 1960)

In order for equation 5. 2 .1. 1. to be solved, data for "... the quantity of material carried by flows of various magnitudes are known and the frequency of occurrence of each flow is also known ..." (Wolman and Miller, p. 56; 1960). To obtain this data on frequency and sediment yields, Wolman and Miller (p. 55; 1960) recommended that " ... a record of twenty five to fifty years, considerably longer than most river records, may be sufficient to provide an adequate sample of a river's regime of flow ..."

Wolman and Miller (p. 73; 1960) set the caveat on frequency and magnitude analysis that the "Evaluation of the relative importance of various geomorphic processes in a given region, as well as the relative effectiveness of events of different frequency, will require more detailed observations of the landforms themselves and the processes operating on them." In other words, the specific combination of frequency and magnitude that produces the most work, will differ from region to region, and needs individual analysis.

Having made this caveat, Wolman and Miller (pp. 54 and 66; 1960) equated the discharge that achieved the most work (i.e. the discharge which occurred with moderate frequency and magnitude) to the bankfull discharge and this particular measure of flow has been widely adopted as being the cause, or at least as a first approximation to the cause (in the absence of river gauging - see Richards, p. 70; 1977) of channel morphology.

Wolman and Miller (1960) provided a rational basis for choosing a single representative measure of discharge that could be related to channel morphology - the discharge that achieved the most work could legitimately be thought of as the dominant or the causal discharge. Table 5. 2. 1. 2. (see p. 197 this thesis) lists some of the representative measures of discharge that have been related to channel morphology.

Wolman and Miller qualified what they perceived the concept of dominant discharge represented;

> *"Because of the range of discharge to which most natural channels are subject, it is logical to assume that the channel shape is affected by a range of flows rather than by a single discharge. Blench (1951, sec 6. 16) for example, refers to a dominant discharge as 'the discharge that would actually produce the same result as the actual varying discharge."*
>
> Wolman and Miller (p. 65; 1960)

In other words, Wolman and Miller viewed other discharges as capable of changing river channel morphology, but those changes blended in to the morphology created by the dominant discharge (the discharge that achieves the most work). To use Wolman and Miller's (p. 73; 1960) analogy, a dwarf makes little indentations in a tree with his axe, and then a man comes along and hits exactly the same spot on the tree with his axe. Although the dwarf had an effect on the tree, his efforts are now obscured by the man's much larger indentation.

Some authors however, have proposed that different discharges may have visible effects - that is to say that channel morphology reflects the range of discharges that have acted upon it, and not just the effects of the dominant discharge (see Hickin, pp. 249 - 250; 1977). For example, Pickup and Warner (1976) specified three discharges (return periods of; 1.15 -1.40 yrs: 1.58 yrs: 4 - 10 yrs) that operated on different elements of the channel and Osterkamp and Hedman (p. 19; 1982) wrote that "... channel size and shape are the integrated results of all water and sediment discharges conveyed by the channel and ... the concept of a specific channel-forming discharge seems inappropriate."

In tandem with discharge, sediment is the other major control on alluvial channel form, both in terms of the type of sediment and the amount supplied. The sediment that composes river channels is often a complex mix of different materials, each with different weights, sizes, shapes etc. Because the type of sediment influences the resistance of a channel to the force applied by some aspect of the discharge (velocity, shear stress or stream power), it is essential that a single representative value of sediment (boundary condition) can be related to any given value of cause (discharge) and effect (morphology).[120]

[120] The effects of different discharges and sediments have been tested in the flume (see **Appendix,** Section G).

Considerable attention has been given to defining a representative measure of a distribution of particle size (Lane, p. 132; 1935: Leopold, Wolman and Miller, pp. 188 - 195; 1964: Richardson, pp. 6-10 to 6-16; 1971: Simons, pp. 20-8 to 20-9; 1971). Table 5. 2. 1. 3. shows some of the criteria that have been used to present a single representative measure of sediment size; the most commonly used measure is the median particle diameter. The use of the median diameter was advocated by Richardson (p. 6-16; 1971) who noted that "It is commonly held by both geologists and hydraulic engineers that the size distribution of natural sediments will plot as a straight line on log probability paper.[121] If this is true, then a natural sediment can be completely described by the median diameter (the size of the sediment of which 50% is finer and 50% is larger by weight)." However, Mahmood and Shen (p. 30-16; 1971) note that in the canals of the Punjab, for which regime equations were derived, under constant sediment and discharge conditions, channel cross-sections could have right or left skewed distributions of sediment size.

The specification of channel sediment (boundary condition), in terms of a single value, has considerable problems. Factors such as friction angle, projection and exposure (Kirchner et al.; 1990), density and sorting (Knighton; 1984), shape (Lane; 1935) spatial distribution (Leopold, Wolman and Miller; 1964) turbulence fluctuations (Drake et al.; 1988) all affect the resistance and motion of individual grains - variations not described by median values of grain size. In addition to these uncertainties surrounding the individual grain size, further uncertainties are introduced by the cumulative properties of sediment behaviour in bedforms (Simons et al.; 1965).

The use of statistical values to describe cause and effect, imply that geomorphology is primarily concerned with epistemic relationships between aleatory distributions. Purely causal laws that examine individual behaviour (for example individual channel width) in terms of the individual controls, have not generally been applied in geomorphology.[122] A justification for the use of aleatory values, and the fact that modern science recognises that certain behaviour can only be understood by the properties of group behaviour, has been discussed in detail in chapter three. Therefore, it is expected of the fluvial laws suggested by the case studies in this chapter, that for a given distribution of control, an *invariant* distribution of effect should be produced. That is, current understanding in fluvial geomorphology predicts a causal/deterministic relationship at the scale of the distribution. Any departures from this relationship are thought to be due to error, equifinality or equilibrium, reviewed below. The view of science in Chapter Three is essential to this perception of fluvial theory.

[121] i.e. a normal distribution.

[122] See the case studies in this chapter. Nearly all of them are concerned with the properties of distributions, not individual cases.

5. 2. 2. Error.

As stated above, the application of causal laws to complex situations, often makes use of the techniques of epistemic probability (such as regression, correlation etc.), which are designed to test the truth of a proposition.[123] Any statistical variation or deviation from the assumed true relationship, is ascribed to error or noise (unexplainable variance). Krumbein and Graybill (1965), Chorley (1966), and Chorley and Kennedy (1971) have provided a list of possible sources of error or noise that may occur in geomorphological investigations. These are listed below and an example of that form of error is provided from the previous review of causal laws in geomorphology or from a wider literature.

1. "Standard error noise ... arising solely from the assumed normal distribution of the scatter ... there are many possible reasons for this, varying from errors in measurement to some assumed natural variation inherent in the phenomena being expressed."

Chorley and Kennedy (p. 31; 1971)

2. "Other variable noise ... this noise varies in amount with the particular value of Y which is considered, suggesting that one or more other independent variables (X_2, X_3 ... etc.) are exerting an influence on Y."

Chorley and Kennedy (p. 34; 1971)

"It is important to keep in mind that these data describe certain natural channels. In natural channels specific variables often occur in association. For example, steep slopes are associated with coarse material. In a system which contains ... a minimum of seven variables, a diagram ... which treats only two of these cannot be expected to describe the mechanism of adjustment or all theoretically possible conditions."

Leopold and Wolman (p. 60; 1957)

"The variability in any natural environment is the product of the happenings in many geologic periods and of the unknown habits of some grotesque animals. To argue that this variability could ever be entirely explained is absurd."

Melton (p. 445; 1958)

"The variety of exponents describing downstream trends within catchments reflects the influence of variables other than discharge on the channel morphology,"

Richards (p. 73; 1977)

[123] See **pp. 50** to **53 this thesis** for a discussion on epistemic probability.

"channel reaches that differ significantly from typical gradient-discharge relations either are influenced by anomalous conditions, which should be definable, or represent a form of instability, which should ultimately result in naturally occurring channel changes."

Osterkamp (pp. 1254 and 1253; 1978)

3. "Errors of method occur when there is a discrepancy between the conceptual definition of the quality to be measured and the operational definition used to make the measurement."

Krumbein and Graybill (p. 45; 1965)

'The precise measurement of meander wavelengths is difficult and subjective."

Carlston (pp. 873 - 874; 1965)

"... discriminant functions for meandering and braided channels vary across a wide range. One of the main reasons for this lies in differences between operational definitions both of the slope and discharge and meandering."

Alabyan and Chalov (p. 470; 1998)

4. "Errors of measurement ... resulting from errors in measuring, counting, computing etc.

Chorley (p. 291; 1966)

"... The choice of independent variables was not ideal and not perfectly representative of the underlying controlling factors and ... the measurement and sampling procedures were imperfect ..."

Mosley (p. 136; 1981)

"... all correlations of measured load of natural streams with other factors is influenced by the limitations of ... measurements ... Measurements of sediment load are available in quantity only on that portion of the load caught ... Where total load and the load caught in a modern sampler have been compared, it has been shown that the device catches on the order of two - thirds of the total load in streams carrying principally sand and silt."

Leopold, Wolman and Miller (p. 220; 1964)

5. 2. 3. Equifinality

Causal laws relate the unique value of a cause to a unique value of an effect, and state that there is a proportional relationship between them such that if the cause changes by a certain amount, the effect changes commensurately. Epistemic probability assumes that this true relationship exists in data and that departures from this relationship are due to error or ignorance. In a work that has prompted much interest among geomorphologists (Culling; 1957: Chorley; 1962: Conacher; 1969: Chisholm; 1967: Karcz; 1980: Haines-Young and Petch; 1983: Culling; 1987b: Thorn and Welford; 1994), Bertalanffy (1950: 1969) inadvertantly provided a detailed theoretical mechanism for fluvial theory that had the capability of reconciling persistent deviations in regression relationships with the concept of ignorance. Bertalanffy drew on thermodynamics and chemical equilibria for his ideas, and a brief review of some core concepts is required in order to understand Bertalanffy's contribution to fluvial theory.

Chemical equilibria occur when the rate of the forward reaction (reactants to products) equal the rate of the backward reaction (products to reactants). Zumdahl (1995) gives the following analogy:

> *"Equilibrium is not static; it is a highly dynamic situation. The concept of equilibrium is analogous to two island cities connected by a bridge. Suppose the traffic flow on the bridge is the same in both directions. It is obvious that there is motion, since one can see the cars travelling across the bridge; but the number of cars in each city is not changing because equal numbers are entering and leaving. The result is no net change in the car population."*
>
> Zumdahl (p. 186; 1995)

Any chemical reaction obeys the 'law of mass action' which relates the concentration of the products to the concentration of reactants.

Equation 5. 2. 3. 1:

$$k = \frac{[C]^l[D]^m}{[A]^j[B]^k}$$

Where:

K	=	Equilibrium Constant
CD	=	Products
A B	=	Reactants

Equation 5. 2. 3. 1 forms a constant (the *equilibrium constant* - see above) which states that for any given chemical reaction at a given temperature, the ratio of products to reactants will be constant, or in the language of Zumdahl's analogy, the relative size of the car population of the reactant island and the product island will be constant. This relationship is determined by many factors; "...the relative energies of the reactants and products, and the relative 'degree of organisation' of the reactants and products[124]," (Zumdahl, p. 187; 1995). However, the *equilibrium position*, which determines the absolute amount of products or the amount of reactants at the equilibrium state, is determined by the initial concentrations.

A rule of thumb known as 'Le Châtelier's principle,' is a qualitative interpretation of the law of mass action[125] , that states what happens when changes in concentration occur to a system that is in equilibrium, but Le Châtelier's principle also predicts what happens when changes in external conditions such as temperature and pressure are imposed upon a system in equilibrium. For example, if more reactant is added to a system that is in equilibrium, the law of mass action states that the ratio between products and reactants stays the same, so the reaction moves to a state which favours consumption of the added reactant and production of the product until the ratio is restored. If temperature is increased in an endothermic reaction (one that consumes energy), Le Châtelier's principle predicts that the equilibrium position will shift to a new state that favours the products and absorbs the excess energy. If temperature is increased in an exothermic reaction (one that releases energy), the equilibrium position will shift to a new state that favours the reactants and absorbs the added energy. Le Châtelier's principle states that "If a change is imposed upon a system at equilibrium, the position of the equilibrium will shift in a direction that tends to reduce the effect of that change," (Zumdahl, p. a36; 1995).

Bertalanffy defined a state for open systems[126] that was not thermodynamic equilibrium (maximum entropy) but was a stable state that existed away from thermodynamic equilibrium. This condition was termed the 'Steady State;'

[124] A measure of disorganisation refers to the thermodynamic concept of entropy discussed on **pp. 60** and **61 this thesis.**

[125] Because Bertalanffy draws on the idea of the law of mass action, Le Châtelier's principle, is of value for understanding the concept of steady state and its use in equifinality, as described by Bertalanffy.

[126] Chorley and Kennedy (p. 357; 1971) define an open system as "A system with boundaries which allow the import and export of both mass and energy."

"In open systems ... the steady state is not reversible as a whole nor in many individual reactions ... an open chemical system may attain a time independent steady state, where the system remains constant as a whole and in its (macroscopic) phases though there is a continuous flow of component materials ... We therefore see that in the steady state a constant ratio between the components is established .. moreover, the ratio of components in the steady state depends only on the reaction constants and not on the inflow; the system thus shows 'self regulation' where the ratio of the components is maintained in changing inflow, changing absolute size,"

Bertalanffy (pp. 124 - 131; 1969)

Bertalanffy (1969) described the steady state of an open system in terms of the law of mass action. The primary idea of steady state is the concept of a constant ratio between reactants and products discussed above. It is the relative sizes of the reactants and products which is the steady state, not the absolute size which is determined by the initial concentrations. Steady state is the equilibrium constant for open systems, not the equilibrium position.

Having given the definition of steady state as the equilibrium constant for open systems, Bertalanffy went on to state that the open system would always strive to reach steady state and that it could attain it in various ways.

"One important characteristic of biological systems is circumscribed by terms like 'purposiveness,' 'finality,' 'goal-seeking etc. It has often been emphasised that every system attaining an equilibrium shows, in a certain way, 'finalistic,' behaviour ... the same final state, the same 'goal,' may be reached from different initial conditions and in different pathways ... if open systems attain a steady state, this has a value equifinal or independent of initial conditions."

Bertalanffy (pp. 131 - 132; 1969)

"...the final state may be reached from different initial conditions and in different ways... in an open kinetic system, irrespective of the content in the beginning or at any other time, the steady state values will always be the same because they are determined only by the constants of reaction and of inflow and outflow."

Bertalanffy (pp. 157 and 158; 1950)

Given the definition of steady state above, which refers to the equilibrium constant, equifinality should primarily show that the same equilibrium constant can be produced over any range of inputs. For example;

$$\text{Steady State } (k = 0.5) = \frac{200}{400}$$

$$\text{Steady State } (k = 0.5) = \frac{5}{10}$$

This is not a remarkable result though, and merely suggests that there is a constant relationship between the products and reactants that does not change with scale. Furthermore the above example does not account for what Bertalanffy termed the different ways a system can reach its steady state. Equifinality, in the sense proposed by Bertalanffy, requires that the system reaches a steady state, but a steady state that has the same absolute magnitude, or in equilibria parlance, the same equilibria position. For example, using the steady state (k = 0.5), assume for simplicity that a system is composed of 5 different reactants and only one product.; because the equilibrium position is constant, the concentrations of the reactants and the products must be constant:

$$\text{Steady State } (k = 0.5) = \frac{200}{[5]^2\,[2]^2\,[2]\,[2]\,[1]} = 400$$

$$\text{Steady State } (k = 0.5) = \frac{200}{[0.5]\,[1]\,[10]^2\,[4]\,[2]} = 400$$

The above example illustrates how the same steady state with the same equilibrium position can be achieved in different ways. As long as the multiplication of all the reactant terms is constant, they can be arranged in any order, and the product will be the same because it is determined by the constant of reaction.

This interpretation of Bertalanffy's concept of equifinality as applying to steady states with equal equilibrium positions, finds some support in an example used by Bertalanffy (p. 142; 1969). The concept of equifinality was illustrated by reference to an experiment on two rats where one rat was allowed to develop normally and the other rat's growth was inhibited by vitamin deficiency. Bertalanffy's point was that the rat who suffered the vitamin deficiency eventually achieved the same steady state as the normal rat. Note that Bertalanffy does not suggest that the deprived rat developed a similar ratio between vitamin intake and weight, but at a position lower down the chart (same equilibrium constant but different equilibrium position). Bertalanffy's point is that the deprived rat reaches the same equilibrium constant and the same equilibrium position i.e. the same quantitative value of weight, as the normal rat, but that this value was reached in a different manner. Equifinality then requires two components; 1) an equilibrium constant or constant ratio between reactants and products which is independent of concentration and which will be reached by the system eventually; 2) a given equilibrium position or quantitative value of a steady state which can be reached by different arrangements of the components but whose behaviour is governed by the equilibrium constant.

Equifinality in fluvial theory provides a degree of freedom to the use of causal laws in geomorphology. The definition of steady state in terms of the law of mass action requires that there is a determined proportionality between concentration of reactants, or, in the case of fluvial systems, the magnitude causal/boundary condition variables, and concentration of products, or in the case of fluvial systems, the magnitude of effects such as channel morphology. The law of mass action, or Le Châteliers principle as it applies to the concentration of products is explicitly causal; changing the magnitude of the causes leads to a fixed proportional change in the effects such that the ratio of cause to effect is constant. The definition of steady state for fluvial systems (an open system), means that as the absolute concentration or magnitude of causes increases, the effect will increase to restore the fixed ratio or steady state condition.

Equifinality states though, that for any fixed steady state, that is for any defined magnitude of causes and effects, although the magnitude of the total causes and effects remains the same, their individual constituents may vary, such that the same steady state is reached in different ways. For example, assuming that these variables are dimensionless and quantifiable in fluvial theory then:

$$\text{Steady State } (k = 0.5) = \frac{[Xs][Wl]\,[w/d]\,[v]\,[Bi]\,[Cs]}{[Q]\,[Bs]\,[bs]\,[Vs]\,[Ve]} = \frac{10}{20}$$

Where:

Effects are:	[Xs]	= Cross-Sectional Area	[Wl]	= Meander Wavelength;
	[w/d]	= Width/ Depth Ratio	[v]	= Flow Velocity
	[Bi]	= Braiding Index	[Cs]	= Channel Slope
Causes are:	[Q]	= Discharge	[Bs]	= Bank Sediment Size
	[bs]	= Bed Sediment Size	[Vs]	= Valley Slope
	[Ve]	= Vegetation		

As shown above, equifinality states that these variables can be rearranged in any way, providing that the totals of the individual elements do not exceed the magnitudes that describe the given equilibrium position of the given steady state (10 for the effects and 20 for the causes shown above) - for example an increase in one causal variable must lead to a decrease in another causal variable.

This means that in regression relationships, which do not take account of every variable of cause and effect (especially bivariate but also multivariate regressions), an explanation exists for the scatter on charts. Whilst the total magnitude of all the causes involved in producing a given effect has remained constant, the individual component may fluctuate. Thus in regression charts in fluvial geomorphology, different values of the same causal factor can produce the same effect, but an increase or decrease in other factors is assumed. In a system with multiple components of effects, equifinality also states that different effects may be produced by the same causes, though the total magnitude of effect will remain constant.

Equifinality requires concepts of aleatory probability at the level of the individual components. For example, assume that different values of an individual causal component can produce a unique effect. Because an individual causal component can have a distribution of values for a single value of an individual effect component and these values are assumed not to be error, but to be an intrinsic part of the system (i.e. the system can reach the same equilibrium position of steady state in different ways), all values must be treated as valid values for the system. The values of the causal components are assumed to be normally distributed, and the mean value[127] is assumed to be the most probable state of the causal variable, given a value of an effect. A relationship is thus formed between the mean values of the causal component (e.g. discharge) and the effect (e.g. channel cross-section). It is assumed that as the mean value of the individual causal component changes, the magnitude of the total of all causal components (20 in the above example) changes, and a new equilibrium position of steady state emerges.

However, equifinality in fluvial theory is not taken as a suggestion that an aleatory statistical approach is the correct one (that is, mean cause can produce a distribution of mean effect).[128] At the scale of total causal components, the scatter on for example bivariate relationships, is due to error caused by ignorance. If all the variables were analysed, a perfect proportional relationship between the total magnitude of causes and the total magnitude of effects would be formed, governed by the equilibrium constant of reaction. Equifinality can be thought of as a complex concept for justifying causal/deterministic relationships and stating that scatter on regression charts is due to the partial analysis of the system and therefore ignorance.

[127] Note that this is the mean value of a mean measure of cause such as mean annual discharge or mean sediment size.

[128] If an aleatory approach was taken, the scatter on the graph would be incorporated into a distribution, which had as its fundamental data the mean values of effect. Determinism would become possible because the statistical properties of this distribution would be invariant. This requires an increase in the scale at which fluvial phenomena are predictable. See **Section 7. 4. 2.** for the application of aleatory laws to mean values of controls and morphology at the reach level.

Equifinality suggests that 'other variable noise' will be a persistent feature of regression plots that only analyse part of the system, but it puts this noise in a defined causal framework.

5. 2. 4. Equilibrium

"Equilibrium - a condition in which some kind of a balance is maintained,"

Chorley and Kennedy (p. 348; 1971)

"... One's whole attitude to natural systems stems from one's time perspective,"

Chorley and Kennedy (p. 248; 1971)

There is considerable confusion regarding the concept of equilibrium in geomorphology (Thorn and Welford, p. 666; 1994: Mayer, p. 278; 1992: Renwick, p. 267; 1992). Equilibrium usually refers to the ideal behaviour of a single landform over time (See Howard; 1965) - with different types of equilibrium occurring, depending on the particular timescale used (Summerfield, p. 12; 1991: Thorn, p. 67; 1988: Knighton, p. 91; 1984: Chorley and Kennedy, p. 202; 1971). However, causal laws in geomorphology are derived empirically from a sample of various different rivers and it is in this sense that we are examining equilibrium and its role in supporting causal laws.

The notion of equilibrium theory in fluvial geomorphology is founded on the concept of the steady state as outlined by Bertalanffy (1969), though there is some disagreement as to the properties of the steady state as it applies to geomorphology. Our present understanding of fluvial systems is that there is a constant proportionality or equilibrium constant that defines the relationship between average cause and average effect. This equilibrium constant or the quantitative relationship between cause and effect is found in fluvial theory by epistemic probability (empirical regression equations). For any value of the causes, we should be able to predict the equilibrium position of the effects. This unique relationship between cause and effect is complicated by empirical difficulties such as an error and equifinality, which suggest that, unless all the cause and effect factors are taken into account, some scatter will be present on regression charts, because a unique relationship exists between total cause and total effect, whilst individual cause and effect factors can assume a distribution of values.

The empirical causal relationship is further complicated by the temporal nature of process and form. Any system takes a finite amount of time to react to the causes operating on it and produce the effect usually associated with that cause. This can be seen within the laboratory environment. For example, if in a flume a given discharge (cause) is known to produce a given channel cross-section form (effect), and that discharge is only run for 20 seconds in the flume, it is unlikely that the expected effect will be achieved - the expected cross-sectional form may only occur after an hour of flow. This period of time taken for a characteristic form to emerge from an applied magnitude of cause was termed by Howard (p. 305; 1965) 'inertia,' and by Brunsden and Thornes (p. 466; 1979) the 'relaxation time.'

In the natural environment, different rivers have different histories of sediment and water discharges or different drainage basin adjustments, and the response of river morphology to the current average cause may be influenced by this different history. An investigator may be comparing the morphology of a river channel that has constantly had a given water discharge to the morphology of a river that has just acquired that same water discharge after experiencing a much higher discharge. Thus for some time, the morphology will be different between the two channels even though all the controls on them are currently equivalent. Similarly, two different magnitudes of cause may produce the same effect because of previous process history.

The concept of equilibrium, like equifinality explains noise and justifies the use of causal relationships. Equilibrium can be viewed as 'other variable noise' where the noise is time or history. On a regression plot, channels on or near the regression line are in the steady state - they are 'in equilibrium;' channels far away from the regression line are out of the steady state because they have not had time to adjust to prevailing conditions, perhaps because of past histories - they are 'out of equilibrium' channels; "Disequilibrium landforms are those that tend towards equilibrium, but have not had sufficient time to reach such a condition," (Renwick, p. 268; 1992).

Because we can only define any particular steady state or equilibrium constant for any set of external variables in terms of empirical data, geomorphologists have to make an assumption about the data and about the effects of time on river form. The assumption is that the effects of time or histories on river channels are normally distributed and that the most probable state is one in which the effects of time or history are zero - that is, the mean value of the normal distribution of time is the steady state where the cause has the correct effect according to the equilibrium constant. Ferguson writes that;

" In comparing different rivers in order to infer general relationships between channel form and environmental conditions, the problem can be minimised by studying as large a sample as possible. If information is available for a large number of rivers, at the same or at different times, their deviations from equilibrium may tend to cancel out."

Ferguson (p. 236; 1977)

The difficulty with this approach is that to derive the steady state from empirical data, we must already assume that it exists and is the most frequent form. The steady state value is defined and is a necessary consequence of our presupposition about the nature of time and process.

As pointed out earlier, the concept of steady state as defined by Bertalanffy is based upon the law of mass action. In chemistry, the constant which is derived from this law and which defines the equilibrium, is determined by the aleatory laws of thermodynamic principles. Steady state in geomorphology, despite the thermodynamic overtones, is not defined by aleatory statistical laws and appears to be more akin to mechanical equilibrium - a balance between force and resistance - the solution to which is not solved deterministically in fluvial theory but by epistemic statistics where the average is the true value and deviations from it are due to errors, equifinality and differences in history.

Table 5. 2. 1. 1- Common Descriptors of Channel Morphology

Author	Variable	Context
Wolman and Brush (pp. 208 -209; 1961)	mean channel depth	investigation of independent controls on river form (experiment)
Ackers (p.10; 1964)	mean channel depth	channel depth related to constant discharge (experiment)
Brice (p. D16; 1964)	mean channel depth	cross - sectional area related to mean channel width
Leopold, Wolman, and Miller (p.215; 1964)	mean channel depth	at-a-station hydraulic geometry
Hickin (p. 770; 1972)	mean channel depth	investigation of pseudomenaders (experimental)
Knighton (p. 101; 1977)	mean channel depth	at-a-station hydraulic geometry
Park (p. 137; 1977)	mean channel depth	channel cross section related to drainage area
Schumm et al (p. 172; 1981)	mean channel depth	channel depth related to valley slope
Osterkamp and Hedman (p. 6; 1982)	mean channel depth	at-a-station hydraulic geometry
Orlowski, Schumm and Meilke, Jr. (pp. 226 - 227; 1995)	mean channel depth	reach classifications
Shields Jr. (p. 37; 1996)	mean channel depth	reach hydraulic geometry
Wolman and Brush (pp. 208 -209; 1961)	mean channel width	investigation of independent controls on river form
Ackers (p.10; 1964)	mean surface width	channel width related to constant discharge (experiment)
Brice (p. D16; 1964)	mean effective width	channel characteristics related to mean channel width
Orlowski, Schumm and Meilke, Jr. (pp. 226 - 227; 1995)	mean channel width	reach classifications
Shields Jr. (p. 37; 1996)	mean channel width	reach hydraulic geometry
Knighton and Nanson (p.614 -615; 1993)	mean width/depth ratio	discharge - slope relationships and channel pattern
Nanson and Knighton (pp. 220 -224; 1996)	mean width/depth ratio	classification of anabranching channels
Brice (p. D16; 1964)	mean slope	slope related to mean channel width
Hickin (p. 770; 1972)	mean water surface slope	investigation of pseudomenaders (experiment)
Shields Jr. (p. 32; 1996)	mean channel slope	meander geometry related to independent variables
Shields Jr. (p. 37, 1996)	mean channel slope	reach hydraulic geometry
Carlston (pp. 873 - 874; 1965)	mean meander wavelength	meander wavelength related to discharge
Dury (pp. 93 and 94; 1973)	cross-sectional area	investigation of magnitude - frequency analysis

Table 5. 2. 1. 2- Common Measures of Discharge

Author	Variable	Context
Leopold and Wolman (pp. 58 - 59; 1957)	bankfull discharge	channel patterns related to slope and discharge
Leopold, Wolman and Miller (p. 219; 1964)	bankfull discharge	effective discharge analysis
Leopold, Wolman and Miller (p. 241; 1964)	mean annual discharge	downstream hydraulic geometry
Carlston (p. 864; 1965)	bankfull discharge	meander wavelength related to discharge
Carlston (p. 864; 1965)	mean discharge of the month of maximum discharge	meander wavelength related to discharge
Dury (pp. 150-152; 1969)	bankfull discharge	hydraulic geometry
Schumm (p. 256; 1969)	mean annual discharge	channel geometry related to sediment type and transport
Schumm (p. 256; 1969)	mean annual flood	channel geometry related to sediment type and transport
Dury (p. 272; 1970)	bankfull discharge	meander wavelength related to discharge
Schumm (1971b)	mean annual discharge	width - depth relationships to discharge
Dury (1973)	bankfull discharge	defining a fixed recurrence interval for bankfull discharge from drainage basin area
Ferguson (p. 39; 1973)	mean annual flood	channel pattern type related to sediment and discharge
Dury (p. 221; 1976)	bankfull discharge	discharge prediction from channel dimensions
Pickup (p. 371; 1976)	optimum discharge for bed load transport	adjustment of channel shape to discharge
Pickup (p. 371; 1976)	effective discharge	adjustment of channel shape to discharge
Osterkamp (p. 1253; 1978)	mean discharge	braiding in relation to gradient, particle size and discharge
Carson (p. 320; 1984)	mean annual flood	channel pattern related to sediment type and discharge
Carson (p. 323; 1984)	2 yr. discharge	channel pattern related to sediment type and discharge
Hey and Thorne (1986)	mean annual discharge	hydraulic geometry
Mosley (p.128; 1991)	mean annual flood	hydraulic geometry
Simon (p. 612; 1995)	bankfull discharge	equation for channel stability
Nanson and Knighton (pp. 220 -224; 1996)	bankfull discharge	controls and classification of anabranching rivers
Nanson and Knighton (pp. 220 -224; 1996)	maximum discharge	controls and classification of anabranching rivers
Nanson and Knighton (pp. 220 -224; 1996)	2 yr. discharge	controls and classification of anabranching rivers
Nanson and Knighton (pp. 220 -224; 1996)	2.3 yr. discharge	controls and classification of anabranching rivers
N. R. A. (p. 2; 1996)	mean annual discharge	river habitat survey classification
Alabyan and Chalov (p. 471; 1998)	effective discharge	channel patterns related to discharge
Brewer and Lewin (p. 1001; 1998)	bankfull discharge	comparison of unstable reaches with conventional slope/discharge relationships

Table 5. 2. 1. 3.- Common Measures of Sediment

Author	Variable	Context
Schumm (1960)	Silt-clay % bed and bank sediment	channel shape related to sediment type.
Leopold, Wolman and Miller (p. 190; 1964)	median particle size	description of the stream bed
Ackers and Charlton (p. 291; 1970)	median particle size	dimensional analysis of meander wavelength
Holland and Pickup (p. 76; 1976)	median particle size	knickpoint development in stratified sediment (experiment)
Pickup (p. 365; 1976)	mean particle size	adjustment of channel shape to discharge
Osterkamp (p. 1261; 1978)	median particle size	braiding in relation to gradient, particle size and discharge
Hey, (p. 373; 1979b)	d84	flow resistance in gravel bed rivers
Mosley (p. 130; 1981)	median particle size	hydraulic geometry
Mosley (p. 130; 1981)	mean particle size	hydraulic geometry
Mosley (p. 130; 1981)	d75	hydraulic geometry
Mosley (p. 130; 1981)	silt-clay % bank sediment	hydraulic geometry
Kennedy (pp.1261 - 1262; 1983)	median particle size	model of river flow
Carson (pp. 320 - 327; 1984)	median particle size	channel pattern related to sediment type and discharge
Hey and Thorne (1986)	d84	hydraulic geometry
Knighton (p. 80; 1984)	mean particle size	exponential decrease in particle size with distance downstream
Knighton (p. 80; 1984)	median particle size	exponential decrease in particle size with distance downstream
Drake et al (p. 193; 1988)	median particle size	motion picture photography of bedload transport
Kirchner et al (p. 670; 1990)	median particle size	variability of sediment transport in water worked sediments (experiment)
Ashworth et al (p. 125; 1994)	median particle size	prototype modelling of channels (experiment)
Simon (p. 612; 1995)	median particle size	equation for channel stability
Lane, Richards and Chandler (p.115; 1995)	median particle size	numerical modelling of stream adjustment
Brookes (pp. 262 -263; 1996)	median particle size	general guidance for river restoration

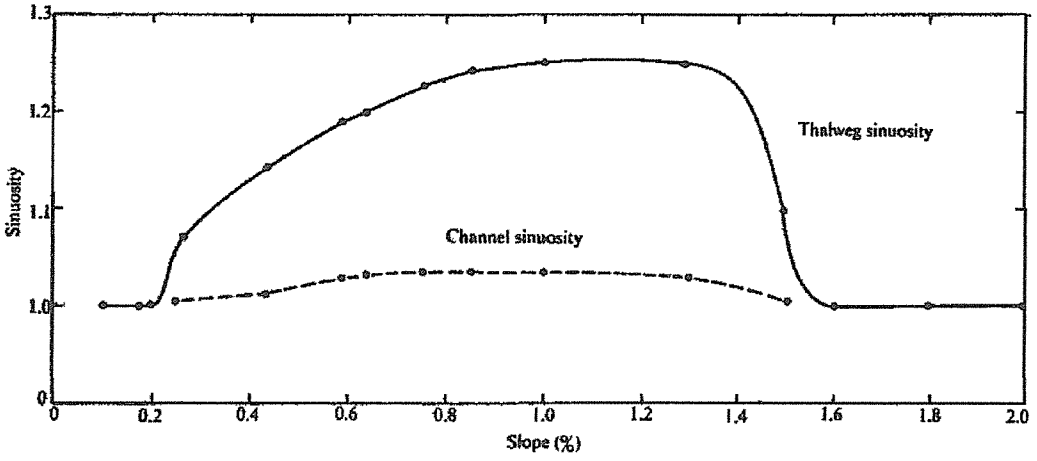
5. 3. The Application of Chaos Laws in Fluvial Theory.

5. 3. 1. Previous Developments

The study of chaotic systems and the laws that govern them is a new branch of physical science, its initiating work being published in 1963. As such, it is to be expected that chaos laws have not had as much application to problems of alluvial river channel morphology as have causal laws, which are predominantly based on the much older framework of Newtonian mechanics. However some important precursors to a fully developed chaos approach are present in the geomorphic literature.

Principal among forerunners of chaos in geomorphology is Schumm (1973: 1979). Schumm and Khan (1971: 1972) presented a non-linear threshold model for thalweg planform (Figure 5.1.3.1).

Figure 5.3.1.1. Schumm and Khan's (1971) Nonlinear Model of Channel Development



From Schumm and Khan (p. 409; 1971)

The diagram shows that with increasing slope, sinuosity does not increase proportionally. Rather, the system is able to absorb changes in the rate of energy distribution within a given morphology, until it reaches the threshold (0.2 %), where a slight progressive change in slope produces a disproportionately large change in thalweg configuration. An opposite threshold occurred at (1.28%).

The threshold result in Schumm and Khan (1971) seems to have set the stage for Schumm's (1973: 1979) theoretical treatise for geomorphology. In these papers on geomorphic thresholds and complex response, an explicitly non-linear mode of operation was propounded for geomorphic phenomena; "That is, increasing stress may not produce commensurate strain." (p. 308; 1973); non-linearity is essential for a system to obey chaos laws[129]. This non-linear change in form with increases in control variables was expressed in the term 'extrinsic thresholds' and was placed under the rubric of catastrophe theory by Graf (1979). However, most importantly, from a chaos viewpoint, Schumm identified that non-linearity could stem from the internal structure of a system; Schumm termed internal non-linearity in geomorphic systems "intrinsic thresholds," and defined them as "...a threshold that is developed within the geomorphic system by changes in the system itself through time." (p. 304; 1973).

Schumm's model of intrinsic thresholds suggested two things. Firstly that a single river system could evolve over time without any change in its controlling variables, due to changes in internal behaviour. Nicholas *et al.* (1995) illustrate this concept for the case of braided rivers: "... the braided river evolves continuously, transferring sediment in the form of macroslugs ... Flume workers have noted that these process-form interactions occur under conditions of constant discharge and sediment inputs to a system." (Nicholas *et al.*, p. 503; 1995). Secondly, Schumm's model suggested that a uniform force applied to different rivers may not have the same effect because their internal structure was different, a result that Schumm termed Complex Response. In illustrating complex response, Schumm (1979) cited equivocal data on the effect of large magnitude events; the floodplain of the Cimmaron river was destroyed by rare floods whilst the Connecticut river was not significantly affected (Schumm and Lichty; 1963: Wolman and Eiler; 1958).

Schumm's recognition of the non-linearity of alluvial systems is an important precursor to the application of chaos laws to alluvial river channel problems.

Retrospectively, chaos laws and their application to alluvial river channel problems can be identified in the experimental literature of fluvial geomorphology. Three papers, with diverse aims and areas of investigation, present results which suggest that chaos concepts were applicable to the phenomena being investigated.

[129] See **Section 4. 2.** for the importance of non-linearity in chaos.

Friedkin (1945) is the classic monograph of experimental fluvial geomorphology, providing a comprehensive study on the causes of meandering. A minor but persistent theme is present in this work. Whilst Friedkin was able to reproduce his experiments (at least qualitatively), he noted that "Every phase of meandering represents a changing relationship between three closely related variables: the flow and the hydraulic properties of the channel, the amount of sand moving along the bed, and the rate of bank erosion. These three variables constantly strive to reach a balance, but never do, even with a constant rate of flow," (p. 3; 1945).

In addition to this condition, and possibly contributing to it, the continual 'trading' of sediment between reaches and its changeability in space and time was observed and Friedkin wrote; "...it seems clear that in meandering rivers the rate of trading must vary considerably, because no two bends are eroding their banks at the same rate, and in some sections little or no bank caving occurs. Therefore no set rules can be laid down as to the source of the sand in each bend." (p. 6: ibid).

The study concluded that bank caving was the most important variable in determining channel planform and cross-section (p. 11 and 17; ibid) and thus this spatial and temporal change in bank erosion is significant. Friedkin, by his own admission, accentuated the role given to bank caving[130] by adding another level of spatial resolution to the problem; "These tests go one step further and show that the shape of the cross-sections depends upon the location in the cross-sections at which the sand enters. Probably the most important conception of a meandering river is that over each square foot of the bed, shoaling or deepening takes place depending upon the relationship between the sand entering that area and the ability of the flow over that area to carry sand." (p. 11; ibid). Finally; "It is important to note that even with a constant rate of flow, the small scale rivers did not reach a stable condition..." (p. 15; ibid)

The seminal paper by Simons *et al.* (1965), develops the theme present in Friedkin (1945). This experimental flume work elucidated the sequence of bedform regimes that occurred with increased power acting on movable sediment (figure 5. 3. 1. 2). Simons *et al.* (1965) defined the primary controls on this series, but the paper pointed out several exceptions that interfered with this relationship; exceptions that prompted the caveat -"A completely satisfactory method of predicting form of bed roughness has not been developed." (p. 50; 1965).

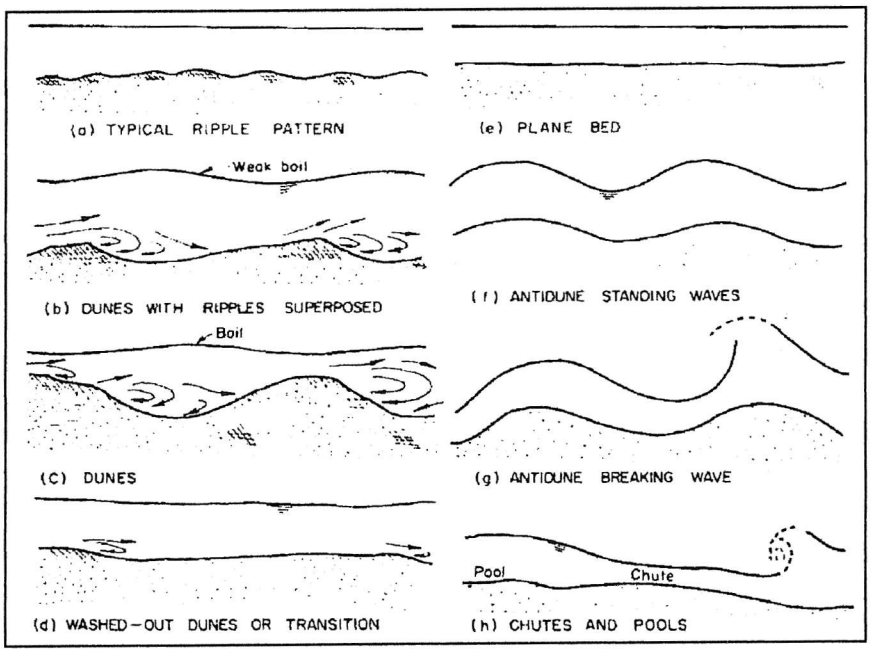
[130] Sediment was derived solely from bank caving in the majority of tests.

Of particular note was the observation that internal system state altered the force acting on it. In the transitional regime, the final form of the bed varied "depending primarily on antecedent conditions" (p. 37; ibid). If a plane bed was in existence, the flow could be reduced to levels commensurate with the low flow regime without producing the associate bedforms of ripples or dunes. If the bed was dunes, the converse occurred, the bed supporting upper flow regimes without a change in form.

Existing morphology also governed how a change in an external variable was applied. "If the bed material of a stream moves at one discharge, but does not move at a smaller discharge, the bed configuration at the lower discharge will be a residual of the bed configuration formed when the sediment was moving. The problem in this case is knowing when beginning of the bed material movement occurs." (p. 37; ibid).

In other words, the morphology determines the internal evolution of the system or in the terminology of Simon *et al.* "... changes in flow conditions may change the role of a dependent variable into that of an independent one." (p. 46; ibid). Kennedy (p. 1260; 1983) believed that this "interdependency of the variables and their roles as dependent variables at some times and independent variables at others," was key to progress in fluvial understanding.

Figure 5. 3. 1. 2.- Sequence of Bedforms with Increasing Power

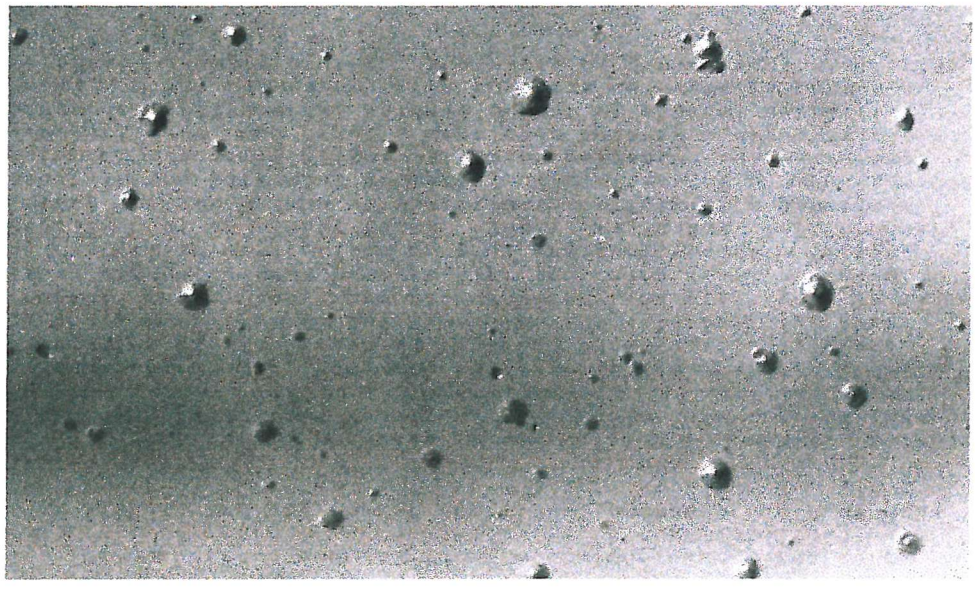


From Simons *et al.* (1965)

Karcz (1973) conducted experiments that investigated longitudinal bedforms. More precisely, Karcz's objects of interest were "flow aligned elements of streambed scour" (p. 149; 1973), or features of erosion, not deposition.

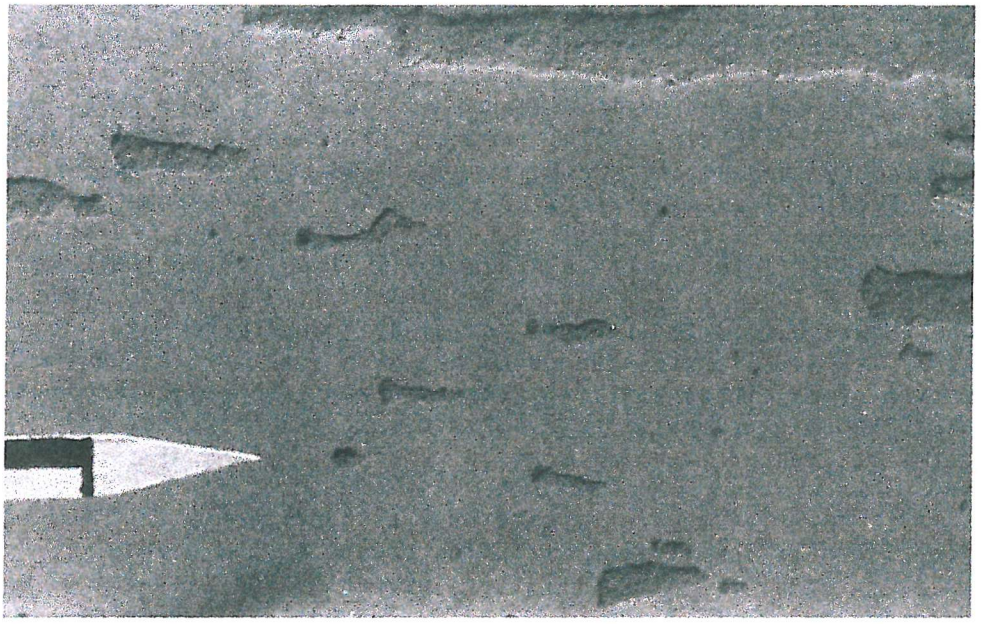
One of Karcz's experiments is of significance. A mechanism in the literature had been proposed that scour is introduced by bed defects. Karcz, developing this further, suggested that processes of early primary consolidation in sediment, provided an inherent source of defects. To test this idea, beds of montmorillinite soil clay were deposited from suspension and were then subjected to constant, prolonged flow conditions. After initial erosion, the clay bed became stable, forming what would be regarded as an equilibrium surface. Three to 24 hours later, pits and mounds appeared at the bed surface and formed foci for erosion (**figure 5. 3. 1. 3**). At higher velocity, the circular pits evolved into flow aligned grooves which eventually led to the '... removal of the entire upper part of the bed,' (p. 157; ibid: See **figure 5. 3. 1. 4**). Karcz ascribed these erosive incidents to slow and progressive change in the clay aggregates, which led to increased pore pressure and the subsequent expulsion of excess fluid from within the bed. This led to a weakening of the bed and the defects that appeared.

Figure 5. 3. 1. 3.- Pits and Mounds in Karcz's (1973) Experiments



From Karcz (p. 156; 1973)

Figure 5. 3. 1. 4.- Flow Aligned Grooves in Karcz's (1973) Experiments



From Karcz (p. 156; 1973)

The above experimental papers suggest that concepts of sensitive dependence may be applicable to fluvial geomorphology. The bank erosion in Friedkin (1945) fluctuated with time and space under constant conditions; similar flows in Simons *et al.* (1965) could produce different morphology depending on the state of the inherited, internal bedform condition and Karcz's stable clay bed could spontaneously change over time, without change in the controlling conditions.

The above papers, although perhaps showing examples of chaotic behaviour in geomorphic systems, are not an explicit use of chaos concepts to examine fluvial phenomena. Possibly the first person to recognise the significance of chaos for geomorphology in general was Culling, who produced two foundational philosophical and conceptual treatises on the subject (Culling; 1987a: 1987b) and numerous technical papers that employed chaos to geomorphological phenomena (Culling; 1986a: 1986b: 1987: 1988: Culling and Dakto; 1987). In fluvial geomorphology, most attention seems to have been focused on the dissection of landscapes by river features and the possible chaotic nature of the pattern of this dissection (i. e. Tarboton *et al.*; 1988: Rosso *et al.*; 1991: Ijjasz - Vasquez *et al.*; 1992: Rodriguez-Iturbe *et al.*; 1992: Phillips; 1995: Claps and Oliveto; 1996: Gao and Zong; 1996), or in other words, most attention has focused on phenomena that comes under the investigation of the wider, inclusive definition of fluvial geomorphology.[131]

[131] See **Section 1. 1.**

Little work has been done on how chaos applies to within channel phenomena; Nikora (1991) has examined the fractal structure of river planforms but arguably the most important contribution to this field has been the connection made by Phillips (1990: 1995: 1999) between hydraulic geometry and chaos and Phillips' approach to this problem is considered in some detail below.

The ethos behind Phillips' considerable contribution to the application of chaos in geomorphology generally, is stated in the summary of his work to date (Phillips; 1999). Phillips (pp. 10 - 12; 1999) prefaces his work with a definition of the type of science he practises, and provides caricatures of reductionist, systematist and nonlinearist scientists. Phillips 'presumes to join the ranks of the systematist' (p. 12; ibid) and he defines that brand of science (using the analogy of understanding a machine) in the following terms:

"The systems approach would involve carefully observing the intact machine. The systematist would catalogue all the components and how they are linked together. She would observe the machine in operation, and systematically measure the flows and cycles of energy and matter into and out of the machine, and among the system components."

Phillips (p. 10; 1999)

Phillips (p. 14; ibid) goes on to write that in the study of earth surface systems, geomorphologists are often concerned with partially specified systems, or more particularly, those systems " ... where the direction or basic nature of systems interactions are known, but the specific magnitudes (or the specific governing equations) are unknown." Phillips (p. 17; ibid) summarises this approach as the ability to describe geomorphic systems in terms of box and arrow diagrams.

Phillips thinks that the problem of hydraulic geometry fits into the category of system that these two statements describe; a system where the links between variables can be described and their directions are known. In order to describe these links Phillips (p. 741; 1990: p. 315; 1995) reduces the nine dimensional system of Hey (1997) into a four dimensional system composed of velocity (V), slope (s), hydraulic radius (R) and friction factor (f). Having done this, Phillips proposes to describe the direction of the relationship between any one of the four variables in terms of the other three, and to this end he proposes the four equations given below:

$$V = R^{0.5}s^{0.5}f^{-0.5}(\rho g)^{-0.5}$$

$$s = V^{2}R^{-1}f(\rho g)^{-1}$$

$$R = V^{2}s^{-1}f(\rho g)^{-1}$$

$$f = V^{2}R^{-1}s(\rho g)^{1}$$

Where:

V	=	Velocity
s	=	Slope
R	=	Hydraulic Radius
f	=	Friction Factor
g	=	Acceleration due to gravity
ρ	=	Unit weight of water

Phillips places such emphasis on the direction of the relationship between variables, because the direction of the relationship can indicate whether a system is governed by chaos concepts or not. This direction of relationship involves a useful tool of chaos research termed the Lyapunov exponent. For every variable that composes a system's phase space, there is a Lyapunov exponent: that is for an *n* dimensional system there are *n* Lyapunov exponents. The sign (positive / negative / neutral) of a Lyapunov exponent for any variable, indicates the direction of change in this variable over time. The direction of change in all variables describes the behaviour of the system over time and the form of attractor that governs the system. Table 5. 3. 1. gives the form of attractor which results when the variables in a three dimensional system have different signs. The presence of a positive Lyapunov exponent indicates the presence of chaotic behaviour because that particular variable diverges or stretches over time[132] and indicates the presence of sensitive dependence on initial conditions. Berge et al. (p. 288; 1984) write that "...finding a positive Lyapunov exponent is an unambiguous signature of a chaotic regime."

Table 5. 3. 1- Lyapunov Exponents and Attractors Types in Three Dimensional Phase Space

Type of Attractor	Sign of Exponents
Fixed Point	(-,-,-)
Limit Cycle	(0,-,-)
Torus T2	(0,0,-)
Strange Attractor	(+,0,-)

From Berge *et al.* (p. 287; 1984)

[132] See **Sections 4. 3.** and **4. 4.**

Lyapunov exponents are most commonly used as an analytical tool once a trajectory in phase space or an attractor has been formed. In this situation, the Lyapunov exponent provides a quantitative measure of the degree of sensitive dependence, expressing the average divergence per iteration over the length of the attractor (for details of the method see: Alligood *et al.*, p. 106; 1996: Stewart, p. 293; 1997: Baker and Gollub, p. 120; 1990). However, if for a deterministic system, the direction of change in variables is known, then it can be derived that a positive Lyapunov exponent exists and that the system will be obeying chaos laws.

Phillips' construction of the four equations above seeks to make hydraulic geometry a deterministic system in order that directions of change and Lyapunov exponents can be inferred. The connection is made that, because there are positive exponents present in the equations, positive Lyapunov exponents exist and the system exhibits chaotic behaviour (Phillips, p. 742; 1990)

Phillips does not give any basis for the form of these equations, i.e. why some variables have a positive or negative effect on the variable under consideration, but the approach appears similar to that of Schumm's considered earlier, where the variables are treated in isolation.

Phillips wrote that the systematist 'considered the intact machine' (see above) and that the systematist '... should not take the machine apart," (Phillips, p. 11; 1999) but it is contended here that Phillips, in spite of his ethos, has taken the machine apart and that when reassembled, the individual parts do not fit. It is suggested that if these equations were tested experimentally, they may not consistently and deterministically give the same result. For example, if in experimental conditions the value of the hydraulic radius was measured, it may not always be described by $V^{2}\ s^{-1}\ f^{1}$. Indeterminacy results from the simultaneous interaction of all variables, the number of combinations that the values for these variables can have and the multitude of different relationships that are possible between variables. Phillips has written that:

> *"...my own work ... is to detour from the traditional route of attempting to analyse a time series to see whether a low - dimensional chaotic attractor exists. Rather, given what is known of the structure and functioning of an earth surface system, the approach is to see whether deterministic chaos is a possible or probable cause of some of the observed complexity."*
>
> Phillips (p. 391; 1994)

It is suggested here that an analysis of in-channel fluvial geomorphology is possible only by the construction and identification of a strange attractor from empirical experimental data, because the structure and functioning of the nine components of Hey's (1997) variables is not known, and cannot be solved in any form by rational methods. The next section aims to suggest a basis for the existence of such an attractor in fluvial geomorphology.

5. 3. 2. Lorenz, Culling and a New Hypothesis for Fluvial Geomorphology

"Toward a Theory of the Morphologic Significance of Turbulence in the Flow of Water in Streams."

Leighly (1932)

Lorenz (p. 158; 1993) commenting on the scope of application for chaos analysis wrote that the falling of a leaf from a tree could be considered an example of chaos because the weather system that prompted its descent from the tree was controlled by chaos laws. Culling (1987b) has proposed a similar line of reasoning for geomorphology;

"...it is known that chaotic motion and strange attractors enter into the heartland of physical geography for turbulent flow is irregular, intermittent, self-similar, and whether we like it or not ubiquitous."

Culling (p. 68; 1987b)

Lorenz and Culling's argument is that those phenomena which are controlled entirely or partly by other phenomena which obey chaos laws, are in themselves subject to the laws of chaos. Such a state exists in fluvial geomorphology; the motivation for examining chaos laws in geomorphology stems from the fact that turbulent fluid flows are the main way in which energy is distributed to land masses (Summerfield, p. 196; 1991), and that substantial evidence suggests that turbulent flows are governed by the laws of chaos.[133] By implication then, we have reason for investigating whether chaos laws apply to the indeterminacy of river channels.

On the basis of the reasoning of Lorenz and Culling, a new hypothesis is proposed for the application of chaos laws in fluvial geomorphology. Firstly, most process research since the 1950's has focused on sediment and discharge as the controls on river form and has been able to establish some form of epistemic probability relationship between these two controls and the morphology of river channels. These controls on river form are maintained in the new hypothesis and form the 'virtual constants' of the fluvial system.

The virtual constants of sediment size and discharge form limits on the total amount of energy and the distribution of energy for the nine components of Hey's (1997) equations for river form. Sediment and discharge may therefore determine both the volume and the shape of the phase space for river morphology; that is the maximum possible values, or the lengths or the axes in phase space for each variable.

[133] See **Sections 4. 5. 3.** and **4. 5. 4.**

Within this phase space determined by sediment and discharge, the actual location of the river morphology (its description at any time in terms of values of the nine variables) is dependent on the precise nature of the equations which govern turbulence; these equations are assumed to be chaotic. Because turbulence and sediment interact almost instantaneously in river systems, and have a mutual affect on each other, the equations actually refer to turbulence-sediment behaviour.

Turbulence is sensitively dependent on initial conditions. In river systems, this would mean that the values of Hey's (1997) nine components of river morphology could not be predicted at a given time, if the initial starting conditions were known for turbulence-sediment interaction. However, over a period of time, given a set of virtual constants, the behaviour of turbulence should form a strange attractor whose behaviour is predictable and determined. In river systems, this means that given constant values of sediment and discharge, turbulence-sediment interaction should produce an attractor of morphology in nine dimensional phase space that is predictable, although the precise location on the attractor is not predictable at any given time.

Finally, the equations of turbulence-sediment interaction and the attractor they produce in nine dimensional morphology phase space, apply over space and time. The position of any given cross-section of river in nine dimensional morphology phase space is determined by the complex interaction of space and time and their effect on initial conditions. Assume that our interest is in a given cross-section S_2; a given flow of water (F_1) with given turbulence-sediment interaction passes cross-section S_1 upstream; the turbulence-sediment interaction and the value of the nine dimensional morphological phase space at cross-section S_2 will be governed by the conditions it has inherited from the flow at S_1 coupled with the in situ initial conditions in the absence of flow (F_0). If our attention remains focused on cross-section S_2, the value of the nine dimensional morphological phase space will be governed by the conditions it has inherited from flow F_2 at cross-section S_1 upstream coupled with the *in situ* initial conditions left by the previous flow F_1. This complex interaction means that the attractor for river morphology in a downstream direction should be the same as the attractor for a given cross-section.

The next chapter proposes the predictive statements which causal, statistical and turbulence-sediment chaos relationships between entities imply, and outlines the details of an experiment to test these predictive statements.

CHAPTER 6

Experimental Design

6. 1. The Predictive Statements of Causal, Aleatory, Statistical and Chaos Laws.

It has been stated that the relationship between entities that hypotheses generate must be capable of being critically assessed or tested.[134] It has been suggested that the manner in which this assessment takes place is by examining the predictions that a connection between entities makes. Keeping in mind the difficulties with the relationship between prediction and knowledge,[135] the three types of connections between entities are proposed for fluvial systems, along with their appropriate boundary conditions and associated predictions.[136]

Causal Relationships Between Entities for Fluvial Geomorphology

For a given discharge acting on sediment of a given size, after a given amount of time, the prediction is made that a given morphology will be produced.[137]

Aleatory Statistical Relationships Between Entities for Fluvial Geomorphology

For a given discharge acting on sediment of a given size, after a given amount of time, the prediction is made that different morphologies will be produced, but at *some scale* these different morphologies will result in an invariant distribution.[138]

Chaos Relationships Between Entities for Fluvial Geomorphology

For a given discharge and sediment size, after a given amount of time, the prediction is made that different morphologies will be produced, but at *some scale* these different morphologies will produce a strange attractor.[139]

---

[134] See **Section 3. 3.** for a discussion on hypotheses.

[135] See **Section 3. 6.** for an outline of the major philosophies of science and their views on prediction.

[136] The boundary conditions of constant controls and constant time elapsed seek to minimise the application of error, equifinality and equilibrium to the results (see **Sections 5. 2. 1** to **5. 2. 4**).

[137] See **Section 3. 4. 1.** for a discussion of causal thinking.

[138] See **Sections 3. 4. 2** and **3. 4. 3. 2.** for an outline of statistical thinking and the deterministic nature of aleatory concepts at some scale.

[139] **Chapter Four** outlines the principles of chaos theory. See also section for a discussion of chaos laws as being intermediate forms of law.

In the field, difficulties with variability in controls and boundary conditions, and the additional theoretical constructs of equifinality and equilibrium, makes testing of these predictive statements problematic;[140] any variation in form which could be due to fundamental causal, aleatory statistical or chaos laws operating, could be ascribed to differences in magnitude of cause, boundary conditions or equifinality/equilibrium. The best means of testing these hypotheses and their predictive statements is in laboratory conditions where the conditions can be rigourously controlled. An outline of the experimental set up used to test these hypotheses is given below.

6. 2. Experimental Design

In order to test which relationships between entities could best explain alluvial river channel indeterminacy, and possibly form a law for alluvial channels, a series of flume experiments was conducted. **Figure 6. 2. 1.** shows the outlay of the experiments.[141]

A distance of 7.50m was measured from the start of the flume. This length was divided into measuring intervals of 0.05 m and was split up into fifteen sections of 0.50 m. The flume was set to a gradient of 1:100 that remained constant for all the runs. An end board (0.009 m deep x 1.37 m wide x 0.30 m long) placed at the top end of the flume, was secured to the concrete base of the flume using double sided adhesive tape and waterproofed using sealant. Towards the upstream edge of this end board, a vertical board (0.065 m deep x 1.37 m wide x 0.012 m long) was secured at right angles to the end board and sealed (A in **figure 6. 2. 1.**). In the middle of this vertical board, a cut was made so that a hole 0.065 m deep x 0.05 m wide was formed. This allowed water to enter from the stilling pool to the drainage area, and defined the width of the initial channel. On the downstream side of the vertical board and in line with the entrance hole, two steel plates (0.12 m deep x 0.22 m long) set at right angles were secured to the horizontal and vertical boards, and sealed. The purpose of these steel plates was to prevent leakage and surface flow during formation of the initial channel and to keep the flow within the steel runners.[142]

[140] See **Sections 5. 2. 2.** to **5. 2. 4.** for a discussion of error, equifinality and equilibrium.

[141] See **Appendix** (Section A) for details of experimental equipment.

[142] See **p. 213 this thesis** for a description of experimental procedure.

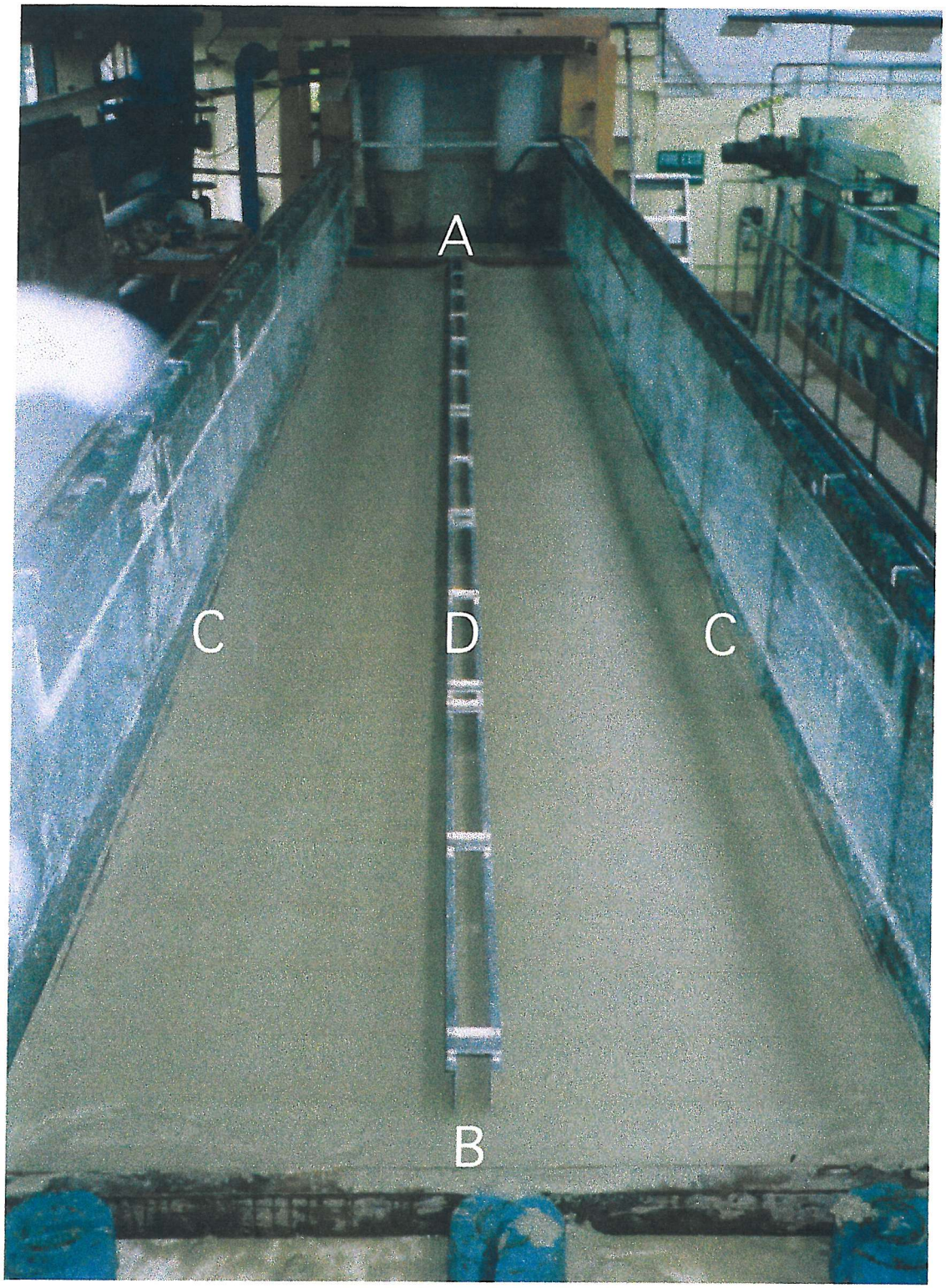
Initially the experimental design included an end board at 7.50 m, which had a hinged vertical wall that could be lowered (the distance of 7.50 m was measured between the downstream edge of the top vertical board and the upstream edge of the downstream vertical board). This hinged vertical wall was removed because the flow could not create its own channel (see below). However, the end board attached to the base of the flume which supported this vertical wall was retained. Its dimensions were the same as those of the top end board (0.09 m deep x 1.37 m wide). During the construction of a run, a removable block of wood 0.074 m deep was placed at the end of the flume to support the saturated sand surface (B in figure 6. 2. 1.) .

Wooden runners (7.50 m long: 0.074 m deep: 0.012 m wide - C in figure 6. 2. 1.) were attached to both sides of the flume and to the top and bottom end boards (a 0.009 m groove was cut at either end of the wooden runners so that their height along the length of the flume was 0.074 m). The wooden runners were sealed where they joined the concrete base of the flume, at the joins with the end boards, and at the top where they joined the perspex side walls.

The end boards and the wooden runners formed a rectangular box with dimensions 7.50 m (length) x 1.346 m (width) x 0.074 m (depth) and a total volume of 0.747 m^3. This rectangular box was filled with sorted medium to fine sand (0.50 - 0.10 mm diameter). This dimension of sand was chosen because it is easily erodible and gives a large degree of change in a short time. The maximum height of the two end boards (i.e. 0.009 m) was taken as the base level of the drainage system, and measurements were made relative to this point. The maximum height of the drainage surface relative to base level was 0.065 m (i.e. 0.074 - 0.009 m).

A baffle was constructed out of square wire mesh with dimensions 0.0120 m x 0.0120 m, and placed before the end board at the top of the flume. Water passed through the baffle before entering the drainage area.

Figure 6. 2. 1.- Photograph of a Constructed Run, Looking Upstream.



6. 3. Experimental Procedure.

Prior to the experiments being conducted, five representative test sections were selected that would represent the surface formed by fluid-sediment interaction. These five test sections were chosen objectively before the experiments began, so that measurements would not be dictated by the flow but would be independent of it. This independence of the test sections meant that sometimes the measurements would not be in the formed channel, but on the uneroded drainage surface. Because of severe time constraints, pilot studies could not be carried out after it was established that the metal sections succeeded in producing a channel. Because it was unknown how wide the final channel would be, the width of the drainage area (1.346 m) was divided into four equal sections (0.3365 m) labelled a, b, c, d, from left to right when looking upstream.[143] It was assumed that the channel would definitely occupy the central area of the drainage area and so the middle sections b and c were chosen as the independent test sections. These test sections formed a rectangle (0.50 m x 0.3365 m) and a grid was constructed (0.05 m x 0.0673 m) resulting in 66 measurements that characterised each grid.

The test sections were located away from the beginning and end of the flow in order to eliminate any transient behaviour and were made at half meter intervals. In this manner, the diversity of the eroded fluid-sediment surface was captured given the limited data gathering technique (i.e. point gauge). Table 6. 3. 1. shows the name of the five test sections and their respective distances downstream and cross-stream (left to right).

Table 6. 3. 1.- Flume Test Section Locations

SECTION NAME	DISTANCE DOWNSTREAM	DISTANCE CROSS-STREAM
B1	1.50 - 2.00 M	0.3485 - 0.6850 M
C2	2.50 - 3.00 M	0.6850 - 1.0215 M
B3	3.50 - 4.00 M	0.3485 - 0.6850 M
C4	4.50 - 5.00 M	0.6850 - 1.0215 M
B5	5.50 - 6.00 M	0.3485 - 0.6850 M

[143] Four sections were delineated because it was initially planned that random tests would show how differences in sampling location in the field could lead to different results in morphology. This procedure was not carried out because the flow stayed within sections b and c, and sections a and d were left relatively untouched.

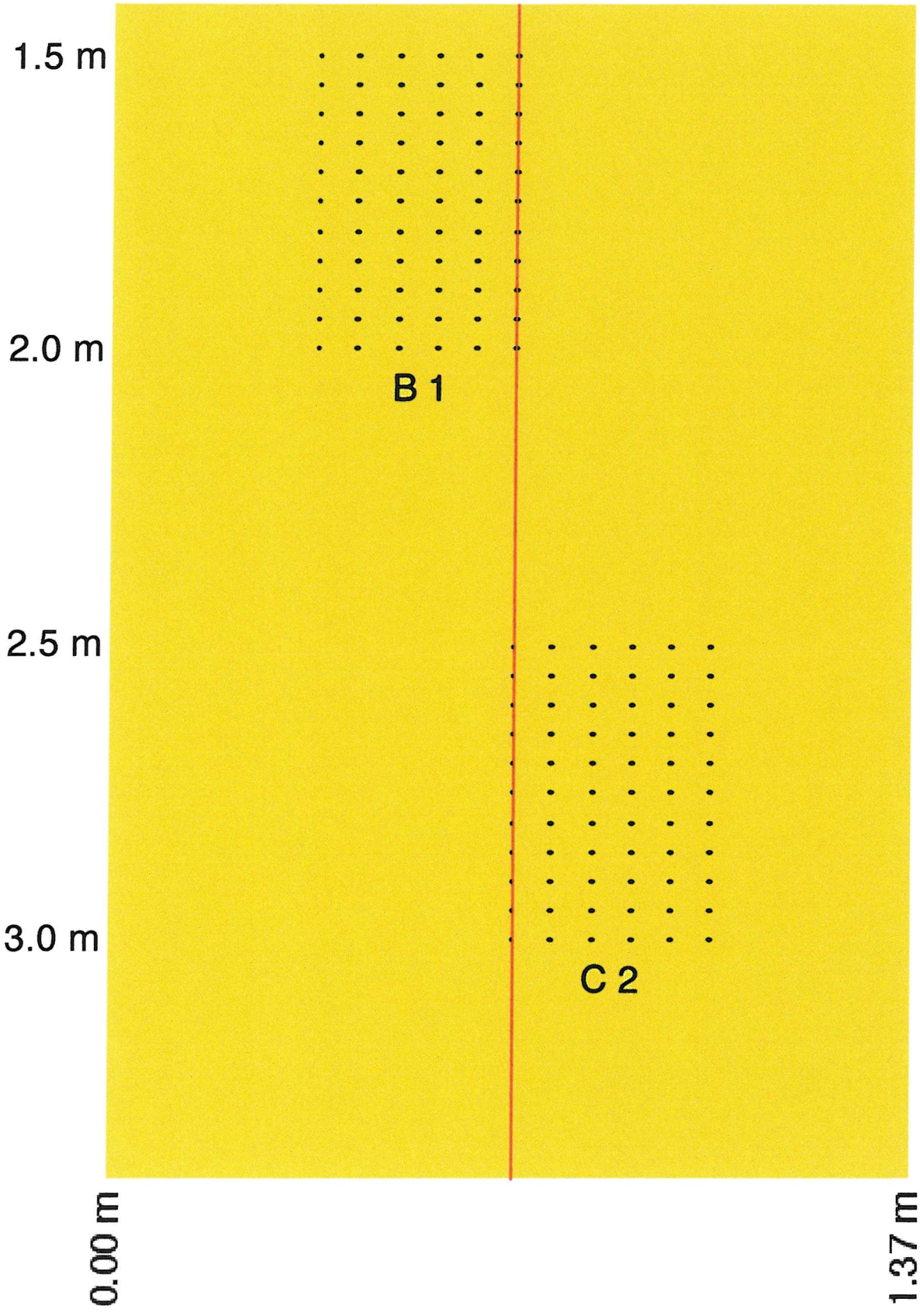
The rationale of the experiments was to keep all possible control variables constant for different experimental runs. This meant that if similar or different morphologies were produced for the runs, then these similarities or differences could be imputed to the governing equations and not to error.

The first stage in the experimental procedure was to control the water level in the initial sand bed. Water levels in sediment affect cohesion and the erodibility of the sediment; fluctuating water levels result in differences in erodibility. In order to eliminate this variation, the sand had to be completely dry or completely saturated. The first alternative has obvious problems, because a large volume of sand needs to be dried after each run. The second alternative was adopted. A removable block of wood was placed at the end of the sand bed (i.e. at 7.50 m) to support the free face of the sediment (B in figure 6. 2. 1.); the sand was mixed using a shovel and water flowed into the area for a fixed period of time.

The relative degree of saturation of the sediment was measured both within runs and between runs, because any difference in saturation is a difference in a boundary condition of the experiment. Piezometers do not work in the shallow depths of sediment that this experiment uses. In order to circumvent this problem, samples of saturated sediment were taken at specified distances downstream and weighed in their saturated state. These samples were then oven dried for a specified length of time and reweighed. The difference in weight was the weight contributed by water. The weight contributed by the water was expressed as a percentage of the total weight of the sample. This percentage saturation is expressed in the variable 'PSS' in Chapter Seven.

Once the sand bed was saturated, the fluidised bed was levelled by sliding a board across the wooden runners to achieve a flat drainage surface. This drainage surface extended from the top end board to the removable end board (A to B in figure 6. 2. 1.) and from the outside edge of one runner board to the outside edge of the other runner board (C to C in figure 6. 2. 1.). The state of the flume surface is illustrated in figure 6. 3. 1 , using two of the test locations (b1 and c2). At this first stage of the experiment the aim is to achieve a completely flat surface. Any departures from a flat surface represent a difference in initial conditions and at this stage the variable 'Initial Height of Drainage Surface' (used in Chapter Seven) is measured for each of the test sections. This variable describes the initial state of the 'flat' drainage surface, so that a potential boundary condition to any law between fluid-sediment flow and morphology is represented.

Figure 6. 3. 1. - First Stage of The Flume Experiments; 'Flat' Drainage Surface



1:10 scale diagram of flume surface. The red line represents the central transect 0.6850, which is used for analysis in **Chapter Seven**. The black dots represent the individual measurements that make up the test sections b1 and c2; their scale is exaggerated in order for them to be visible in this diagram.

Because saturated sand is fluid and has no cohesion, it was not possible to follow normal flume procedure and cut an initial channel into the sand bed; the sand that composed the banks of this cut channel would collapse and flow into the hole. A possible alternative for constructing a channel in saturated conditions was outlined by Stebbings (1963):

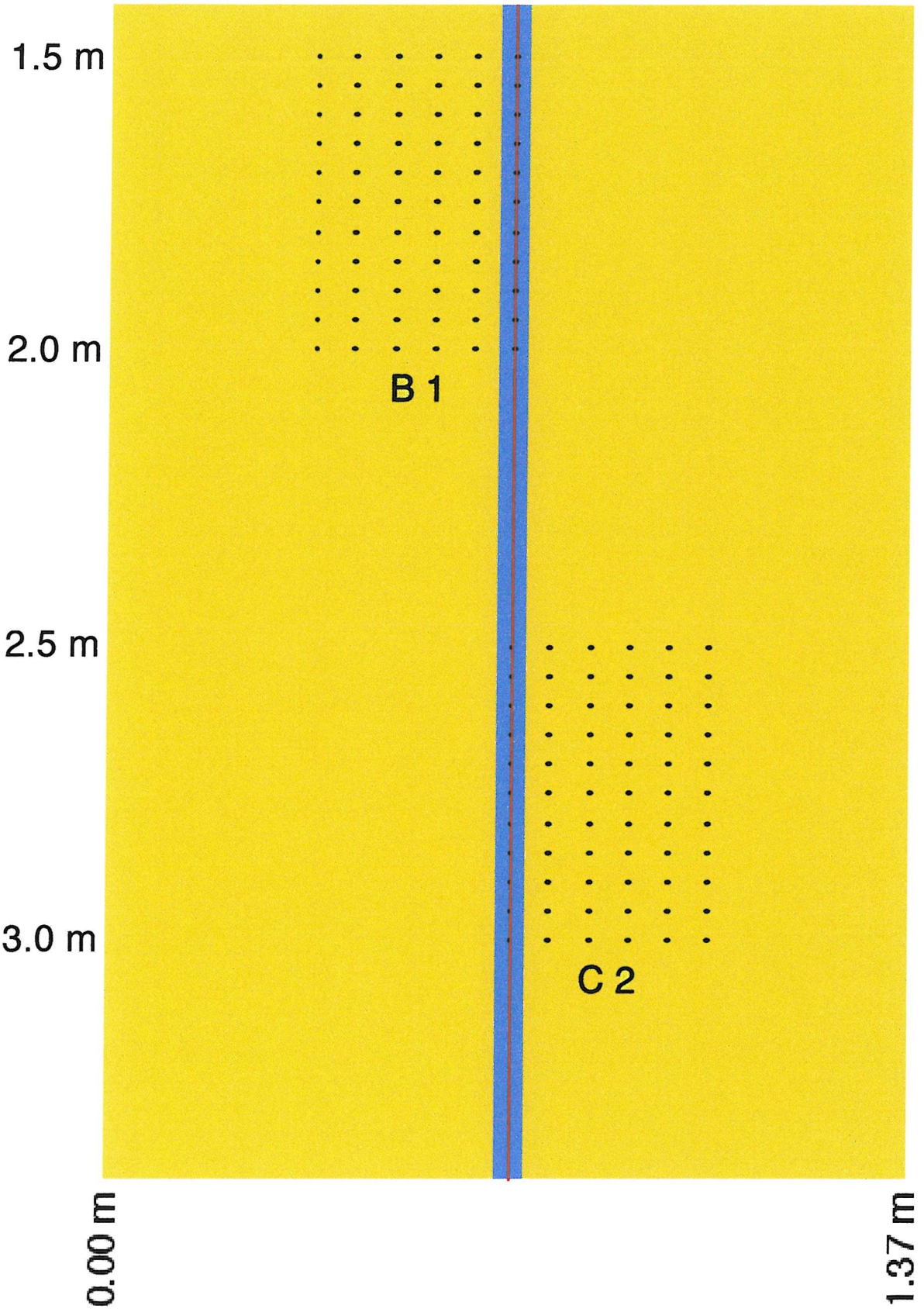
"In preliminary trials an attempt was made to obtain a self formed channel in sand screeded to a flat surface and sloping only in a longitudinal direction. This was only partially successful since, although a channel was formed, only part of the discharge was confined in it and available for erosive action. The remainder of the flow extended over the sand surface to the flume sides at shallow depth. However, the trial demonstrated that if a test were prolonged sufficiently, a channel with upstanding banks would form at the upstream end of the flume."

Stebbings (p. 487; 1963)

In a pilot study, a similar result to Stebbings (1963) was found. The discharge acted on the drainage surface as sheet flow, and a channel did not form after three hours of flow. The time constraints on these flume experiments were severe, and it was not possible to allow a channel to form by flow alone. To get round this difficulty, six metal sections (1.25 m (length) x 0.05 m (width) x 0.14 m (depth)) were constructed. These sections interlocked, and had lips on the top edge so that they could be pushed downwards. Once the sand was saturated, these metal sections were inserted into the sand and interlocked, so that they formed a metal channel 7.50 m long and 0.05 m wide for the whole depth of the sand bed (D in figure 6. 2. 1). The metal section located furthest upstream was placed over the steel plates set at right angles, so that the flow did not leak into the surrounding sand, but stayed within the metal channel.

Prior to water filling this metal channel, the temperature of the water was measured (temperature affects viscosity and therefore turbulence and is a boundary condition for the experiment) and is described by the variable 'temp' in Chapter Seven. The fixed discharge of one litre per second was started and when the flow reached the end of the metal sections (7.50 m) the removable block of wood (B in figure 6. 2. 1.) at the end of the sand bed which supported the free face was taken away. The discharge flowed between the metal sections for a period of twenty minutes, during which the sediment in between the metal sections was excavated and the flow formed its own bedforms. Figure 6. 3. 2, shows the flume in this second stage of the experiment, as the constrained flow excavates the sand in the central portion of the drainage surface. Following this period of twenty minutes, the metal sections were removed in strict order, proceeding from the end (7.50 m) of the drainage surface and finishing at the start (0.00 m), without a cessation in flow.

Figure 6. 3. 2. - Second Stage of The Flume Experiments; Constrained Flow Excavating Central Portion of Saturated Drainage Surface.



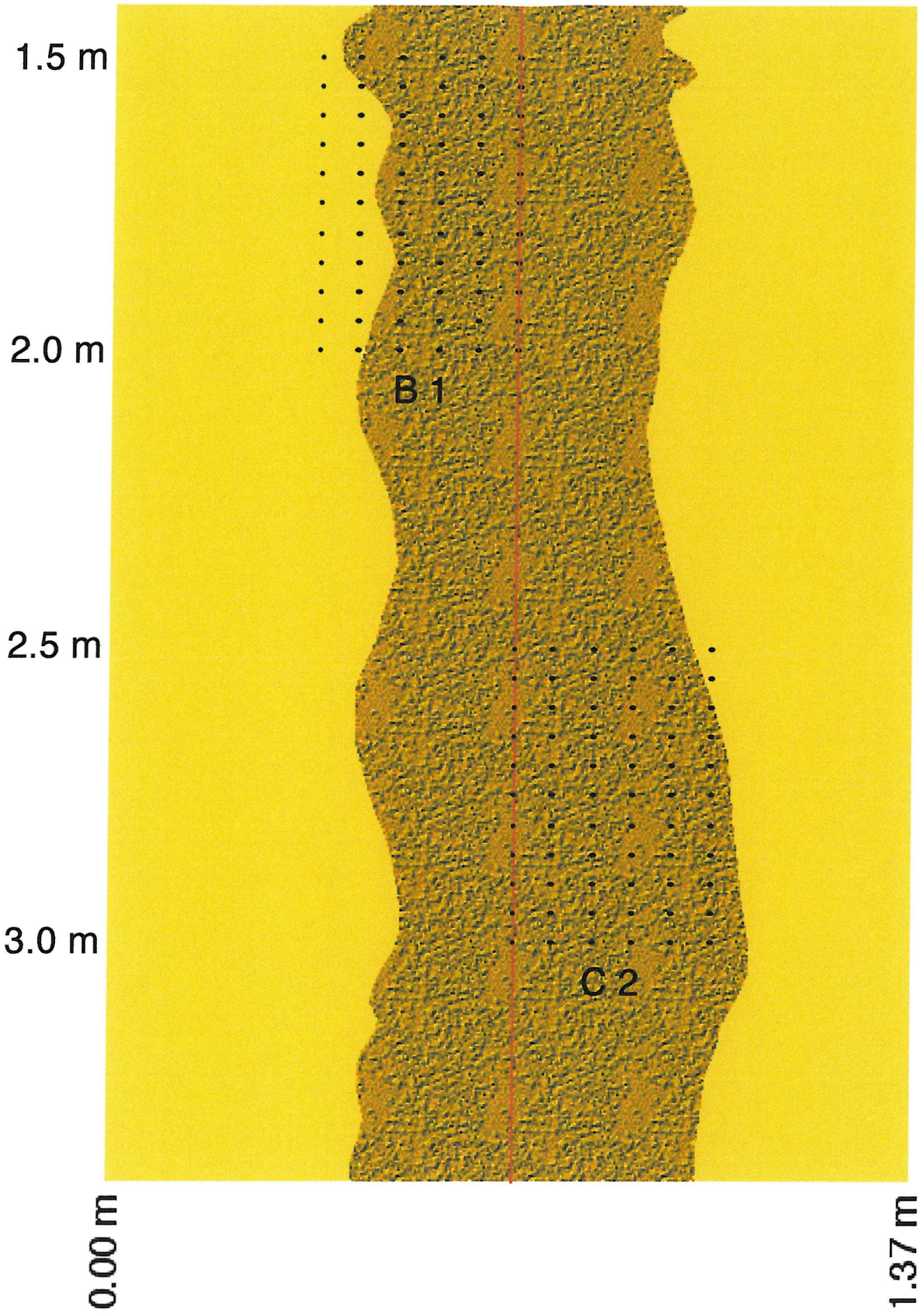
1:10 scale diagram of flume surface. The red line represents the central transect 0.6850, which is used for analysis in **Chapter Seven**. The black dots represent the individual measurements that make up the test sections b1 and c2; their scale is exaggerated in order for them to be visible in this diagram. The blue section represents the extent of the constrained flow, which excavates the saturated sand within these confines. The flow is constrained in order to form an initial channel for the flow to occupy.

The experimental clock was started, once the metal boards had been removed, and the flow continued for three hours, forming its own dimensions. On the hour, for hours one and two, velocity measurements were made at each of the five test sections, followed by photographs of every section (15 sections); a note was made of the time taken for this procedure. After three hours of flow, the discharge was stopped and the surface water was allowed to drain out of the drainage area. Photographs were taken of the final morphology of the bed and measurements of the heights of the bed were taken using the point gauge at each of the five test sections. These measurements of the eroded surface are termed 'final height' in Chapter Seven and are used as a quantitative description of morphology. Figure 6. 3. 3, shows the final state of the drainage surface and the relative positions of the test sections.

Once these measurements had been acquired, the procedure was repeated and a new experimental run was conducted. A total of eight runs were conducted in this manner.[144] The next chapter outlines the results of these eight runs for each of the different predictive statements of causal, aleatory statistical and chaos hypotheses.

[144] See **Appendix Section G** for a full description of all experimental runs.

Figure 6. 3. 3. - Final Stage of The Flume Experiments; Eroded Surface and Relative Position of Test Sections



1:10 scale diagram of flume surface. The red line represents the central transect 0.6850, which is used for analysis in **Chapter Seven**. The black dots represent the individual measurements that make up the test sections b1 and c2; their scale is exaggerated in order for them to be visible in this diagram. The darker, textured area represents the extent of the eroded surface. Because the test sections are independent of the flow, some measurements may include the uneroded drainage surface. The extent to which this occurs differs between flows and is therefore valuable information.

Chapter 7

Analysis and Discussion of the Fundamental Data Set

Outline

This chapter is concerned with analysing the results of the experiments outlined in Chapter Six. It has been argued in previous chapters that geomorphology is science and that science is a form of knowledge characterised by laws. A particular feature of scientific laws is their ability to be tested by the predictions they make.[145] This chapter tests the predictions that causal, aleatory statistical, and chaos hypotheses make for fluvial morphology.[146] The results of these tests have implications for previous work in geomorphology[147] and for future investigations into the indeterminacy of rivers. The success of these hypotheses in experimental conditions also has implications for the scientific status of geomorphology. The chapter begins by outlining the fundamental data set and giving a glossary of abbreviations used herein (7. 1.). The complexity which scientific laws must predict is shown through contour plots of several data sets (7. 2.). The causal hypothesis (which attempts to predict individual data points) is tested using linear regression at the run level and the reach level (7. 3 - 7. 3. 3) and the appropriateness of linear regression is examined through residual analysis (7. 3. 4.). The aleatory statistical hypothesis (which attempts to predict the statistical properties of distributions) is tested by comparing statistical similarity and forming epistemic relationships between the statistical properties of the controls and the statistical properties of the morphology at the run level and the reach level (7. 4. - 7. 4. 9.). Finally, the chaos hypothesis (which attempts to predict certain properties of a data set) is tested by attempting to find a constant fractal dimension at the reach level (7. 5.). The programme, Fractal Dimension Calculator, written by Paul Bourke is used. This programme calculates the box-dimension from two-dimensional wireframe images. From the analysis of the flume data using causal, aleatory statistical and chaos approaches, it is found that only the aleatory statistical hypothesis at the run level passes predictive scrutiny; for all other hypotheses there is no consistently produced morphology and no consistent relationship between the controls and the final channel.

[145] See **Chapters Two, Three** and **Four** for an outline of this scientific framework.

[146] See **p. 209 this thesis** for the predictive statements of these three laws.

[147] See **Chapter Five**. This is particularly important for assessing epistemic relations between aleatory values, and the theoretical constructs of equifinality and equilibrium.

7. 1. The Fundamental Data Set

The data on which all of the subsequent analyses are based are the Final Height measurements for the five different test sections of runs one to eight, which had fixed and constant discharge of one litre per second.[148] In the flume, the x and y co-ordinates were fixed by the *a priori* selection of the test sites, but the z co-ordinate or the height of the drainage area varied according to the indeterminate behaviour of the fluid flow and any laws that govern its behaviour. The height of the drainage bed is used as a quantitative measure of the morphology of the experimental channel. The lack of a quantitative measure of morphology in some fluvial studies was discussed in Chapter Five.

The data set of drainage bed heights for each of the eight runs is presented in the Appendix (section B) using the co-ordinate system that was used to collect the data in the flume; i.e. the data moves from left to right along rows and moves downstream from top to bottom in columns. A brief description of the statistics of this data set show that it is not normally distributed, but has a bi-modal distribution. This bi-modality arises from the objective locating of the test sites and the inclusion of two different populations in the data; the channel, with one distribution and the flat or uneroded drainage area with another distribution.

In addition to the fundamental data set, information on the controls that are thought to govern the formation of morphology in the flume channel is also documented; these controls are the discharge, the slope, the temperature of the water,[149] the initial height of the drainage surface,[150] and the percentage saturation of the sediment.[151] These data are included in the Appendix.

Several abbreviations are used when describing the fundamental data set and the controls thought to affect morphology in the flume. For clarity these are given below:

Final Height	FHT
Temperature	TEMP
Initial Height of Drainage Surface	IHDS
Percentage Sediment Saturation	PSS
Distance From Source	DFS

[148] See **p. 218 this thesis** for a definition and description of final height in the experiments.

[149] See **p. 216 this thesis** for a description of the measurement of temperature in the experiments.

[150] See **p. 214 this thesis** for a definition and description of initial height of drainage surface in the experiments

[151] See **p. 214 this thesis** for a definition and description of percentage sediment saturation in the experiments

7. 2. A Brief Description of the Fundamental Data Set.

The flow in the flume generated complicated and diverse forms of morphology. An example of each of the five sections (b1, c2, b3, c4, b5), which get progressively further from the source of the flow, has been selected at random from the eight runs. A contour plot showing the final height of these sections is given in figures 7.2.1. - 7.2.5.

Figure 7.2.1. - Contour Plot of Section b1: Run Two

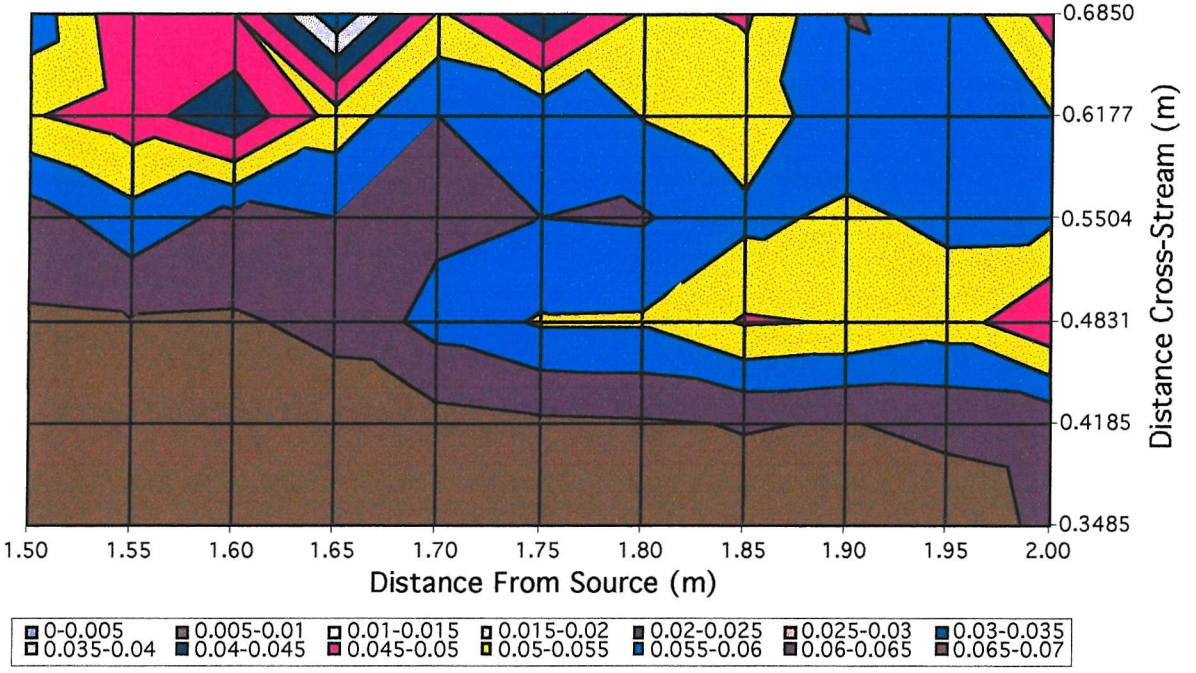


Figure 7.2.2.- Contour Plot of Section c2: Run Seven

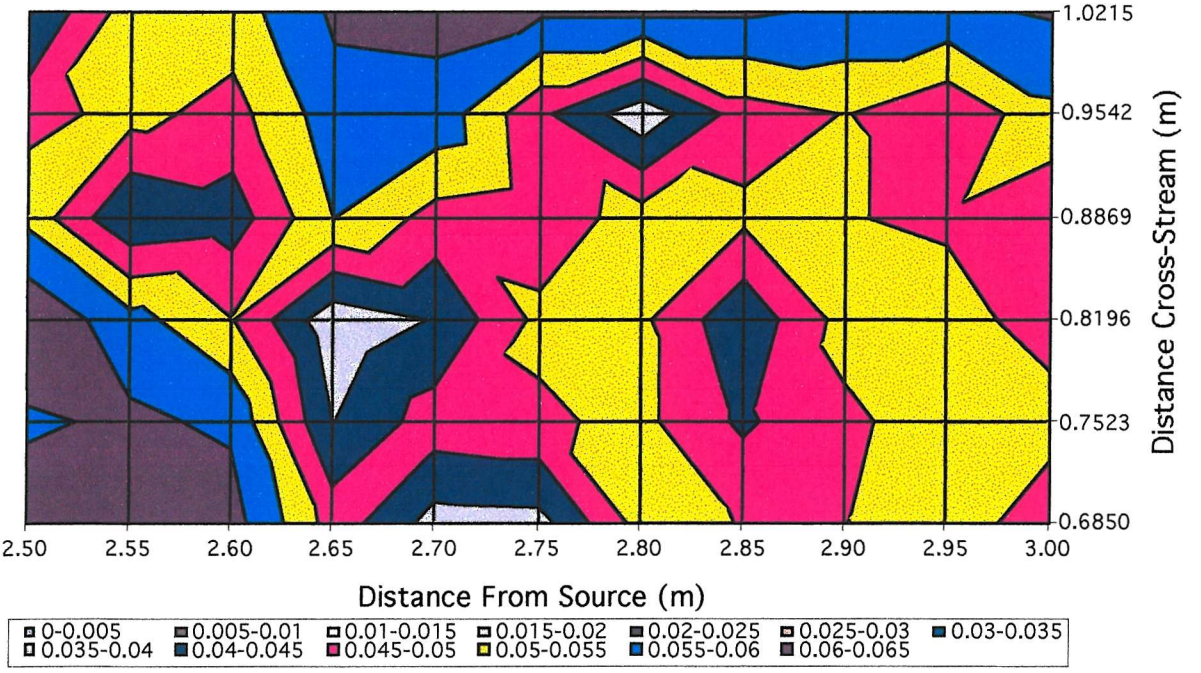


Figure 7.2.3- Contour Plot of Section b3: Run Four

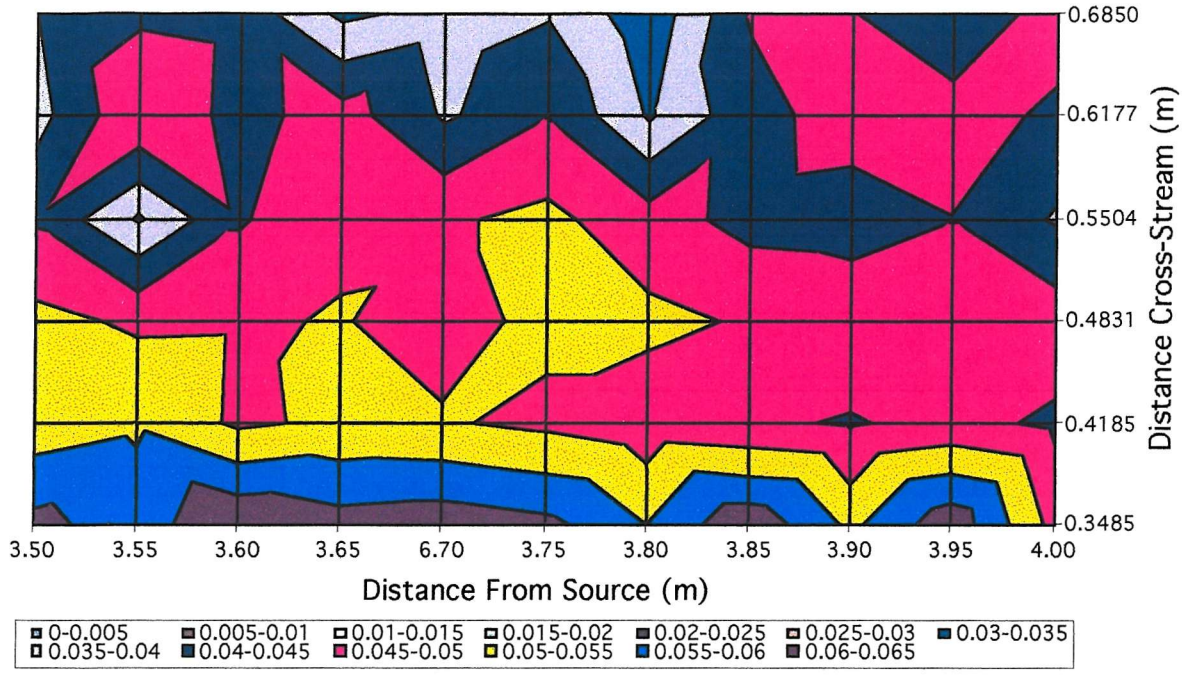


Figure 7.2.4- Contour Plot of Section c4: Run Five

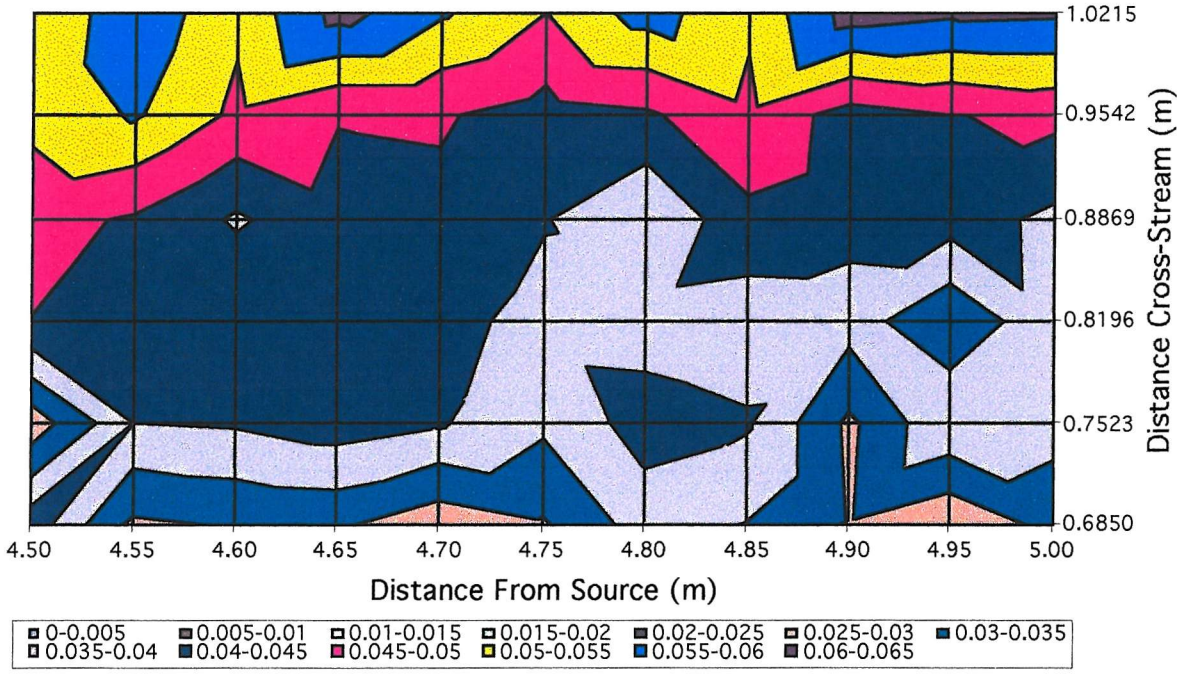
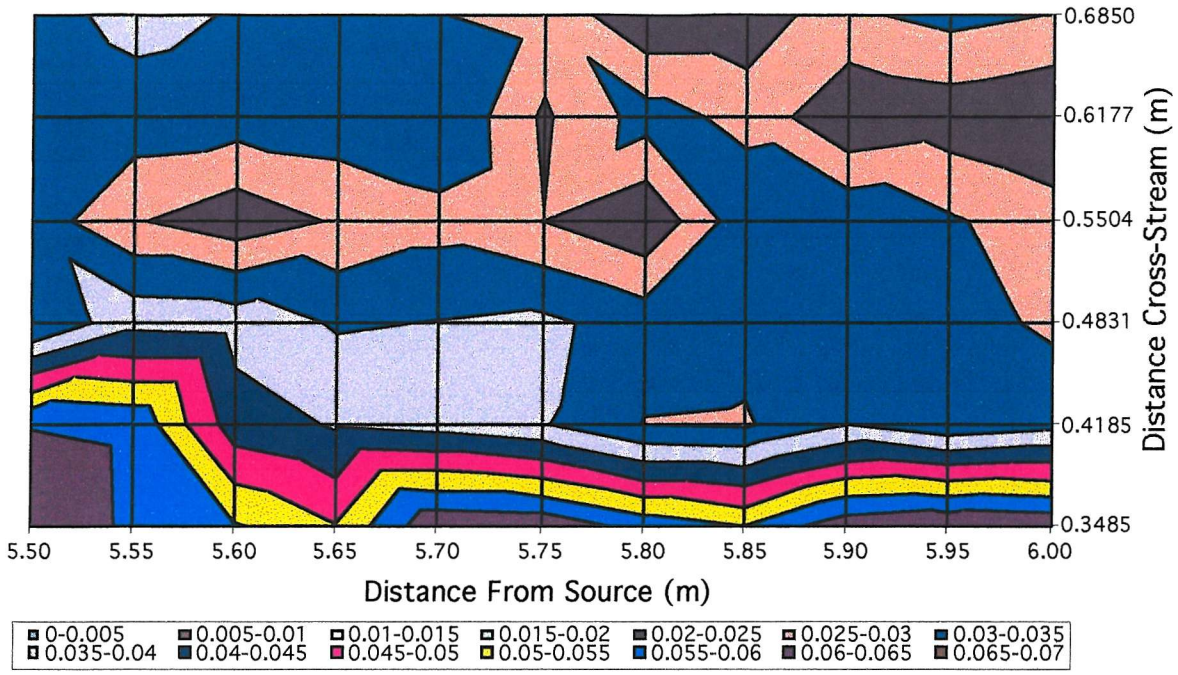


Figure 7.2.5- Contour Plot of Section b5: Run Three



The contour plots show the complexity of fluvially generated morphology. It is expected, from the viewpoint of science as presented in Chapter Three, that a law, at some scale, should be able to explain the diversity of the morphology created by fluvial flows.

When faced with complexity, scientists often try to simplify the problem in order to begin to understand the behaviour in question. In a particularly relevant illustration, Stewart (p. 184; 1997) writes of Feigenbaum that "... like any sensible research scientist [he] made no attempt to beat his brains out on the full complexity of real turbulent flow." A similar approach is adopted in the subsequent analysis of the above contour plots. For many of the statistical techniques used, it is assumed that the data is independent - that is the value of one piece of data does not affect another. Intuitively this statement of independence cannot be supported for fluvial flows which evolve explicitly over time and space. Rather than attempt to define the whole interaction of dependency over the grid of data, a single transect (0.6850) that is present in all sections is used for analysis. The analysis then tries to model or include this dependency as it develops over a run. The exception to this is in the application of fractal methods, the precise nature of which demands the use of all the grid data.

In terms of the philosophy of science, the failure of a law to predict this single transect means that confidence in that type of law for explanation of fluvial morphology will be reduced. Success of a type of law in predicting the single transect suggests further analysis is required that aims to explain the whole grid of data.[152]

[152] See **Section 3. 6.** for an outline of views on knowledge and prediction.

7. 3. Testing the Causal Hypothesis for the Fundamental Data Set

This section tests the hypothesis that "Causal relationships between entities best explain the indeterminacy of alluvial river channel cross-section and alluvial river channel planform." This is a level of analysis which uses individual values of Final Height (FHT) and is more detailed than most previous work in geomorphology, which has made use of mean values of the controls and effects.[153]

The above contour plots of the Final Height measurements have shown the complexity of the morphological information generated by the flow in the flume. This section aims to explain that complexity in terms of linear causal laws at the level of individual measurements.

The ideal conditions for testing the hypothesis "Causal relationships between entities best explain the indeterminacy of alluvial river channel cross-section and alluvial river channel planform," would have occurred if the control variables had been perfectly constant. If this had been the case, analysis of the data could have been achieved using correlation between the different final heights of each run. If the data was correlated, a causal relationship could have been deemed to apply to the data - a given set of controls produce a given morphology. If the data was not correlated then it could have been stated that the casual model was not successful in explaining the data - a given set of controls produces different morphology.

The above situation was violated, because small changes in the control variables occurred. In this case, epistemic probability is used (specifically linear regression) to relate changes in the control variables to changes in morphology. If changes in the control variables explain changes in morphology for a given run, and the relationship between the controls and morphology is the same between runs, then causal laws can be thought to apply at this level. Epistemic probability treats departures from any derived relationships as error.[154]

The controls used for a linear causal explanation of the Final Height (FHT) data are the variables of discharge, slope, water temperature, Percentage Sediment Saturation (PSS - a measure of sediment strength), and the Initial Height of the Drainage Surface (IHDS - a measure of the initial conditions for the fluid flow). Out of these controls, discharge and slope are constant for all runs. In principle then, FHT should be explicable in terms of PSS, IHDS and temperature fluctuations.

[153] See **Section 5. 1.** and **5. 2. 1.** for a discussion on the use of mean values in fluvial geomorphology.

[154] See **Section 3. 4. 1.** this thesis for the theory of epistemic probability and error.

7. 3. 1. Linear Prediction of the Final Height at the Run Level.

The first analysis of the FHT data was concerned with forming linear predictive relationships between FHT and PSS and between FHT and IHDS data for the central transect of a whole run (i.e. sections b1, c2, b3, c4, b5 taken together). At this juncture, it is necessary to state that another intrinsic control must be included in the analysis. The data for FHT and for the controls of PSS and IHDS are inevitably collected at different locations along the flume. Because spatial location varies, it must be classed as an independent variable, and is included in the regression analysis under the label Distance From Source (DFS). As DFS increases, measurements are made progressively further downstream.

Tables 7. 3. 1. 1. - 7. 3. 1. 3. show the results of regressing FHT on DFS, PSS and IHDS. A cursory glance at the r^2 values for these regressions indicate that DFS is the best linear explanation for the height data. The relationship between FHT and DFS is fairly strong in all runs, although some unexplained variance remains even for run five which exhibits the strongest relationship ($r^2 = 0.823$).

Table 7. 3. 1. 1.- Regression of Final Height on Distance From Source

Run No.	$\hat{\beta}_0$	$\hat{\beta}_1$	r^2
1	0.0606	- 0.00602	0.630
2	0.0589	- 0.00453	0.596
3	0.0639	- 0.00604	0.695
4	0.0690	- 0.00643	0.708
5	0.0703	- 0.00748	0.823
6	0.0666	- 0.00623	0.763
7	0.0609	- 0.00489	0.548
8	0.0618	- 0.00537	0.632

Table 7. 3. 1. 2.- Regression of Final Height on Percentage Sediment Saturation

Run No.	$\hat{\beta}_0$	$\hat{\beta}_1$	r^2
1	0.1440	-0.00560	0.091
2	0.1110	-0.00359	0.071
3	0.1500	0.01010	0.210
4	0.2920	-0.01310	0.294
5	0.3680	0.02140	0.336
6	0.2190	0.01380	0.175
7	0.1480	0.01050	0.346
8	0.1720	-0.00770	0.241

Table 7. 3. 1. 3.- Regression of Final Height on Initial Height of Drainage Surface

Run No.	$\hat{\beta}_0$	$\hat{\beta}_1$	r^2
1	-0.2500	4.5950	0.289
2	-0.1100	2.4060	0.224
3	-0.2200	4.0430	0.252
4	-0.1200	2.5360	0.203
5	-0.3600	6.1930	0.338
6	-0.2100	3.8530	0.289
7	-0.1200	0.8300	0.043
8	-0.2300	4.2960	0.656

The square root of the r^2 value is the correlation coefficient and the value of this coefficient is significant[155] at the 95% confidence limit for all runs for the relationship between FHT and DFS. The strength of the linear relationship between FHT and DFS is not constant between runs, but varies. Run five has 82.30% of the variance in FHT explained by DFS, whilst run seven has only 54.80 % of the variance in FHT explained by DFS. This variation in the strength of the linear relationship between FHT and DFS may be due to the effects of PSS and IHDS in individual runs, and this is investigated in subsequent analyses. The nature of the linear relationship is unambiguous; in all eight runs the constant for DFS ($\hat{\beta}_1$) is negative and the predictive linear relationship is consistently inverse; that is, as DFS increases, FHT decreases.

Table 7. 3. 1. 2. shows the predictive regression relationships between FHT and PSS. The r^2 values show that this relationship is quite weak, although the correlation coefficient is significant at the 95% confidence limit for all runs, with the exception of run two, where it is not significant and suggests that FHT appears to be randomly related to PSS for run two. PSS was measured for each grid location and thus one data value for PSS is related to eleven values of FHT. As with the predictive relationship between FHT and DFS, the predictive relationship between FHT and PSS is not constant in strength. Run seven has 34.60% of its variance in FHT explained by PSS whilst run two has 7.10% of its variance in FHT explained by PSS.

[155] Significant difference in the context of correlation implies that the null hypothesis (there is no relationship between two variables) is rejected in favour of the alternative hypothesis (there is a relationship between two variables).

In addition to the relatively weak and variable predictive relationship between PSS and FHT, the situation is complicated by the ambiguous nature of the relationship. Exactly half of the runs (3, 5, 6 and 7) have positive $\hat{\beta}_1$ constants, indicating a proportional predictive relationship which states that, as PSS increases, FHT increases. The other half of the runs (1, 2, 4 and 8) suggest the opposite, having negative $\hat{\beta}_1$ constants and indicating an inverse predictive relationship which states that, as PSS increases, FHT decreases. This ambiguous nature of the relationship clearly presents problems for the predictive testing of linear laws.

It was mentioned that the variation in the strength of the linear relationship between FHT and DFS, may be due to the effects of PSS and IHDS in individual runs. Having derived some statistics for PSS, its effects on the relationship between FHT and DFS can be assessed. There is no significant linear relationship between the r^2 values of FHT and DFS and the r^2 values of FHT and PSS. Similarly, there is no significant linear relationship between the r^2 values of FHT and DFS and the $\hat{\beta}_1$ values of FHT and PSS. In other words, knowing the strength and the direction (proportional or inverse) of the relationship between FHT and PSS, does not explain the variance in the strength of the relationship between FHT and DFS.

Table 7.3.1.3. shows the predictive linear relationship between FHT and IHDS. The relationship is again quite weak, but all the runs have significant correlation coefficients at the 95% confidence limit, with the exception of run seven which is not significant. Because of severe experimental time constraints, the height of the initial drainage surface was sampled at twice the interval of FHT, or half as many measurements were made of the height of the initial drainage surface than were made of FHT. Rather than waste data and use a section average for the regression relationships, average values were derived for the missing values of the height of the initial drainage surface via linear interpolation between the nearest upstream and downstream values. As with the previous two relationships, the predictive relationship between FHT and IHDS is not constant in strength; run eight has a relatively strong linear relationship with 65.60% of the variation in FHT being explained by IHDS, whilst run seven has only 4.30% of the variation in FHT explained by IHDS. The direction of the relationship between FHT and IHDS is constant and proportional; that is, as IHDS increases, FHT increases.

As with PSS, it can be assessed how the strength of the relationship between FHT and DFS is affected by the relationship between FHT and IHDS. The r^2 values for the relationship between FHT and IHDS have no significant linear relationship with the r^2 values for the relationship between FHT and DFS. That is, knowing the strength of the relationship between FHT and IHDS, does not explain the variance in the strength of the relationship between FHT and DFS.

Multiple linear regression is a technique for estimating the combined linear relationship of the three control variables of DFS, PSS and IHDS with FHT. However, before a multiple regression is performed, Weisberg (p. 39; 1985) recommends that the possible relationships between the control variables are explored. This has been done partially above, in the examination of whether the strength of the relationship between FHT and DFS is related to a relationship between FHT and PSS/IHDS. Tables 7. 3. 1. 4 to 7. 3. 1. 6. explore the explicit relationships between the control variables without reference to FHT (the dependent variable).

Table 7. 3. 1. 4.- Regression of Percentage Sediment Saturation on Distance From Source

Run No.	$\hat{\beta}_0$	$\hat{\beta}_1$	r^2
1	18.2668	0.1723	0.176
2	18.3544	0.2217	0.257
3	19.6381	-0.1879	0.327
4	18.1481	0.2012	0.403
5	19.6719	-0.1279	0.327
6	19.4881	-0.1229	0.323
7	18.9376	-0.1976	0.284
8	15.8050	0.2583	0.366

Table 7. 3. 1. 5.- Regression of Initial Height of Drainage Surface on Distance From Source

Run No.	$\hat{\beta}_0$	$\hat{\beta}_1$	r^2
1	0.0644	-0.0005	0.353
2	0.0647	-0.0009	0.566
3	0.0672	-0.0005	0.278
4	0.0680	-0.0007	0.230
5	0.0647	-0.0005	0.377
6	0.0673	-0.0006	0.328
7	0.0688	-0.0009	0.311
8	0.0671	-0.0009	0.480

Table 7. 3. 1. 6.- Regression of Initial Height of Drainage Surface on Percentage Sediment Saturation

Run No.	$\hat{\beta}_0$	$\hat{\beta}_1$	r^2
1	0.0768	-0.0008	0.123
2	0.0767	-0.0007	0.062
3	0.0595	0.0003	0.013
4	0.0688	-0.0002	0.002
5	0.0572	0.0004	0.016
6	0.0362	0.0002	0.109
7	0.0882	-0.0001	0.080
8	0.0988	-0.0002	0.488

The results in Tables 7. 3. 1. 4 to 7. 3. 1. 6. are governed by a logical constraint placed on the data. Because the relationship between FHT and DFS is the strongest relationship, it necessarily follows that if PSS and IHDS are to have any effect on FHT they must vary significantly in some way (whether inversely or proportionally) with DFS. Tables 7. 3. 1. 4. to 7. 3. 1. 6. indicate that this is the case. The relationships between PSS and DFS are relatively weak, but all have significant correlation coefficients at the 95% confidence limit. As with all the other regressions discussed so far, the strength of the relationship is not constant but varies. The relationships between IHDS and DFS are in general slightly stronger than those for PSS, but are particularly strong for runs two and eight.

The relationship between IHDS and DFS is influenced to a large extent by the fact that the first measurement grid (b1) appears to have had a consistently higher initial drainage surface throughout all the runs, whilst the rest of the measurement grids appear to have been much similar in initial height (see Appendix, Section C). A probable explanation for this difference is that the drainage area was constructed by placing sediment at the top of the flume and smoothing it by working from source to mouth; it appears that not all of the sediment was effectively redistributed, and this has led to slightly higher heights at the top of the channel.

If the first measurement grid (b1) is removed from the regression relationship between IHDS and DFS, then the new regression relationships have the r^2 values 0.151 and 0.054 for runs two and eight respectively, a considerable reduction in the level of explanation achieved. However, because the height of the initial drainage surface is held to be a control on the final morphology, the first measurement grid (b1) is included in the analysis and the regression relationships in table 7. 3. 1. 5. stand.

The relationships between PSS and DFS and IHDS and DFS are the opposite to the relationships between FHT and PSS and FHT and IHDS. For example, the relationships between IHDS and DFS are consistently inverse, whereas the relationships between FHT and IHDS are consistently proportional. This switching of the direction of relationship is a result of the logical constraint that the relationship between FHT and DFS place on the controls. The direction of the relationships in Tables 7. 3. 1. 4. to 7. 3. 1. 6. can in fact be derived from the two statements made by the previous regressions. For example, table 7. 3. 1. 1. makes the statement that as DFS increases, FHT decreases, whilst table 7. 3. 1. 3. makes the statement that as IHDS increases FHT increases. Because FHT decreases downstream, and IHDS is proportionally related to FHT, the IHDS must decrease downstream, and this is what is shown by table 7. 3. 1. 5. The more varied relationships of PSS similarly exhibit this switching of direction between tables.

The relationships between IHDS and PSS are very weak, with the exception of run eight, where the relationship is relatively strong. In four of the runs (1, 6, 7 and 8) the correlation coefficient is significant at the 95% confidence limit, whilst in the remaining four runs (2, 3, 4 and 5) the correlation coefficient is not significant at the 95% confidence limit. The null hypothesis is only marginally overturned for run seven, with the critical limit just being exceeded. Most critical values for the correlation coefficient are given to the nearest ten degrees of freedom, and some error is introduced by rounding up. A more accurate check using the p value for r^2 provided by the program Minitab, indicated that the null hypothesis should not be rejected for run seven and that the relationship between height of the initial drainage surface and PSS appears to be random for this run.

The direction of the relationship for table 7. 3. 1. 6. should be predictable from the relationships between PSS and DFS, and IHDS and DFS. Where the change in PSS and IHDS is similar (i.e. negative) with DFS, the relationship between IHDS and PSS should be proportional. Where the change in PSS and IHDS is different with DFS (i.e. PSS is positive change and IHDS is negative change) the relationship between IHDS and PSS should be inverse.

This direction switching enforced by the necessary changes in control variables with DFS, occurs in table 7. 3. 1. 6. with the exception of run seven, where a proportional relationship is predicted but an inverse relationship is present. An explanation for this behaviour comes from the random association between IHDS and PSS. Because no significant linear relationship exists, the regression coefficient $\hat{\beta}_1$ may be randomly distributed about a mean of zero, including positive and negative values.

Having considered the interrelationships between variables, table 7. 3. 1. 7. lists the multiple regression of FHT on DFS, PSS and IHDS. For four of the runs (1, 3, 4 and 5) the addition of PSS and IHDS improves the explanation of FHT by less than 1.00%. Explanations are improved by 1.10%, 6.10.%, 6.30% and 16.40% for runs six, two, seven and eight respectively. There is no consistent or unique set of relationships between FHT and the individual variables or between the different variables that account for the better performance of the multiple regression for runs two, seven and eight in particular.

Table 7. 3. 1. 7.- Regression of Final Morphology Height on Distance From Source, Percentage Sediment Saturation and Initial Height of Drainage Surface

Run No.	$\hat{\beta}_0$	$\hat{\beta}_1$	$\hat{\beta}_2$	$\hat{\beta}_3$	r^2
1	-0.0169	-0.0057	0.0009	0.9360	0.638
2	0.1120	-0.0066	0.0030	-1.6000	0.657
3	0.0202	-0.0057	-0.0001	0.6790	0.701
4	0.0602	-0.0059	-0.0002	0.4370	0.712
5	-0.0726	-0.0065	0.0043	0.8560	0.832
6	0.1140	-0.0065	-0.0038	0.4070	0.774
7	0.0930	-0.0054	0.0016	-0.9030	0.611
8	-0.2340	-0.0035	0.0042	0.0042	0.796

Summary of Section 7. 3. 1.

The primary result of the above analyses is that DFS is the main control on the FHT of a surface deformed by fluid flow. DFS had the strongest regression relationship with FHT. Thereafter the strength of this relationship forms a logical constraint on the other control variables. The control variables may appear to be randomly related to FHT, but if any type of significant linear relationship is present, then it must, by logical necessity, be inversely or proportionally related to DFS. Similarly, because the control variables may change either randomly or significantly with DFS, the interrelationships between variables are either random or determined by the nature (proportional or inverse) of change with DFS. The conclusion is that the two determining variables, namely PSS (bank cohesion in the field) and IHDS are only auxiliary to the internal variable of DFS. This is why the multiple regression equations do not yield significant improvements in the explanation of variance in FHT, when compared with the explanations provided by distance from source alone.

The significance of this result is that DFS obscures the effect of the controls on flume morphology, because within a run, morphology is more related to where in the flume the measurements are made than to the controls acting on that location.[156]

At this point in the analysis, it seems appropriate to ask why FHT varies so markedly with DFS, and therefore obscures the effect of any differences in control variables? The initial drainage surface can be considered as a flat surface, with no internal gradients, that was tilted to a constant slope in order to let the input of water flow under gravity; that is the surface of the sand runs parallel to the bed of the flume. However, once flow has begun and sediment entrainment occurs, the formation of an internal gradient is an inevitable part of the system. Sediment takes a finite time to be eroded and transported out of the system and the drainage area can be thought of as a queue for sediment (Langbein and Leopold; 1968) have proposed a model of sediment transport that explains sediment movement in terms of traffic queues). At the end of the drainage area, or maximum DFS (section b5), where the sand stops and the flume base becomes visible, sediment moves quickly out of this area and on to the open area of the flume base. This occurs because the concentration of sediment at the end of the flume is lower and transport rate of the particles is regulated only by the amount of fluid discharge. The concentration of particles increases upstream (DFS is smaller) and the rate of sediment movement slows because the sediment is in a queue; the rate of transport depends not only on the flow of water but on the movement of the particles in front. The result of this difference in concentration and sediment transport rate, is that the FHT decreases with DFS because the sediment concentration becomes lower and the sediment transport rate increases.

[156] See comment on hydraulic geometry (**p. 144 this thesis**) where it was stated that in principle, a universal quantitative law for morphology in different physiographic settings should not be possible. This result suggests that in terms of the relationship between external controls and morphology, a universal law may not be applicable within the same, controlled, physiographic setting.

7. 3. 2. Linear Prediction of the Final Height of Sections Within a Run.

The variable DFS for a whole run, includes regions of the flume that are invariably different from one another due to the rate of sediment transport, and the strength of this relationship between regions of the flume places a strong logical constraint on changes in the control variables. If, however, regression relationships were made for each region of the flume individually, then it should be possible to identify what effect the control variables of PSS and height of the initial drainage surface have on FHT, because the FHT is more homogeneous and is not influenced by the trend between regions of the flume.

This strategy to reduce the influence of DFS and highlight the underlying controls on flume morphology was employed by forming five individual regression relationships for each run. The final heights of the central transect 0.6850 were treated in isolation for each of the five measurement grids. Although DFS must be included in any regression relationship (because measurements are made at specific locations) the distance is limited to 0.50 m and the change in FHT over this distance should be reduced, because the sediment transport conditions should be more similar within a region. One problem with reducing the effects of DFS by examining individual measurement grids is that PSS becomes a constant and cannot be included in the regression relationship. PSS was measured for each measurement grid and so a single value covers the eleven measurements made downstream. The influence of PSS is analysed later.

Tables 7. 3. 2. 1 to 7. 3. 2. 3. document the parameters $\hat{\beta}_0$ $\hat{\beta}_1$ and r^2 for the regression of section FHT on DFS. Of the 40 regression relationships, 29 of the correlation coefficient values are not significant at the 95% confidence limit, suggesting an apparently random relationship between FHT and DFS in these cases. Significant relationships are marked in red. Out of the 11 significant relationships (marked in red), four occur in run two, 10 have negative $\hat{\beta}_1$ parameter values indicating an inverse relationship (as DFS increases FHT decreases) and one has a positive $\hat{\beta}_1$ parameter indicating a proportional relationship (as DFS increases FHT increases). The strength of the relationship between FHT and DFS is not constant but varies; the maximum amount of variance in FHT explained by DFS is 78.20% whilst the minimum amount of variance in FHT explained by DFS is 0.00%.

Table7. 3. 2. 1.- Regression of Final Height on Distance From Source for transect 0.6850 at individual measurement grids; temperature, discharge, slope and Percentage Sediment Saturation constant.[157]

			$\hat{\beta}_0$		
Run No.	b1	c2	b3	c4	b5
1	0.0440	0.1172	-0.0034	0.1326	0.0209
2	0.0277	0.0798	0.1464	0.1196	0.1479
3	0.0724	0.0793	0.0335	0.1681	0.1145
4	0.0274	0.0026	0.0082	0.0381	0.1761
5	0.0744	0.0283	0.0738	0.0802	0.1565
6	0.1127	0.0703	0.1001	0.0824	0.1081
7	0.0247	0.1201	-0.0200	0.0918	-0.0079
8	0.0720	0.0119	-0.0480	0.0706	0.0333

Table 7. 3. 2. 2

			$\hat{\beta}_1$		
Run No.	b1	c2	b3	c4	b5
1	0.0043	-0.0269	-0.0114	-0.0224	0.0015
2	0.0123	-0.0109	-0.0285	-0.0173	-0.0199
3	-0.0101	-0.0123	-0.0184	-0.0278	-0.0148
4	-0.0171	0.0191	0.0087	0.0000	-0.0249
5	-0.0094	0.0080	-0.0087	-0.0102	-0.0220
6	-0.0329	-0.0081	-0.0144	-0.0096	-0.0136
7	0.0138	-0.0250	0.0170	-0.0111	0.0066
8	-0.0086	0.0112	0.0228	-0.0063	-0.0005

Table 7. 3. 2. 3.

			r^2		
Run No.	b1	c2	b3	c4	b5
1	0.0440	0.3898	0.1320	0.1570	0.0060
2	0.0580	0.3970	0.6050	0.6530	0.7240
3	0.0920	0.0900	0.0050	0.3230	0.2570
4	0.6830	0.3280	0.0840	0.0000	0.3750
5	0.1300	0.1020	0.0990	0.1100	0.3400
6	0.4540	0.0530	0.7820	0.1280	0.3020
7	0.1180	0.2200	0.2970	0.5760	0.0530
8	0.1400	0.1760	0.4310	0.0840	0.0000

[157] Significant relationships are marked in red in these tables.

Tables 7. 3. 2. 4. to 7. 3. 2. 6. document the parameters $\hat{\beta}_0$, $\hat{\beta}_1$ and r^2 for the regression of section FHT on IHDS. Of the 40 regression relationships, 32 of the correlation coefficient values are not significant at the 95% confidence limit, suggesting an apparently random relationship between FHT and IHDS in these cases. Out of the eight significant relationships (marked in red), three have negative $\hat{\beta}_1$ parameter values indicating an inverse relationship (as IHDS increases, FHT decreases) and five have positive $\hat{\beta}_1$ parameter values, indicating a proportional relationship (as IHDS increases, FHT increases). The strength of the relationship between FHT and IHDS is not constant but varies; the maximum amount of variance in FHT explained by IHDS is 76.20% whilst the minimum amount of variance in FHT explained by IHDS is 0.00%.

Table 7. 3. 2. 4.- Regression of Final Height on Initial Height of Drainage Surface for transect 0.6850 at individual measurement grids; temperature, discharge, slope and Percentage Sediment Saturation constant.[158]

			$\hat{\beta}_0$		
Run No.	b1	c2	b3	c4	b5
1	0.2059	1.3806	0.0509	0.5258	-0.0250
2	0.0894	0.0585	0.3903	-0.3000	-0.1500
3	0.0207	0.5555	0.1505	0.3306	-0.6400
4	0.8749	0.1804	0.0440	-0.0120	-0.3200
5	-0.1000	0.1302	-0.0890	0.1608	-0.1600
6	-0.6000	-0.0740	-0.1800	0.3328	-0.3900
7	0.1974	-0.5700	0.3410	0.3978	-0.1400
8	-0.1800	-0.1600	0.1452	0.1576	-0.1800

Table 7. 3. 2. 5.

			$\hat{\beta}_1$		
Run No.	b1	c2	b3	c4	b5
1	-2.3836	-21.6553	-0.1876	-8.1156	4.4676
2	-0.5974	-0.1337	-5.5191	5.3871	2.9016
3	0.5052	-7.9380	-1.6799	-4.4583	10.4304
4	-11.8814	-1.9339	-0.0498	0.7730	5.4598
5	2.3370	-1.2265	1.9881	-1.9857	2.9096
6	9.6152	1.9031	3.5420	-4.5792	6.5296
7	-2.1246	9.6522	-4.6693	-5.5900	2.5832
8	3.6000	3.3035	-1.6772	-1.8318	3.4803

[158] Significant relationships are marked in red in these tables.

Table 7. 3. 2. 6.

			r^2		
Run No.	b1	c2	b3	c4	b5
1	0.0960	0.6490	0.0000	0.0840	0.1810
2	0.0010	0.0010	0.3270	0.3980	0.2130
3	0.0040	0.1320	0.0320	0.0330	0.4440
4	0.6060	0.0500	0.0000	0.0240	0.3940
5	0.0300	0.0360	0.0580	0.0460	0.1030
6	0.7620	0.0090	0.4410	0.3560	0.3430
7	0.0380	0.2950	0.2470	0.5820	0.1030
8	0.2330	0.1550	0.0260	0.0260	0.3270

Tables 7. 3. 2. 7. to 7. 3. 2. 9. document the parameters $\hat{\beta}_0$, $\hat{\beta}_1$ and r2 for the regression of the control variable IHDS on the control variable DFS. Of the 40 regression relationships, 19 values are not significant at the 95% confidence limit, suggesting an apparently random relationship between IHDS and DFS in these cases. Out of the 21 significant relationships (marked in red), 17 have negative $\hat{\beta}_1$ parameter values indicating an inverse relationship (as DFS increases, IHDS decreases) and four have positive $\hat{\beta}_1$ parameter values, indicating a proportional relationship (as DFS increases, IHDS increases). The strength of the relationship between IHDS and DFS is not constant but varies: the maximum amount of variance in IHDS explained by DFS is 96.10% whilst the minimum amount of variance in IHDS explained by DFS is 0.009%.

Table 7. 3. 2. 7.- Regression of Initial Height of Drainage Surface on Distance From Source for transect 0.6850 at individual measurement grids; temperature, discharge, slope and Percentage Sediment Saturation constant.[159]

			$\hat{\beta}_0$		
Run No.	b1	c2	b3	c4	b5
1	0.0689	0.0581	0.0702	0.0663	0.0666
2	0.0701	0.0672	0.0514	0.0710	0.0783
3	0.0736	0.0652	0.0574	0.0643	0.0705
4	0.0710	0.0734	0.0718	0.0819	0.0907
5	0.0700	0.0753	0.0646	0.0586	0.0817
6	0.0748	0.0670	0.0712	0.0711	0.0771
7	0.0748	0.0703	0.0590	0.0590	0.0633
8	0.0710	0.0544	0.0707	0.0605	0.0735

[159]Significant relationships are marked in red in these tables.

Table 7. 3. 2. 8.

			$\hat{\beta}_1$		
Run No.	b1	c2	b3	c4	b5
1	-0.0024	0.0013	-0.0023	-0.0010	-0.0007
2	-0.0017	-0.0014	0.0032	-0.0016	-0.0027
3	-0.0036	-0.0004	0.0022	0.0004	-0.0011
4	-0.0013	-0.0031	-0.0022	-0.0035	-0.0044
5	-0.0014	-0.0037	0.0002	-0.0014	-0.0029
6	-0.0040	-0.0011	-0.0018	-0.0014	-0.0022
7	-0.0028	-0.0023	0.0013	0.0011	0.0003
8	-0.0023	0.0030	-0.0019	0.0007	-0.0020

Table 7. 3. 2. 9.

			r^2		
Run No.	b1	c2	b3	c4	b5
1	0.8100	0.6990	0.5920	0.2450	0.1520
2	0.3310	0.1280	0.7270	0.4150	0.5150
3	0.8050	0.0370	0.5680	0.0350	0.3560
4	0.8720	0.6620	0.2690	0.2810	0.8810
5	0.5120	0.9140	0.0040	0.1690	0.4740
6	0.8290	0.3860	0.3310	0.1520	0.9610
7	0.5760	0.5660	0.1430	0.2910	0.0090
8	0.5620	0.8780	0.3070	0.1400	0.2450

Tables 7. 3. 2. 10. to 7. 3. 2. 13. document the parameters $\hat{\beta}_0$ $\hat{\beta}_1$ $\hat{\beta}_2$ and r^2 for the multiple regression of section FHT on DFS and IHDS. Out of the 40 regression relationships, 23 are not significant at the 95% confidence limit, suggesting an apparently random relationship between FHT and DFS/IHDS. Of the 17 significant values (marked in red), 10 had negative $\hat{\beta}_1$ parameter values indicating an inverse relationship between FHT and DFS and seven had positive $\hat{\beta}_1$ parameter values indicating a proportional relationship between FHT and DFS.

Out of the 17 significant values (marked in red) eight had negative $\hat{\beta}_2$ parameter values indicating an inverse relationship between FHT and IHDS and nine had positive $\hat{\beta}_2$ parameter values indicating a proportional relationship between FHT and IHDS. The strength of the multiple regression relationship between FHT and DFS/IHDS is not constant but varies; the maximum amount of variance in FHT explained by DFS/IHDS is 87.50% whilst the minimum amount of variance in FHT explained by DFS/IHDS is 0.034%.

Table 7. 3. 2. 10.-Multiple Regression of Final Height on Distance From Source and Initial Height of Drainage Surface for transect 0.6850 at individual measurement grids; temperature, discharge, slope and Percentage Sediment Saturation constant.[160]

			$\hat{\beta}_0$		
Run No.	b1	c2	b3	c4	b5
1	0.3810	1.5620	-0.4830	1.3248	-0.3546
2	-0.1656	0.1698	-0.0187	0.0052	0.2992
3	0.7477	0.7015	0.3209	0.2901	-0.5083
4	0.0861	-0.4490	-0.0894	-0.0503	-0.1727
5	0.2361	-0.6130	-0.0660	0.1311	0.2707
6	-1.3510	0.1757	0.0116	0.5560	-0.9210
7	-0.1023	-0.4280	0.4350	0.3075	-0.1614
8	-0.1725	0.0090	-0.2610	0.1123	-0.3000

Table 7. 3. 2. 11.

			$\hat{\beta}_1$		
Run No.	b1	c2	b3	c4	b5
1	-0.0073	0.0061	0.0269	-0.0403	0.0058
2	0.0169	-0.0127	-0.0389	-0.0147	-0.0251
3	-0.0426	-0.0157	0.0127	-0.0271	-0.0050
4	0.0161	0.0384	0.0118	0.0038	-0.0080
5	-0.0127	0.0400	-0.0091	-0.0090	-0.0260
6	0.0344	-0.0098	-0.0122	-0.0187	0.0154
7	0.0186	-0.0074	0.0266	-0.0072	0.0058
8	-0.0006	0.0111	0.0284	-0.0058	0.0087

Table 7. 3. 2. 12.

			$\hat{\beta}_2$		
Run No.	b1	c2	b3	c4	b5
1	-4.8900	-24.8700	6.8300	-17.9860	5.6420
2	2.7580	-1.3380	3.2100	1.6130	-1.9310
3	-9.1780	-9.5400	-5.0070	-1.8980	8.8370
4	-0.8270	6.1500	1.3600	1.0800	3.8470
5	-2.0309	8.5100	2.1600	-0.8710	-1.3980
6	16.6830	-1.5740	1.2416	-6.6610	13.3400
7	1.6980	7.7940	-7.7100	-3.6530	2.4260
8	3.4430	0.0540	3.0100	-0.6900	4.5350

[160] Significant relationships are marked in red in these tables.

Table 7. 3. 2. 13.

			r^2		
Run No.	b1	c2	b3	c4	b5
1	0.1210	0.6550	0.3000	0.4690	0.2510
2	0.0740	0.4710	0.6360	0.6740	0.7700
3	0.3220	0.2730	0.1260	0.3290	0.4630
4	0.6840	0.5010	0.1120	0.0340	0.3980
5	0.1450	0.2530	0.1670	0.1170	0.3530
6	0.8470	0.0570	0.8180	0.7660	0.3580
7	0.1280	0.3040	0.8750	0.7520	0.1440
8	0.2330	0.1760	0.4900	0.0870	0.4190

What conclusions can be inferred about the controls on river morphology from these results? Before any conclusions are made, a caveat to the interpretation of these results must be stated. The results in tables 7. 3. 2. 1. to 7. 3. 2. 13. are derived without any reference to the controls of PSS and the temperature of the water. These two factors (PSS and temperature) may interfere with any relationship that is present between FHT and DFS/IHDS. PSS and temperature cannot be implicitly included in a causal analysis at the scale of sectional analysis because they are singular values or constants. A method of incorporating their control on FHT that is independent of DFS is discussed in the next section.

With the above limitation in mind, several things can be shown from these sectional regressions. Firstly, the control of DFS is limited as a linear predictor of FHT over short distances. This was expected, because the sediment transport rate in the flume probably approaches a constant value with a reduction in the level of analysis. Nonetheless, this indicates that the best linear predictor available (despite the fact that it obscured the effect of different controls) for the flume as a whole, does not function at smaller levels of analysis. Out of 40 samples, only in 10 or 25% of cases was FHT significantly related to DFS with a consistent parameter $\hat{\beta}_1$ (i. e. negative). Of these 10 significant values, only five or 12.50% of the samples, explained more than 50% of the variability in FHT. It does not seem possible to predict section FHT given DFS.

Having considered the expected result that the control of DFS would not exert a strong control within a region of the flume, attention is turned to the results given for the control of IHDS. Without the logical constraint exerted by DFS when the run was considered as a whole, the true affect of IHDS should be shown by considering its influence on areas of similar sediment transport rate. The results given above suggest that even in the context of similar regions, IHDS is a poor linear predictor of FHT. Out of 40 samples, only in five or 12.50% of cases was FHT significantly related to IHDS with a consistent parameter $\hat{\beta}_1$ (i.e. positive). Of these five significant values, only one or 2.50% of the samples explained more than 50% of the variability in FHT.

Multiple regression of FHT on DFS/IHDS does not improve predictive capabilities. Out of the 40 samples, only in 17 or 42.50% of cases was FHT significantly related to DFS/IHDS; the consistent values (negative $\hat{\beta}_1$ parameter) for DFS remained at 10, and the consistent values for IHDS increased to nine (positive $\hat{\beta}_1$ parameter). At first sight this seems to be improvement on individual linear regression, with an increase in significant values and an increase in consistent values for IHDS. However, if the consistent values in DFS are matched with the consistent values from IHDS, only five sections have matching pairs, i.e. a negative $\hat{\beta}_1$ value in DFS and a positive $\hat{\beta}_1$ value in IHDS.

This matching is essential for a predictive relationship. An investigator needs to decide what kind of linear relationship, whether proportional or inverse, exists between a control and the effect. One way of deciding this in the presence of ambiguous information, is to assume that the most frequently occurring form of relationship is the correct one. Once this choice has been made, those relationships which occur that indicate a different kind of linear relationship to the chosen form, even if statistically significant, must be discounted as error.

The poor performance of the control IHDS in explaining FHT, as suggested in the caveat mentioned above, may be due to the interfering effects of PSS and water temperature. The influence of these variables is investigated below.

7. 3. 3. The Influence of PSS and Temperature on Linear Explanation of the FHT of Sections Within a Run.

The influence of PSS was obscured in the analysis of runs when treated as a whole, because DFS was the dominant control. In the sectional analysis above, PSS was a constant, because one value covered the 11 values of final data. One way of assessing the influence of PSS without reference to Distance From Source, is to use the parameters of the sectional regression analysis between FHT and IHDS and regress them on the values of PSS for each section. This test examines whether differences in the strength or direction of the relationship between Final Height and Initial Height of the Drainage Surface are affected linearly by Percentage Sediment Saturation.

Table 7. 3. 3. 1. lists the parameters for the linear regression of sectional r^2 values (for the relationship between FHT and IHDS) against sectional PSS for each run (i.e. not between runs). Temperature within a run is constant and is therefore not included in the regression equation. The relationship given by the regression equation is ambiguous: half of the samples have negative $\hat{\beta}_1$ values indicating an inverse relationship; that is, as PSS increases the strength of the relationship between FHT and IHDS decreases. The remaining half of the samples have positive $\hat{\beta}_1$ values indicating a proportional relationship; that is; as PSS increases the strength of the relationship between FHT and IHDS increases. This ambiguous nature of the relationship is made somewhat irrelevant by the fact that none of the regressions between sectional r^2 values and PSS are significant at the 95 % confidence interval, suggesting that there is an apparently random relationship between the strength of the relationship between FHT/IHDS and PSS. That is, knowing the values of PSS does not explain why the regression equation for FHT and IHDS is so weak.

Table 7. 3. 3. 1.- Regression of Sectional Regression r^2 values for each run against Sectional Percentage Sediment Saturation Values for each run

Run No.	$\hat{\beta}_0$	$\hat{\beta}_1$	r^2
1	-7.7011	0.4141	0.377
2	-0.2500	0.0228	0.008
3	2.7293	-0.1373	0.154
4	-3.1600	0.1799	0.109
5	2.0917	-0.1061	0.271
6	0.5720	-0.0099	0.000
7	0.3847	-0.0071	0.000
8	-0.2500	0.0239	0.015

There is no suggestion that as PSS increases, the predictive relationship between FHT and IHDS will improve. Conversely, there is no suggestion that as PSS decreases, the predictive relationship between FHT and IHDS will improve.

Table 7. 3. 3. 2. lists the parameters for the linear regression of sectional $\hat{\beta}_1$ values (for the relationship between FHT and IHDS) against sectional PSS for each run. The direction of this relationship is constant, with the exception of run three, with all of the $\hat{\beta}_1$ values positive for this regression, indicating a proportional relationship; that is as PSS increases, the nature of the relationship between FHT and IHDS will become increasingly proportional. This suggests that if the PSS was exactly constant, then the direction of the relationship between FHT and height of initial drainage surface would be constant also. This finding is negated by the fact that none of the regressions, (except run two) between sectional $\hat{\beta}_1$ values and PSS are significant at the 95 % confidence interval, suggesting that an apparently random relationship exists between PSS and the direction of the relationship between FHT and IHDS.

Table 7. 3. 3. 2.- Regression of Sectional Regression $\hat{\beta}_1$ values for each run against Sectional Percentage Sediment Saturation Values for each run.

Run No.	$\hat{\beta}_0$	$\hat{\beta}_1$	r^2
1	-9.9072	0.0762	0.000
2	-110.5780	5.7847	0.962
3	180.5620	-9.5699	0.519
4	-147.8790	7.7139	0.320
5	-153.4710	8.0745	0.322
6	-217.9600	11.6340	0.561
7	-149.1770	8.2893	0.691
8	-3.9043	0.3147	0.006

From these results, the following conclusion can be drawn; there is no linear relationship between the value of PSS and the strength or direction of the relationship between FHT and IHDS. Any weaknesses in the relationship between FHT and IHDS are not due to variations in PSS.

These findings can be tested further by examining the relationship between PSS and the r^2, $\hat{\beta}_1$, values for FHT/IHDS for different runs; that is, keeping the section (for example b1) the same, the eight regression parameters for this section (for example r^2 for b1 in runs 1, 2, 3 ...) are regressed on the eight PSS values for this section (for example PSS for b1 in runs 1, 2, 3 ...). The results of this test are important for assessing the influence of temperature, discussed later.

The results in table 7. 3. 3. 3. show the relationship between sectional PSS and the strength of the relationship between sectional FHT and sectional IHDS. The r^2 values for this regression indicate an apparently random relationship. The results in table 7. 3. 3. 4. show the relationship between sectional PSS and the direction of the relationship between sectional FHT and sectional height of initial drainage surface. The r^2 values for this regression also indicate an apparently random relationship.

Table 7. 3. 3. 3.- Regression of Sectional Regression r^2 values against Sectional Percentage Sediment Saturation Values over eight runs.

Section	$\hat{\beta}_0$	$\hat{\beta}_1$	r^2
b1	-0.1800	0.0226	0.007
c2	-0.7600	0.0511	0.036
b3	-0.9400	0.0612	0.120
c4	0.3900	-0.0106	0.001
b5	0.2560	0.0003	0.000

Table 7. 3. 3. 4.- Regression of Sectional Regression $\hat{\beta}_1$ values against Sectional Percentage Sediment Saturation Values over eight runs

Section	$\hat{\beta}_0$	$\hat{\beta}_1$	r^2
b1	2.2649	-0.1111	0.000
c2	99.2233	-5.3511	0.190
b3	-18.1651	0.9466	0.111
c4	-42.9544	2.1705	0.172
b5	2.3442	0.1327	0.002

Given the results of the above, which suggest that PSS does not seem to alter the strength or direction of the predictive relationship between sectional FHT and IHDS over different runs, the influence of temperature on this predictive relationship can be assessed.

The r^2 and $\hat{\beta}_1$ values for the relationship between FHT and IHDS, for each of the sections (b1, c2, b3, c4, b5) were listed over eight runs and were regressed on the temperature values for the eight runs. Because the eight temperature values are the same (each section had water of the same temperature flowing over it in any given run), this procedure amounts to fitting five different sets of response variables to a constant predictor series. Tables 7. 3. 3. 5. and 7. 3. 3. 6. show that temperature does not alter the strength or direction of the relationship between FHT and IHDS.

Table 7. 3. 3. 5.- Regression of Sectional Regression r^2 values against Temperature Values over eight runs

Section	$\hat{\beta}_0$	$\hat{\beta}_1$	r^2
b1	-0.9600	0.0889	0.010
c2	0.4136	-0.0153	0.000
b3	-9.3588	0.7044	0.858
c4	1.8881	-0.1250	0.040
b5	1.3065	-0.0770	0.042

Table 7. 3. 3. 6.- Regression of Sectional Regression $\hat{\beta}_1$ values against Temperature Values over eight runs

Section	$\hat{\beta}_0$	$\hat{\beta}_1$	r^2
b1	-85.0179	6.3063	0.122
c2	236.8020	-17.7244	0.323
b3	-10.0292	0.6621	0.003
c4	-26.9397	1.7999	0.023
b5	8.6006	-0.2771	0.001

The r^2 values for these regressions indicate an apparently random relationship, and suggest that a knowledge of the temperature of the water does not help in explaining the strength or weakness of the predictive relationship between FHT and IHDS. Section b3 has a relatively high r^2 value, but even this is not significant at the 95% confidence limit.

7. 3. 4. Residual Analysis, Transformations and Dependence.

The analysis above has suggested that a linear causal model is quite poor in explaining the FHT data. However, the technique of linear regression depends on three key requirements being fulfilled; the data should be 1) Normally distributed 2) Independently distributed and 3) Exhibit constant variance. Statistical theory had developed extensive critical techniques that evaluate these assumptions and allow for appropriate remedial action (Cook and Weisberg; 1982: Weisberg; 1985: Ryan; 1997). Before a conclusion is drawn as to the failure of linear regression in predicting the FHT data, these critical tests must be performed to examine whether the model is appropriate for the data.

Perhaps the most useful statistical technique for evaluating the assumptions of the regression model, is the residuals versus fits plot. This graphical description of the regression model graphs the fitted values of FHT ($\hat{y}$), against the residuals or error terms of the equation, which describe the discrepancy between the predicted value and the actual value of y - that is: ($\hat{e} = y - \hat{y}$).

Residuals versus fits plots provide information on two important features of the data. The first is the test for non-constant variance in the data. The regression model assumes that the variance of the response variable is constant for all values of the independent variable. In other words, the values the response variable can take, are dependent solely on their relationship to the independent variable and not to some other factor that interferes with the response variable. For example, an experimenter might measure the height a rocket attains depending on the fuel supplied to it. At low fuel levels, the variance in the height obtained may be low; at higher fuel levels this variance may increase, due to unaccounted effects in the performance of the engine at higher altitudes. In this case, an exclusive relationship between fuel and height is being affected by other factors and the effects of these factors need to be removed if the regression model is to be deemed appropriate.

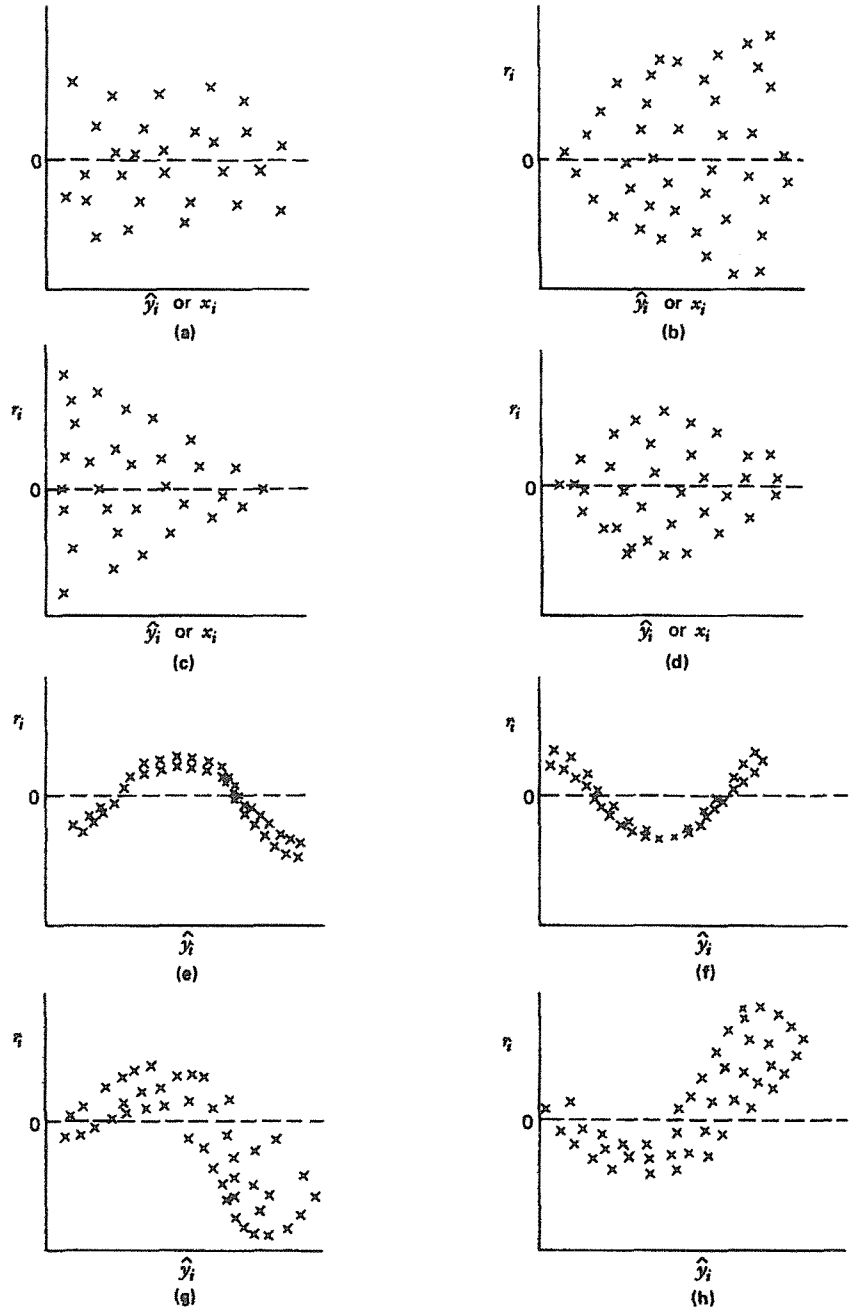
Secondly, residuals versus fits plots can show whether non-linear relationships need to be included to improve the model. The values of y may depend on some non-linear function of x such as an exponential equation of the form $y = a.x^b$. Alternatively, the value of any given y may depend on a linear relationship to x plus a non-linear function of proceeding y values - i.e. $y_i = a.\ x_i.\ y_i^{0.5}$. Residual versus fits plots should identify either type of non-linear relationship, if present.

Weisberg (p. 24; 1985) summarises the usefulness of the residuals versus fits plot and provides examples of plots which indicate non constant variance and/or non-linearity (Figure 7. 3. 4. 1.):

"Systematic features in this plot are of interest. Curvature might indicate that the fitted model is inappropriate and suggest a transformation of the data. Residuals that seem to increase or decrease in magnitude with [fitted value] might indicate non-constant residual variance... if the plot of [residuals] versus [fits] shows no systematic features then we would have little reason to suspect that the fitted model was inappropriate for the data."

Weisberg (p. 24; 1985)

Figure 7. 3. 4. 1- Examples of Nonlinearity and Non-Constant Variance in Residual-Fits Plots



Residual plots: (*a*) null plot; (*b*) right-opening megaphone; (*c*) left-opening megaphone; (*d*) double outward bow; (*e*) nonlinearity; (*f*) nonlinearity; (*g*) nonlinearity and nonconstant variance; (*h*) nonlinearity and nonconstant variance.

From Weisberg (p. 132; 1985)

The residuals versus fits plots for the multiple regression of FHT on DFS, IHDS, PSS, and temperature of water, were examined for non-constant variance for the whole of a run. An analysis of the pattern of variance in the residual versus fits plots of figures 7. 3. 4. 2. - 7. 3. 4. 9., shows that runs three, four, five and eight can be viewed as having a random pattern of variance, or constant variance over all fitted values of the control variables. Run one shows limited evidence that it may have a left opening megaphone pattern of variance, or that the variance increases as the fitted value of FHT decreases. Runs two, six and seven show limited evidence that they may have right opening megaphone patterns of variance, or that the variance increases as the fitted value of FHT increases. At this stage of the analysis, the assumption of constant variance only holds for runs three, four, five and eight and therefore the regression model only applies for these runs.

The residuals versus fits plots for the multiple regression of FHT on DFS, IHDS, PSS, and temperature of water, were examined for non-linearity, for the whole of a run. None of the residuals versus fits plots in figures 7. 3. 4. 2. - 7. 3. 4. 9. suggest any obvious non-linearity that may be present either in the effect or in the control variables. The conclusion to be made from the data, is that the use of a non-linear regression model would not improve predictive capabilities.

Another useful tool in the critical assessment of the regression model assumptions is the probability plot of residuals. The probability plot (in conjunction with histograms of residuals) indicates whether the distribution of the residual values is normal or not. The normality of residuals is an important assumption in regression, because hypothesis testing and confidence intervals depend on a normal distribution (Montgomery and Peck; 1992). Testing to see whether estimated values $b_o\ b_1 \ldots b_n$ of the parameters $\beta_o\ \beta_1 \ldots \beta_n$ are significant depends on a normal distribution of errors (Freund, p. 416; 1982). This is particularly significant for empirical testing, where the true value of β is unknown and comparisons can be made between $b_o\ b_1$ to examine whether a statistically consistent relationship has been derived. Similarly, for predictive uses of the derived regression relationship, (i.e. to predict unobserved values of y based on parameter values and a known value of x) the normality assumption provides confidence intervals for the derived values; i.e. what values may be deemed to correspond to the regression relationship and the distribution either side of the fitted line, and which lie outside it (Weisberg, p. 21; 1985).

The probability plots and histograms of residuals in figures 7. 3. 4. 2. - 7. 3. 4. 9. show a similar pattern to those of the residuals versus fits plots. For runs four, five and eight, the probability plots indicate a normal distribution (straight line) with the majority of the values centred near zero and with a few extreme values at the top and bottom of the distribution. Run one and run six show distributions that have high frequencies of extreme negative residuals, with run one including several very negative values. Run two and run seven show distributions that have large negative and positive tails. Run two displays very negative and positive values, whilst run seven has some extreme positive values. Thus far, the runs display patterns similar to those seen in the test for non-constant variance, where runs four, five and eight display normality, whilst runs one, two, six and seven are the same and show non-normal behaviour. Run three however, which showed predominantly random variance along with runs four, five and eight, does not display normal behaviour. It has a tail of negative residual values and several very positive residual values that make it non-normal. At this stage of the analysis, the assumption of constant variance and normality for the residuals only holds for runs four, five and eight and therefore the regression model only applies for these runs.

Figure 7. 3. 4. 2.

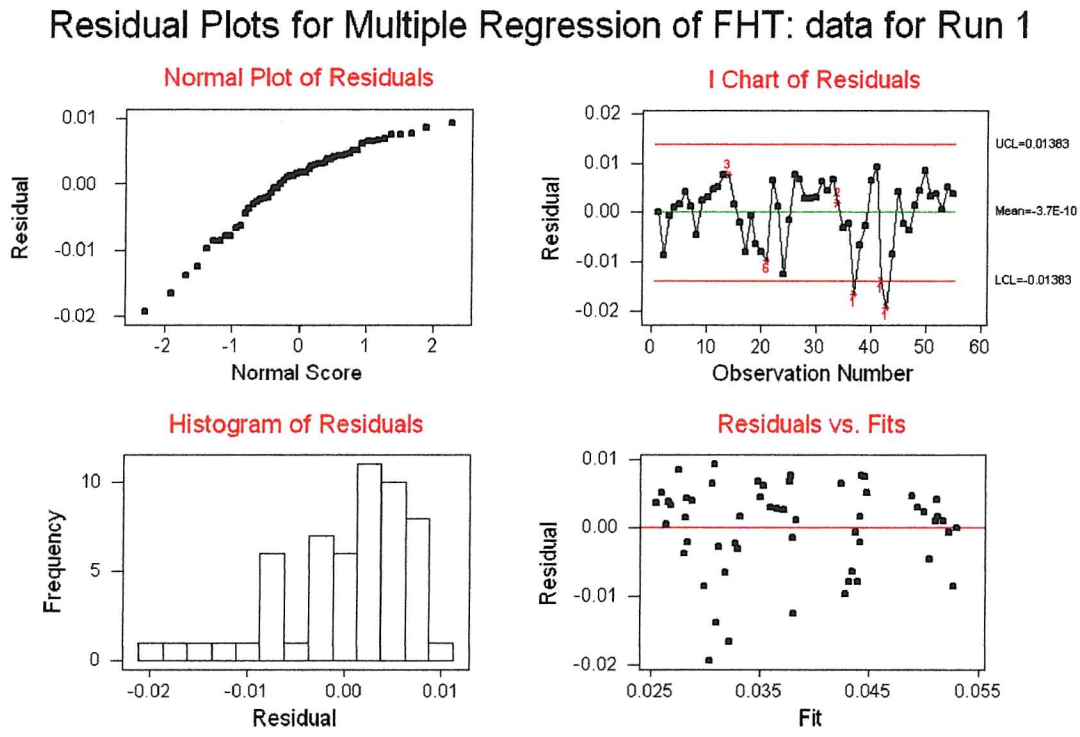


Figure 7. 3. 4. 3.

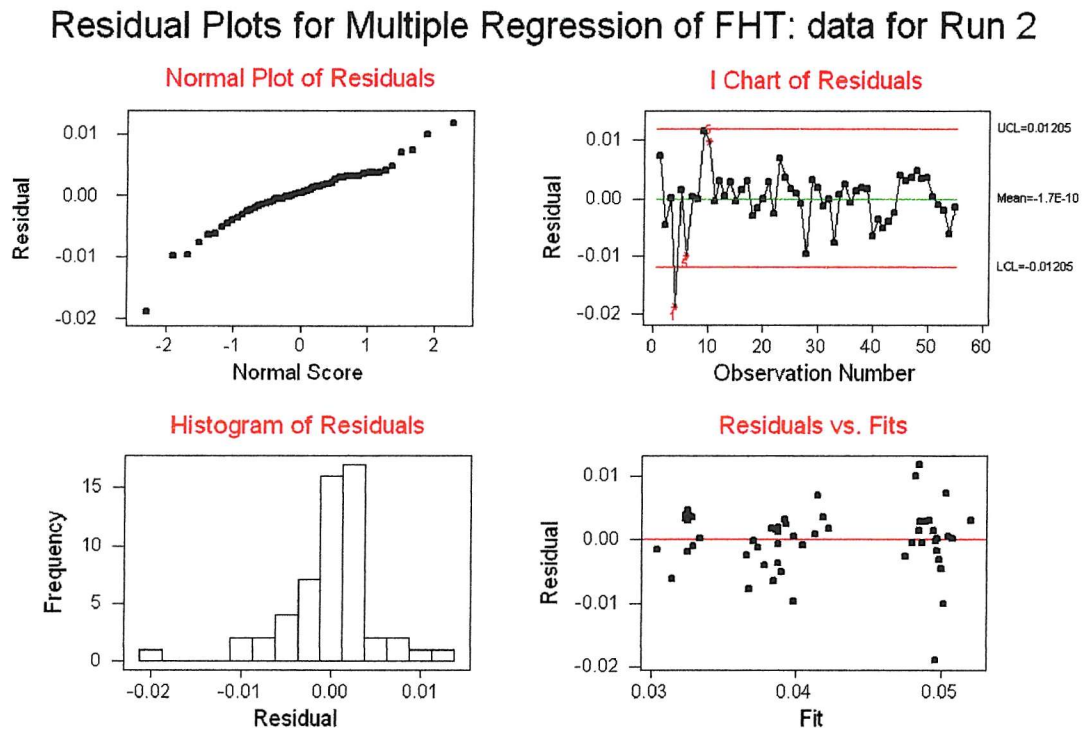


Figure 7. 3. 4. 4.

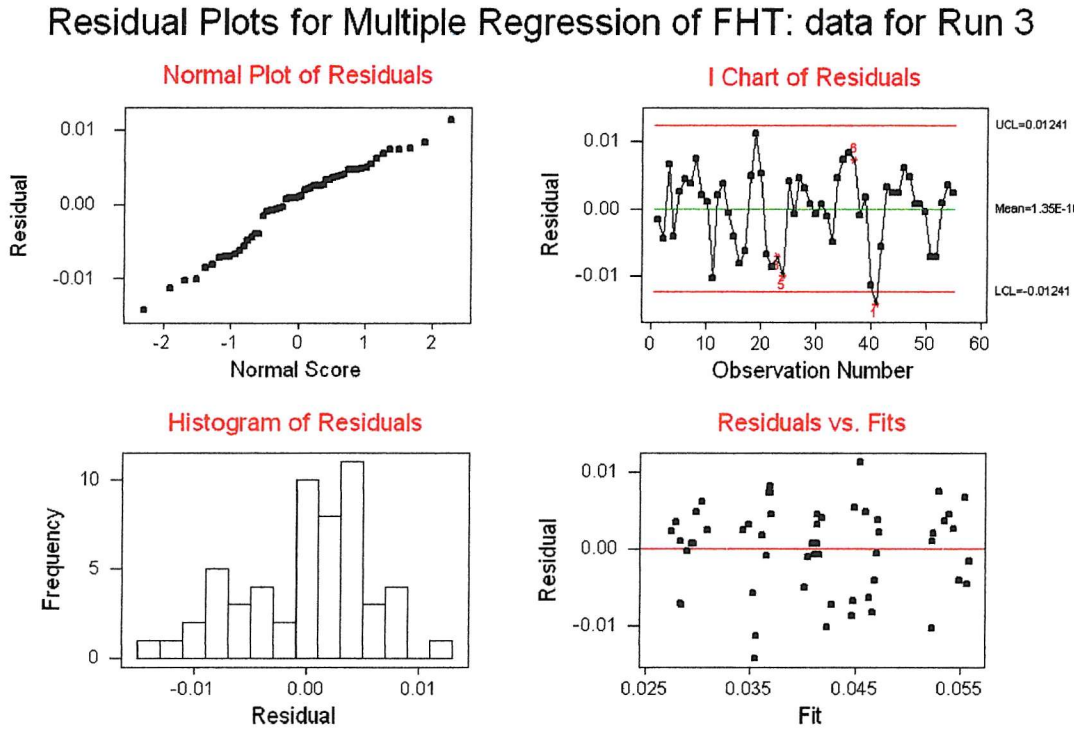


Figure 7. 3. 4. 5.

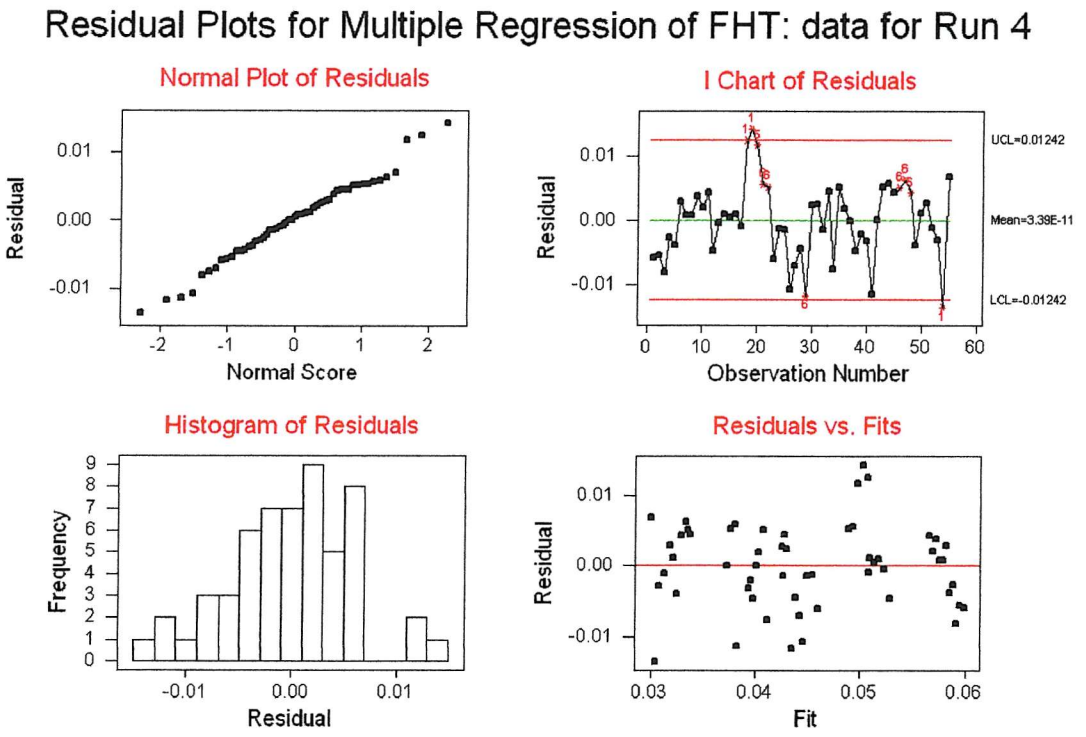


Figure 7. 3. 4. 6.

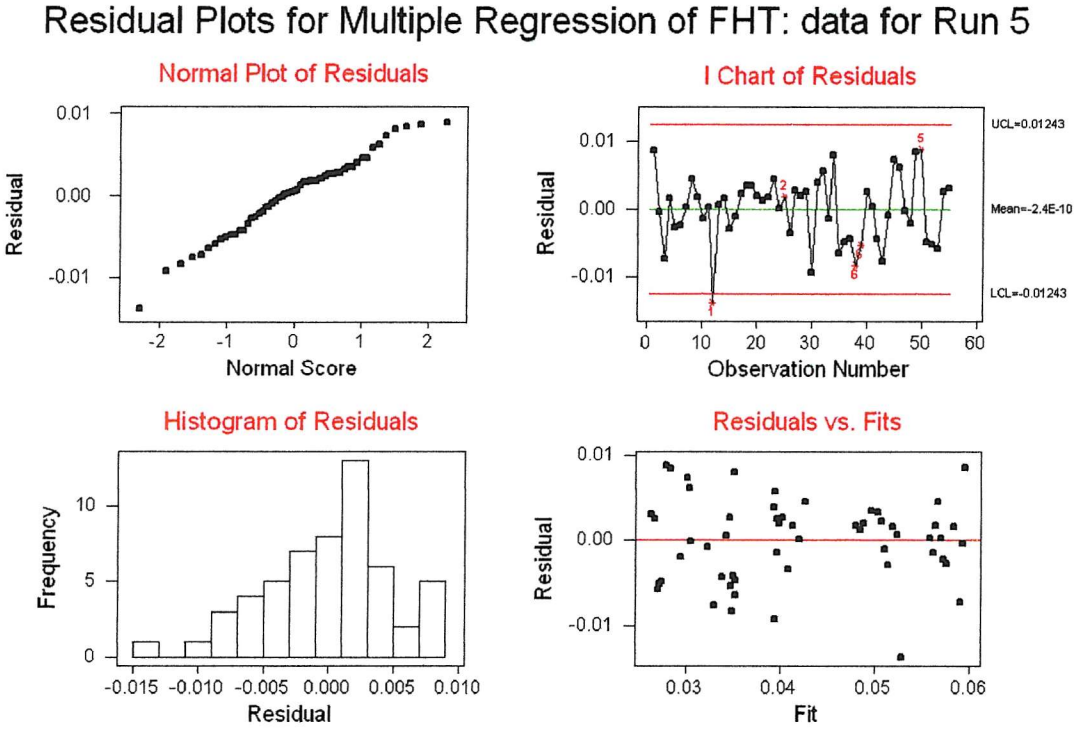


Figure 7. 3. 4. 7.

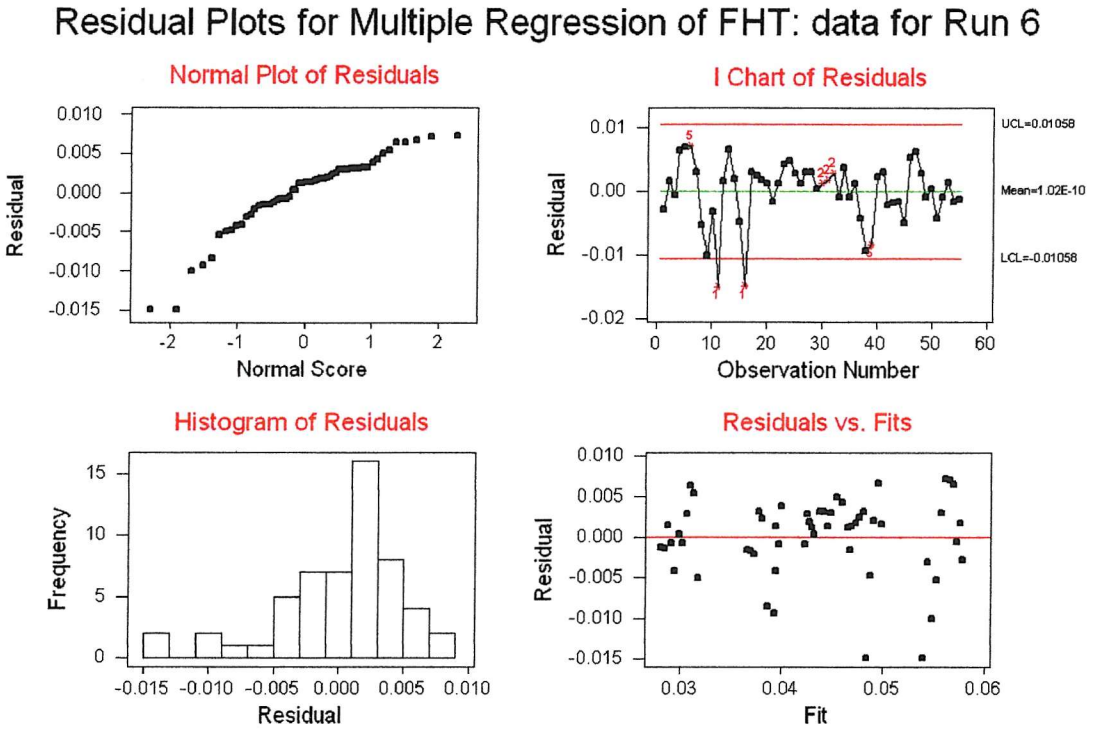


Figure 7. 3. 4. 8.

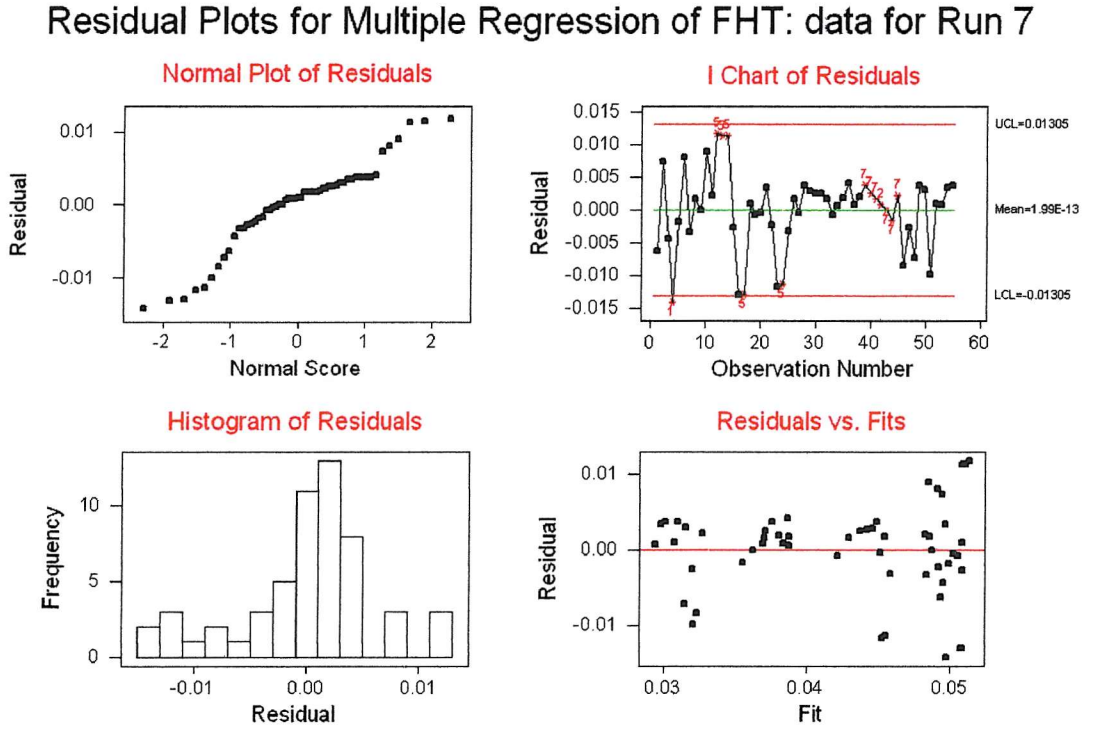
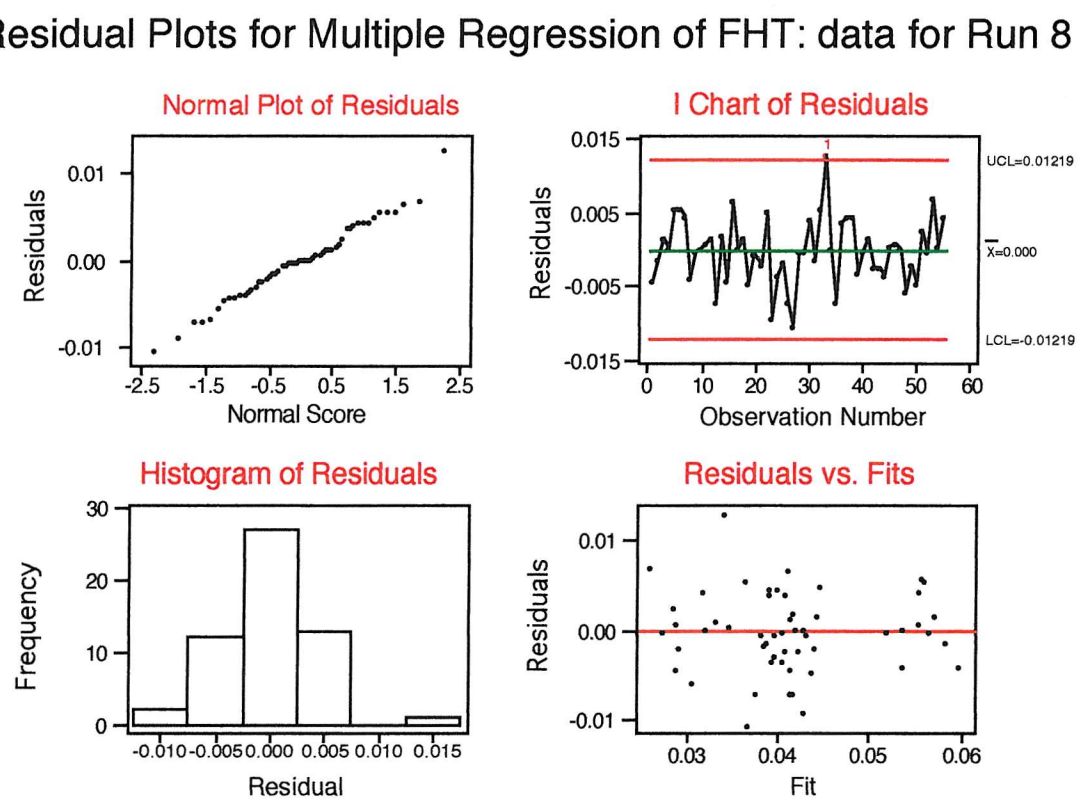


Figure 7. 3. 4. 9.



Non-constant variance may make the distributions of residuals non-normal. If this is the case, a transformation of the data that makes the variance constant should produce distributions more closely approximating the normal. Weisberg (p. 134; 1985) produced table 7. 3. 4. 1. outlining the major transformations available for data. Cook and Weisberg (p. 59; 1982) noted that in the case where the precise non-normal distribution of the data is unknown, and a non-linear function that can be transformed to yield linearised data is also not available, ... a specific single rationale for choosing a transformation is lacking... Accordingly, each transformation was applied to the FHT data in an attempt to stabilise the variance of residuals and produce distributions that were normal. None of the transformations listed succeeded in altering the pattern of variance or changing the distributions to normal in runs one, two, three six and seven.

Table 7. 3. 4. 1. - Functions for Transformations of the Dependent Variable.

Transformation
$\sqrt{Y}$
$\sqrt{Y}+\sqrt{Y+1}$
$\log Y$
$\log Y+1$
$1/Y$
$1/(Y+1)$

From Weisberg (p. 135; 1985)

The failure of the transformations to effect any change in the variance pattern or the distribution structure, suggests that the large values of residuals are not due to some obvious process that is consistently changing the values of FHT in a way that can be modelled from the data. The alternative to a consistent process altering the values of FHT, is that the large residuals that suggest non-constant variance and which lead to a non-normal distribution are outliers (see above observations on residuals). Residual outliers are data points that are inconsistent with the regression relationship and may be due to error in measurements, recording, or because exceptionally large or small values of the independent variables, for which the regression relationship does not extrapolate, have been used (see Ryan, p. 351; 1997).

The claim that outliers in the data are due to *error in recording* (i.e. the data points are assumed to be correct but have been entered wrongly into the computer for analysis) is easily evaluated and it has been verified that the data points are correct. Similarly the idea that the range of the independent variables is too big, does not fit the data, where the control variables have been reasonably well controlled and differ only over small values.[161] Finally, a judgement can be made as to whether the outlier points correspond to errors in measurement. Errors in measurement can be defined as the difference between the true value of the sediment surface and the recorded measured value; i.e. how accurate are the measurements or to what degree do they approach the true value. Without the setting up of a complex evaluation procedure, it is difficult to quantify the typical spread of errors in measurements made on saturated sand. The medium of sand is such that measuring topography using a point gauge actually changes the value of the measurement being made by indenting the surface (an example of the Heisenberg uncertainty principle).

[161] A related point concerning regression is that the values of the independent variables used, are too close together and do not allow the relationship between the independent variables and the dependent variable to be expressed. This claim is valid where the values of the dependent variable are distributed about the mean or the intercept and show no variation with control variables, but only variation due to pure chance. This is not the case with the FHT data, where the data is not uniform but shows significant departures from the mean value, that require an explanation.

A possible technique for assessing error experimentally, would have been to glue or freeze the bed surface after measurements and measure the discrepancy between the point gauge indent and the surrounding topography. Strict time constraints did not permit this technique however.

An attempt was made to limit the error in measurement by making measurements to the highest level of precision possible; that is to $1/10$ of a millimetre or $1/10000$ of a metre. A reasonable estimate of the error in measurement seems to be +/- 0.5 mm or +/- 0.0005m. A generous estimate of error based on this level of precision may be +/- 1.0 mm or +/- 0.001m. It can be seen that even working with the generous estimate +/- 1 mm error, it is improbable that the outlier values are due to errors in measurements, when the measurements were taken to this precision.

The outlier residuals (which are not the raw data but which are departures from the fitted line) in the residual versus fits and probability plots are of an order of magnitude greater than the generous estimate of error of 1.0 mm. For example in run one, three residuals have the respective values of -19.0 mm, -16.6 mm and 9.2 mm; that is they differ from the predicted value by the given amounts. Similarly, run three which has a fairly random pattern of variance before transformation, has residual values of - 14.2 mm, -11.3 mm and 11.3 mm. The data seems to suggest that it is not probable if measurements are being made to an accuracy of a tenth of a millimetre, that errors in measurement as great as 19 mm are being committed. In short, the variance in the data is much greater than even the generous margin of error(+/- 1 mm), and a good degree of confidence can be placed in the view that the outliers are not due to errors in measurement.

When the alternatives of error in measurements or recording, or because exceptionally large or small values of the independent variables over which the regression relationship does not extrapolate have been explored as explanations for the outliers, a plausible conclusion is that they are a part of the natural behaviour of the phenomenon that is under investigation. Dealing with outliers that are a part of the data's behaviour is difficult. The offending points can be removed to give a constant variance and a normal distribution, so that confidence limits and hypothesis testing can be applied to the parameters. However, if the outliers are an intrinsic part of the response, this procedure is equivalent to removing the features of the data that the regression relationship is meant to explain. In the case of the flume experiments, those large residuals often correspond to the most interesting features of the topography, such as deep erosional pits. Montgomery and Peck (p. 81; 1992) provide the following statement on outliers:

"Sometimes we find that the outlier is an unusual but perfectly plausible observation. Deleting these points to improve the fit of the equation can be dangerous, as it can give the user a false sense of precision in estimation or prediction. Occasionally we find that the outlier is more important than the rest of the data because it may control many key model properties .. Outliers may also point out inadequacies in the model..."

Montgomery and Peck (p. 81; 1992)

Given that outliers seem an intrinsic part of the response of FHT, it does not seem desirable to remove the data that causes them from the regression relationship. However, their presence means that variance is not constant for runs one, two, six and seven and non-normal distributions result for runs one, two, three, six and seven. This result means that the regression model is inappropriate for these runs, because the assumptions of normal distributions with constant variance have not been met. The regression parameters cannot be evaluated by the use of hypothesis testing nor can confidence intervals be derived for prediction or estimation purposes.

The above situation, where the regression model satisfies the assumptions for three runs, may be improved if the residuals are tested for dependence. If dependence is detected, this can be modelled and incorporated into the regression model. Weisberg (p. 159; 1985) gives the following comment on dependence in data:

"Another distributional assumption concerning the errors is that they are uncorrelated. This would mean that the value of the error for one case does not depend on the value of the error for any other case . This assumption will be violated in some problems, especially if the cases are ordered in time or in space... In general, diagnosing correlated errors is very difficult, the best diagnostics coming form careful consideration of the process that generates the data."

Weisberg (p. 159; 1985)

In spatial processes, a reasonable way of modelling correlation between error terms is to suggest that the previous residual alters the value of the current residual. The proposition that morphology in time and space affects subsequent values has already been touched upon in Section 5. 3. 1. 2. It is expected therefore, that the values of FHT in the fundamental data set at y_i will affect the value at y_j in some manner. A plot that tests the residuals for dependence takes the form; residuals (y axis) versus residuals-1 (x axis). This models the influence that the residual at r_i has on the residual at r_j.

Figures 7. 3. 4. 10. - 7. 3. 4. 14. show regression plots of residuals versus residuals-1 for the runs one, two, three, six and seven which had non-constant variance and/or non-normal distributions. In figure 7. 3. 4. 10. (run one) the graph does show some trend; that negative residual values are preceded by negative residual values (residual-1) and positive residual values are preceded by positive residual values (the r^2 value for this plot is 0.205). However, large negative outliers remain, which do not appear to be correlated to any previous value. Run two (figure 7. 3. 4. 11.) shows no trend in the relationship (the r^2 value for this plot is 0.0208). Large positive and negative outliers that are not correlated to any value persist in the plot. Run three (figure 7. 3. 4. 12) shows a significant trend (r^2 value is 0.204) in the relationship and there are no large outliers from the trend line. This suggests that the data of run three does include spatially correlated data points and the effect of this dependence should be modelled in order for the regression model to apply. Figure 7. 3. 4. 13. indicates that run six has a degree of spatial correlation between error terms (r^2 value is 0.162), but as with runs one and two, large negative outliers from the trend exist, which are uncorrelated to previous error terms.

Finally figure 7. 3. 4. 14. shows a complex relationship for run seven. There is a slight spatial dependency in the data (r^2 value is 0.124) but the data points are scattered widely, but uniformly about the trend line. As such, there are no real outliers, but the whole data set is more dispersed than for that in run three. The inclusion of spatial modelling may slightly improve the normality and the variance of the regression relationship between FHT and the control variables for run seven.

The plots of residual versus residual-1 for the runs four, five and eight which had normal distributions and constant variance, showed the expected result that there was no structure to the variance that was described by spatial dependence.

Figure 7. 3. 4. 10.

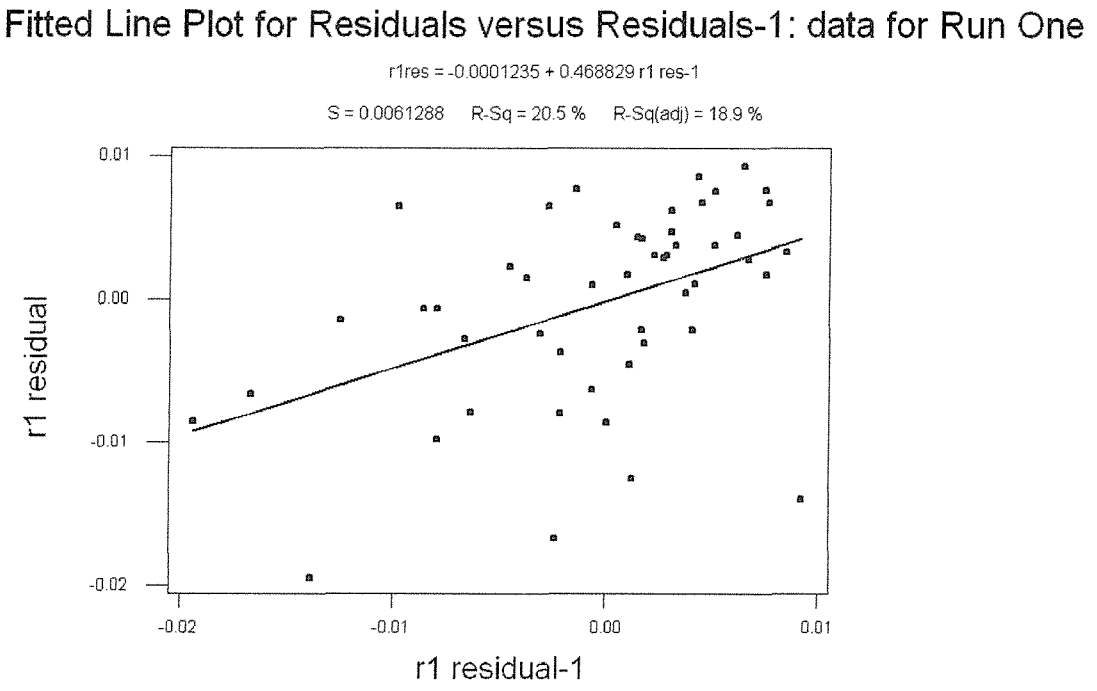


Figure 7. 3. 4. 11.

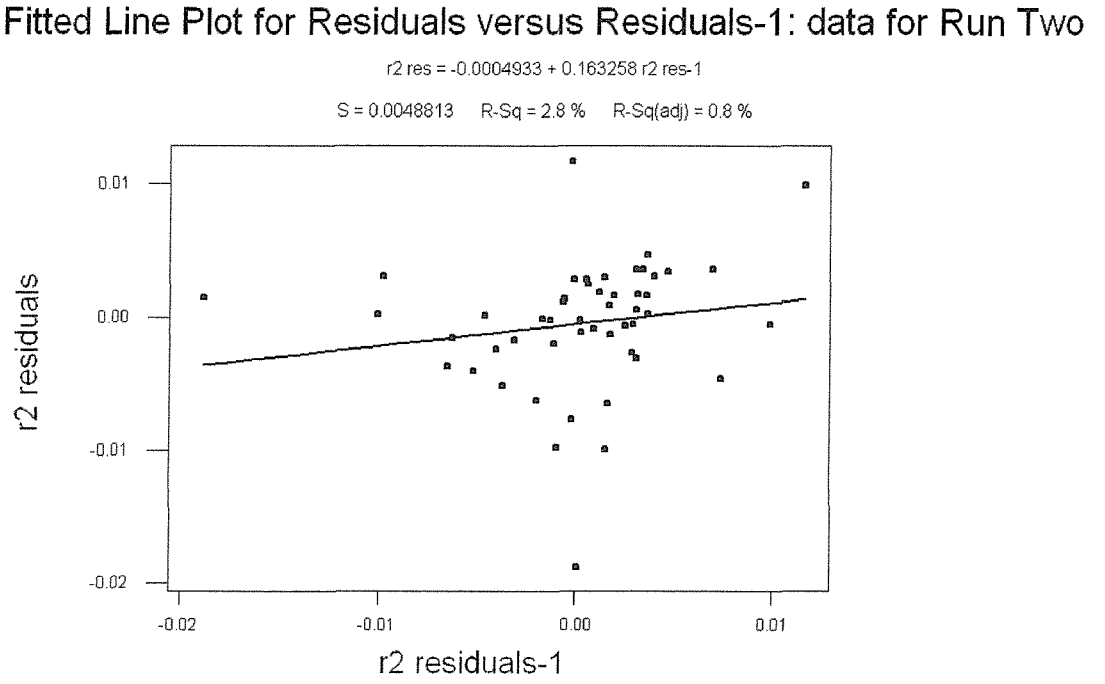


Figure 7. 3. 4. 12.

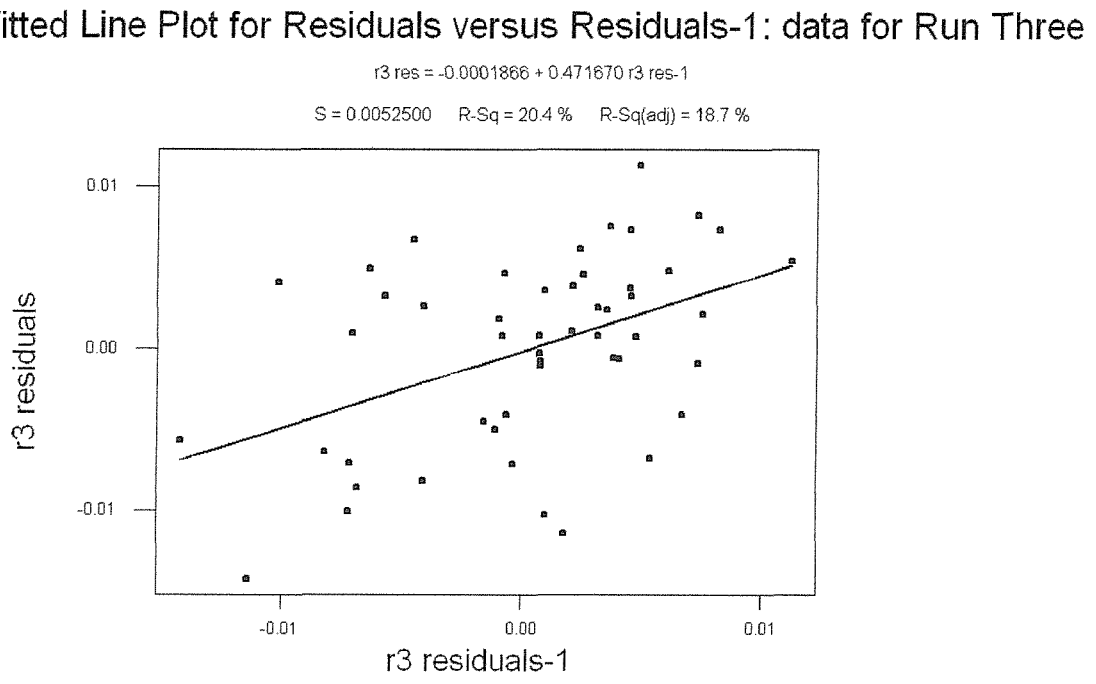


Figure 7. 3. 4. 13.

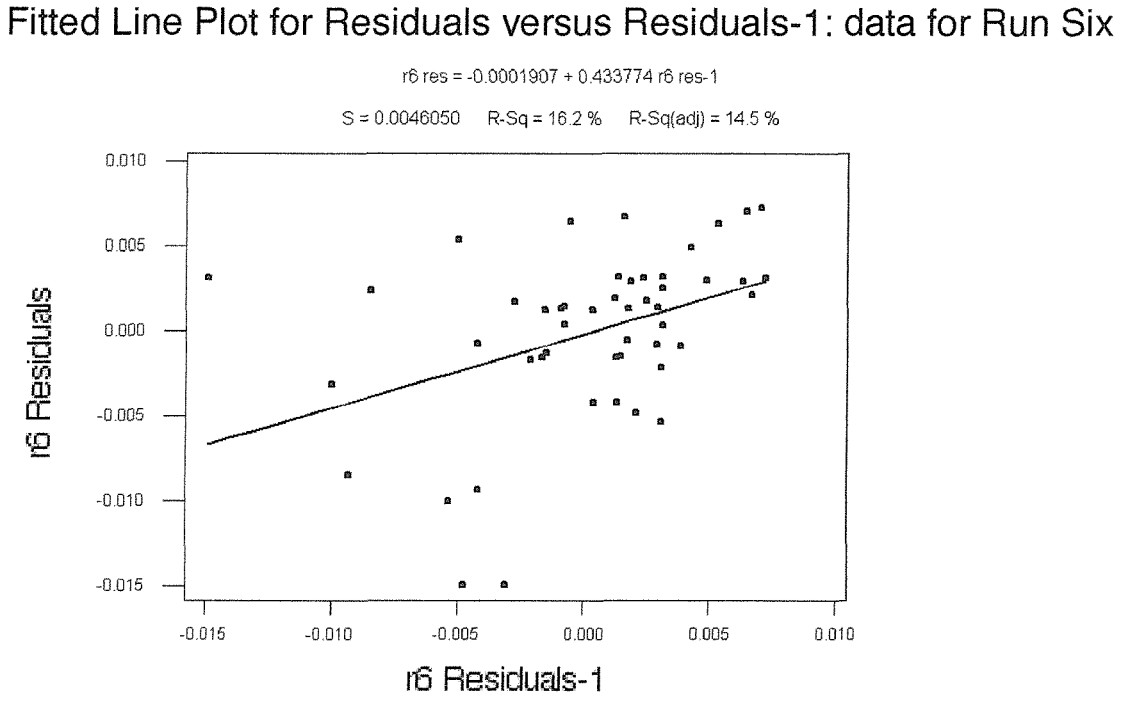
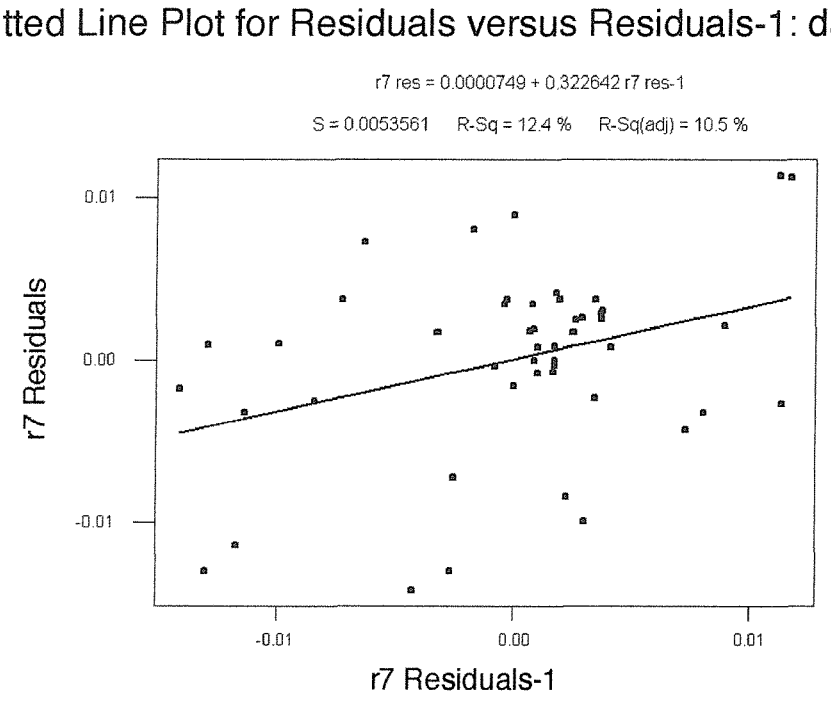


Figure 7. 3. 4. 14.



Having seen that correlated residuals may be responsible for the variance patterns and for contributing to the non-normality of distributions, particularly for runs three and seven, this dependence needs to be included into the regression model to examine whether its inclusion removes outliers, removes the variance structure, and makes the residual distributions normal. This is possible by including the control variable of FHT-1 into the multiple regression of FHT on DFS, IHDS, and PSS. This variable models how the value of FHT at y_i affects the value of FHT at y_j. Table 7. 3. 4. 2. shows the individual regression relationship between FHT and FHT-1, whilst table 7. 3. 4. 3. shows the effect FHT-1 has on the multiple regression relationship. Table 7. 3. 4. 3. indicates that the inclusion of the FHT-1 variable slightly improves the r^2 value for all runs, except runs two and eight, where the r^2 value decreases (compare with table 7. 3. 1. 7.).

Table 7. 3. 4. 2.- Regression of Final Morphology Height on Final Height-1

Run No.	$\hat{\beta}_0$	$\hat{\beta}_1$	r^2
1	0.0065	0.8192	0.635
2	0.0118	0.6959	0.495
3	0.0062	0.8356	0.711
4	0.0069	0.8588	0.722
5	0.0052	0.8624	0.765
6	0.0054	0.8590	0.758
7	0.0116	0.7242	0.566
8	0.0061	0.8602	0.728

Table 7. 3. 4. 3.- Regression of FHT on FHT-1, DFS, PSS and IHDS

Run No.	$\hat{\beta}_0$	$\hat{\beta}_1$	$\hat{\beta}_2$	$\hat{\beta}_3$	$\hat{\beta}_4$	r^2
1	-0.0002	0.4755	-0.0031	0.0003	0.423	0.710
2	0.0925	0.1727	-0.0055	0.0028	-1.4328	0.658
3	0.0180	0.4767	-0.0031	-0.0009	0.4736	0.762
4	0.0489	0.4524	-0.0032	-0.0016	0.2711	0.784
5	-0.0995	0.1499	-0.0054	0.0060	0.6182	0.881
6	0.0355	0.4372	-0.0033	-0.0011	0.3344	0.803
7	0.1045	0.3321	-0.0042	-0.0005	-0.7733	0.685
8	-0.1777	0.2129	-0.0026	0.0027	2.7377	0.813

Figures 7. 3. 4. 15. - 7. 3. 4. 22. give the residuals versus fits plots, histograms and probability plots for the multiple regression including FHT-1 and should be compared with figures 7. 3. 4. 2. - 7. 3. 4. 9. In run one, the effect of modelling for short scale dependence has made the variance pattern slightly more constant by pulling several large outliers towards the centre of the x (fit) axis, but the outliers still affect the data and the distribution of errors is not normal. In run two there is little change in the pattern of variance and the presence of residual outliers means its distribution of errors remain non-normal. Run six shows some improvement in the constancy of variance, but larger positive residuals are created by the inclusion of FHT-1 and again it remains non-normal. In run four, which had constant variance and a normal distribution of residuals before the inclusion of FHT-1, the addition of this variable has increased the value of the outliers and made the tails of the distribution longer. The result has been to decrease the normality of the distribution, though it still remains normal. In runs five and eight, which again had constant variance and normal distributions of residuals before the inclusion of FHT-1, the variance and the distributions are relatively unaffected by the modelling of short scale dependency.

Run three, as suggested by the residual versus residual-1 plot, shows a large improvement with the inclusion of FHT-1. The structure of the variance is constant and the probability plot shows a straight line. The number of positive residual values has been decreased and very negative residual values have been removed. In run seven, the change is not so noticeable, but the variance pattern has been made more constant by the pulling of larger residuals into the centre of the x axis (fits) and the curvature of the distribution has been reduced, making the distribution just normal.

Figure 7. 3. 4. 15.

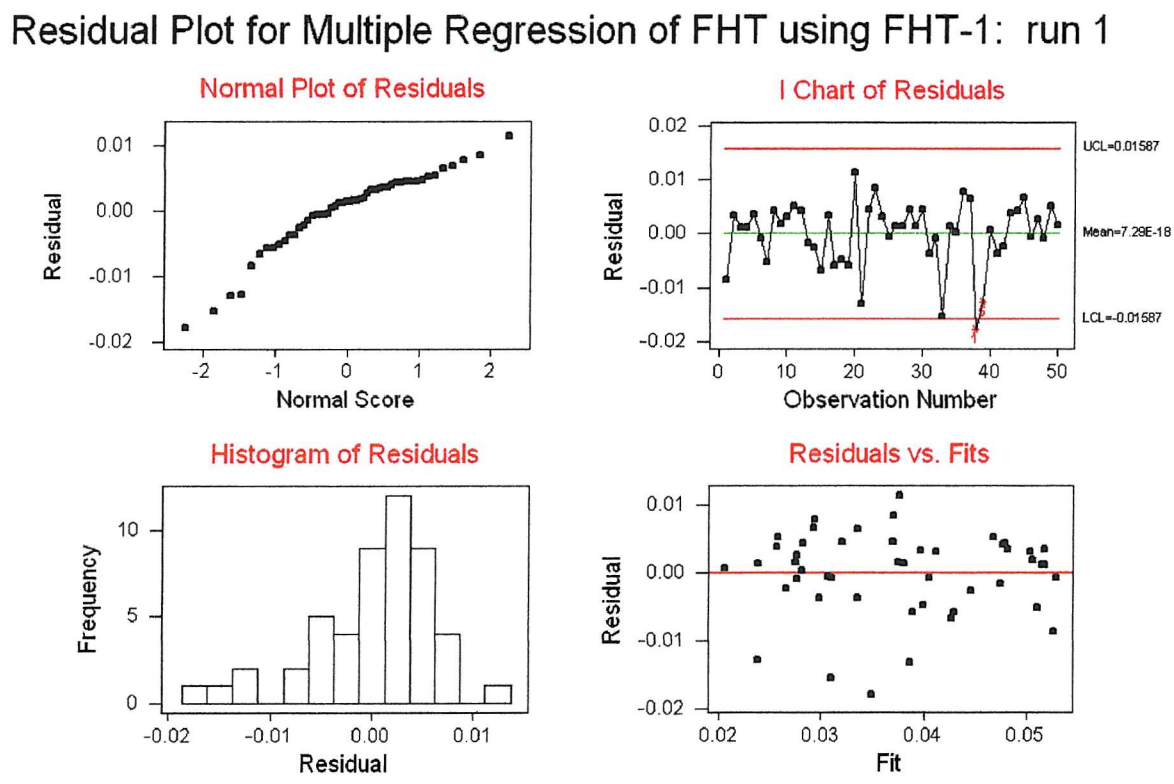


Figure 7. 3. 4. 16.

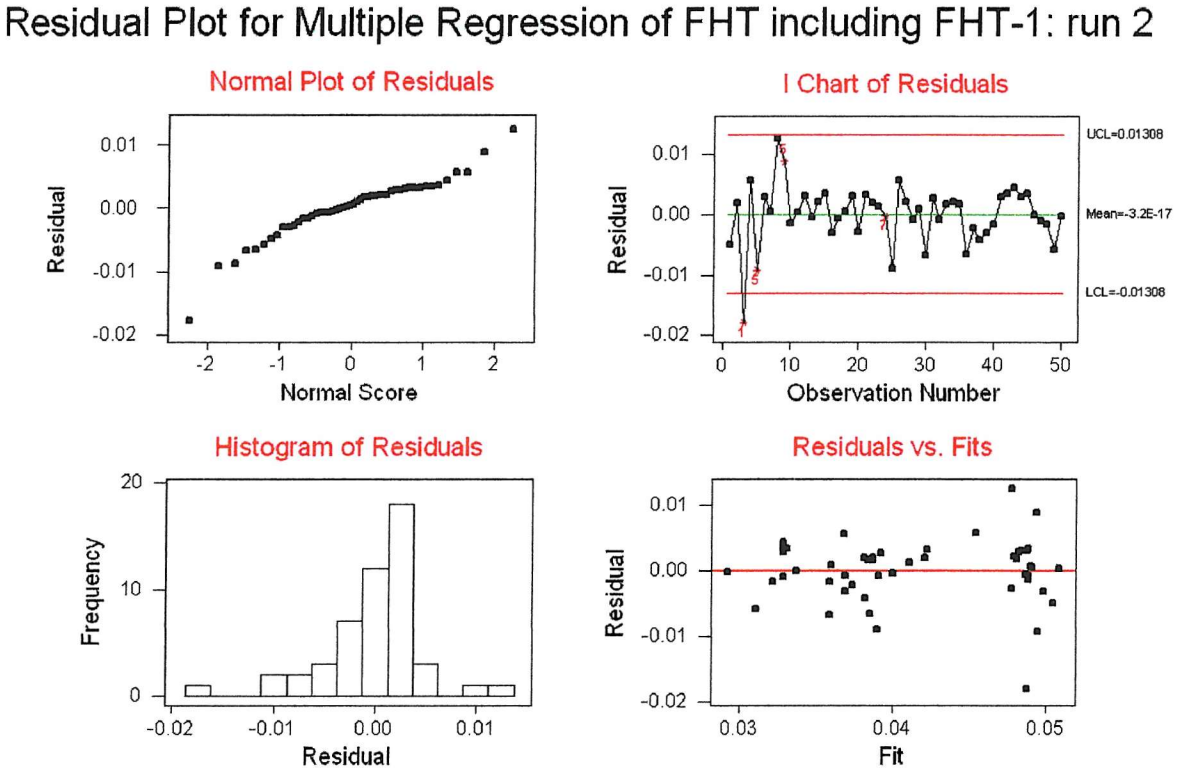


Figure 7. 3. 4. 17.

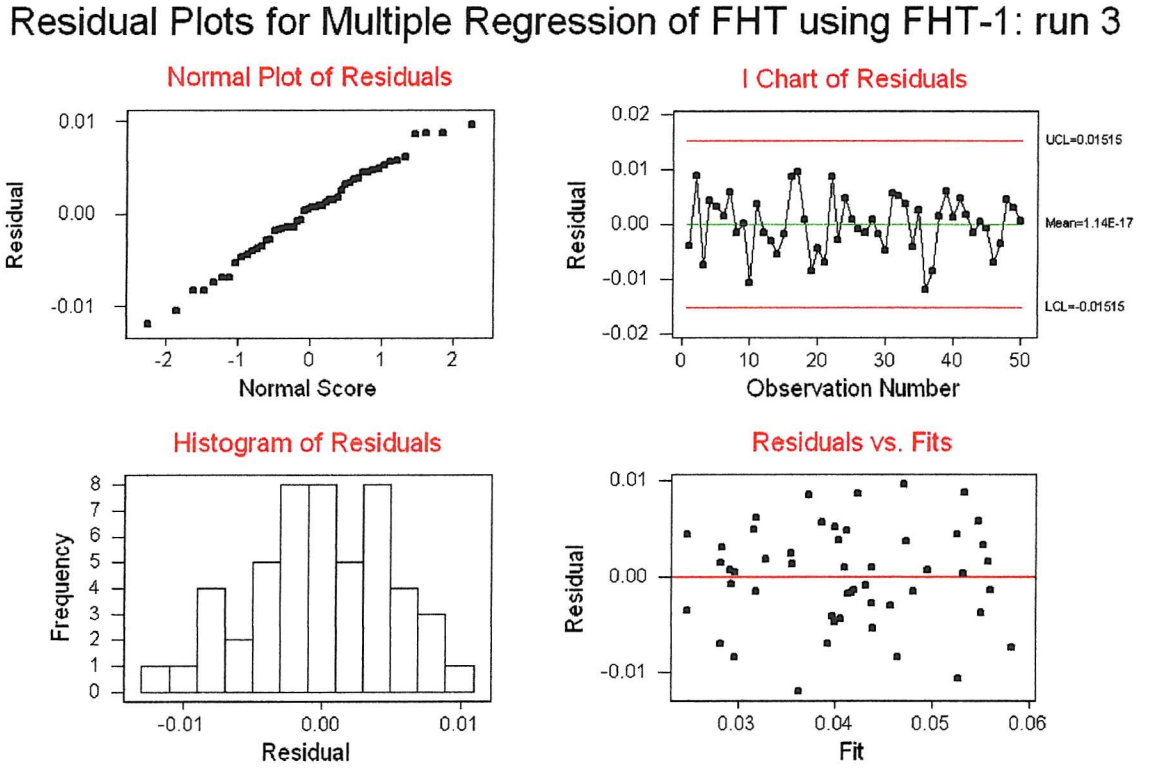


Figure 7. 3. 4. 18.

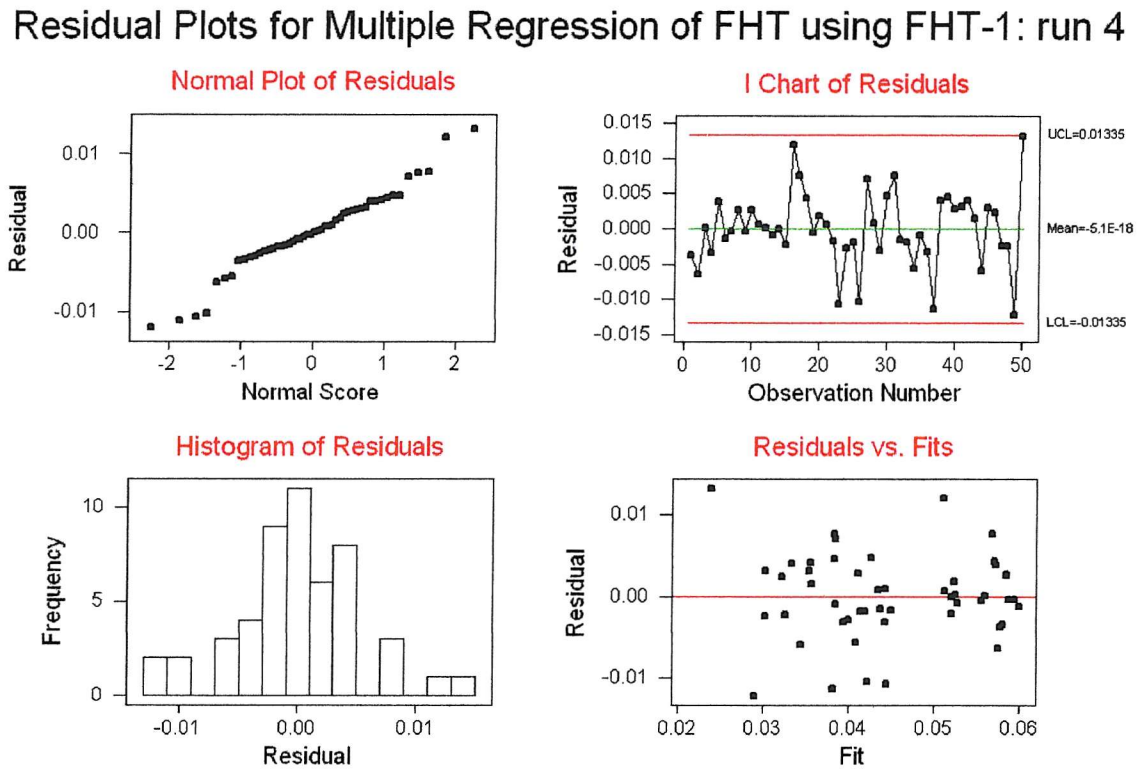


Figure 7. 3. 4. 19.

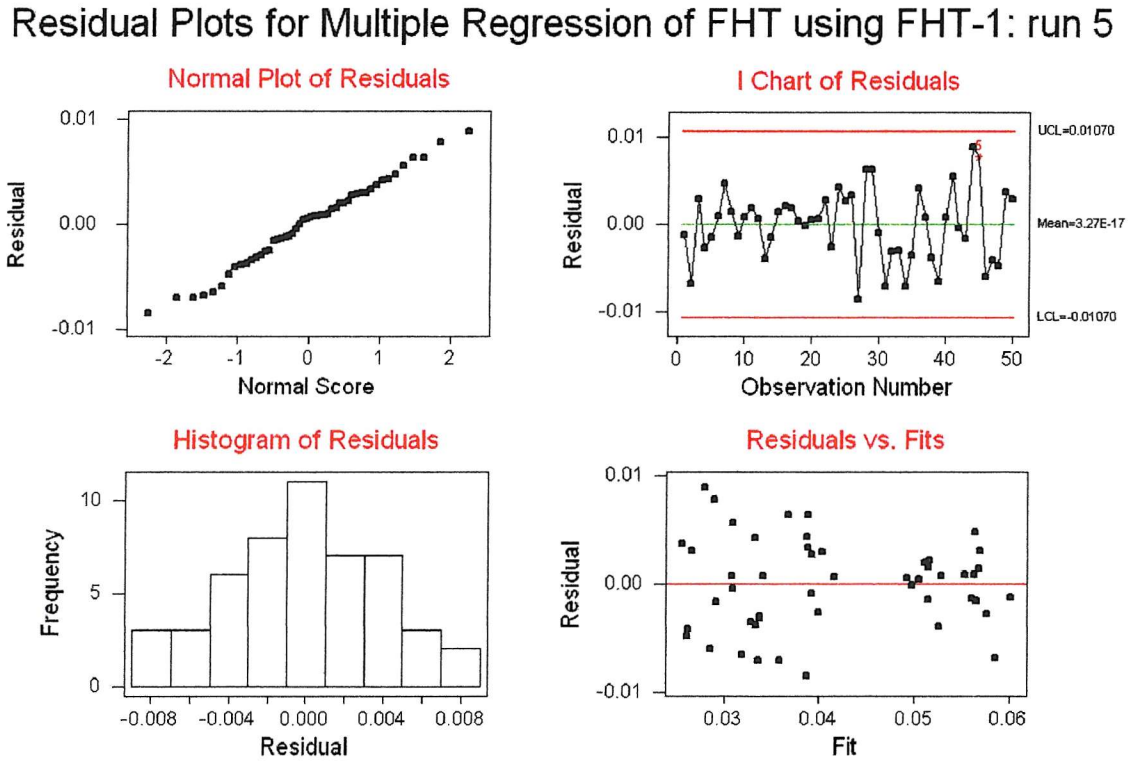


Figure 7. 3. 4. 20.

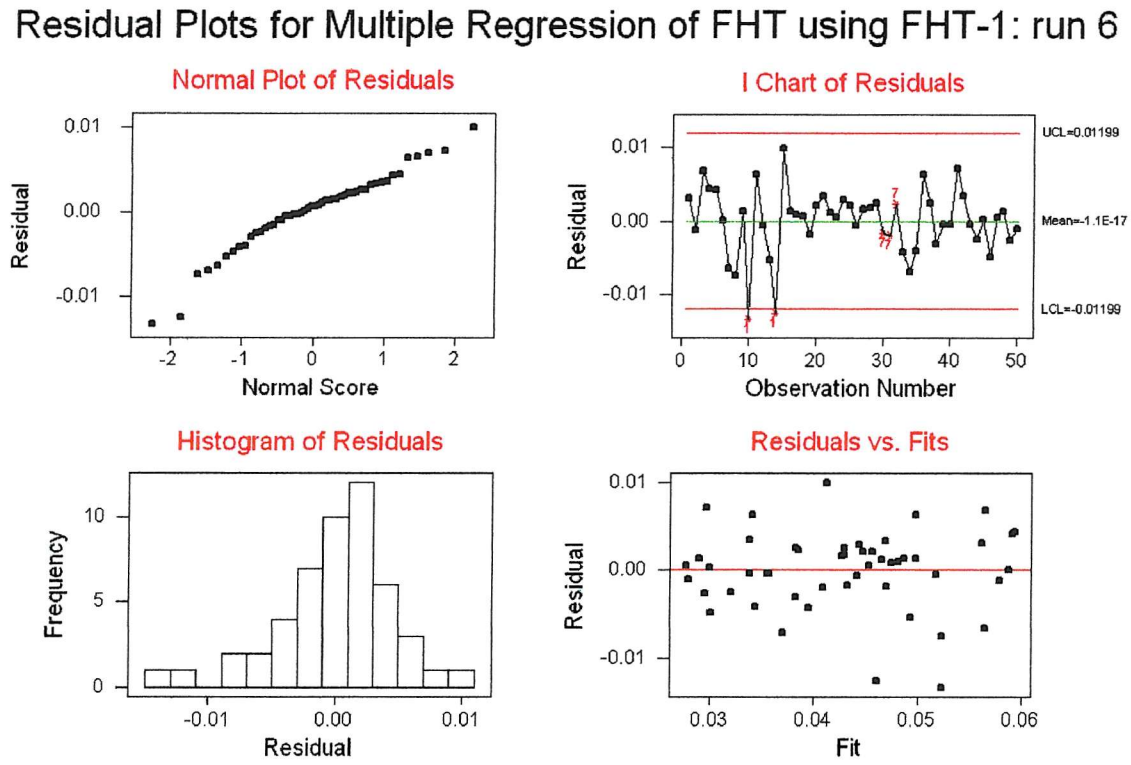


Figure 7. 3. 4. 21.

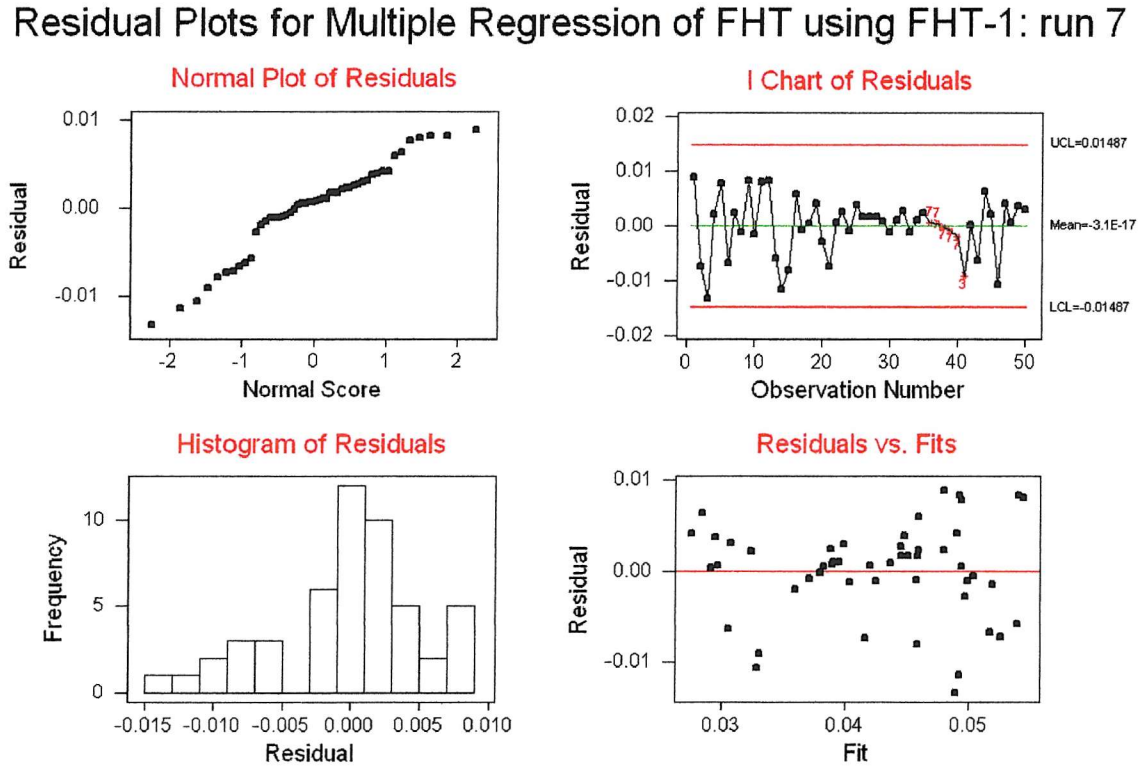
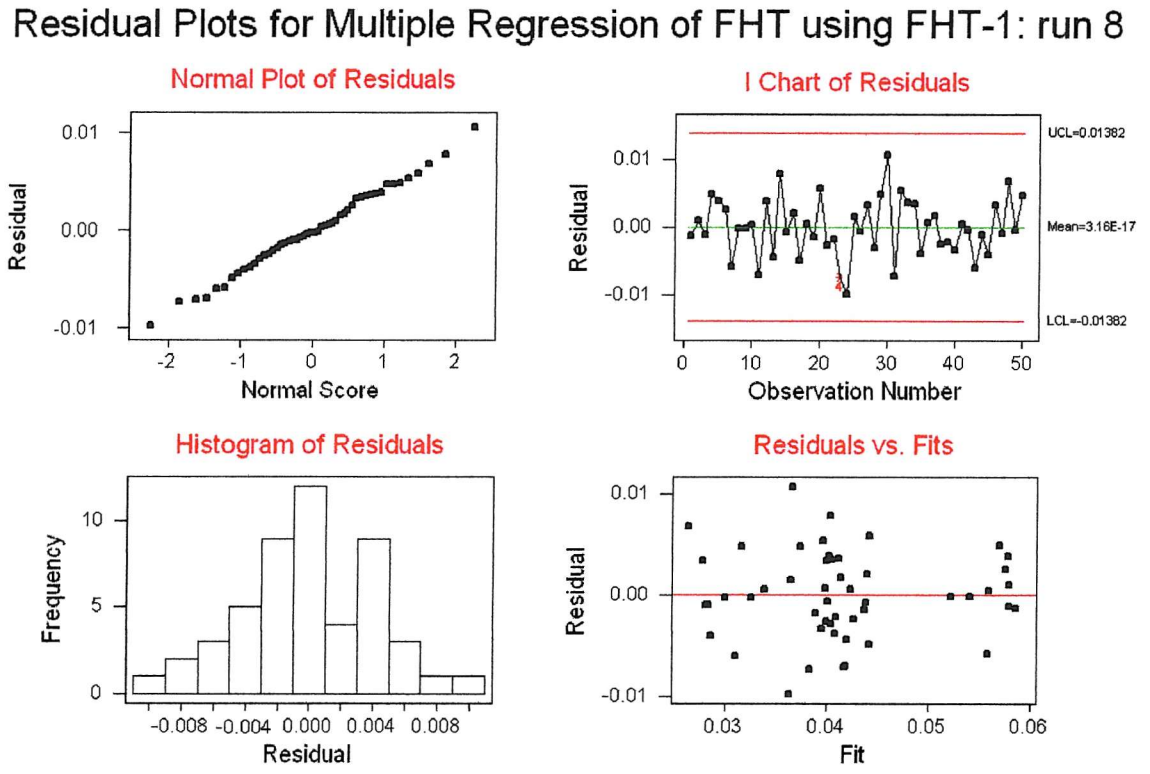


Figure 7. 3. 4. 22.



The result of modelling spatial dependence is that the regression model can be applied to runs three, four, five, seven and eight. The regression parameters of these equations can be tested to examine whether they are statistically similar or not; that is, the regression parameters for these five runs are the same except for deviations due to chance. Weisberg (p. 179 - 184; 1985) has proposed four models for regression lines. The first is the general model where the slopes and intercepts of the lines are different for all runs. The second model is termed parallel regressions, where the slopes of the lines are the same but the intercept values differ for all runs. The third model, termed concurrent regression, is where the slopes of the lines are different, but the intercept values are the same for all runs. The fourth model, termed coincident regression, is where there is perfect agreement between the different runs and the slopes and intercepts of the lines are the same for all runs. Models two, three and four which have greater explanatory power for the FHT data, are compared to model one, which suggests that each run is independent and that none of the regression parameters are consistent. This comparison is done by way of an hypothesis test (see equation 7. 3. 4. 1.) using the percentage points of the F distribution[162].

[162] See Weisberg (p. 182 - 183; 1982) for details on the construction of these model types.

Equation 7. 3. 4. 1.- Hypothesis Test for the F-distribution

$$F_1 = \frac{(RSS_1 - RSS_1)/(df_1 - df_1)}{RSS_1 / df_1}$$

Where:

RSS_1 = Residual Sum of Squares for models 2, 3, 4.

RSS_1 = Residual Sum of Squares for model 1.

df = Degrees of Freedom.

The values of these models and whether they are significant or not, are presented in table 7. 3. 4. 4. They are based on the multiple regression of FHT on FHT-1, and DFS, (the two influential variables) for runs three, four, five, seven and eight.

Table 7. 3. 4. 4.- RSS, df and F Values for Weisberg's (1982) Four Regression Models.

	Model 1	Model 2	Model 3	Model 4
RSS	0.0103115	0.006392	0.0104804	0.00662
d.f	237	243	241	247
F value		15.01	0.97	8.48
Significant		YES	NO	YES

The hypothesis test uses model one as the alternative hypothesis. Thus if an F test between models two, three or four and model one is significant, it suggests that model one (the alternative hypothesis) should be accepted and the null hypothesis (models two, three or four) should be rejected. The hypothesis statement for model two compared to model one, is that a fit which has a common slope but different intercepts for all runs, is as good as a fit which has different slopes and different intercepts for each run. It can be seen from table 7. 3. 4. 4. that the null hypothesis has to be rejected in this case, and the alternative of the general model has to be accepted. The hypothesis statement for model four compared to model one, is that a fit which has common slopes and common intercept is as good as a fit which has different slopes and different intercepts for each run. Again from table 7. 3. 4. 4. the null hypothesis in this case has to be rejected and the alternative of the general model has to be accepted. The hypothesis statement for model three compared to model one, is that a fit which has different slopes but a common intercept for all runs is as good as a fit which has different slopes and different intercepts for each run. From table 7. 3. 4. 4. it is seen that the null hypothesis has to be accepted in this case; the third model which is of greater explanatory power for the flume data than the general model, gives as good a fit as the general model.

What does the applicability of model three to runs three, four, five, seven and eight indicate about the application of deterministic methods to the FHT data? Firstly, it means that prediction in terms of deterministic modelling of fluctuations in the control variables is not possible using individual data points. DFS was shown to be the most successful predictor of FHT data within a run at the scale of the whole run (source to mouth), and the variable FHT-1 was shown to model spatial dependency and make the distributions of residuals normal. However, the fitted line that is formed for these data is specific to the run in question; the slope of the fitted line differs between the runs for which regression applies. This means that it is not possible to state the relationship between DFS, FHT-1 and FHT *a priori*.

Consider a scenario that does not extrapolate the data, but stays within the range of the control variables used in the five runs above. Given values of discharge, IHDS, PSS, and temperature of water, a new run is allowed to develop and create its own morphology. Because these control variables do not converge to a unique solution for FHT, the investigator is required to take a measure of FHT (this could be a point measurement or an average measurement at say the source of the flow). Given this information, the investigator should be in a position to specify the FHT of subsequent points downstream because they change with DFS and FHT-1. However, model three shows that the investigator is not in such a position. Although the relationship between FHT and DFS/ FHT-1 is fairly strong within a run, it changes between runs; that is the magnitude of the relationship between FHT and DFS/ FHT-1 is unique for any flow.

All the data collected, the *a priori* specification of discharge, IHDS, PSS, and temperature of water, and the posterior collection of a measure of FHT, amount to the statement that FHT will decrease with DFS (the direction of the relationship is consistent), but it can not forecast by how much. Similarly there is spatial dependence (FHT-1) but the extent of this is not constant between runs. The addition of variables does not make fluvial morphology determinate at the individual level,

Model three, however, does present a more positive aspect than the general model, and suggests that the intercept values are the same for all the runs. The intercept for multiple regression is given by equation 7. 3. 4. 2.;

Equation 7. 3. 4. 2.- Equation for the Intercept of Multiple Regression

$$\hat{\beta}_O = \bar{y} - \sum^{p} \hat{\beta}_j \bar{x}_j$$

Where:

$\hat{\beta}_o$	=	Estimated value of intercept
$\bar{y}$	=	Mean value of response variable
P	=	Number of predictors
$\hat{\beta}_j$	=	Estimated value of parameter
$\bar{x}_j$	=	Mean Value of control variable

From Weisberg (p. 40; 1985)

For the data above, which uses the two most successful predictors of FHT (FHT-1 and DFS), equation 7. 3. 4. 2. shows that the intercept is calculated by summing the products of the mean of FHT-1 x $\hat{\beta}_1$ and the mean of DFS x $\hat{\beta}_2$ and then subtracting this figure from the mean value of FHT.

The term $\sum^{p} \hat{\beta}_j \bar{x}_j$ in equation 7. 3. 4. 2. reveals several interesting things about the derivation of the intercept value. Firstly, the means of the control variables are subject to strict constraints; mean of DFS is a constant for any run, because the data is collected at fixed intervals along the channel and the mean of FHT-1 is equal to the mean of FHT (the response variable), minus one piece of data. Any variation in the means of the control variables between runs has to be produced by a variation in mean FHT.

Secondly, the parameter values ($\hat{\beta}_1$,$\hat{\beta}_2$) have been shown to be significantly different through the fitting of model three mentioned above. However, whilst they are different, they also naturally remain small compared to the means of the control variables.

What these two observations amount to is that differences in the intercept cannot be produced by changes in the parameter values alone, and the only source of difference is a change in the mean value of FHT-1 which is nearly equivalent to the mean of the response variable FHT. It can be concluded then that the intercept values are similar for runs three, four, five, seven and eight, because the mean value of FHT does not differ significantly.

Model three (concurrent) for the flume data suggests that although the morphology at any given location cannot be predicted in terms of the control variables, the mean FHT value of the whole run (source to mouth) is statistically constant and can be successfully predicted for the values of the control variables. This claim will be evaluated more thoroughly in the testing of aleatory statistical laws in Section 7. 4.

Summary of Section 7. 3.

The above analysis has attempted to use the controls of DFS, IHDS, PSS, and temperature of water, to predict FHT by a linear relationship. It has been shown that at a large scale, within a run, DFS is a reasonably effective linear predictor of change in FHT, because gross changes in sediment transport rates exist and lead to the regionalisation of areas of the flume. If this intrinsic control on FHT is removed and a linear predictive relationship is attempted within a region, using the controls of IHDS, PSS, and temperature of water to predict FHT, it has been shown that no linear relationship exists between these controls and FHT.

It seems that an attempt to explain flume morphology in terms of linear laws, fails predictive tests because a constant form of relationship cannot be established. Relationships between FHT and control variables appear to be generally random within a region, and ambiguous in direction (i .e. some values are inverse and some are proportional). The state of knowledge of the flume system in terms of linear laws may be stated as following; given a set of control variables with known values, it is not possible to predict the final morphology of a region; all that is known is that this region will generally be either lower or higher in elevation than the region above it or below it respectively.

Linear regression is a tool of epistemic probability, which aims to distinguish the true value of a response from error. A trivial observation shows that linear regression of individual values is not the most appropriate tool for tackling the indeterminacy of fluvial morphology (FHT). For any single value of discharge or PSS or temperature, a diverse and varied bed morphology results. For linear regression to apply, a smooth morphology, with a uniform quantitative value of FHT (save for errors in measurements or in initial conditions) would have to be the result.

It is intuitively expected that a varied morphology is the normal product of sediment/discharge interactions in a flume, because whilst discharge, PSS and temperature, affect the sediment-fluid interaction, it is the complex dynamics of the flow that actually produce the final morphology or FHT of the bed. In short, linear regression requires a single true value of FHT for each single value of the control variable; intuitively it is known that this assumption is not correct because complex flow dynamics produce true, but diverse values of FHT. Only if the complex flow dynamics and their relationship to both the controls and the morphology were understood, would a deterministic law at the level of the individual become possible.

7. 4. Testing the Aleatory Statistical Hypothesis for the Fundamental Data Set.

This section aims to test the hypothesis "Aleatory statistical relationships between entities best explain the indeterminacy of alluvial river channel cross-section and alluvial river channel planform." This is the type of law that has been applied most in fluvial geomorphology, where attempts to derive epistemic relationships between mean values of control and mean values of morphology have been made. This section examines whether scatter on such charts as those presented in Chapter Five, is likely to be due to the proposed concepts of error, equifinality or equilibrium, or whether scatter in aleatory relationships at this scale of investigation is an intrinsic part of the system.

Aleatory statistical methods place a different interpretation on a data set to those made by epistemic techniques. Aleatory statistical methods expect a consistent pattern to emerge from the collective properties of individual measurements and not in the measurements themselves. This means that the values taken by individual measurements are not significant, providing that they do not alter the measures of the collective properties. A justification for aleatory concepts in geomorphology and science was provided in Chapters Two and Three.

If aleatory statistical laws can be applied to the flume data, it is expected that three defining measures of the collective data will remain constant for any set of control variables; the shape of the distribution, the variance or standard deviation of the distribution and the mean of the distribution. The shape of the distribution indicates the general form of the response variable. In a normal shape distribution, moderate values of the response are most common - larger and smaller values of the response occur less often as departures are made from the moderate values. In a positively skewed distribution shape, smaller values of response occur most frequently but larger values of the response variable are present. In negatively skewed distribution shape, the opposite is the case; larger values of the response variable occur most frequently, but smaller values of the response variable are present. In a uniform distribution shape, all values of the response variable are the same. For any set of control variables, we would expect the overall nature of the response to be similar if an aleatory statistical law was appropriate; that is, if a negatively skewed distribution was obtained for run one, we would expect to obtain a negatively skewed distribution for run two.

The variance or standard deviation of the data is a measure of the average deviation from the mean and is affected by the range of the data. As an example, consider two samples of data given in table 7.4.1. Both these data sets are normally distributed and have a mean that is zero, but data set A has a standard deviation of 6.02 and data set B has a standard deviation of 14.36. The data in set A is distributed closely to the mean so that the average departure from the mean is fairly small, whilst in data set B the data is more dispersed around the mean and the average departure from the mean is greater. For constant controls, it is expected that distributions with similar average deviations from the mean will result; it is not expected that the same constant controls can produce responses with significantly different maximum and minimum values between trials. As shown by table 7. 4. 1. two distributions can have the same shape and mean, but different variances.

Table 7. 4. 1.- Similar Shape and Mean but Different Variance

Set A	Set B
-5	-30
-6	-10
-10	-5
0	0
0	0
2	5
8	10
6	10
5	20

The mean of the data is a measure of its central location. The mean describes the middle of the data set or the most moderate value amongst the data. In a normally distributed sample, the mean is the value of the data which occurs most often, but this definition does not hold for skewed data. For constant controls, it is expected that the mean of the data will be similar amongst samples. For example, if in run one a mean value of 0.001 m is produced for FHT, we do not expect in run two that for the same set of controls, a mean value of 0.006m is produced for FHT. A shift in mean or central location indicates that the response of the data has increased or decreased. It is possible for two distributions to have the same shape and variance, but different mean values.

Because all three measures of distributions define different aspects of the collective nature of the response, for two distributions to be equivalent, their shape, variances and means must all be judged to be statistically similar; that is any differences between them are due to chance alone and not to real differences in them.

The ideal conditions in which to test for aleatory statistical laws would have occurred if all the control variables had been perfectly constant and consistent between runs.[163] The analysis of the data would then have taken the form of comparing the shape, variance and mean of the FHT data between the runs. If all three criteria were similar for all comparisons, it could have been stated that an aleatory statistical law existed between the controls of discharge, gradient, PSS, IHDS, and FHT. Because the data would yield consistent values, the shape variance and mean of the FHT could be predicted for that set of control variables. If the comparisons gave different results for shape, variance and mean, then it could have been stated that aleatory statistical laws were not adequate to explain and predict the response of FHT for the control variables of discharge, gradient, PSS, and IHDS.

These ideal conditions are violated, because as was seen in Section 7. 3. it was not possible to keep the control variables absolutely constant[164]. In this situation if there are differences in the shape, variance and mean of the FHT distributions, it could be stated that these are due to differences in the control variables, and that if these are taken into account a constant aleatory statistical relationship would emerge. Therefore, the following analysis compares the shape, variance and mean of FHT data, and also analyses the shape variance and mean of the control variables of IHDS and PSS. The data is then tested to examine whether differences in the collective properties of the control variables affect the collective properties of the FHT data.

An issue in the analysis of aleatory data is whether statistical difference is equivalent to physical difference, i.e. just because data is judged to be different in statistical tests is it really different or significant in a physical environment? It is suggested here that statistical significance gives some objectivity to the assessment of whether two measures are different or not; without the concept of statistical significance the judgement of whether data is different or not is a subjective matter. The problem of statistical versus physical difference requires more careful study.[165]

[163] See experimental rationale given in **Chapter Six.**

[164] Improving the constancy of controls and developing a dynamic, non-invasive measurement procedure would represent a significant improvement in the instigation of this experimental design. See **Chapter Eight** for a discussion.

[165] See **p. 63 this thesis** for a discussion of the historical basis of statistical inference.

Outline of Test Data

The principal data used for the aleatory statistical tests is the FHT data for the central transect 0.6850, for runs one to eight. Tests are initially performed at the section level, where the final height, in effect, behaves as a random variable.[166] The statistical properties of IHDS for the sections are derived from the six real values of IHDS used in the testing of causal laws. PSS and temperature are constants for a section and have no statistical properties.

Tests are carried out on the FHT data at the run level, where both IHDS and PSS have statistical properties.

The comparison of statistical properties requires that the variable in question behaves as a random variable. Crucially, the random variable should be independent - that is one value of the variable does not affect another (Johnson, p. 222; 1988). The hypothesis outlined in Section 5. 3. 1. 2. suggests that this is not the case and that the value of a preceding datum point influences the next datum point. This dependence is seen in the causal tests above; the data differs with distance from source, but significantly it does not differ consistently with distance from source between runs; the preceding value of morphology in some way alters the succeeding value of morphology. At the section level this dependence can be placed to one side for two reasons; firstly the data behaves effectively as a random variable at this level (see footnote); we are unable to detect the dependence over such short distances. Secondly, we deal with relatively small sample size; with large sample sizes (i.e. the 330 data points for a run) and dependent data, the confidence interval of the standard error shrinks to very small proportions, because the standard deviation of the sample is smaller than expected. This leads to the result that comparisons between statistical properties are more likely to be judged significant than if the data was independent. This means that any significant differences obtained are not justifiable because the result could be imputed to the dependence of the data.

7. 4. 1.- Outline of Statistical Tests

The shape of the distributions was analysed using probability plots and the Anderson-Darling test for normality (see Minitab, p. 19-45; 1995 and Weisberg, p. 159; 1985). Minitab produces p-values for the Anderson Darling test statistic and the 95% confidence limit was used, such that values below or equal to $p = 0.05$ were judged to be significant and the null hypothesis of 'the sample is normally distributed' was rejected in favour of the alternative hypothesis 'the sample is not normally distributed.'

[166] I.e. there is no observable pattern. Although the system may be determined (i.e. by chaotic non-linear dynamics) it is not possible to deduce form the data what the determining equations may be.

The equivalence of the variance of two distributions was assessed using Levenes test (see Minitab, p. 8-23; 1995). This test statistic is appropriate for distributions which are continuous but not necessarily normally distributed. Minitab produces p - values for Levenes test and the 95% confidence limit was used, such that values below or equal to p = 0.05 were judged to be significant and the null hypothesis 'the variances of the two samples are the same' was rejected in favour of the alternative hypothesis 'the variances of the two samples are different.'

The equivalence of the mean of two distributions was assessed using the unpooled, twosample t-test (see Minitab, p. 8-15; 1995). The unpooled twosample procedure is appropriate for samples which may have different shapes or variances (Minitab, p. 12-12; 1995). Minitab produces p-values for the t-test and the 95 % confidence limit was used, such that values below or equal to p = 0.05 were judged to be significant and the null hypothesis 'the means of the two samples are the same' was rejected in favour of the alternative hypothesis 'the means of the two samples are different.'

7. 4. 2. Section Level Results

The Anderson-Darling test for normality was performed on the section FHT data. Table 7. 4. 2. 1. shows that the six of the 40 sections tested gave p-values below the 95% confidence interval , suggesting that these sections are non-normally distributed. Those values that are significantly different are marked in red.

Table 7. 4. 2. 1.- P-values for the Anderson-Darling Test of Normality: FHT data at the Section Level

run/section	b1	c2	b3	c4	b5
1	0.001	0.36	0.011	0.885	0.892
2	0.376	0.936	0.643	0.070	0.061
3	0.498	0.349	0.537	0.222	0.131
4	0.294	0.05	0.587	0.787	0.114
5	0.309	0.004	0.045	0.149	0.135
6	0.406	0.073	0.911	0.271	0.147
7	0.492	0.269	0.026	0.515	0.060
8	0.894	0.749	0.721	0.676	0.655

Levene's test for the equivalence of sample variances was performed on the section FHT data. Table 7. 4. 2. 2. shows that 14 out of 140 comparisons gave p-values below the 95% confidence interval, suggesting that these sections have significantly different variances. Those values that are significantly different are marked in red.

Table 7. 4. 2. 2.- P-values for Levene's Test for the Equivalence of Variance: FHT data at the Section Level.

Comparison/section	b1	c2	b3	c4	b5
1/2	0.058	0.003	0.336	0.021	0.682
1/3	0.165	0.883	0.771	0.524	0.489
1/4	0.426	0.247	0.531	0.105	0.131
1/5	0.382	0.026	0.957	0.069	0.258
1/6	0.024	0.205	0.519	0.050	0.526
1/7	0.061	0.763	0.704	0.003	0.258
1/8	0.402	0.049	0.540	0.018	0.282
2/3	0.363	0.004	0.396	0.112	0.801
2/4	0.118	0.201	0.627	0.366	0.269
2/5	0.167	0.684	0.274	0.790	0.550
2/6	0.885	0.283	0.050	0.693	0.898
2/7	0.678	0.025	0.538	0.056	0.550
2/8	0.130	0.368	0.723	0.812	0.672
3/4	0.382	0.298	0.668	0.361	0.370
3/5	0.532	0.035	0.698	0.224	0.969
3/6	0.242	0.249	0.185	0.206	0.879
3/7	0.549	0.685	0.884	0.021	0.742
3/8	0.422	0.065	0.669	0.096	0.908
4/5	0.832	0.400	0.450	0.648	0.509
4/6	0.053	0.896	0.072	0.643	0.279
4/7	0.143	0.249	0.830	0.040	0.509
4/8	0.939	0.583	0.941	0.304	0.357
5/6	0.089	0.500	0.510	0.960	0.592
5/7	0.228	0.060	0.640	0.169	1.000
5/8	0.886	0.706	0.474	0.689	0.777
6/7	0.534	0.215	0.229	0.086	0.592
6/8	0.061	0.700	0.140	0.582	0.739
7/8	0.163	0.094	0.801	0.113	0.777

The unpooled two sample t-test for the equivalence of sample means was performed on the section FHT data. Table 7. 4. 2. 3. shows that 44 out of 140 comparisons gave p-values below the 95% confidence interval, suggesting that these sections have significantly different means. Those values that are significantly different are marked in red.

Table 7. 4. 2. 3.- P-values for unpooled twosample t-test for the equivalence of sample means: FHT data at the Section Level.

Comparison/section	b1	c2	b3	c4	b5
1/2	0.430	0.011	0.930	0.004	0.031
1/3	0.110	0.450	0.600	0.016	0.920
1/4	0.001	0.000	0.490	0.002	0.200
1/5	0.001	0.010	0.350	0.120	0.820
1/6	0.200	0.099	0.001	0.005	0.870
1/7	0.270	0.030	0.078	0.001	0.820
1/8	0.002	0.870	0.470	0.001	0.650
2/3	0.089	0.066	0.700	0.680	0.057
2/4	0.012	0.016	0.580	0.620	0.820
2/5	0.009	0.760	0.450	0.007	0.100
2/6	0.110	0.330	0.005	0.790	0.065
2/7	0.920	0.660	0.120	0.210	0.100
2/8	0.018	0.000	0.450	0.048	0.110
3/4	0.220	0.002	0.830	0.490	0.210
3/5	0.160	0.057	0.650	0.130	0.780
3/6	0.910	0.370	0.002	0.820	0.810
3/7	0.038	0.110	0.160	0.290	0.780
3/8	0.310	0.280	0.220	0.130	0.640
4/5	0.720	0.041	0.830	0.008	0.300
4/6	0.430	0.009	0.007	0.510	0.260
4/7	0.002	0.240	0.250	0.690	0.300
4/8	0.790	0.000	0.170	0.270	0.350
5/6	0.330	0.270	0.007	0.019	0.950
5/7	0.002	0.800	0.320	0.001	1.000
5/8	0.570	0.001	0.110	0.000	0.860
6/7	0.066	0.330	0.130	0.180	0.950
6/8	0.520	0.030	0.001	0.046	0.800
7/8	0.004	0.014	0.021	0.260	0.860

The Anderson-Darling test for normality was performed on the section IHDS data. Table 7. 4. 2. 4. shows that none of the 40 sections tested gave p-values below the 95% confidence interval. This suggests that all the sections are normally distributed.

Table 7. 4. 2. 4.- P-values for the Anderson-Darling Test of Normality: IHDS data at the Section Level.

run/section	b1	c2	b3	c4	b5
1	0.562	0.118	0.297	0.118	0.117
2	0.149	0.263	0.606	0.263	0.458
3	0.643	0.442	0.656	0.442	0.833
4	0.410	0.794	0.795	0.794	0.471
5	0.103	0.229	0.352	0.229	0.079
6	0.234	0.542	0.368	0.542	0.505
7	0.157	0.443	0.205	0.443	0.747
8	0.264	0.305	0.956	0.305	0.740

Levene's test for the equivalence of sample variances was performed on the section IHDS data. Table 7. 4. 2. 5. shows that six out of 140 comparisons gave p-values below the 95% confidence interval, suggesting that these sections have significantly different variances. Those values that are significantly different are marked in red.

Table 7. 4. 2. 5.- P-values for Levene's Test for the Equivalence of Variance: IHDS data at the Section Level.

Comparison/section	b1	c2	b3	c4	b5
1/2	0.869	0.035	0.585	0.931	0.286
1/3	0.375	0.583	0.850	0.937	0.807
1/4	0.150	0.100	0.424	0.518	0.147
1/5	0.382	0.195	0.602	0.290	0.470
1/6	0.371	0.836	0.962	0.788	0.582
1/7	0.208	0.241	0.640	0.939	0.231
1/8	0.900	0.027	0.461	0.578	0.039
2/3	0.362	0.082	0.658	0.880	0.340
2/4	0.322	0.946	0.722	0.509	0.618
2/5	0.566	0.769	0.982	0.336	0.934
2/6	0.349	0.056	0.657	0.762	0.434
2/7	0.221	0.310	0.983	0.883	0.846
2/8	0.982	0.626	0.869	0.716	0.457
3/4	0.034	0.180	0.466	0.529	0.171
3/5	0.107	0.314	0.678	0.288	0.539
3/6	0.802	0.753	0.915	0.815	0.738
3/7	0.729	0.467	0.716	1.000	0.272
3/8	0.410	0.088	0.511	0.499	0.042
4/5	0.640	0.837	0.708	0.780	0.624
4/6	0.082	0.133	0.479	0.636	0.216
4/7	0.013	0.438	0.726	0.530	0.764
4/8	0.358	0.760	0.819	0.421	0.917
5/6	0.154	0.243	0.673	0.668	0.635
5/7	0.051	0.624	1.000	0.301	0.812
5/8	0.582	0.981	0.850	0.143	0.512
6/7	1.000	0.334	0.699	0.818	0.345
6/8	0.510	0.054	0.541	0.211	0.055
7/8	0.266	0.456	0.864	0.514	0.632

The unpooled two sample t-test for the equivalence of sample means was performed on the section IHDS data. Table 7. 4. 2. 6. shows that 97 out of 140 comparisons gave p-values below the 95% confidence interval, suggesting that these sections have significantly different means. Those values that are significantly different are marked in red. The IHDS mean has so many significant differences because the variance of the variable is small and well controlled. Any slight difference in mean in the IHDS data is then judged to be significant.

Table 7. 4. 2. 6.- P-values for unpooled twosample t-test for the equivalence of sample means: IHDS data at the Section Level.

Comparison/section	b1	c2	b3	c4	b5
1/2	0.000	0.002	0.000	0.000	0.055
1/3	0.000	0.000	0.000	0.000	0.000
1/4	0.000	0.000	0.003	0.001	0.000
1/5	0.000	0.000	0.000	0.000	0.000
1/6	0.000	0.000	0.000	0.000	0.000
1/7	0.000	0.000	0.000	0.000	0.000
1/8	0.000	0.017	0.000	0.000	0.420
2/3	0.620	0.068	0.000	0.000	0.012
2/4	0.000	0.015	0.750	0.012	0.001
2/5	0.210	0.006	0.001	0.001	0.001
2/6	0.250	0.180	0.013	0.004	0.001
2/7	0.000	0.130	0.580	0.007	0.001
2/8	0.580	0.044	0.640	0.037	0.037
3/4	0.003	0.140	0.001	0.340	0.012
3/5	0.590	0.050	0.940	0.015	0.019
3/6	0.500	0.290	0.035	0.003	0.047
3/7	0.000	0.750	0.001	0.000	0.019
3/8	0.340	0.001	0.001	0.000	0.001
4/5	0.000	0.670	0.001	0.560	0.670
4/6	0.024	0.056	0.018	0.240	0.090
4/7	0.015	0.120	0.430	0.098	0.690
4/8	0.001	0.000	0.480	0.048	0.000
5/6	0.740	0.017	0.063	0.300	0.170
5/7	0.000	0.042	0.002	0.037	0.970
5/8	0.084	0.000	0.002	0.009	0.000
6/7	0.001	0.600	0.034	0.290	0.160
6/8	0.130	0.002	0.034	0.066	0.000
7/8	0.000	0.001	0.034	0.210	0.000

7. 4. 3. Analytical Procedure for Analysis of Aleatory Test Results

Tables 7. 4. 2. 1 to 7. 4. 2. 6. attempt to construct a profile of complex phenomena. Two criteria are being analysed in these tables. The first criterion is similarity - the similarity of the FHT between runs and the similarity of the control IHDS between runs. The second criterion is the relationship between FHT and the control of IHDS. Both these criteria need to be studied in order to meet predictive requirements. The difficult interaction of similarity and regression is discussed below before the analysis is presented.

In the flume experiments, the *comparison* between data is key to understanding; the significant results are relative, not absolute. In the regression analysis above, relationships were derived for each run individually and then compared. The *comparison* showed that model three was appropriate - the data had similar mean values of FHT but the relationship between FHT and DFS was not constant. Comparisons are so important because fluid flows are indeterminate; it is not possible to work out from a set of equations *a priori* what the final form will be[167] . The best that can be hoped for *en route* to those equations is to discover elements of morphology that are constant for a set of controls. As more constant elements are discovered, it may be possible to make some form of predictive statement and perhaps derive *a priori* equations that will reproduce those constant elements.

In aleatory analysis, the elements of morphology that are being tested for constancy are not individual data points but their cumulative statistical properties. The aim is to examine whether statistical properties are similar and if they are not, why that may be the case. For example, differences in statistical properties of FHT may be due to differences in the statistical properties of the control properties of IHDS.

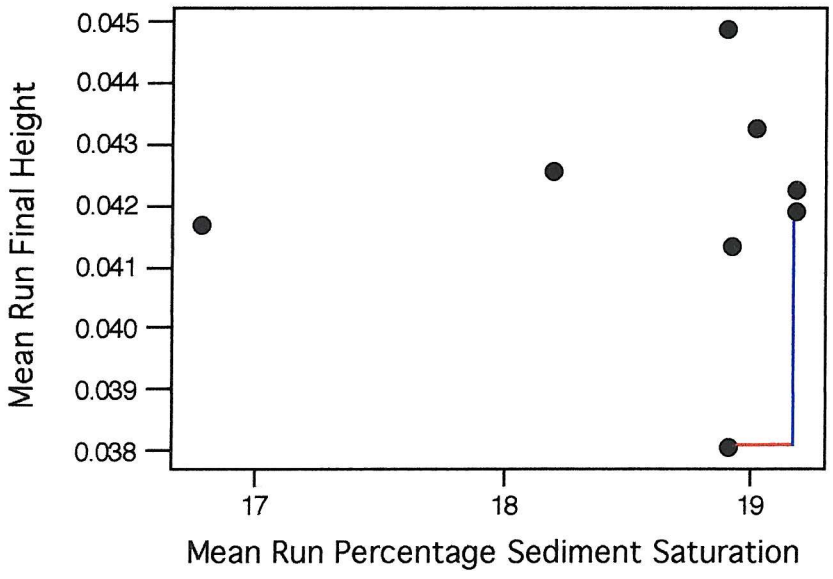
In regression analysis, relationships between FHT and the control variables were derived internally between runs and then compared; in aleatory analysis the opposite will be the case; comparisons will be made between runs and then relationships will be derived. This approach is taken because of the ideas of statistical similarity and difference, and the scale of analysis. It is possible to examine whether statistical properties are similar and therefore constant, by way of an hypothesis test. These tests have two possible results 1) The statistical properties are similar and any difference between them is due to chance 2) The statistical properties are not similar because of real difference between them. Statistical similarity essentially states that the two values are the same and this is important for discussing relationships.

[167] See **Chapter One** for a description of indeterminacy.

When the statistical properties of the controls and the statistical properties of FHT are being assessed, four scenarios are possible: 1) The controls can be statistically similar (same shape, variance and mean) and the FHT can be statistically similar 2) The controls can be statistically different (one or more of shape, mean or variance is different) and the FHT can be statistically different 3) The controls can be statistically similar and the FHT can be statistically different 4) The controls can be statistically different and the FHT can be statistically similar.

Regression analysis does not apply to the first of these scenarios because it involves trying to form relationships within a distribution space, where any differences are due to chance. A regression relationship is applied where differences in variables exist, but statistical similarity implies a constant and a regression relationship should not be applied. For example, consider the relationship between FHT mean for run one and FHT mean for run two and the corresponding values of PSS mean for both runs. Figure 7. 4. 3. 1. shows a plot for FHT mean versus PSS mean. The red line shows that the horizontal distance between the two PSS values is not great enough for them to be considered statistically different values; similarly the blue line shows that the vertical distance between the two values of FHT is not great enough for them to be considered statistically different values. These two points therefore form a distribution space. It is thought in aleatory theory that the distribution is the best resolution with which to view the data; any difference within statistically similar distributions is due to chance. Therefore, those comparisons which fit scenario one are removed from the regression analysis.

Figure 7. 4. 3. 1- Plot of Mean Run FHT Versus Mean Run PSS



Fitted line plots for the regression of data involving the remaining three scenarios are more complex than usual because they involve comparisons rather than actual data. The data for the comparison regression is the difference between the statistical property of the control for the given run (or the distance travelled on the x axis) and the difference between the statistical property of FHT for the given runs (or the distance travelled on the y axis). One or both of these distances has been judged to be statistically significant and therefore not a constant. The quantity difference is clear for variance and mean - it is one value subtracted from another. For the difference between the shape of a distribution, the best estimate available is the difference between p-values for the Anderson-Darling test statistic.

The data is ordered such that the control variable is always increasing; for example, for the regression Difference of FHT variance on Difference of IHDS mean, the data used for the comparison between run seven and eight is not IHDS mean for run seven minus IHDS mean for run eight, but IHDS mean for run eight minus IHDS mean for run seven, because run eight has the smaller mean. This leads to the x axis for the comparison regression chart starting at the most negative value and proceeding to zero. The x axis for the comparison regression goes from large increases in the control variable between runs to small increases in the control variable between runs. The y axis for the comparison regression chart varies from negative to positive values; negative values indicate an increase in the variable between runs, positive values indicate a decrease in the variable between runs.

A proportional relationship between the control and the FHT parameter would be indicated on these fitted line plots by a large increase in the control variable (negative x) between runs leading to large increases in the FHT parameter (negative y). Small changes in the control variables (near zero x) leads to small changes in the FHT parameter (near zero y). An inverse relationship would be a large increase in the control variable between runs (negative x), leading to a large decrease in the FHT parameter between runs (positive y). Small changes in the control variables between runs (near zero x) leads to small changes in the FHT parameter between runs (near zero y).

Finally, data involving scenario one (same controls/same results) fits the aleatory model; constant controls give constant statistical properties of FHT. Data involving scenario two (different controls/different results) have the potential to fit the aleatory model if the relationship between the controls and FHT can be established. The absence of a relationship between statistical properties of the controls and statistical properties of FHT means that we do not know how changing the properties of the controls will affect the FHT. Crucially this implies that for any given set of controls with fixed statistical properties, we cannot know the statistical behaviour of FHT or flume morphology. Data involving scenarios three (same controls/different results) and four (different controls/same results) do not fit the aleatory model at this level of analysis. Their inclusion in regression relationships will weaken any trend; however it is expected that the control variables should be general predictors of morphology and not be applied as posterior knowledge.

7. 4. 4. Section Level Analysis

Table 7. 4. 4. 1. analyses the results of the statistical tests for section FHT shape and categorises the individual relationships between the properties of IHDS and FHT shape according to the four scenarios listed above. FHT shape was judged to be different between runs if one or both of the runs had a non-normal distribution.

Table 7. 4. 4. 1.- Scenarios for section FHT shape

	IHDS shape	IHDS variance	IHDS mean
Scenario 1	102	95	36
Scenario 2	0	0	30
Scenario 3	38	38	8
Scenario 4	0	7	66

Table 7. 4. 4. 1. shows that 102 of the 140 comparisons involve FHT shape having a normal distribution. IHDS shape and IHDS variance show a fairly strong aleatory relationship with FHT shape with 102 and 95 comparisons respectively having similar controls and similar FHT shape. However, significant differences in FHT shape are not explainable in terms of IHDS shape or IHDS variance. IHDS shape has similar controls but different FHT shape in 38 comparisons. IHDS variance has 38 comparisons which show this divergent behaviour and seven comparisons which show convergent behaviour, where significantly different variances produce similar FHT shape. IHDS mean is poorly related to FHT shape having only 36 comparisons with similar controls and similar FHT shape. FHT shape is not explainable in terms of IHDS mean; IHDS mean has 66 comparisons which are significantly different but which give similar FHT shape. It also has eight comparisons which have similar means but yield significantly different FHT shape. Table 7. 4. 4. 2. gives the regression relationships between FHT shape and the properties of IHDS for scenarios two, three and four. Standard deviation was used in place of variance for the regression relationships.

Table 7. 4. 4. 2. - Regression Relationships for FHT shape: Scenarios Two, Three and Four

Variable	n	$\hat{\beta}_0$	$\hat{\beta}_1$	r^2
IHDS shape	38	-0.3200	-0.4059	0.023
IHDS variance	45	0.0002	247.0560	0.005
IHDS mean	104	0.0939	-11.8046	0.001

Table 7. 4. 4. 3. analyses the results of the statistical tests for section FHT variance and categorises the individual relationships between the properties of IHDS and FHT shape according to the four scenarios listed above.

Table 7. 4. 4. 3.- Scenarios for section FHT Variance

	IHDS shape	IHDS variance	IHDS mean
Scenario 1	126	122	43
Scenario 2	0	2	10
Scenario 3	14	12	3
Scenario 4	0	4	84

Table 7. 4. 4. 3. shows that IHDS shape and IHDS variance have a strong aleatory relationship with FHT variance having 126 and 122 comparisons respectively with similar controls and similar FHT variance. IHDS shape has 14 comparisons which show divergence, and IHDS variance shows divergence (12 comparisons) and convergence (four comparisons). IHDS mean is poorly related to FHT variance with 84 comparisons showing convergence (different IHDS mean producing similar FHT variance) and 3 comparisons showing divergence (similar IHDS mean producing different FHT variance). Table 7. 4. 4. 4. gives the regression relationships between FHT variance and the properties of IHDS for scenarios two, three and four. Standard deviation was used in place of variance for the regression relationships.

Table 7. 4. 4. 4.- Regression Relationships for FHT variance: Scenarios Two, Three and Four

Variable	n	$\hat{\beta}_0$	$\hat{\beta}_1$	r^2
IHDS shape	14	0.0021	0.0017	0.002
IHDS variance	18	0.0032	6.0754	0.110
IHDS mean	97	-0.0001	-0.0001	0.000

Table 7. 4. 4. 5. analyses the results of the statistical tests for section FHT mean and categorises the individual relationships between the properties of IHDS and FHT mean according to the four scenarios listed above.

Table7. 4. 4. 5.- Scenarios for section FHT Mean

	IHDS shape	IHDS variance	IHDS mean
Scenario 1	96	92	33
Scenario 2	0	2	35
Scenario 3	44	42	9
Scenario 4	0	4	63

Table 7. 4. 4. 5. shows that IHDS shape and IHDS variance have a fairly strong aleatory relationship with FHT mean, having 96 and 92 comparisons respectively which have statistically similar controls and statistically similar effects. However, divergence is present in these controls with IHDS shape having 44 comparisons and IHDS variance having 42 comparisons which have similar controls but different FHT mean. IHDS mean again shows a poor relationship to FHT with 63 comparisons showing convergence from significantly different values of IHDS mean to similar values of FHT mean. Table 7. 4. 4. 6. gives the regression relationships between FHT mean and the properties of IHDS for scenarios two, three and four. Standard deviation was used in place of variance for the regression relationships.

Table7. 4. 4. 6.- Regression Relationships for FHT mean: Scenarios Two, Three and Four.

Variable	n	$\hat{\beta}_0$	$\hat{\beta}_1$	r^2
IHDS shape	44	-0.0027	0.0032	0.008
IHDS variance	48	0.0008	6.6351	0.028
IHDS mean	107	-0.0002	0.8505	0.027

Having tested the statistical properties of FHT against the statistical properties of IHDS, it is necessary to examine whether differences in the constants at this level of investigation, namely PSS and temperature, produce differences in the statistical properties of FHT. The regression relationships formed between these differences are given below in tables 7. 4. 4. 7. and 7. 4. 4. 8.

Table 7. 4. 4. 7.- Regression relationships between difference in PSS for section and difference in statistical properties of section FHT.

Variable	n	$\hat{\beta}_0$	$\hat{\beta}_1$	r^2
FHT Shape	140	-0.0550	-0.1604	0.098
FHT variance	140	0.0001	0.0004	0.020
FHT mean	140	-0.0002	0.0002	0.001

Table 7. 4. 4. 8.- Regression relationships between difference in Temperature and difference in statistical properties of section FHT.

Variable	n	$\hat{\beta}_0$	$\hat{\beta}_1$	r^2
FHT Shape	140	0.0425	-0.1738	0.012
FHT variance	140	0.0001	-0.0001	0.000
FHT mean	140	-0.0005	-0.0022	0.015

None of the regression relationships give significant results at the 95% confidence limit.

7. 4. 5. Run Level Results

The Anderson-Darling test for normality was performed on the run FHT data. Table 7. 4. 5. 1. shows that none of the runs tested gave p-values below the 95% confidence interval , suggesting that all runs are normally distributed.

Table 7. 4. 5. 1.- P-values for the Anderson-Darling Test of Normality: FHT data at the Run Level.

Run No	1	2	3	4	5	6	7	8
p-value	0.218	0.195	0.790	0.227	0.057	0.371	0.768	0.130

Levene's test for the equivalence of sample variances was performed on the run FHT data. Table 7. 4. 5. 2. shows that four out of the 28 comparisons have significantly different variances. Those values that are significantly different are marked in red.

Table 7. 4. 5. 2.- P-values for Levene's Test for the Equivalence of Variance: FHT data at the Run Level.

Run No.	1	2	3	4	5	6	7	8
1								
2	0.081							
3	0.656	0.200						
4	0.834	0.043	0.504					
5	0.323	0.004	0.147	0.426				
6	0.620	0.215	0.962	0.471	0.132			
7	0.256	0.562	0.493	0.167	0.029	0.520		
8	0.289	0.548	0.533	0.195	0.039	0.562	0.967	

The unpooled two sample t-test for the equivalence of sample means was performed on the run FHT data. Table 7. 4. 5. 3. shows that four out of the 28 comparisons have significantly different means, all involving run one. Those values that are significantly different are marked in red.

Table 7. 4. 5. 3.- P-values for unpooled twosample t-test for the equivalence of sample means: FHT data at the Run Level.

Run No.	1	2	3	4	5	6	7	8
1								
2	0.041							
3	0.110	0.740						
4	0.001	0.110	0.083					
5	0.056	0.860	0.660	0.230				
6	0.011	0.450	0.320	0.430	0.630			
7	0.022	0.700	0.510	0.240	0.880	0.710		
8	0.068	0.900	0.850	0.110	0.780	0.410	0.630	

The Anderson-Darling test for normality was performed on the run IHDS data. Table 7. 4. 5. 4. shows that six out of the eight runs had p-values below the 95% confidence interval indicating that these runs are non-normally distributed. This high percentage of non-normal distributions arises from the fact that section b1 generally had higher than expected initial drainage surface heights.

Table 7. 4. 5. 4.- P-values for the Anderson-Darling Test of Normality: IHDS data at the Run Level.

Run No.	1	2	3	4	5	6	7	8
p-value	0.000	0.000	0.267	0.061	0.017	0.000	0.000	0.018

Levene's test for the equivalence of sample variances was performed on the run IHDS data. Table 7. 4. 5. 5. shows that two out of the 28 comparisons had values below the 95% confidence interval, indicating significant differences in variance for those runs. Values that are significantly different are marked in red.

Table 7. 4. 5. 5.- P-values for Levene's Test for the Equivalence of Variance: IHDS data at the Run Level.

Run No.	1	2	3	4	5	6	7	8
1								
2	0.345							
3	0.303	0.836						
4	0.022	0.240	0.082					
5	0.895	0.352	0.276	0.015				
6	0.660	0.609	0.660	0.076	0.710			
7	0.068	0.293	0.178	0.868	0.065	0.139		
8	0.076	0.475	0.255	0.634	0.063	0.198	0.604	

The unpooled two sample t-test for the equivalence of sample means was performed on the run IHDS data. Table 7. 4. 5. 6. shows that 17 out of the 28 comparisons have significantly different means. Those values that are significantly different are marked in red.

Table 7. 4. 5. 6.- P-values for unpooled twosample t-test for the equivalence of sample means: IHDS data at the Run Level.

Run No.	1	2	3	4	5	6	7	8
1								
2	0.000							
3	0.000	0.001						
4	0.000	0.003	0.780					
5	0.000	0.000	0.510	0.830				
6	0.000	0.015	0.900	0.330	0.120			
7	0.000	0.015	0.990	0.840	0.680	0.530		
8	0.001	0.550	0.000	0.001	0.000	0.004	0.005	

The Anderson-Darling test for normality was performed on the run PSS data. Table 7. 4. 5. 7. shows that two out of the eight runs had p-values below the 95% confidence interval, indicating that these runs are non-normally distributed. Those values that are significantly different are marked in red.

Table 7. 4. 5. 7. - P-values for the Anderson-Darling Test of Normality: PSS data at the Run Level.

Run No	1	2	3	4	5	6	7	8
p-value	0.233	0.486	0.277	0.045	0.592	0.443	0.308	0.032

Levene's test for the equivalence of sample variances was performed on the run PSS data. Table 7. 4. 5. 8. shows that none of the 28 comparisons had values below the 95% confidence limit, suggesting that all runs had constant variance.

Table 7. 4. 5. 8.- P-values for Levene's Test for the Equivalence of Variance: PSS data at the Run Level.

Run No.	1	2	3	4	5	6	7	8
1								
2	0.943							
3	0.561	0.655						
4	0.378	0.464	0.736					
5	0.184	0.285	0.521	0.830				
6	0.165	0.274	0.518	0.848	0.961			
7	0.700	0.780	0.874	0.639	0.439	0.433		
8	0.963	0.996	0.730	0.565	0.435	0.436	0.825	

The unpooled two sample t-test for the equivalence of sample means was performed on the run PSS data. Table 7. 4. 5. 9. shows that 10 out of the 28 comparisons have significantly different means. Those values that are significantly different are marked in red.

Table 7. 4. 5. 9.- P-values for unpooled twosample t-test for the equivalence of sample means: PSS data at the Run Level.

Run No.	1	2	3	4	5	6	7	8
1								
2	0.540							
3	0.960	0.540						
4	0.980	0.490	0.930					
5	0.430	0.990	0.390	0.330				
6	0.740	0.670	0.750	0.660	0.480			
7	0.110	0.046	0.075	0.081	0.018	0.035		
8	0.002	0.001	0.001	0.001	0.000	0.001	0.010	

7. 4. 6.- Run Level Analysis

Table 7. 4. 6. 1. analyses the results of the statistical tests for run FHT shape and categorises the individual relationships between the properties of IHDS, PSS and FHT shape according to the four scenarios listed above. FHT shape was judged to be different between runs if one or both of the runs had a non-normal distribution.

Table 7. 4. 6. 1.- Scenarios for run FHT shape

	IHDS shape	IHDS var.	IHDS mean	PSS Shape	PSS var.	PSS mean
Scenario 1	1	26	13	15	28	18
Scenario 2	0	0	0	0	0	0
Scenario 3	0	0	0	0	0	0
Scenario 4	27	2	15	13	0	10

All the comparisons have similar FHT shape. In general FHT shape is not well explained by the statistical properties of the control variables; four of the measures have large numbers of scenario four comparisons where different controls give rise to similar FHT shape. The exceptions to this are the variance properties of IHDS and PSS, which have 26 and 28 comparisons respectively, where the controls and the FHT are statistically similar. Table 7. 4. 6. 2. gives the regression relationships between run FHT shape and the properties of IHDS and PSS for scenario four. Standard deviation was used in place of variance for the regression relationships.

Table 7. 4. 6. 2.- Regression relationships between difference in run FHT shape and statistical properties of run IHDS and PSS.

Variable	n	$\hat{\beta}_0$	$\hat{\beta}_1$	r^2
IHDS shape	27	0.0683	1.9676	0.269
IHDS stdev	-	-	-	-
IHDS mean	15	-0.0210	-8.2840	0.000
PSS shape	13	-0.0460	-0.7655	0.175
PSS stdev	-	-	-	-
PSS mean	10	0.7575	0.4287	0.354

Table 7. 4. 6. 3. analyses the results of the statistical tests for run FHT variance and categorises the individual relationships between the properties of IHDS, PSS and FHT variance according to the four scenarios listed above.

Table7. 4. 6. 3.- Scenarios for run FHT variance

	IHDS shape	IHDS var.	IHDS mean	PSS Shape	PSS var.	PSS mean
Scenario 1	1	22	12	13	24	16
Scenario 2	4	0	3	2	0	2
Scenario 3	0	4	1	2	4	2
Scenario 4	23	2	12	11	0	8

Table 7. 4. 6. 3. shows a similar situation to that for run FHT shape. In general the statistical properties of the controls are poorly related to FHT variance with most showing convergence from statistically different values of control to statistically similar values of FHT variance. Again the exceptions are the variances of PSS and IHDS which have 24 and 22 comparisons respectively which have statistically similar controls and statistically similar FHT variance. Table 7. 4. 6. 4. gives the regression relationships between run FHT shape and the properties of IHDS and PSS for scenarios two, three and four. Standard deviation was used in place of variance for the regression relationships.

Table7. 4. 6. 4.- Regression relationships between difference in run FHT variance and statistical properties of run IHDS and PSS.

Variable	n	$\hat{\beta}_0$	$\hat{\beta}_1$	r^2
IHDS shape	27	-0.0003	0.0005	0.000
IHDS stdev	-	-	-	-
IHDS mean	16	-0.0025	-0.9035	0.240
PSS shape	15	-0.0012	-0.0027	0.074
PSS stdev	-	-	-	-
PSS mean	12	-0.0006	-0.0000	0.000

Table 7. 4. 6. 5. analyses the results of the statistical tests for run FHT mean and categorises the individual relationships between the properties of IHDS, PSS and FHT mean according to the four scenarios listed above.

Table7. 4. 6. 5.- Scenarios for run FHT mean

	IHDS shape	IHDS var.	IHDS mean	PSS Shape	PSS var.	PSS mean
Scenario 1	1	23	13	12	24	14
Scenario 2	4	1	4	1	0	0
Scenario 3	0	3	0	3	4	4
Scenario 4	23	1	11	12	0	10

Table 7. 4. 6. 5. shows a familiar pattern - run FHT mean is poorly related to the majority of variables, except IHDS and PSS variance. Table 7. 4. 6. 6. gives the regression relationships between run FHT mean and the properties of IHDS and PSS for scenarios two, three and four.

Table7. 4. 6. 6.- Regression relationships between difference in run FHT mean and statistical properties of run IHDS and PSS.

Variable	n	$\hat{\beta}_0$	$\hat{\beta}_1$	r^2
IHDS shape	27	-0.0018	-0.0086	0.129
IHDS stdev	-	-	-	-
IHDS mean	15	0.0014	2.0001	0.518
PSS shape	16	-0.0014	-0.0054	0.064
PSS stdev	-	-	-	-
PSS mean	14	-0.0004	0.0003	0.007

7. 4. 7. Discussion of Analysis at the Section Level.

It would appear from the data that the statistical properties of FHT are independent of the control of IHDS at the section level. Whilst some properties such as IHDS shape and IHDS variance have a good aleatory relationship with the properties of FHT, the controls and the response exhibiting similar statistical properties, it would appear that the success of these relationships is primarily dependent on the fact that FHT arrives at a statistically similar value fairly constantly; when FHT does not arrive at this value, there is not an aleatory relationship present.

For example, the shape of the FHT distributions has a fairly good relationship with both the shape and the variance of IHDS distributions; when the FHT distribution is similar, the IHDS distribution is similar in 96 and 89 comparisons respectively. However, when a significant difference in FHT shape does occur, as it does in 44 comparisons, the shape and variance of IHDS cannot explain why this difference occurs. Indeed, table 7. 4. 4. 1. shows that whilst FHT shape differs, the variance and shape of IHDS distributions remain the same.

Similar situations occur for the variance of the FHT distributions and the shape and variance of IHDS distributions. Table 7. 4 .4. 3. shows that in the majority of cases the variance of FHT and the shape and variance of IHDS are similar; however, in those cases in which the FHT variance is not similar, this difference cannot be explained in terms of IHDS shape and variance; sometimes similar IHDS variance gives different FHT variance and sometimes different IHDS variance gives similar FHT variance.

A conclusion that can be inferred from this behaviour is that it is coincidence that the statistical properties of FHT are well related to the statistical properties of IHDS shape and variance; it seems to be that these variables are fairly consistent in the experimental design and it just so happens that the properties of FHT tend toward a consistent value; this leads to the result that the controls and the final product are statistically similar. However, in those cases where FHT does vary, it is shown to be independent of changes in the properties of IHDS.

This conclusion draws support from the behaviour of IHDS mean. Whilst the variance was well controlled within a run, this led to the result that any differences in mean were judged to be significant and it was difficult to make the mean consistent between runs. As a result, a converse situation results to that applied to IHDS shape and IHDS variance - the control variable is rarely statistically similar. However, with IHDS mean, although the control variable differs, the statistical properties of FHT are independent of any changes in it. The relationship between the statistical properties of FHT and IHDS mean appear random with all types of scenario occurring. Scenarios one and four collectively dominate the relationship because FHT converges to a statistically similar solution in 68.50% comparisons. However, this result does seem independent of the properties of IHDS.

For all the statistical properties of IHDS, the relationship to FHT appears to be random - sometimes similar values of IHDS will produce similar values of FHT, sometimes similar values will produce different values of FHT - sometimes different values of IHDS will produce different values of FHT and sometimes different values will produce similar values of FHT.

Intuitively it is clear that the statistical properties of IHDS, and in particular, the mean value of IHDS should affect the final height of morphology; reasoning by extremes, a zero value of mean IHDS will produce different morphology to a 10 m value of mean IHDS. Given that the response to the statistical properties of IHDS is apparently random at the reach level, how can the affect of IHDS on experimental conditions be understood?

The seemingly random nature of the relationship between IHDS and FHT in aleatory terms may suggest that we are operating within a larger distribution space than expected. The distribution space which may be appropriate is the collective distributions of shapes, variances and means, rather than the shapes, variances and means of the collective data points; for example, the shape, variance and mean of the distribution of variance may be analysed. This is moving up a level of analysis and coarsening the resolution at which determinism becomes possible. This coarsening of scale and the formation of the distributions of shape, variance, and mean may be done in two ways 1) Within a run, so that all the section values are incorporated into the statistical features of a run distribution 2) Incorporating all section values over all runs into a distribution. The first strategy for forming larger distributions (the formation of a run distribution) is performed in the next section. The second strategy would require the repeating of the experiments in order to compare the distribution of section values performed over eight runs.

If one of the scenarios of divergence or convergence was predominant, it would be possible to coarsen the scale of resolution in one dimension only. For example, if constant statistical properties of IHDS gave rise to different values of the statistical properties of FHT it would be possible to work with the scale of the shape, variance and mean of the data for IHDS and devise a larger distribution space for FHT which would use the collective distribution of shapes, variances and means. If different statistical properties of IHDS gave rise to similar values of the statistical properties of FHT then it would be appropriate to devise a larger distribution space for IHDS which would use the collective distribution of shapes, variances and means, whilst FHT would use the shape, variance and mean of the data. However, because both convergence and divergence are present, it seems necessary to use larger distribution spaces in the two dimensions of FHT and IHDS.

Given that at this level of analysis it appears that FHT is randomly related to IHDS, this leaves the result that with constant discharge, the shape of section FHT is statistically similar in 68.58% of comparisons, the variance of FHT is statistically similar in 90% of comparisons and the mean of FHT is statistically similar in 68.58% of comparisons.

The most immediately striking feature of these statistics is that sectional shape and sectional mean both have the same number of sections that differ significantly (31.42%). This leads to the issue of whether those sections which have different shape also have different mean. The answer is that only 16 (36.36%) out of the 44 comparisons which have different mean also have different shape. There appears to be no substantial connection between mean and shape.

The prediction of section final height given constant discharge is fairly good (68. 58%). However a judgement needs to be made on the remaining 31. 42% of the comparisons which are different. The question arises whether we have produced a deterministic relationship at this level of analysis and the 31. 42% of comparisons that are significantly different are due to errors in measurement. Two pieces of evidence seem to refute the view that the 31. 42% of comparisons which show significant differences are due to error. The first, considered in the application of causal laws, was the precision with which measurements were made (see p. 254 - 255 this thesis for a discussion of error and the precision of measurements).

The second piece of evidence refers to the effect of sediment transport, which was also considered in the application of causal laws. It has been suggested that the sediment transport rate and its variance with distance from source, acts as a strong constraint on final height. Although variance in sediment transport rate with distance from source leads to a heterogeneous regionalisation of the final height over the whole of the channel, (high heights near source, low heights near mouth), it must also necessarily result in a progressive smoothing of heights from source to mouth, which has the consequence that within individual regions of the flume, final height should be fairly homogeneous.[168] As has already been discussed, dependence cannot be seen at the level of the section, because there is no trend; data varies randomly about a flat mean, whereas at the run level, data varies randomly about a decreasing mean.

Given the homogenising action of sediment transport within a reach, there is a process at work that should force convergence to statistically similar values of FHT at the reach level. This, combined with the accuracy of the measurements, suggests that the presence of significantly different FHT shapes and means indicates that there is a mechanism at work in the channel that promotes differences in the shape and mean of the reaches, given constant discharge. That is, the reaches that are significantly different are not due to error, but are a fundamental response of the fluid-sediment system.

Given this fact that the differences in shape and mean FHT are considered a fundamental response of the system, it needs to be asked at which level of analysis does the shape and mean of FHT become deterministic; again there are two possibilities for coarsening the level of investigation 1) Within a run so that all the section values are incorporated into the statistical features of a run distribution 2) Incorporating all section values over all runs into a distribution. Comments on the behaviour of FHT at the section level must be set in the context of the behaviour of FHT at the run level. This is examined next.

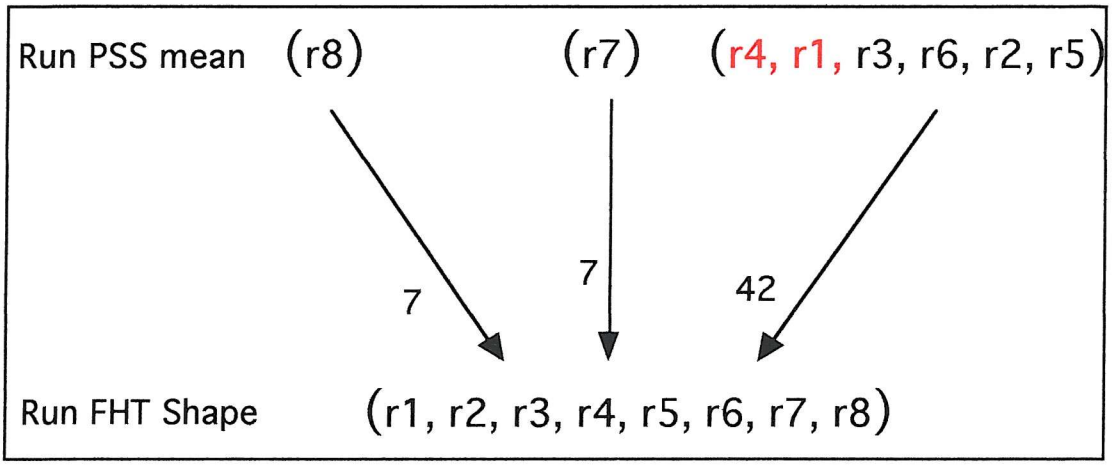
[168] This statement results from the idea that sediment *en masse* cannot jump the queue; it is highly improbable that in the middle of the channel length (half way between source and mouth), the channel will reach base level, because all the sediment has been removed from that region and deposited downstream. Rather the sediment in this section must, in general, wait until the reaches nearer the mouth have had their sediment removed.

7. 4. 8. Discussion of Analysis at the Run Level.

The data at the run level suggests a similar conclusion to that in the section analysis - that the statistical properties of the control variables of IHDS and PSS are not well related to the statistical properties of FHT. The exceptions to this are IHDS variance and PSS variance which have a large proportion of scenario one cases for all of the relationships with FHT. Again it is expected that the relation of the variance of IHDS and PSS to the properties of FHT is largely coincidental - they do not vary for runs, but because FHT is converging to similar solutions it appears as if they are influential in bringing this convergence about. The evidence for this coincidence is less than that for the sectional level because FHT converges to a statistically similar solution much more often. For example, PSS variance has no significantly different comparisons and neither does FHT shape. In this situation it is not possible to refute the statement that the presence of constant PSS variance causes the shape of the FHT distribution to become constant, because there is no evidence to the contrary. Disproval of this claim would require further experimentation where every variable, apart from the variance of PSS was kept constant, in order to examine the effect that changing PSS variance had on FHT shape. However, it can only be stated that it is strongly suspected that the relationship between the variance variables does not cause the statistical properties of FHT to converge, but is coincidental to that convergence.

Because the statistical properties of FHT converge, the scenario situations appear to be less complex than for the section level. Figure 7. 4. 8. 1. is a diagram which shows the different distributions present for PSS mean and the different distributions present for FHT shape. A distribution is indicated by brackets, and runs within those brackets are judged to have statistically similar values. Because statistical properties are continuous, there is a spectrum of distributions. In some cases, runs might be judged to belong to two distinct distributions; these are shown on the diagram as red values. A red value belongs to the distribution it is placed in, but equally could be placed in the distribution to its immediate left. For example in figure 7. 4. 8. 1. r4 and r1 have similar PSS mean to r3, r6, r2 and r5, but are also judged to be similar to r7. The arrows and their numbers indicate how many comparisons move from a given distribution in PSS mean to the distribution in FHT shape. The maximum number of comparisons for each run is seven, giving a total of 56.

Figure 7. 4. 8. 1.- Membership of PSS Distributions Proceeding to Membership of FHT Distributions



The relationships between FHT shape and the control variables follow the same pattern as that shown in Figure 7. 4. 8. 1. Significantly different values of the control variables all produce the same form of response - i.e. a normal distribution. This suggests that the shape of FHT is not affected by changes in the control variables, over the range used in these experiments. Because all the different distributions converge to a similar solution, a deterministic relationship can be found by combining the three PSS distributions into one. The shape of FHT is then deterministically related to the shape, variance and mean of the combined distribution.

The above data suggest that FHT shape is stable to changes in the control variables. Given constant discharge, it can be predicted that the FHT values will assume a normal distribution for a run, even if other control variables differ.

The situation is complicated for FHT variance and mean, because of the presence of significantly different values of these variables. FHT variance and mean are generally predictable in terms of the control variables, but explanation of the different values of FHT variance and mean in terms of the control variables is difficult. Figure 7. 4. 8. 2. presents a diagram showing the different distributions for IHDS mean and FHT mean and the relationship between them. The notation is the same as that for figure 7. 4. 8. 1. (see above).

Figure 7. 4. 8. 2.- Membership of IHDS Distributions Proceeding to Membership of FHT Distributions

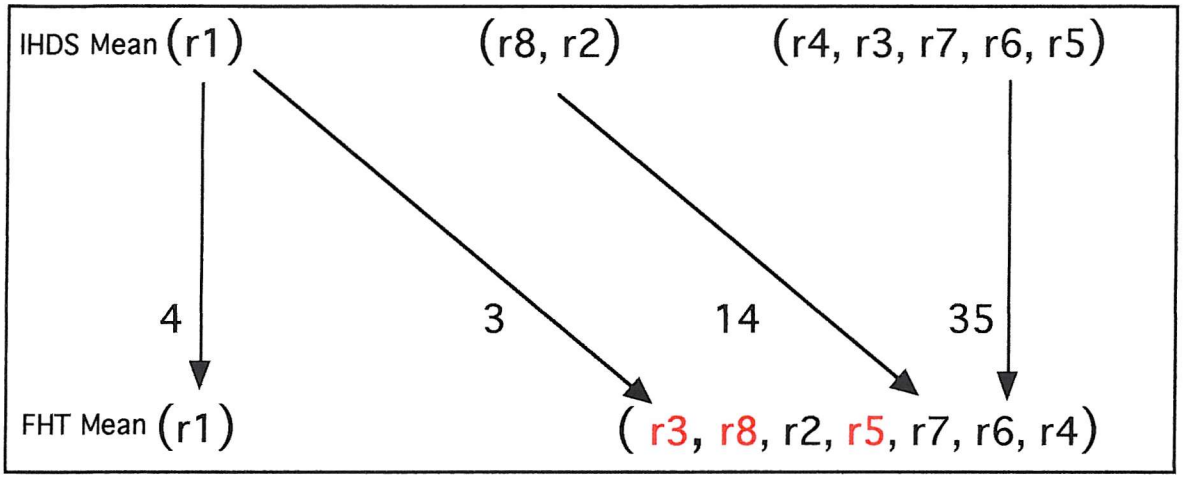


Figure 7. 4. 8. 2. shows that the two distributions of IHDS mean (r8, r2) (r4, r3, r7, r6, r5) converge to the same distribution of FHT mean (r3, r8, r2, r5, r7, r6, r4). Every comparison for these runs involve membership to a given FHT mean distribution. It can be suggested that if the properties of these two distributions are taken together, then a deterministic relationship exists between IHDS mean and FHT mean.

The difficulty is due to run one (r1): although its IHDS mean belongs to a single distribution, it can produce FHT data that suggest it belongs to two different FHT distributions. If the IHDS mean distribution (r1) produced exclusive membership to the FHT distribution (r3, r8, r2, r5, r7, r6, r4), then the three IHDS mean distributions could be taken together to produce a deterministic relationship between IHDS mean and FHT mean. Alternatively, had the IHDS mean distribution (r1) produced exclusive membership to the FHT mean distribution (r1), a deterministic relationship between IHDS mean and FHT mean would have existed, plus information on the critical limits of that relationship. These critical limits would have been defined by the range between the minimum and maximum values of the combined IHDS mean distribution (r8, r2, r4, r3, r7, r6, r5) which are run 8 and run 5 respectively (the range is 0.00182 m or 1.82 mm).

Figure 7. 4. 8. 2. suggests that the relationship between IHDS mean and FHT mean is complex and most likely non-linear. This conclusion is derived from the fact that whilst the IHDS mean distribution which (r1) is part of, is distinct from the other two IHDS distributions, it produces seemingly random behaviour in terms of FHT. When data can belong to two distributions with different means, it is expected that the piece of data may share characteristics associated with either distribution. However the interaction between IHDS (r1) and FHT is complete; for example run one which has the lowest IHDS mean produces the same morphology as that of run five which has the highest IHDS mean. Conversely run one can produce different morphology to run two, even though these runs have comparable IHDS mean. IHDS mean for the distribution which r1 belongs to, can produce the same characteristics as those runs which have values of IHDS mean that are located well within an alternative distribution.

This phenomenon may be explained by the suggestion that there is considerable interference between mean values of IHDS and FHT over a given range. The process that is active in fluid flow, whatever it may be, that causes convergence from different IHDS means to similar FHT means, obscures or limits the effect of changing IHDS mean over a given range. Only when the difference in IHDS mean reaches a critical limit will the differences in FHT be free from noise - it is not possible to state from these experiments what that difference is, except it involves a lower IHDS mean than that present in run one. In other words, we may only compare the effects of IHDS mean if large enough gaps are left in the distributions of interest. Using integers to represent distributions, there may be a deterministic relationship if distributions 1 and 3 are compared and 2 and 4 are compared. Alternatively, a deterministic relationship may only be present using distributions 1 and 4 and distributions 2 and 5 etc.

The question of statistical significance versus physical significance may suggest that the difference in IHDS mean is negligible and the difference in FHT mean is negligible and that the situation really represents constant IHDS mean giving rise to constant FHT mean. The difference in IHDS mean is fairly small; if we take the highest value of IHDS mean as 100%, then the range between the lowest and highest value of IHDS represents only 4.98% of that total. However this small difference produces larger differences in FHT mean; it seems reasonable to suggest that the maximum height of the drainage surface does not represent 100% for the FHT, because from experimental evidence it has been shown that a flow of one litre per second cannot remove the entire drainage surface. A more reasonable estimate, based on experimental evidence, is to suggest that the value equal to 100% of the FHT surface should be the highest IHDS height minus the lowest FHT value - or the greatest erosional capability of the flow. Using this figure, the range between the highest and lowest value of FHT represents 24.69% of that total. Subjectively, this seems to be a physically significant value.

With the exception of four comparisons with significantly different means and four comparisons with significantly different variances, given constant discharge, the statistical properties of FHT at the run level is predictable. This predictability remains even when changes in the control variables of PSS and IHDS occur; a key area of investigation is defining the limits of this stability for the control variables.

The aleatory stability of run FHT suggests several important points. Firstly, the sediment transport system taken as a whole, produces statistically similar morphology when discharge is constant. This is an expected result; the discharge provides the energy for sediment transport and therefore sets strong constraints on the diversity of morphology that is present. Secondly, the stability is an aleatory one and this places a different interpretation on the data. Even at the run level of analysis there is diversity in the final form. If run one is ignored, because its control variables show some sign of being at the critical limits of stability, this diversity is not due to differences in the controls. As far as present knowledge goes, this diversity is not due to a failure to consider the whole range of control variables (equifinality) nor due to differences in the time evolution of the system (equilibrium). Diversity is not error, but a natural and inherent part of the system. Discharge sets limits to the forms of morphology that can be developed, but within those limits, there is freedom for diversity.

In order to properly ascertain the distribution of statistical properties for a given discharge, another set of eight runs would have to be performed under similar conditions and the distribution of the statistical properties of those runs compared to the eight runs given here.

7. 4. 9. Reconciling the Section and the Run Level

Two factors that promote a uniform morphology given constant discharge, have now been discussed. The first of these is the idea of the sediment transport queue; this mechanism promotes homogeneous morphology within a section - section b1 will never have a lower mean morphology than section b5. Secondly, constant discharge limits the amount of energy for sediment entrainment, and given constant discharge, the sediment transport system as a whole, forms statistically similar morphology. Two pertinent questions are how can differences in morphology at the section level occur given these constraints and can these differences be predicted?

For a stable distribution, aleatory theory attributes differences to the entities within that distribution as due to chance. This is best illustrated by a visual example (see Figure 7. 4. 9. 1.)

Figure 7. 4. 9. 1.- An Example of a Population with Defined *a priori* Probabilities

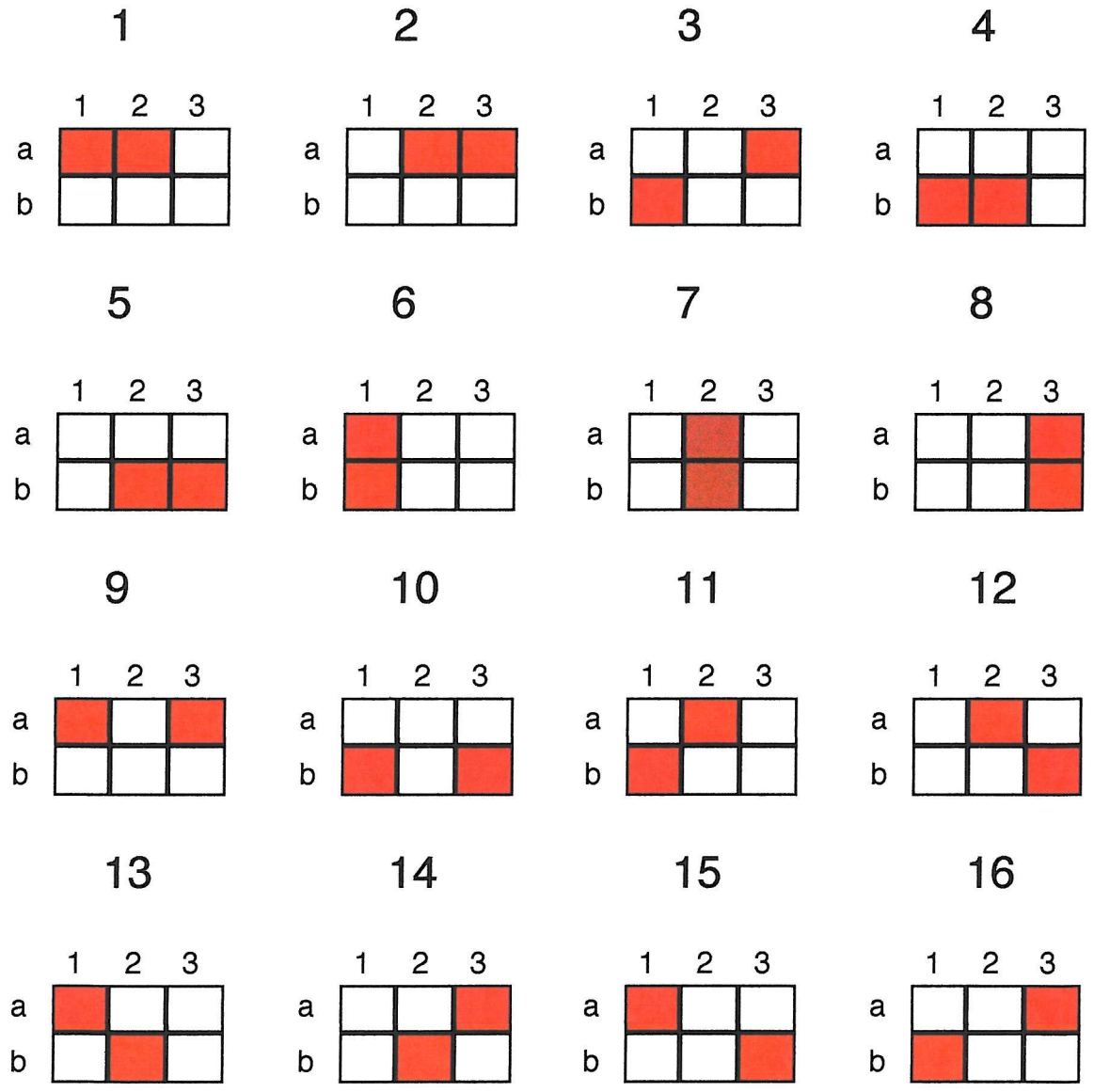


Figure 7. 4. 9. 1. shows a system of six squares which can have two states, either red or white. All possible arrangements for this system are shown. The grid of six squares is divided into a co-ordinate reference system, so that it can be stated whether a square for example at a1 is red or white.

If numeric values of zero are assigned to the red or empty squares and one to the white tiled squares, it is easily seen that each of the sixteen grids has the same overall pattern or numeric average (4/6 or 0.67). With absolute certainty, it could be predicted that an average for all six grids would be 0.67. What happens though if this system is treated in the same way as the flume, and attention focuses on individual sections. Comparing between all combinations of arrangements for the cell a1, three possible outcomes are possible; both a1 cells may be red (as in arrangements one and six); both a1 cells may be white (as in arrangements two and three) or one a1 cell may be red and the other may be white (as in arrangements one and two). If comparisons are made between all possible arrangements for all cells, 720 different comparisons are made (120 for each cell, multiplied by six cells). Of these 720 comparisons, 70 (9.72%) result in two red squares, 310 (43.05%) result in two white squares, and 340 (47.23%) result in one white and one red square.

The sixteen arrangements above are exhaustive for six squares with two red and four white values. In this respect they represent the true statistical population where all possible variations are accounted for. The percentage results above, and the associated probabilities that they represent (0.0972, 0.4305, 0.4723) give the true probabilities of finding red/red, white/white, red/white, respectively. It would be expected that if grids were randomly generated and compared, that as the number of comparisons approached 720, (the total number of combinations for six grids and all sixteen arrangements) these percentages or probabilities would be approached.

What these probabilities indicate, is that for a system defined by six squares with two red (numeric value zero) and four white (numeric value one) and a fixed average, when any individual square is measured, 52.77% of the time (9.72% - red/red + 43.05% white/white) they will have the same value. Conversely, when any individual square is measured, 47.23 % of the time different values will be derived, solely because the same system can have many different arrangements; at the instant it is measured, a system defined by a given criterion (six squares - two red, four white) may be in a different arrangement to another system defined by exactly the same criterion.

Aleatory theory assumes that differences in section morphology are due to chance; whilst the run level has a deterministic distribution, any variations within that distribution are due to different arrangements of the system. In figure 7. 4. 9. 1. the number of times that we can expect to find a different result for an individual section is specified by the defined properties of the system. In the flume, this is not the case. Hypothesis tests in statistics deal with crucial limits of difference, but the phenomenon they measure is continuous; that is, there are different degrees of difference. There are a large and unspecified number of ways that the flume can generate morphology that fits the mean FHT for a run. It is clear from the data that the number of sections in a run that will be significantly different to other runs is not fixed. It seems probable that the number of sections that are different in the whole of the experimental ensemble (i e. 31.42%) is also not a fixed value, given that the number of arrangements of states for the fluvial system is large. That is, if another set of eight runs was performed, it is unlikely that 31.42% of the sections would be significantly different from one another due to the large varieties of arrangements the system can take within the mean value of a whole run.

In terms of aleatory theory, the differences in section are due to chance arrangements of the sediment system. It is also unlikely, although it requires further experimentation, that the probability of getting a significantly different section, given constant controls, can be defined because the number of arrangements is large and unspecified.

7. 5. Testing the Chaos Hypothesis for the Fundamental Data Set

This section tests the hypothesis that "Chaos relationships between entities best explain the indeterminacy of alluvial river channel cross-section and alluvial river channel planform." Chaos is thought to be applicable to fluvial geomorphology through the transmission of force via turbulence in fluid flows. Work has shown that turbulence is governed by chaos mathematics.[169]

The experiments were initially constructed with the aim of identifying sensitive dependence on initial conditions by default. That is, if constant controls produce different morphology and these differences cannot be explained by causal or aleatory methods, it suggests that sensitive dependence is present in the system.

In several ways, this scheme has met with limited success. The causal model, through differences with distance from source, has indicated that the morphology for the runs is not the same. Similarly, the aleatory model has suggested that an invariant distribution of morphology is not present at the reach level. Both of these models have shown that the differences in morphology are independent of differences in control variables. Given that the control variables do not produce the changes, the alternative is that the internal development of the fluid-sediment system is mainly responsible for any given morphology within a run, and that this internal development of the system can take different forms given similar controls.

A positive or pro-active identification of sensitive dependence and strange attractors is difficult, primarily because of the amount of data required and secondly because of the equipment required to collect this information. It could be possible to perform a Ruelle Takens reconstruction of varying FHT data at a single point in the flume, sampled over time. However, experiments which use this method require large data sets (Yepez *et al.* (1989) use 2000 data points) and would need measuring equipment that would not interfere with the flow. A possible data collection method would be two-phase photogrammetry using a digital camera, but this technique is a developing field, not without its problems (Winterbottom and Gilvear; 1997). There was neither the time nor the resources to collect data in this manner.

[169] See **Section 5. 3. 2.** for a new hypothesis in fluvial geomorphology and **Sections 4. 5. 3.** and **4. 5. 4.** for a description of turbulence as a chaos phenomenon.

An alternative method to constructing a strange attractor, is to identify whether the topography produced by the flow is fractal or not.[170] The self similarity of strange attractors on all scales and the ability to fill finite space without their trajectories intersecting is a consequence of their fractal dimension (Rasband, p. 76; 1990 - See p. 115 this thesis for a brief description). Rasband (p. 71; 1990) states that "For a practical definition we take a strange attractor to be an attracting set with fractal dimension." Detailed discussions of fractals and how they relate to strange attractors are given in Mandelbrot (1982; 1992) and Baker and Gollub (1990).

The presence of a constant non-integer dimension for the flume morphology data over different runs would be contributing evidence that the turbulence-sediment system is chaotic. Again it is this constancy in dimension, given constant controls, that is the key issue, not the value of the dimension itself. At present, there is relatively little understanding of the relationship between fractal dimension and physical process (See Rasband, p. 71; 1990). Xu *et al.* comment that the only way to justify the claim that a landscape is self similar is to "...calculate a constant fractal dimension of complex landscape surfaces." If constant controls produce constant fractal dimension, it suggests some property of the morphology is remaining constant, and this is a valuable foundation for a better understanding of fluvial indeterminacy.

An attempt to define the fractal dimension of the flume topography was made using the box-counting method (see Castillo; 1998: Stewart, p. 286; 1997: Xu *et al.;* 1993: Grassberger; 1986 for theoretical details). The programme Fractal Dimension Calculator designed by Paul Bourke (1993) was used to calculate the box dimension for two dimensional contour plots of the data. This programme for Macintosh computers is available from the url address:

http://astronomy.swin.edu.au/pbourke/fractals/fracdim/

The programme Fractal Dimension Calculator uses data in the form of black and white images and overlays boxes of increasing size over the image. The programme counts the number of boxes of a given size r required to cover the black portion of the image. A log10 plot of 'number of boxes of given size r required to cover the image' (y axis) versus 'box size r'(x axis) should produce a linear plot. The trend of this plot is then analysed using least squares regression, and the slope of the line or the $\hat{\beta}_1$ parameter is the fractal dimension of the image.[171] Equation 7. 5. 1. shows the regression model:

[170] Detailed reviews of the use of fractals in topographic problems have been provided by Gao and Zong (1996) and Xu *et al.* (1993).

[171] The fractal dimension of the topography is assumed to be the fractal dimension of the image plus one.

Equation 7. 5. 1.- The Fractal Dimension Regression Model

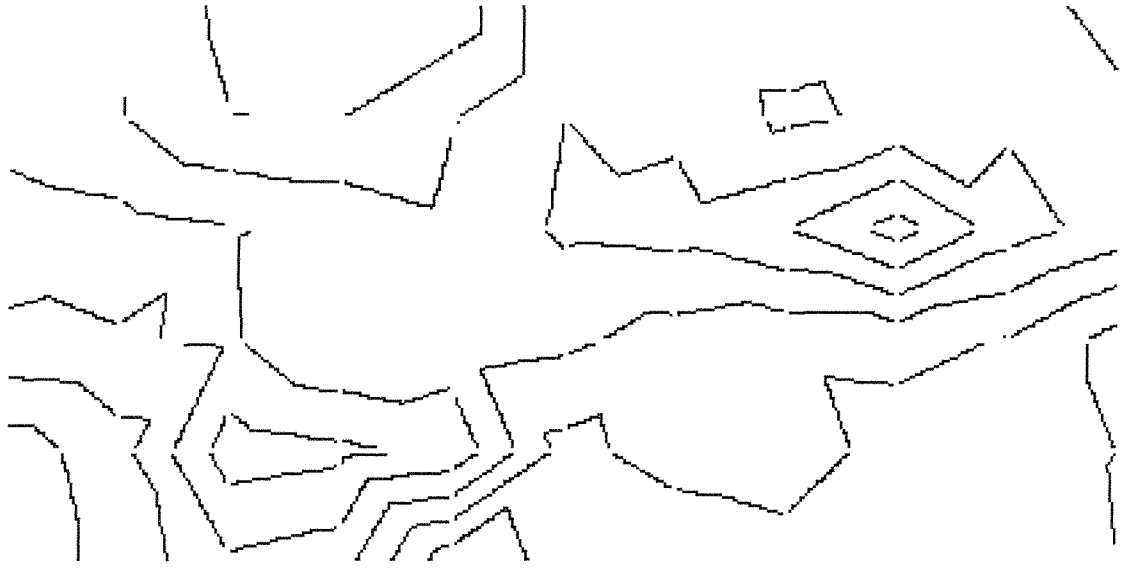
$$\log[N(r)] = \log(c) - D\log(r)$$

Where:

N	=	Number of Boxes
r	=	Side Length of square box
c	=	Intercept
D	=	Box Dimension

The fundamental data set for each section was mapped using the Microsoft Excel wireframe contour plot function. The grid that accompanies these plots was then removed, which caused slight discontinuities to appear in the contours. These wireframe contour plots provided the images for the FDC programme. Figure 7. 5. 1. gives an example of one such image.

Figure 7. 5. 1.- Wireframe Contour Plot of Section c2, Run 4.



A plot of log[N(r)] versus log(r) was derived for each of the 40 sections of flume data, based on the information provided by the FDC. However in none of the reaches was the plot of points linear. The curvature of these plots has been documented by Andrle and Abrahams (1989) and Xu *et al.* (p. 255; 1993). Figures 7. 5. 2. - 7. 5. 5. show the curvature of dimension plots for two sections; the box sizes are the default settings and range from length 1 - 299.

Figure 7. 5. 2.- Fitted Line Plot of Dimension for Wireframe Plot of Section c2, Run Four.

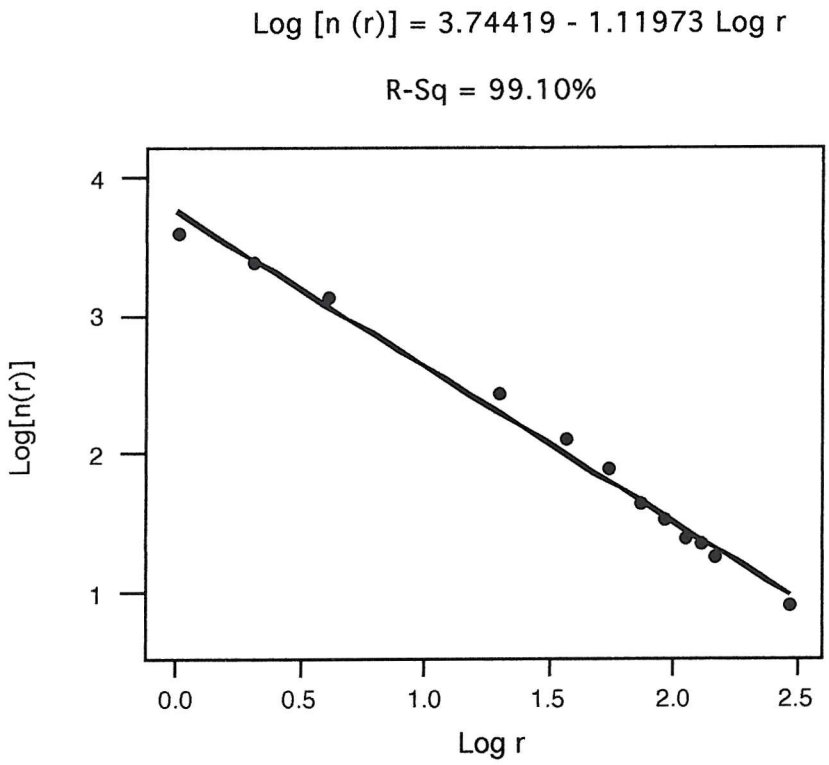


Figure 7. 5. 3.- Residual Data for Fitted Line Plot of Section c2, Run Four.

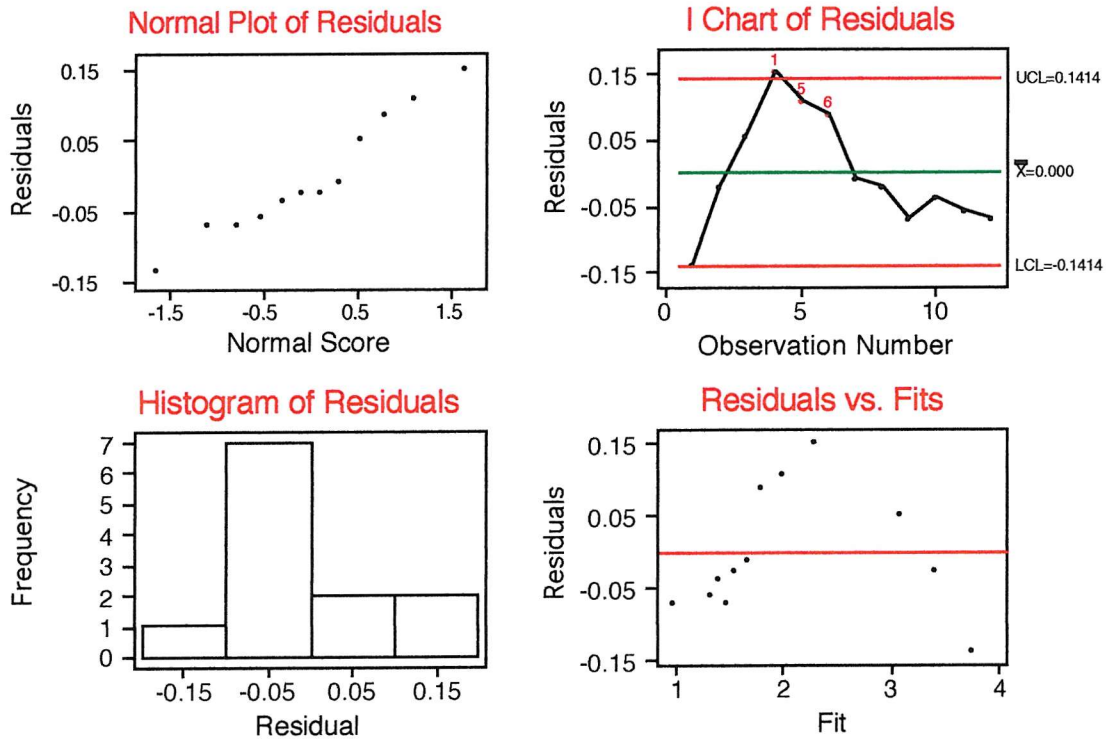


Figure 7. 5. 4.- Fitted Line Plot of Dimension for Wireframe Plot of Section b3, Run Two.

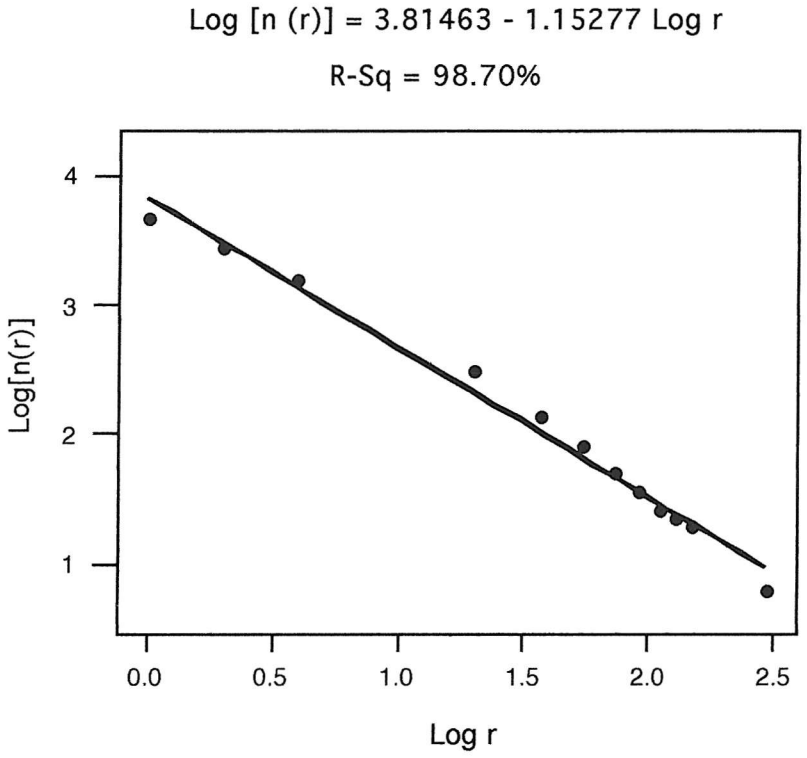
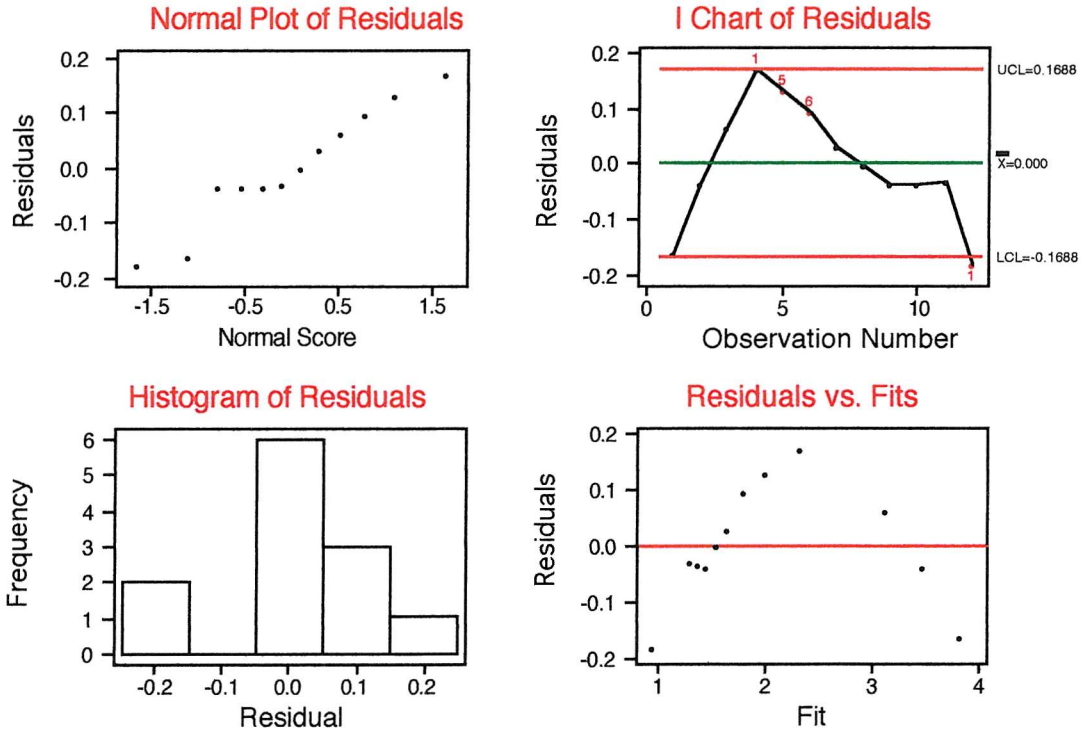


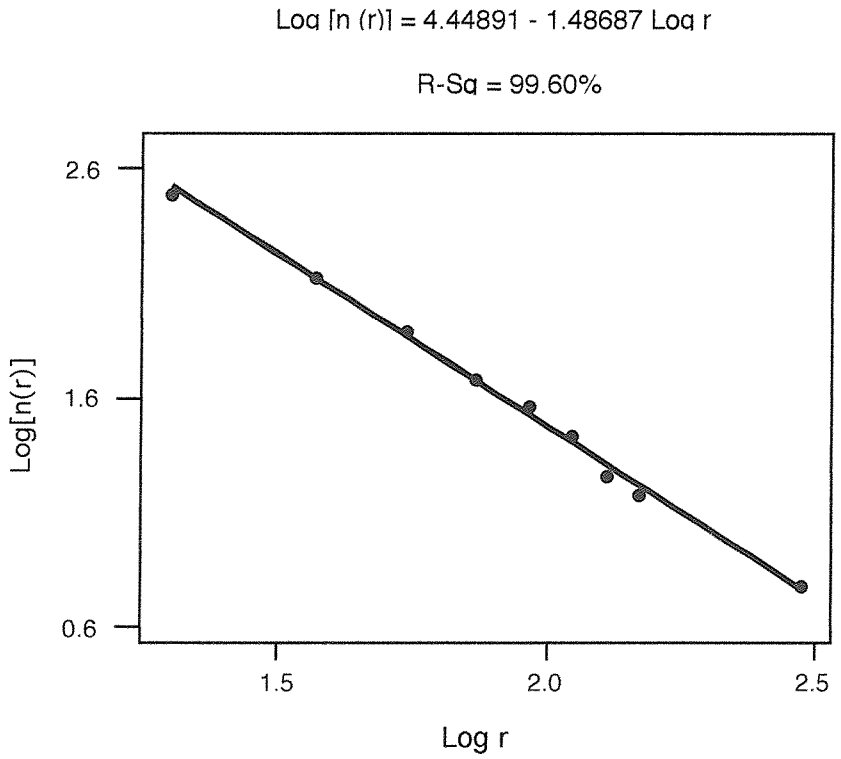
Figure 7. 5. 5.- Residual Data for Fitted Line Plot of Section b3, Run Two.



The residual plots for the regressions indicate that there is consistent curvature in the data. This is particularly evident in the plot for section b3 run two, where the residual versus fit plot shows a near perfect parabolic form. The non-linearity of the log[N(r)] versus log(r) suggests that the data is not self-similar and does not exhibit constant dimension over all scales. The dimension of the topography depends on the scale at which it is viewed (the specific value of log r). This result suggests that the data is not fractal and consequently that the morphology does not have a hallmark of a strange attractor.

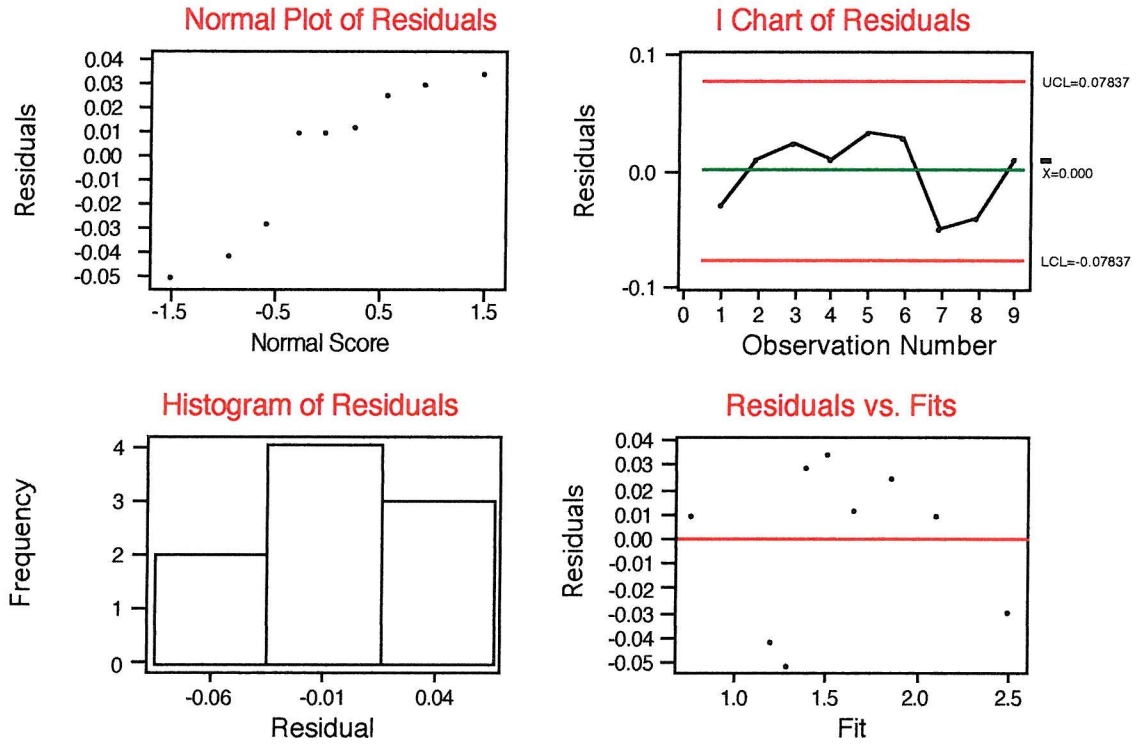
There may be several reasons for this curvature. The first is the possibility of multifractal dimensions, where the curve of the log[N(r)] versus log(r) plot is actually a series of linear segments[172] (Culling and Dakto; 1987: Xu; *et al.*; 1993). The data does not seem to fit the multifractal model; if the data set using the default box sizes is divided into segments, it is clear that these segments are non-linear. In figure 7. 5. 4. it seems reasonable to suggest that there are two slopes present in the data. Figures 7. 5. 5. and 7. 5. 6. show a regression relationship and residual data for the lower slope in figure 7. 5. 5. (between box sizes 20 and 299). Figures 7. 5. 5. and 7. 5. 6. show that curvature is still a feature of this reduced log[N(r)] versus log(r) plot (although the residual versus fit plot is more noisy and the curvature is slightly obscured by some low residuals).

Figure 7. 5. 6.- Fitted Line Plot for Lower Slope in Figure 7.5.4, Box Size 20 to 299.



[172] Mandelbrot (p. 134; 1992) provides an illustration of turbulence as a multifractal phenomenon.

Figure 7. 5. 7.- Residual Data for Lower Slope in Figure 7. 5. 4., Box Size 20 to 299.



It is possible to increase the resolution of the plots by calculating box dimensions for intermediate box sizes. For example, in Figure 7. 5. 5., the first three box sizes are 20, 37 and 55 (in log(r) format they are 1.30, 1.56 and 1.74). It is possible to calculate box dimensions between these intervals and plot the resulting values. One reason for doing this, may be that the non-linearity is due to a large range of box sizes being used, and the dimension is linear only over smaller ranges. Figures 7. 5. 8. and 7. 5. 9. use box sizes of 20, 25, 30, 35, 40, 45, 50 and 55 and therefore fill in the intervals between box size in Figure 7. 5. 5. It is clear from Figures 7. 5. 8. and 7. 5. 9. that the data is non-linear over short increments in box size, the residual versus fit plot again forming a parabola. Box sizes in multiples of two were plotted for sizes between 2 and 24 and the data remained non-linear. This non-linearity is present even using only three data points and is an inherent feature of the data.

Figure 7. 5. 8.- Fitted Line Plot for Intermediate Box Sizes (20 - 55)

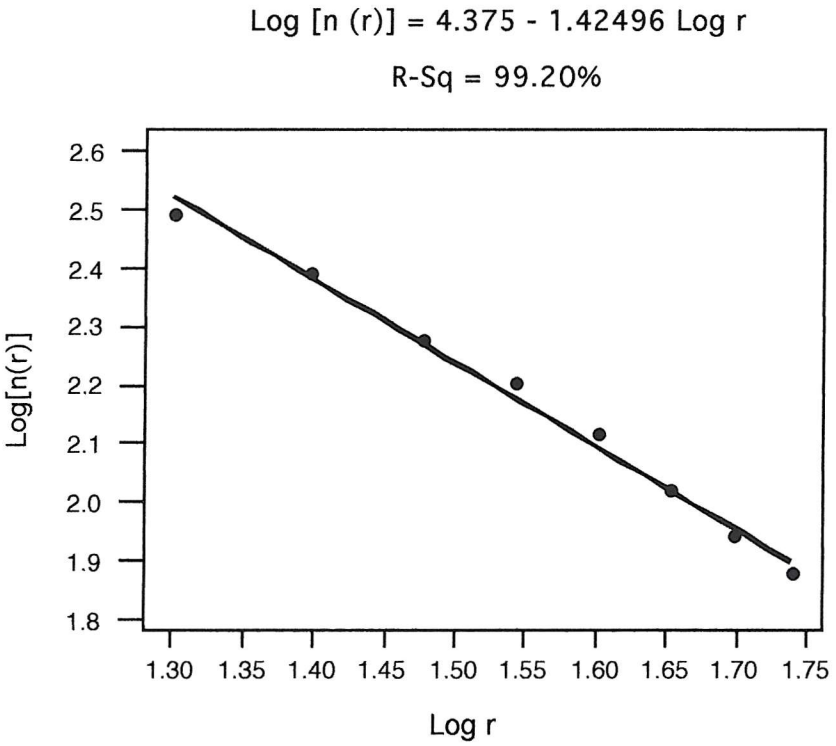
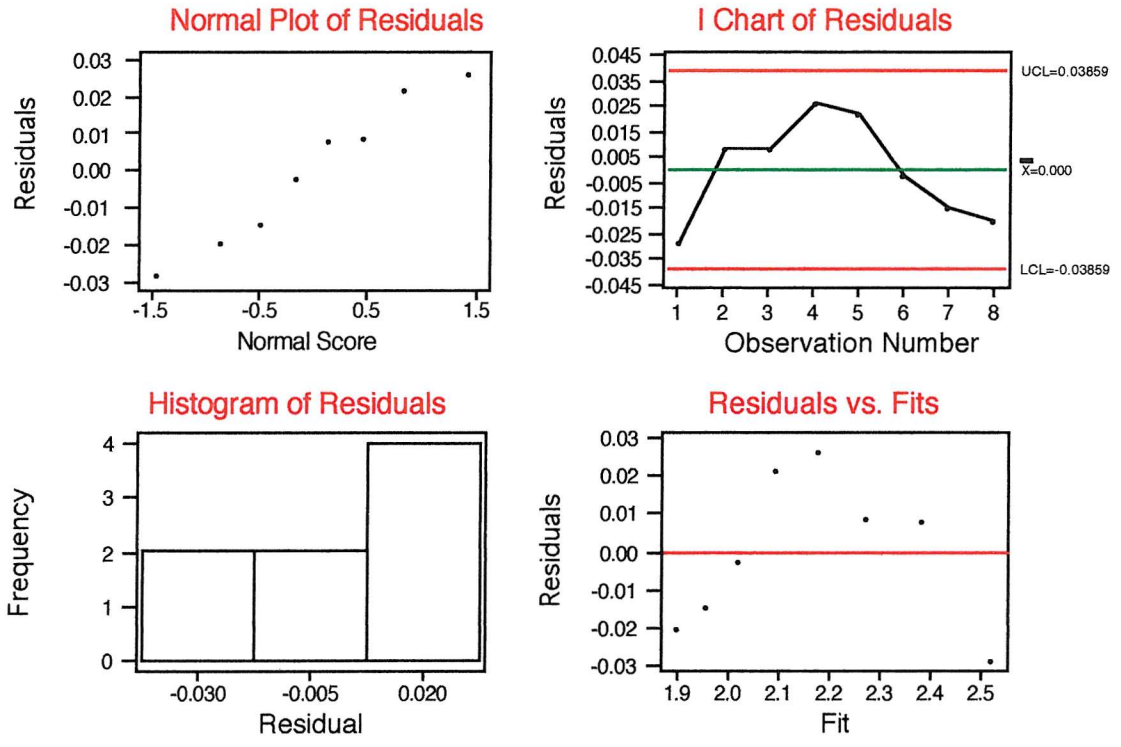


Figure 7. 5. 9.- Residual Data for Intermediate Box Sizes (20 - 55)



A possible explanation for why the data is not fractal/multifractal may be that the data set is not large enough or detailed enough to highlight any fractal properties (see Xu, pp. 256 - 257; 1993). It would certainly be desirable to increase the number of contiguous data points for fractal analysis and this requires further investigation.

An alternative explanation for the lack of a fractal dimension may be the use of the box-counting method. Different means of calculating fractal dimension, for reasons which are not understood, give different fractal dimensions (Klinkenberg and Goodchild; 1992: Xu *et al.*; 1993) and application of another technique may give a fractal plot.

Although the dimension plot is not fractal, it can still provide useful information about the geometric properties of topography (see Kennedy and Lin; 1986: Andrle and Abrahams; 1989). Crucially for this investigation, a comparison can be made to examine whether the dimension plots are similar for the eight runs. Although the data may not be fractal, some aspect of dimension may be constant given the narrow range of controls present in this experiment.

As was seen in Section 7. 3. 4. the residuals for regression analysis should be normally and independently distributed if hypothesis testing on the slope and intercept values is to be carried out. The non-linearity of the trends invalidate these assumptions and comparisons between the log[N(r)] versus log(r) plots cannot be performed using least squares regression analysis.

A close approximation to least squares regressions is to calculate the mean value of slope (dimension) for all possible combinations of data points. The procedure for calculating any individual slope is given in equation 7. 5. 2.

Equation 7. 5. 2.- Approximating Least Squares Slope

$$\beta_1 = \frac{\log[n(r)_{i...n}] - \log[n(r)_{j...n}]}{\log(r)_{i...n} - \log(r)_{j...n}}$$

The mean of all these values approximates the least squares slope. The data for section b1 run one gave a least squares slope of 1.2344; calculating the mean value of slope from all possible combinations of data points gave a slope of 1.2549. These mean values and their distributions can be assessed using Students t test to examine whether they are similar or not. The data that forms the mean distributions (i.e. the individual slope values) can be regarded as independent because it is how the dimension of the topography varies under the application of discrete and independent box sizes.

It is important to realise that neither the least squares slope nor the mean value of slope have any value as predictors of individual log[n(r)] given log(r). Non-linearity implies that each value of dimension is unique; that is why the plots are not self-similar or fractal, because the dimension of the morphology changes with scale or log(r). Use of a single or average value of *D* will give systematic errors and a resulting plot that again has no universal value for *D*. The value in comparing mean slope, is that it highlights any similarities or differences in the data when a constant model is applied. Such similarities or differences will be present under the application of the correct non-linear model, which is difficult to identify (Weisberg, p. 261; 1985: Ryan, p. 417; 1997).

Finally, only the middle six box sizes were used to compose the mean slope distributions. These box sizes are 20, 37, 55, 74, 93 and 112. This was done for several reasons; firstly small or large box sizes may be insensitive to any differences in the image (see FDC manual). This is easily seen with large box sizes; if the box is large enough, one will cover the entire image regardless of any differences in shape. The selection of moderate box sizes removes the possibility of similarity in mean slope (dimension) due to insensitive measures. Secondly, the middle box sizes were selected, because the margin of error between least squares regression and the calculated mean value of slope, increases with the number of values used.

Table 7. 5. 1. gives the results of a Students t-test performed on the distribution of slope (dimension) values for the same section between runs (i.e. b1 in run one is compared with b1 in run 2). Significant values are marked in red. It can be seen that the means of the slope distributions are similar in 129 out of 140 comparisons (significant values are marked in red).

Table 7. 5. 1.- Students t-test on Distribution of Slopes (Dimensions) for Same Section Between Runs.

Comparison/section	b1	c2	b3	c4	b5
1/2	0.140	0.220	0.390	0.140	0.660
1/3	0.096	0.570	0.240	0.550	0.990
1/4	0.270	0.120	0.780	0.490	0.950
1/5	0.057	0.360	0.089	0.530	0.990
1/6	0.180	0.420	0.470	0.980	0.450
1/7	1.000	0.006	0.480	0.340	0.320
1/8	0.031	0.150	0.300	0.990	0.460
2/3	0.570	0.074	0.470	0.650	0.660
2/4	0.540	0.410	0.073	0.550	0.630
2/5	0.390	0.850	0.087	0.410	0.540
2/6	0.700	0.036	0.940	0.110	0.140
2/7	0.140	0.026	0.780	0.950	0.520
2/8	0.270	0.360	0.670	0.150	0.720
3/4	0.290	0.046	0.110	0.970	0.940
3/5	0.790	0.180	0.900	0.870	1.000
3/6	0.900	0.830	0.600	0.540	0.470
3/7	0.096	0.002	0.850	0.690	0.330
3/8	0.630	0.079	0.630	0.560	0.470
4/5	0.180	0.690	0.004	0.890	0.910
4/6	0.410	0.027	0.250	0.470	0.270
4/7	0.270	0.220	0.340	0.640	0.210
4/8	0.110	0.720	0.054	0.500	0.400
5/6	0.700	0.130	0.380	0.500	0.270
5/7	0.057	0.140	0.760	0.560	0.140
5/8	0.840	0.520	0.270	0.550	0.330
6/7	0.180	0.001	0.840	0.330	0.018
6/8	0.570	0.058	0.870	1.000	0.075
7/8	0.031	0.057	0.900	0.350	0.820

Table 7. 5. 2. gives the results of a Students t-test performed on the distribution of slope (dimension) values between different runs (i.e. all values for run one compared with all values for run two). Significant values are marked in red. It can be seen that the means of the slope distributions are similar in 24 out of 28 comparisons. All those comparisons that are not similar occur in run eight.

Table 7. 5. 2- Students t-test on Distribution of Slopes (Dimensions) for Different Runs.

Run No.	1	2	3	4	5	6	7	8
1								
2	0.410							
3	0.380	0.8						
4	0.670	0.68	0.570					
5	0.059	0.23	0.480	0.120				
6	0.570	0.850	0.700	0.850	0.21			
7	0.330	0.700	0.910	0.490	0.58	0.62		
8	0.009	0.046	0.170	0.019	0.43	0.047	0.23	

Table 7. 5. 3. gives the results of a Students t-test performed on the distribution of slope (dimension) values between different sections within a run (i.e. section b1 in run one compared with section c2 in run one). Significant values are marked in red. It can be seen that the means of the slope distributions are similar in 69 out of 80 comparisons.

Table 7. 5. 3- Students t-test on Distribution of Slopes (Dimensions) of Sections Within a Run.

Comparison/Run	1	2	3	4	5	6	7	8
b1/c2	0.036	0.480	0.340	0.036	0.700	0.410	0.000	0.920
b1/b3	0.084	0.200	0.490	0.360	0.580	0.700	0.069	0.830
b1/c4	0.000	0.770	0.890	0.130	0.990	0.360	0.350	0.850
b1/b5	0.031	0.130	0.840	0.038	0.910	0.820	0.000	0.750
c2/b3	0.690	0.180	0.046	0.170	0.120	0.073	0.730	0.910
c2/c4	0.011	0.780	0.290	0.980	0.590	0.000	0.290	0.710
c2/b5	0.290	0.180	0.190	0.880	0.650	0.270	0.770	0.610
b3/c4	0.140	0.390	0.610	0.340	0.420	0.460	0.550	0.480
b3/b5	0.550	0.490	0.600	0.160	0.190	0.350	0.620	0.420
c4/b5	0.520	0.240	0.960	0.930	0.860	0.027	0.240	0.830

Tables 7. 5. 1. - 7. 5. 3. show that *generally* the mean value of slope or the least squares regression value for *D*is the same in all comparisons. In other words, although the morphology does not have a constant dimension because it is non-linear, the degree of non-linearity or the rate of change in dimension with scale is constant.

Between runs, the probability of finding a section with the same rate of change in dimension is high (0.9214 or 92.14%). This represents a considerable improvement on the comparisons that were obtained using mean values of FHT. The stability of the dimension variable is reflected at the run level and significantly between different sections within a run. The probability of finding different sections within a run with similar rate of change in dimension is again high (0.8612 or 86.25%).[173] Previously, comparisons of this sort have not been possible, because it is clear that values of final height differ with Distance From Source. However, dimension is a function of final height that is fairly stable throughout the length of the run.

Given the fact that different methods of calculating *D*give different results, a key question is whether the stability of the rate of change in dimension is a feature of the data, or a reflection of the insensitivity of the box-counting method/FDC programme to differences in topography.

A strategy was developed to assess whether box-counting/FDC was insensitive to changes in morphology. The scheme tested randomly generated grids of data and their associated topographies; it was thought that if the different random grids gave different distributions of mean slope (dimension), then confidence could be placed in the box-counting method/FDC programme, because it could distinguish between different sets of data. The constancy of dimension in the flume data then becomes a matter of physical process generating similar structure, rather than an insensitive measure giving an illusion of stability.

Section b1 was chosen from runs one and four, and a random grid (constructed by sampling from the FHT data for these sections) was generated for each data set. These images given by these random grids are shown along with the images for the original grids in Figures 7. 5. 10. - 7. 5. 13. Students t-test was performed on differences between the two random grids and between the random grids and the original data. The results of this test are given in table 7. 5. 4.

[173] Six (7.50%) of the comparisons which have significant differences involve section one, suggesting that this section is somehow more susceptible to the rate of change in dimension.

Figure 7. 5. 10.- Image One Derived by Random Sampling of Data Points For section b1 Run One.

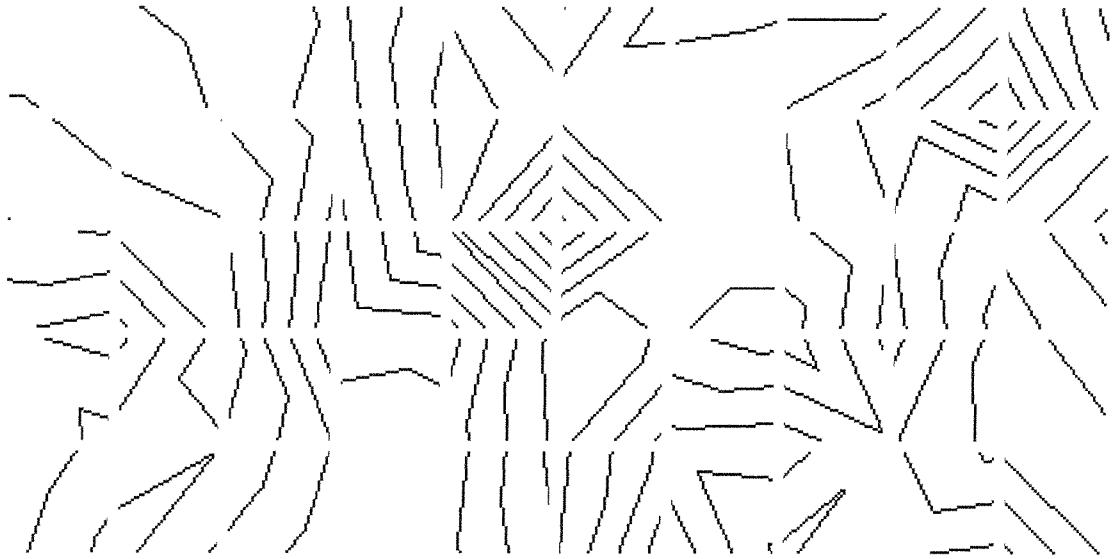


Figure 7. 5. 11.- Image Two Derived by Random Sampling of Data Points For section b1 Run Four.

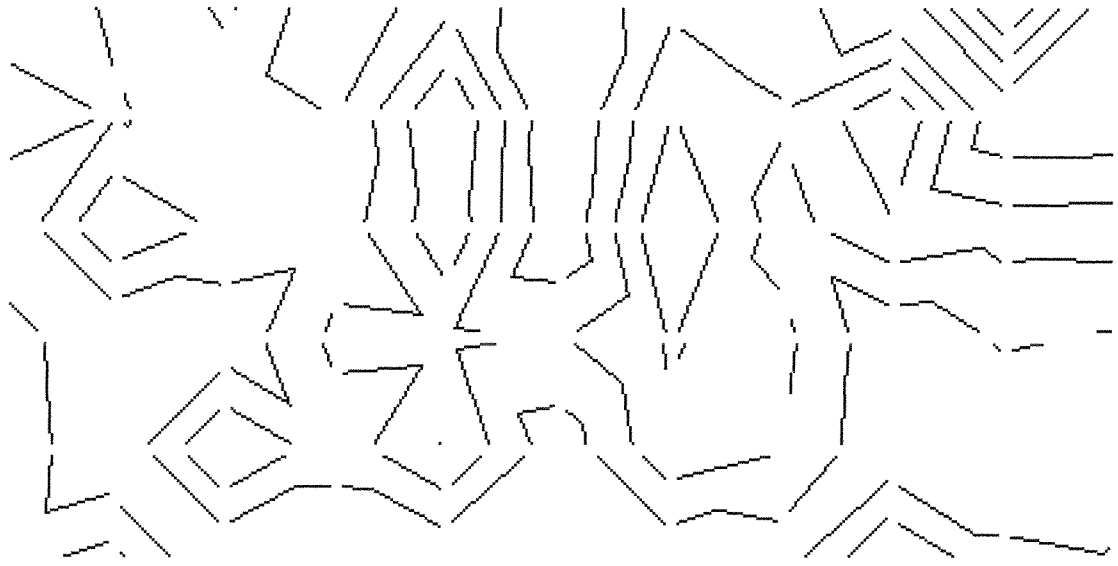


Figure 7. 5. 12.- Image Three - Original Data for Section b1 Run One.

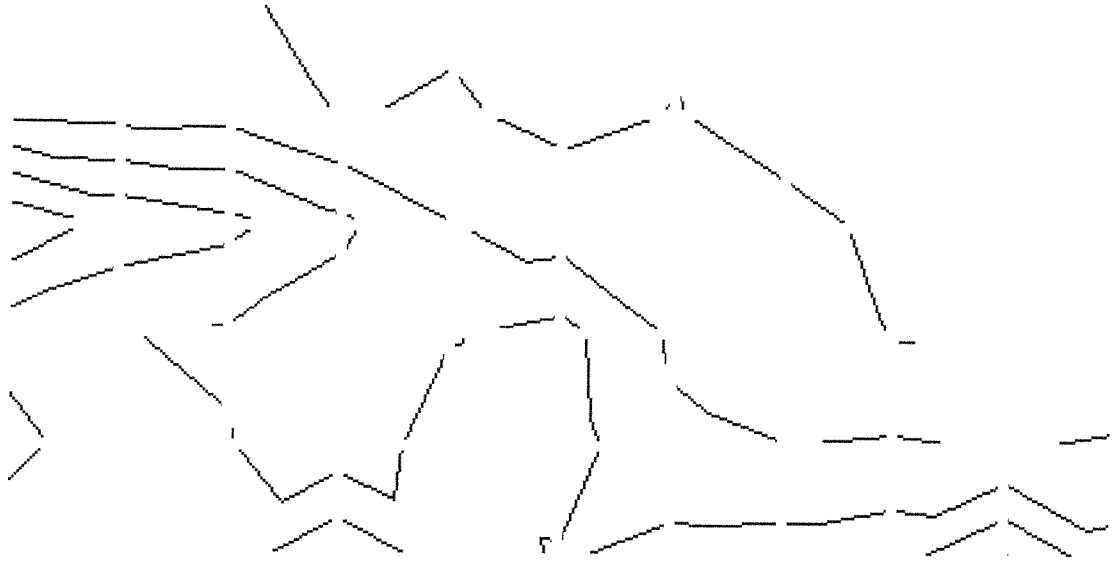


Figure 7. 5. 13.- Image Four- Original Data for Section b1 Run Four.

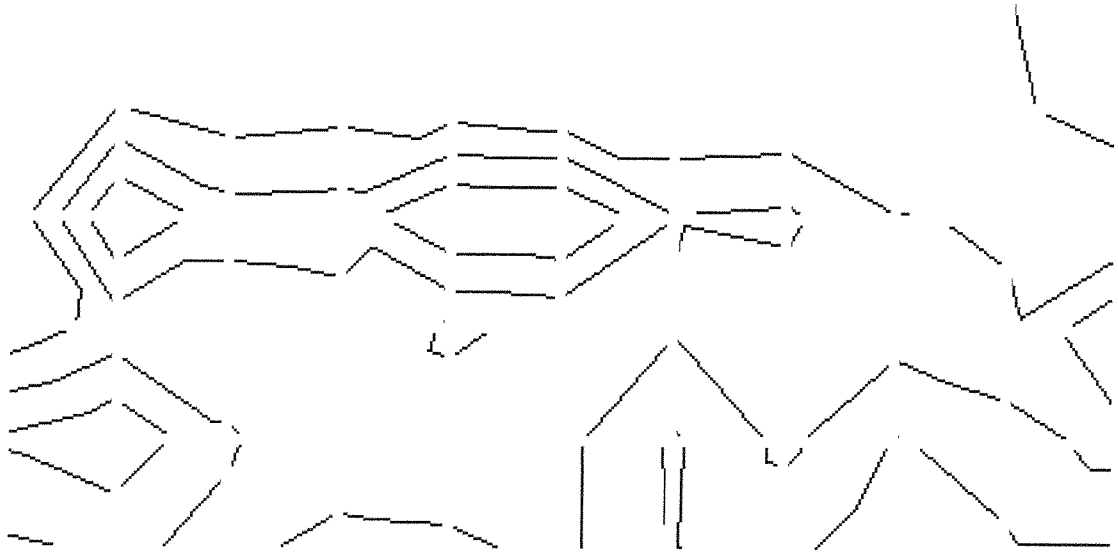


Table 7. 5. 4- Students t-test on Random Data and Original Data.

Image No.	1	2	3	4
1				
2	0.270			
3	0.003	0.010		
4	0.000	0.006	0.270	

Table 7. 5. 4. shows that the box-counting method/FDC programme can distinguish successfully between the randomly generated images and the original images. However, it was initially thought that two sets of randomly generated data should produce differences in rate of change in dimension, because no constant process is at work to bring about similar rate of change in dimension. This has not been the case - the randomly generated images (one and two) have statistically similar values of mean slope (dimension).

In retrospect this is not surprising. Random sampling from a set of points is very unlikely to yield a smooth topography and a complex topography; rather the topography generated will usually be complex, because the data peaks and troughs often. In other words, it is highly probable that the data generated will be roughly similar in character.

Table 7. 5. 4. shows that with different methods of organising data (i.e. fluid organised data points and computer organised data points), the box-dimension method/FDC programme distinguishes between rates of change in dimension. Within a method of organising data, the rate of change in dimension is similar. This result has implications for the understanding that is gained by a constant rate of change in dimension being present in the data. Before this is discussed and analysed more fully, what dimension represents, and the factors involved in producing dimension are considered.

Dimension is a quantitative measure of the geometry of the channel morphology. In a fractal object, dimension is a constant (coefficient), because the object is invariant in its geometric properties over all scales. In a non-fractal object, the geometry changes with scale; for example a circle examined at high enough resolution becomes a straight line (Stewart, p. 202; 1997). In the non-linear graph the mean slope curve represents the average rate of change in dimension over all scales. It is unclear, and beyond the scope of this thesis, to investigate what physical properties of the morphology are responsible for the changes in dimension at each scale.

Dimension, or the rate of change in dimension, is governed by two things - the values of the FHT data points and their spatial organisation. The first of these properties was considered in isolation in aleatory analysis; the FHT data was lumped together for sections and compared as a distribution to similar sections, without any regard for where data values in the distribution were located in space. It was found that differences in the controls could not explain differences in morphology. Epistemic analysis combined both the values of the FHT data points and their spatial location. Epistemic analysis could not explain differences in morphology to differences in control.

The point of the above paragraph is that there is no insight available for the fluvial system in terms of control variables. Within the narrow range of controls used, deviations occur which are not related to error, but are an internal part of the system, that as yet remains unexplained.

Fractal analysis, as suggested above, is a measure that uses both the values of the data points and their spatial organisation. It has already been shown above that the values of FHT cannot be explained. It is possible then, that the organisation of the FHT data points is what provides the stability of rate of change in dimension.

In order to assess the possibility that the fluid flow organises data points in some special way as to produce constant dimension, random grids were generated from the data for sections b1, c2 and b5 in run one. These sections were chosen because they represent a scenario where the statistical properties (i.e. mean and standard deviation) of the data are relatively closely related (b1 and c2) and a scenario where the properties of the data are relatively far apart (c2 and b5/ b1 and b5). The data for these sections was sampled without replacement so that the random grids rearranged the original data points in any combination. Seven grids were generated for each section (i.e. b1) and each of the seven grids was compared to every other grid of another section (i.e. c2). The test was designed to examine whether the random data would respond differently to the flume data in the manner it responds to the changes in the properties of the data.

From 49 randomly generated comparisons between sections b1 and c2, none produced a mean slope (dimension) that was significantly different. From 49 randomly generated comparisons between sections c2 and b5 none produced a mean slope (dimension) that was significantly different. Finally, from 49 randomly generated comparisons between sections b1 and b5, only two produced a mean slope (dimension) that was significantly different.

The results of this random trial, are that given the values of the data, a consistent random ordering process will favour the statistical stability of the rate of change in dimension. That is, there is nothing unique or special about the way the fluid flow organises data; a random selection programme on a computer will produce constant dimension given the values of the data.

The presence of fairly predictable and consistent dimension offers no insight into the processes that are responsible for it. It has already been shown that the differences in the values of the data given by the flow cannot be explained by the control variables. It has been shown here that the spatial ordering of that data does not explain the dimension, because a random process would give the same result.

Perhaps the most significant aspect of dimension that requires further investigation, is that in the flume channel it appears that dissimilarities in dimension occur more frequently than in randomly ordered data. In 49 random comparisons between section b1 and b5, only two of the dimensions were dissimilar, whereas this comparison was dissimilar in only one experimental trial. Similarly, in 49 random comparisons between sections b1 and c2 none of the dimensions were dissimilar, but again in only one experimental trial this comparison was dissimilar.

Finally, neither the flume generated dimension nor the randomly generated dimension have a clear relationship to the statistical properties of the data (within the range used in these experiments). At this point it is unclear what other measures of FHT would affect the rate of change in dimension and possibly lead to a distinction between the behaviour of fluid generated data and randomly generated data,

Summary

The application of fractal analysis to the final height data has suggested that the data is not fractal and therefore not chaotic. The log[N(r)] versus log(r) was not linear for any of the sections, nor could it be split into several linear segments that would have suggested the morphology was multifractal.

Analysis of the log[N(r)] versus log(r) curves using the mean value of slope indicate that the rate of change in dimension is relatively constant and predictable for the morphology data - both within runs and between runs. The sensitivity of the box-counting method/ FDC programme was tested, and it was shown that it could distinguish between different original data and randomly generated data. However, data within the random generated sections were similar.

A further test examined the similarities between the response of random sections and original sections as the properties of the data changed. It was found that random data behaved similarly to the original data, favouring a statistically constant rate of change in dimension.

The constancy of the rate of change in dimension cannot be understood either in the generation of the data values by the flow or in the spatial organisation of these data values; the data values are not explicable in terms of control variables nor are the variations in dimension related to the values of FHT. The spatial organisation of the data values is not unique to fluid flows but can be reproduced by a random process with consistent rules.

Perhaps the main positive contribution to knowledge from the apparent failure of fractal analysis, is that the random scheme used to order data in the fluid flow (whatever that may be) is fairly consistent. This conclusion is derived from the fact that given a random scheme (fluid flow or computer) the rate of change in dimension is constant. This indicates that the rules for the organisation of sediment by a fluid flow are consistent between and within runs, as they are with the selection of data by a computer.[174]

[174] For example in the computer generated sections, a rule is that all the data values may be manipulated and not just a selected few.

Chapter 8

Conclusions and Further Research

8. 1. A Summary of the Key Ideas to Emerge in this Thesis.

8. 1. 1.

It has been argued that geomorphology has always been held as a science by some geomorphologists. Of particular note are Davis and Gilbert, who, it has been suggested, both held that geomorphology was concerned with generating knowledge in the form of laws. It has been shown that two main schools of thought have dominated the intellectual development of geomorphology; the Simpsonian school which advocates a 'scientific' approach that is different to physical science and the Kittonian school which maintains that geomorphology is equivalent to physical science. The Kittonian view is adopted in this thesis and this provides a mandate for examining the form of knowledge that science aims to produce. It has been suggested that in order to review previous theory in geomorphology and to define an approach to indeterminacy problems, a rigourous concept of physical science and some expectations of the knowledge it produces is required.

8. 1. 2.

The characteristics of physical science have been explored. The view that science is explanation by laws has been taken, and it has been proposed that three types of law exist; causal, aleatory statistical, and chaos. All laws are thought to be causal at some scale, where the properties of a distribution or an attractor (in the case of aleatory and chaos laws respectively) become deterministic. In answer to Hume's scepticism, mathematical relationships have been given as the justification for scientific laws. It has been proposed that a hallmark of scientific laws is their ability to be tested by the predictions they make. Although the concept of prediction has difficulties, it has been shown that it occupies a central place in the philosophy of science, and indeed that different philosophies of science are only different because of their treatment of prediction.

8. 1. 3.

Turbulence has been shown to be a chaos phenomenon and based on the reasoning of Lorenz and Culling, it is thought that through the application of force to the channel by turbulent flows, indeterminacy in fluvial channels may be explained by chaos concepts. A new hypothesis for fluvial morphology has been proposed on this basis.

8. 1. 4.

A review of current approaches to fluvial geomorphology shows that there is considerable scatter on charts when techniques of epistemic probability are applied to natural data. Concepts of error, equifinality and equilibrium attempt to explain this scatter. It is also pointed out that current theory in geomorphology has analysed data using epistemic relations between aleatory entities. The concept of science outlined in this thesis expects that an invariant distribution of morphology will be produced for a given distribution of a control at the scale of a reach (which is the scale with which most studies have been concerned).

8. 1. 5.

The causal hypothesis was applied to the morphology created by fluid flows in the flume. The experiments and analysis suggest that in terms of current understanding of the controls on river form, fluvial theory cannot use causal laws at the scale of individual quantitative measurements to explain the observed morphology. It is not possible to predict the final height of any location in the flume, even with the inclusion of all known controls on river morphology; the addition of variables does not improve prediction and make the data determinate. The individual datum points were unrelated to the control variables, both in magnitude and in the type of relationship that was present (i.e. inverse or proportional). The best variable for prediction was Distance From Source, but within a run, this variable left much variance unexplained (Distance From Source explained only 54.80% of the variance for run seven) and between runs, the extent of the relationship was judged to be different; that is the change in final height with distance from source is not the same between runs. Keeping in mind the difficulties with prediction and experiment, it may be that the addition of some variables which are as yet unknown, or perhaps better experimental conditions, may result in a causal relationship being applicable at the scale of individual measurements[175] . The application of causal laws to individual measurements requires a more detailed understanding of the internal structure of the fluid-sediment interaction and its relationship to the controls and its relationship to morphology.

[175] See **p. 329 this thesis** for an outline of improvements to experimental conditions, particularly the use of a non-invasive photogrammetric technique for measurement.

8. 1. 6.

A significant point to emerge from the testing of causal laws was the importance of distance from source and what has been termed the 'internal structure' of the fluvial channel. It was shown how the fluid flow forms an internal gradient and the idea of sediment queues was discussed as a mechanism for this phenomenon. The busyness of the queue and hence the rate of sediment transport is related to the distance from source. An important result is that, in near constant conditions, the sediment transport system functions independently of the external controls and obscures their influence. The understanding of this internal system is key to exploring indeterminacy; two paradoxical features of this system have been shown, On the one hand, it produces heterogeneous areas of the flume channel within a run, which has the result that the same areas of the flume between runs should be broadly similar. However, on the other hand, this mechanism which leads to homogenisation of a region is a complex function within a run (morphology does not vary exclusively with distance from source or sediment transport) and moreover is not similar between runs (the relationship between distance from source and final height is not constant). The internal structure of the system is intimately related to the problem of dependency in the data, and much more research needs to be focused on identifying consistent and predictable aspects of this internal evolution.

8. 1. 7.

The aleatory hypothesis was applied to the morphology created by fluid flows in the flume. It was found at the reach level that aleatory concepts could not adequately explain variations in the statistical properties of the morphology. It was found that in all possible comparisons, only 68.58% of the central transect (0.6850) were statistically similar for shape and mean of the distributions. The similarity or difference of these sections were generally unrelated to the statistical properties of the controls; when the controls were significantly different, the morphology could be statistically similar, or alternatively, when the controls were statistically similar, the morphology could be significantly different. The large degree of uncertainty concerning final height for the central transect, and the lack of relationship to the statistical properties of the controls, shows the inherent lack of predictability contained in the internal structure of the system; the predictability formed by cumulative behaviour or aleatory distributions is limited and unrelated to our current understanding of controls and form. Importantly, in the flume, the weakness in the relationship between controls and morphology (as far as our present understanding extends) is not due to error, nor to the theoretical constructs of equifinality or equilibrium, but rather because diversity in form is an inherent part of the fluvial system.

8. 1. 8.

The aleatory hypothesis was successful in predicting morphology at the run level. Although four comparisons out of 28 resulted in different mean values, all these involved run one and suggested that the controls on run one may be at the limit of forcing a different kind of morphology. Again, although predictability resulted, the statistical properties of the controls were generally unrelated to the statistical properties of final height. An interpretation of this result is that, although changes in controls are statistically significant, they are unable to produce difference in morphology. The exception to this would have to be run one, where a clear shift in the morphology that is being produced takes place. However, the difference between the controls of run one and the other runs is small, and it may be that the limits of statistical stability for final height for the whole sediment system, operates over a small range of controls. It appears that with a given discharge and initial slope, the internal structure of the system is constrained to produce a final height that will be statistically predictable over a given range of other controls (PSS, IHDS temperature etc.). The range of these controls needs to be investigated.

8. 1. 9.

The relationship between predictability at the run level and uncertainty at the reach level has been explained in aleatory terms as chance arrangements of the system. Problems emerge with defining *a priori* probabilities at the reach level, because the characteristics of the channel population distribution are unknown, as are the number of arrangements the system may take.

8. 1. 10.

The chaos hypothesis was applied to the morphology created by fluid flows in the flume. Fractal analysis of the images created by the contour plots of flume topography failed to produce a constant dimension for the data, suggesting that the geometry of the flume morphology was not scale invariant, but depended on the scale at which it was viewed. This implied that a strange attractor (which is fractal) is not evident in the morphology or the product of the fluid-sediment interaction. It was shown that the dimension was not multifractal but was inherently non-linear. Testing of these non-linear relationships by approximating least squares regression through mean slope, showed that the rate of change in dimension with scale was statistically similar for most runs. In other words, given the set of control variables, the rate of change in dimension is a fairly constant and predictable feature of the data. However, this is a case where the predictability of a feature of the data does not give understanding of the phenomenon.

It has already been shown that the values of final height datum points cannot be explained in terms of the control variables. It was shown in fractal analysis that there was nothing unique about the spatial arrangement of the data values, but that a computer generating random spatial arrangements would also give predominantly constant rate of change in dimension, given the values of data. One conclusion that can be determined from this data is that the ordering process in a fluid flow (whatever that may be) is consistent; different types of random process (fluid or computer generated) give different rates of change in dimension, but it is known, for example, that with a computer generated process with a given set of rules (i.e. all data may be manipulated) the rate of change in dimension will mainly be statistically similar. The presence of similar forms of behaviour in the flume channel suggests that a consistent random process is in operation in the different runs. However, the slightly higher incidence of significantly different mean dimensions in the flume data than in the randomly generated data, may be a noteworthy result. The chaotic nature of the flume data may have been obscured due to data limitations and experimental constraints and this merits further, more accurate, testing.

8. 1. 11.

The limited understanding (in terms of the goals of geomorphology and science as outlined in this thesis) suggested in the previous points, seems to warrant the proposal that indeterminacy should once again become the core area of research for fluvial geomorphology. In particular, a detailed understanding of what has been labelled here the 'internal structure' of channels needs to be developed. Until this area is more fully explored, 'true', but varied relationships between controls and morphology will be labelled either as error, or will be explained away by the injudicious use of equifinality or equilibrium. Undoubtedly these concepts have importance in geomorphology (particularly in the field) but they must be attached to a more rigourous and complex relationship between controls and morphology.

8. 2. Suggestions for Future Research

The present thesis has been concerned with attempting to find a type of law that gives an invariant relationship between controls and morphology. It has not succeeded in doing this, apart from at the scale of the aleatory distribution of final heights at the run level. Clearly, the reach level is important in fluvial problems, and attention needs to be turned to developing a more rigourous understanding of the internal structure of the channel system, so that the reach level might be understood in terms other than the chance arrangements of a deterministic distribution's components.

More accurate experimental work that provides absolutely constant values of the control variables would be beneficial to exploring this internal relationship, so that changes in morphology could be studied without constant reference to the values of the controls. In addition, a digitised data gathering and processing system (such as digital photogrammetry) would provide opportunities to fit proactive identification models of chaotic behaviour such as Ruelle and Taken's reconstructions and better informed fractal analysis. Importantly, an experiment that allows dynamic observation of the development of a fluid - sediment surface (i.e. measurements take place occurring flow) would be beneficial.

In addition to the understanding of the fundamental relationship between control and morphology, which in itself is a formidable task, fluvial geomorphology must address questions of what measures of control and morphology are appropriate (see Section 5. 2. 1.) for a discussion of representative measures in fluvial theory). A particularly pertinent example is the question of discharge measures; if a channel experiences the same mean discharge, but the individual flows which make up this mean value are different, will the morphology be similar or different and how does this relate to the fundamental relationship between controls and morphology? Experimental runs (9, 10, 11, 12 and 13) were carried out using flow sequencing and these runs need to be analysed (see Appendix, Section G).

A particularly important area of work must focus on the critical limits of control variables for which a given morphology results. Clearly at present this can only be undertaken for the sediment system as a whole, where the distribution of final height becomes deterministic over a range of control variables. It has been suggested that the relationship between controls and morphology at the reach level is most likely non-linear and complex with the ability for different controls to produce similar or different morphology. Current research goals could not be to identify the exact relationship between controls and morphology, but merely to divide controls into boundaries, for which it is clear that different morphologies will result. Results suggest that these boundaries may be quite narrow. Tests were also carried out by adjusting bank cohesion by the addition of kaolinite (runs 14 and 15) and the results of these tests also require analysis (see **Appendix**, Section G).

8. 3. Concluding Remarks.

It is only the scientific nature of geomorphology that has uncovered indeterminacy. Fluvial theorists, in seeking a consistent explanation of fluvial forms, have formed a metaphorical rod for their own back; they have revealed that at a fundamental level no consistent explanation is possible. The challenge of indeterminacy that geomorphologists have uncovered, has been somewhat obscured by our attempts to resolve that challenge. To underlying scale relationships between morphology and discharge/sediment, we have fitted various theoretical constructs such as error, equifinality and equilibrium that 'explain' the deviations from our derived relationships. Other geomorphologists, faced with the challenge of indeterminacy and the complexity of the environment, give up the challenge and declare that geomorphology is not scientific after all - that consistent explanation by law is not achievable.

This thesis has not lessened in any way the challenge that indeterminacy poses to fluvial theorists; indeed the opposite is the case. This thesis has taken a broad view of the type of scientific laws that are available for explanation in geomorphology, but it has been shown that in laboratory conditions, no scientific law can be found to apply to fluvial morphology, except at the scale of the entire system.

The contribution of this thesis is to encourage further research into the challenge of indeterminacy in fluvial geomorphology. By choosing to be a scientific discipline, fluvial geomorphology has revealed a phenomenon that threatens its scientific status, but this thesis would advocate that geomorphologists continue to pursue a scientific resolution to what is in many ways, the essence and the excitement of fluvial geomorphology - the, as yet, indeterminate nature of alluvial channels.

APPENDIX

Experimental Data

Section A: Experimental Equipment and Material

1. Tilting Flume of dimensions: 25.00 m (length) x 1.37 m (width) x 0.750 m (depth)

2. 0.747 m^3 of sorted fine sand (sediment) of size 0.5 - 0.1 mm.

3. Metal runner guides (total length 7.50 m)

4. Point Gauge with 1 mm divisions

5. Sliding Trolley with mounted ruler in 1 mm divisions.

6. Turbulence Baffle

7. Wooden Runners: 7.50 m (length) x 0.074 m (depth)

8. Camera

Section B: Fundamental Data Set: Final Height of Drainage Surface

Tables 1 - 5: Run 1

B 1.50 - 2.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.3485	0.4158	0.4831	0.5504	0.6177	0.6850
1.50	0.0678	0.0672	0.0673	0.0651	0.0592	0.0531
1.55	0.0672	0.0662	0.0670	0.0649	0.0603	0.0441
1.60	0.0667	0.0668	0.0660	0.0652	0.0594	0.0517
1.65	0.0668	0.0670	0.0638	0.0622	0.0599	0.0528
1.70	0.0658	0.0648	0.0608	0.0608	0.0590	0.0530
1.75	0.0660	0.0662	0.0625	0.0536	0.0529	0.0554
1.80	0.0658	0.0645	0.0599	0.0552	0.0531	0.0522
1.85	0.0665	0.0652	0.0540	0.0579	0.0578	0.0460
1.90	0.0645	0.0618	0.0486	0.0555	0.0548	0.0523
1.95	0.0635	0.0619	0.0465	0.0549	0.0540	0.0525
2.00	0.0631	0.0610	0.0409	0.0520	0.0476	0.0536

C 2.50 - 3.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.6850	0.7523	0.8196	0.8869	0.9542	1.0215
2.50	0.0500	0.0451	0.0512	0.0538	0.0589	0.0590
2.55	0.0521	0.0572	0.0511	0.0519	0.0602	0.0608
2.60	0.0519	0.0465	0.0504	0.0500	0.0590	0.0609
2.65	0.0459	0.0408	0.0474	0.0502	0.0586	0.0599
2.70	0.0420	0.0419	0.0410	0.0490	0.0598	0.0584
2.75	0.0360	0.0482	0.0405	0.0443	0.0601	0.0611
2.80	0.0431	0.0420	0.0385	0.0483	0.0568	0.0602
2.85	0.0371	0.0432	0.0481	0.0498	0.0561	0.0591
2.90	0.0352	0.0389	0.0510	0.0408	0.0513	0.0595
2.95	0.0330	0.0399	0.0482	0.0429	0.0480	0.0595
3.00	0.0490	0.0475	0.0420	0.0410	0.0495	0.0605

B 3.50 - 4.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.3485	0.4158	0.4831	0.5504	0.6177	0.6850
3.50	0.0508	0.0411	0.0480	0.0452	0.0483	0.0395
3.55	0.0492	0.0492	0.0429	0.0461	0.0432	0.0256
3.60	0.0478	0.0485	0.0480	0.0422	0.0411	0.0365
3.65	0.0492	0.0481	0.0468	0.0401	0.0438	0.0455
3.70	0.0471	0.0455	0.0460	0.0351	0.0448	0.0444
3.75	0.0538	0.0452	0.0424	0.0385	0.0418	0.0398
3.80	0.0604	0.0435	0.0432	0.0425	0.0419	0.0394
3.85	0.0604	0.0445	0.0372	0.0409	0.0408	0.0390
3.90	0.0594	0.0460	0.0341	0.0449	0.0321	0.0415
3.95	0.0599	0.0462	0.0441	0.0403	0.0405	0.0395
4.00	0.0610	0.0469	0.0392	0.0395	0.0398	0.0415

C 4.50 - 5.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.6850	0.7523	0.8196	0.8869	0.9542	1.0215
4.50	0.0349	0.0406	0.0402	0.0295	0.0582	0.0594
4.55	0.0298	0.0382	0.0421	0.0398	0.0609	0.0615
4.60	0.0303	0.0395	0.0419	0.0400	0.0550	0.0598
4.65	0.0155	0.0395	0.0418	0.0399	0.0590	0.0595
4.70	0.0251	0.0339	0.0381	0.0382	0.0418	0.0610
4.75	0.0284	0.0315	0.0409	0.0403	0.0445	0.0604
4.80	0.0371	0.0304	0.0395	0.0402	0.0590	0.0600
4.85	0.0400	0.0295	0.0391	0.0391	0.0595	0.0591
4.90	0.0170	0.0210	0.0350	0.0360	0.0460	0.0599
4.95	0.0109	0.0305	0.0270	0.0218	0.0530	0.0589
5.00	0.0212	0.0318	0.0400	0.0341	0.0372	0.0611

B 5.50 - 6.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.3485	0.4158	0.4831	0.5504	0.6177	0.6850
5.50	0.0645	0.0639	0.0385	0.0292	0.0316	0.0327
5.55	0.0605	0.0518	0.0382	0.0305	0.0320	0.0261
5.60	0.0635	0.0629	0.0391	0.0362	0.0312	0.0242
5.65	0.0643	0.0626	0.0362	0.0325	0.0316	0.0295
5.70	0.0642	0.0631	0.0355	0.0302	0.0289	0.0325
5.75	0.0652	0.0490	0.0302	0.0342	0.0341	0.0360
5.80	0.0646	0.0399	0.0362	0.0352	0.0348	0.0300
5.85	0.0638	0.0389	0.0355	0.0329	0.0360	0.0303
5.90	0.0649	0.0394	0.0334	0.0332	0.0322	0.0268
5.95	0.0631	0.0381	0.0342	0.0345	0.0250	0.0310
6.00	0.0635	0.0378	0.0345	0.0279	0.0232	0.0291

Tables 6 - 10: Run 2

B 1.50 - 2.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.3485	0.4158	0.4831	0.5504	0.6177	0.6850
1.50	0.0675	0.0665	0.0655	0.0626	0.0505	0.0578
1.55	0.0661	0.0668	0.0653	0.0569	0.0471	0.0455
1.60	0.0669	0.0671	0.0655	0.0615	0.0411	0.0499
1.65	0.0662	0.0669	0.0640	0.0601	0.0522	0.0308
1.70	0.0670	0.0668	0.0581	0.0613	0.0600	0.0511
1.75	0.0661	0.0658	0.0545	0.0600	0.0582	0.0402
1.80	0.0658	0.0655	0.0543	0.0605	0.0548	0.0511
1.85	0.0665	0.0648	0.0495	0.0561	0.0522	0.0495
1.90	0.0655	0.0652	0.0503	0.0540	0.0581	0.0602
1.95	0.0672	0.0641	0.0525	0.0560	0.0590	0.0582
2.00	0.0642	0.0635	0.0455	0.0558	0.0552	0.0475

C 2.50 - 3.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.6850	0.7523	0.8196	0.8869	0.9542	1.0215
2.50	0.0552	0.0522	0.0520	0.0562	0.0610	0.0621
2.55	0.0511	0.0496	0.0452	0.0502	0.0599	0.0625
2.60	0.0519	0.0478	0.0549	0.0520	0.0619	0.0621
2.65	0.0482	0.0408	0.0532	0.0538	0.0590	0.0623
2.70	0.0500	0.0328	0.0509	0.0512	0.0587	0.0623
2.75	0.0523	0.0479	0.0448	0.0485	0.0619	0.0629
2.80	0.0468	0.0432	0.0521	0.0465	0.0612	0.0621
2.85	0.0481	0.0447	0.0528	0.0522	0.0598	0.0605
2.90	0.0496	0.0435	0.0502	0.0532	0.0530	0.0623
2.95	0.0515	0.0309	0.0358	0.0478	0.0471	0.0630
3.00	0.0450	0.0361	0.0430	0.0482	0.0541	0.0620

B 3.50 - 4.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.3485	0.4158	0.4831	0.5504	0.6177	0.6850
3.50	0.0599	0.0311	0.0495	0.0435	0.0496	0.0485
3.55	0.0628	0.0478	0.0481	0.0429	0.0478	0.0455
3.60	0.0625	0.0490	0.0502	0.0386	0.0412	0.0440
3.65	0.0632	0.0501	0.0482	0.0438	0.0416	0.0423
3.70	0.0599	0.0496	0.0460	0.0449	0.0423	0.0396
3.75	0.0588	0.0515	0.0435	0.0410	0.0281	0.0301
3.80	0.0555	0.0496	0.0438	0.0441	0.0398	0.0424
3.85	0.0642	0.0482	0.0461	0.0432	0.0308	0.0401
3.90	0.0645	0.0454	0.0478	0.0360	0.0421	0.0361
3.95	0.0638	0.0489	0.0486	0.0389	0.0435	0.0369
4.00	0.0634	0.0498	0.0455	0.0311	0.0411	0.0291

C 4.50 - 5.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.6850	0.7523	0.8196	0.8869	0.9542	1.0215
4.50	0.0405	0.0411	0.0352	0.0451	0.0632	0.0633
4.55	0.0419	0.0438	0.0354	0.0452	0.0629	0.0622
4.60	0.0382	0.0444	0.0249	0.0449	0.0626	0.0635
4.65	0.0400	0.0411	0.0284	0.0429	0.0629	0.0625
4.70	0.0407	0.0368	0.0372	0.0411	0.0615	0.0621
4.75	0.0403	0.0411	0.0292	0.0448	0.0628	0.0620
4.80	0.0320	0.0364	0.0422	0.0432	0.0620	0.0607
4.85	0.0351	0.0373	0.0412	0.0408	0.0611	0.0618
4.90	0.0339	0.0418	0.0412	0.0394	0.0611	0.0618
4.95	0.0338	0.0406	0.0419	0.0398	0.0460	0.0615
5.00	0.0342	0.0385	0.0398	0.0369	0.0429	0.0631

B 5.50 - 6.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.3485	0.4158	0.4831	0.5504	0.6177	0.6850
5.50	0.0619	0.0390	0.0322	0.0342	0.0312	0.0364
5.55	0.0622	0.0392	0.0309	0.0261	0.0248	0.0356
5.60	0.0638	0.0385	0.0325	0.0255	0.0285	0.0363
5.65	0.0631	0.0329	0.0331	0.0250	0.0300	0.0372
5.70	0.0632	0.0382	0.0296	0.0259	0.0283	0.0358
5.75	0.0631	0.0414	0.0269	0.0191	0.0332	0.0365
5.80	0.0631	0.0612	0.0272	0.0272	0.0282	0.0336
5.75	0.0616	0.0396	0.0285	0.0302	0.0245	0.0318
5.90	0.0642	0.0580	0.0298	0.0318	0.0279	0.0305
5.95	0.0638	0.0415	0.0239	0.0321	0.0292	0.0252
6.00	0.0642	0.0502	0.0190	0.0320	0.0275	0.0289

Tables 11 -15: Run 3

B 1.50 -2.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.3485	0.4158	0.4831	0.5504	0.6177	0.6850
1.50	0.0674	0.0695	0.0682	0.0618	0.0615	0.0544
1.55	0.0689	0.0695	0.0672	0.0642	0.0472	0.0512
1.60	0.0679	0.0690	0.0671	0.0611	0.0445	0.0622
1.65	0.0684	0.0680	0.0635	0.0633	0.0396	0.0509
1.70	0.0669	0.0675	0.0642	0.0628	0.0629	0.0571
1.75	0.0684	0.0675	0.0600	0.0609	0.0608	0.0586
1.80	0.0662	0.0662	0.0589	0.0627	0.0629	0.0573
1.85	0.0682	0.0682	0.0579	0.0608	0.0611	0.0606
1.90	0.0671	0.0665	0.0539	0.0602	0.0606	0.0546
1.95	0.0671	0.0658	0.0613	0.0565	0.0590	0.0535
2.00	0.0684	0.0618	0.0512	0.0468	0.0593	0.0421

C 2.50 - 3.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.6850	0.7523	0.8196	0.8869	0.9542	1.0215
2.50	0.0495	0.0452	0.0662	0.0639	0.0631	0.0639
2.55	0.0511	0.0485	0.0582	0.0570	0.0613	0.0642
2.60	0.0465	0.0479	0.0565	0.0572	0.0642	0.0638
2.65	0.0428	0.0391	0.0541	0.0599	0.0602	0.0572
2.70	0.0385	0.0535	0.0550	0.0580	0.0609	0.0641
2.75	0.0400	0.0525	0.0548	0.0572	0.0590	0.0649
2.80	0.0510	0.0524	0.0539	0.0569	0.0609	0.0622
2.85	0.0568	0.0488	0.0509	0.0478	0.0600	0.0623
2.90	0.0503	0.0504	0.0531	0.0573	0.0592	0.0623
2.95	0.0380	0.0529	0.0425	0.0560	0.0609	0.0632
3.00	0.0361	0.0502	0.0406	0.0561	0.0630	0.0629

B 3.50 - 4.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.3485	0.4158	0.4831	0.5504	0.6177	0.6850
3.50	0.0580	0.0496	0.0479	0.0490	0.0502	0.0356
3.55	0.0611	0.0556	0.0396	0.0438	0.0474	0.0322
3.60	0.0442	0.0485	0.0478	0.0363	0.0392	0.0459
3.65	0.0448	0.0504	0.0481	0.0444	0.0330	0.0409
6.70	0.0585	0.0506	0.0478	0.0349	0.0428	0.0460
3.75	0.0646	0.0489	0.0482	0.0396	0.0411	0.0446
3.80	0.0593	0.0518	0.0502	0.0379	0.0318	0.0422
3.85	0.0635	0.0566	0.0475	0.0319	0.0402	0.0404
3.90	0.0546	0.0606	0.0592	0.0382	0.0448	0.0418
3.95	0.0506	0.0504	0.0480	0.0445	0.0413	0.0395
4.00	0.0542	0.0497	0.0424	0.0458	0.0407	0.0352

C 4.50 - 5.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.6850	0.7523	0.8196	0.8869	0.9542	1.0215
4.50	0.0415	0.0445	0.0322	0.0309	0.0429	0.0468
4.55	0.0443	0.0420	0.0400	0.0332	0.0462	0.0612
4.60	0.0452	0.0422	0.2980	0.0339	0.0452	0.0641
4.65	0.0441	0.0300	0.0320	0.0437	0.0452	0.0618
4.70	0.0356	0.0408	0.0211	0.0418	0.0452	0.0632
4.75	0.0379	0.0421	0.0350	0.0341	0.0433	0.0578
4.80	0.0242	0.0412	0.0388	0.0402	0.0418	0.0499
4.85	0.0212	0.0319	0.0373	0.0416	0.0407	0.0522
4.90	0.0296	0.0277	0.0360	0.0404	0.0398	0.0438
4.95	0.0380	0.0306	0.0350	0.0381	0.0382	0.0475
5.00	0.0368	0.0372	0.0340	0.0371	0.0400	0.0522

B 5.50 - 6.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.3485	0.4158	0.4831	0.5504	0.6177	0.6850
5.50	0.0649	0.0598	0.0300	0.0329	0.0309	0.0333
5.55	0.0589	0.0588	0.0384	0.0255	0.0329	0.0365
5.60	0.0556	0.0425	0.0379	0.0220	0.0321	0.0346
5.65	0.0505	0.0395	0.0345	0.0256	0.0332	0.0303
5.70	0.0642	0.0384	0.0352	0.0282	0.0348	0.0301
5.75	0.0638	0.0355	0.0360	0.0251	0.0242	0.0285
5.80	0.0582	0.0298	0.0328	0.0207	0.0321	0.0212
5.85	0.0551	0.0295	0.0316	0.0335	0.0285	0.0212
5.90	0.0635	0.0349	0.0338	0.0344	0.0205	0.0292
5.95	0.0652	0.0314	0.0321	0.0305	0.0221	0.0314
6.00	0.0645	0.0334	0.0291	0.0275	0.0200	0.0298

Tables 16 - 20: Run 4

B 1.50 - 2.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.3485	0.4158	0.4831	0.5504	0.6177	0.6850
1.50	0.0701	0.0703	0.0695	0.0555	0.0618	0.0541
1.55	0.0700	0.0699	0.0655	0.0644	0.0575	0.0541
1.60	0.0698	0.0698	0.0647	0.0620	0.0546	0.0512
1.65	0.0694	0.0693	0.0591	0.0625	0.0611	0.0562
1.70	0.0695	0.0696	0.0598	0.0604	0.0544	0.0547
1.75	0.0690	0.0695	0.0495	0.0642	0.0612	0.0612
1.80	0.0685	0.0680	0.0495	0.0655	0.0636	0.0588
1.85	0.0693	0.0668	0.0578	0.0618	0.0629	0.0585
1.90	0.0688	0.0678	0.0581	0.0627	0.0595	0.0612
1.95	0.0691	0.0652	0.0495	0.0629	0.0508	0.0592
2.00	0.0696	0.0689	0.0692	0.0688	0.0543	0.0611

C 2.50 - 3.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.6850	0.7523	0.8196	0.8869	0.9542	1.0215
2.50	0.0482	0.0512	0.0523	0.0671	0.0651	0.0650
2.55	0.0519	0.0508	0.0477	0.0604	0.0602	0.0642
2.60	0.0528	0.0539	0.0378	0.0581	0.0629	0.0639
2.65	0.0519	0.0560	0.0450	0.0585	0.0558	0.0633
2.70	0.0521	0.0510	0.0482	0.0570	0.0579	0.0641
2.75	0.0500	0.0500	0.0491	0.0540	0.0615	0.0635
2.80	0.0632	0.0552	0.0538	0.0528	0.0461	0.0685
2.85	0.0645	0.0601	0.0513	0.0550	0.0439	0.0530
2.90	0.0615	0.0599	0.0496	0.0495	0.0425	0.0509
2.95	0.0550	0.0547	0.0482	0.0503	0.0572	0.0585
3.00	0.0542	0.0523	0.0472	0.0511	0.0630	0.0628

B 3.50 - 4.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.3485	0.4158	0.4831	0.5504	0.6177	0.6850
3.50	0.0615	0.0522	0.0515	0.0445	0.0380	0.0400
3.55	0.0575	0.0542	0.0492	0.0345	0.0494	0.0442
3.60	0.0645	0.0492	0.0494	0.0445	0.0432	0.0434
3.65	0.0623	0.0509	0.0503	0.0492	0.0474	0.0338
6.70	0.0633	0.0505	0.0482	0.0494	0.0392	0.0372
3.75	0.0615	0.0489	0.0512	0.0512	0.0449	0.0395
3.80	0.0552	0.0469	0.0512	0.0472	0.0345	0.0318
3.85	0.0639	0.0458	0.0495	0.0434	0.0435	0.0455
3.90	0.0538	0.0446	0.0482	0.0429	0.0470	0.0452
3.95	0.0635	0.0466	0.0475	0.0449	0.0470	0.0412
4.00	0.0485	0.0441	0.0479	0.0395	0.0441	0.0473

C 4.50 - 5.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.6850	0.7523	0.8196	0.8869	0.9542	1.0215
4.50	0.0335	0.0438	0.0411	0.0452	0.0450	0.0638
4.55	0.0459	0.0389	0.0385	0.0379	0.0649	0.0629
4.60	0.0422	0.0343	0.0318	0.0615	0.0638	0.0622
4.65	0.0400	0.0369	0.0398	0.0408	0.0629	0.0631
4.70	0.0351	0.0391	0.0369	0.0448	0.0571	0.0619
4.75	0.0375	0.0432	0.0405	0.0441	0.0581	0.0572
4.80	0.0362	0.0425	0.0230	0.0431	0.0442	0.0492
4.85	0.0269	0.0382	0.0488	0.0435	0.0435	0.0528
4.90	0.0373	0.0359	0.0398	0.0419	0.0411	0.0472
4.95	0.0429	0.0359	0.0386	0.0403	0.0422	0.0619
5.00	0.0439	0.0371	0.0339	0.0432	0.0442	0.0638

B 5.50 - 6.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.3485	0.4158	0.4831	0.5504	0.6177	0.6850
5.50	0.0659	0.0426	0.0414	0.0385	0.0380	0.0382
5.55	0.0608	0.0412	0.0405	0.0291	0.0352	0.0386
5.60	0.0582	0.0385	0.0404	0.0351	0.0378	0.0396
5.65	0.0519	0.0352	0.0366	0.0355	0.0382	0.0372
5.70	0.0651	0.0412	0.0343	0.0362	0.0391	0.0285
5.75	0.0658	0.0384	0.0328	0.0335	0.0382	0.0333
5.80	0.0652	0.0359	0.0352	0.0309	0.0318	0.0346
5.85	0.0658	0.0339	0.0301	0.0398	0.0311	0.0302
5.90	0.0651	0.0305	0.0259	0.0354	0.0382	0.0278
5.95	0.0662	0.0329	0.0371	0.0359	0.0351	0.0168
6.00	0.0641	0.0335	0.0339	0.0321	0.0361	0.0369

Tables 21 - 25: Run 5

B 1.50 - 2.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.3485	0.4158	0.4831	0.5504	0.6177	0.6850
1.50	0.0572	0.0561	0.0608	0.0683	0.0702	0.0681
1.55	0.0690	0.0685	0.0670	0.0630	0.0584	0.0590
1.60	0.0678	0.0682	0.0645	0.0565	0.0468	0.0518
1.65	0.0680	0.0655	0.0618	0.0580	0.0535	0.0600
1.70	0.0695	0.0675	0.0615	0.0578	0.0545	0.0550
1.75	0.0682	0.0665	0.0579	0.0450	0.0535	0.0551
1.80	0.0669	0.0659	0.0574	0.0522	0.0541	0.0573
1.85	0.0686	0.0664	0.0474	0.0599	0.0518	0.0613
1.90	0.0682	0.0675	0.0512	0.0612	0.0590	0.0583
1.95	0.0681	0.0667	0.0568	0.0555	0.0584	0.0548
2.00	0.0670	0.0672	0.0522	0.0562	0.0586	0.0562

C 2.50 - 3.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.6850	0.7523	0.8196	0.8869	0.9542	1.0215
2.50	0.0391	0.0500	0.0498	0.0575	0.0628	0.0653
2.55	0.0531	0.0545	0.0560	0.0578	0.0611	0.0643
2.60	0.0536	0.0542	0.0561	0.0432	0.0646	0.0635
2.65	0.0487	0.0491	0.0504	0.0552	0.0618	0.0618
2.70	0.0501	0.0504	0.0481	0.0571	0.0562	0.0614
2.75	0.0530	0.0484	0.0504	0.0623	0.0615	0.0598
2.80	0.0538	0.0482	0.0360	0.0561	0.0562	0.0621
2.85	0.0531	0.0429	0.0445	0.0436	0.0581	0.0630
2.90	0.0509	0.0385	0.0395	0.0530	0.0614	0.0613
2.95	0.0497	0.0442	0.0514	0.0502	0.0579	0.0589
3.00	0.0498	0.0465	0.0474	0.0520	0.0566	0.0548

B 3.50 - 4.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.3485	0.4158	0.4831	0.5504	0.6177	0.6850
3.50	0.0564	0.0484	0.0446	0.0371	0.0365	0.0472
3.55	0.0646	0.0455	0.0398	0.0415	0.0495	0.0422
3.60	0.0627	0.0522	0.0453	0.0322	0.0458	0.0432
3.65	0.0540	0.0476	0.0479	0.0448	0.0470	0.0374
6.70	0.0639	0.0430	0.0420	0.0482	0.0346	0.0430
3.75	0.0647	0.0483	0.0389	0.0469	0.0404	0.0420
3.80	0.0601	0.0502	0.0472	0.0340	0.0466	0.0422
3.85	0.0591	0.0490	0.0459	0.0426	0.0486	0.0302
3.90	0.0636	0.0478	0.0483	0.0435	0.0440	0.0432
3.95	0.0533	0.0444	0.0469	0.0448	0.0468	0.0452
4.00	0.0539	0.0449	0.0451	0.0302	0.0444	0.0383

C 4.50 - 5.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.6850	0.7523	0.8196	0.8869	0.9542	1.0215
4.50	0.0432	0.0267	0.0449	0.0464	0.0515	0.0506
4.55	0.0288	0.0400	0.0431	0.0445	0.0555	0.0585
4.60	0.0305	0.0404	0.0443	0.0393	0.0489	0.0510
4.65	0.0308	0.0425	0.0438	0.0435	0.0452	0.0618
4.70	0.0265	0.0404	0.0415	0.0440	0.0454	0.0555
4.75	0.0294	0.0360	0.0385	0.0403	0.0430	0.0500
4.80	0.0374	0.0419	0.0382	0.0351	0.0443	0.0572
4.85	0.0348	0.0406	0.0370	0.0438	0.0489	0.0508
4.90	0.0295	0.0291	0.0372	0.0420	0.0428	0.0622
4.95	0.0254	0.0395	0.0309	0.0424	0.0448	0.0610
5.00	0.0315	0.0370	0.0389	0.0388	0.0462	0.0605

B 5.50 - 6.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.3485	0.4158	0.4831	0.5504	0.6177	0.6850
5.50	0.0528	0.0418	0.0403	0.0365	0.0359	0.0374
5.55	0.0657	0.0392	0.0309	0.0352	0.0348	0.0365
5.60	0.0669	0.0420	0.0398	0.0344	0.0209	0.0304
5.65	0.0663	0.0410	0.0398	0.0310	0.0260	0.0275
5.70	0.0653	0.0399	0.0372	0.0285	0.0278	0.0368
5.75	0.0650	0.0386	0.0354	0.0312	0.0136	0.0367
5.80	0.0641	0.0372	0.0333	0.0250	0.0306	0.0225
5.85	0.0644	0.0339	0.0366	0.0228	0.0237	0.0221
5.90	0.0645	0.0324	0.0372	0.0351	0.0260	0.0212
5.95	0.0658	0.0283	0.0368	0.0355	0.0229	0.0293
6.00	0.0650	0.0314	0.0362	0.0368	0.0237	0.0295

Tables 26 - 30: Run 6

B 1.50 - 2.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.3485	0.4158	0.4831	0.5504	0.6177	0.6850
1.50	0.0684	0.0689	0.0659	0.0629	0.0569	0.0552
1.55	0.0691	0.0685	0.0555	0.0621	0.0558	0.0594
1.60	0.0688	0.0650	0.0568	0.0562	0.0431	0.0568
1.65	0.0689	0.0682	0.0657	0.0452	0.0468	0.0635
1.70	0.0694	0.0692	0.0522	0.0550	0.0571	0.0638
1.75	0.0690	0.0688	0.0564	0.0571	0.0598	0.0635
1.80	0.0678	0.0689	0.0602	0.0539	0.0597	0.0589
1.85	0.0685	0.0681	0.0555	0.0502	0.0532	0.0500
1.90	0.0675	0.0670	0.0655	0.0436	0.0502	0.0449
1.95	0.0682	0.0675	0.0644	0.0532	0.0485	0.0513
2.00	0.0672	0.0670	0.0614	0.0500	0.0510	0.0390

C 2.50 - 3.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.6850	0.7523	0.8196	0.8869	0.9542	1.0215
2.50	0.0515	0.0508	0.0539	0.0519	0.0621	0.0630
2.55	0.0562	0.0465	0.0550	0.0491	0.0628	0.0639
2.60	0.0512	0.0404	0.0509	0.0508	0.0621	0.0622
2.65	0.0440	0.0559	0.0513	0.0538	0.0612	0.0622
2.70	0.0335	0.0567	0.0485	0.0459	0.0620	0.0628
2.75	0.0512	0.0540	0.0440	0.0419	0.0588	0.0613
2.80	0.0502	0.0535	0.0501	0.0501	0.0595	0.0641
2.85	0.0491	0.0380	0.0528	0.0532	0.0598	0.0600
2.90	0.0482	0.0480	0.0511	0.0495	0.0582	0.0610
2.95	0.0452	0.0419	0.0518	0.0540	0.0559	0.0612
3.00	0.0478	0.0440	0.0529	0.0538	0.0562	0.0589

B 3.50 - 4.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.3485	0.4158	0.4831	0.5504	0.6177	0.6850
3.50	0.0552	0.0482	0.0475	0.0498	0.0496	0.0502
3.55	0.0552	0.0458	0.0465	0.0489	0.0473	0.0503
3.60	0.0603	0.0392	0.0483	0.0419	0.0474	0.0478
3.65	0.0541	0.0332	0.0351	0.0450	0.0449	0.0459
6.70	0.0552	0.0502	0.0403	0.0488	0.0411	0.0474
3.75	0.0629	0.0502	0.0469	0.0452	0.0485	0.0469
3.80	0.0642	0.0479	0.0481	0.0434	0.0487	0.0436
3.85	0.0638	0.0488	0.0359	0.0382	0.0381	0.0443
3.90	0.0629	0.0486	0.0448	0.0422	0.0431	0.0447
3.95	0.0543	0.0465	0.0455	0.0322	0.0433	0.0455
4.00	0.0522	0.0438	0.0452	0.0462	0.0368	0.0415

C 4.50 - 5.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.6850	0.7523	0.8196	0.8869	0.9542	1.0215
4.50	0.0438	0.0342	0.0441	0.0459	0.0619	0.0630
4.55	0.0388	0.0430	0.0357	0.0462	0.0632	0.0599
4.60	0.0408	0.0424	0.0445	0.0448	0.0497	0.0530
4.65	0.0352	0.0302	0.0460	0.0443	0.0439	0.0507
4.70	0.0300	0.0250	0.0432	0.0448	0.0390	0.0552
4.75	0.0302	0.0422	0.0390	0.0444	0.0450	0.0628
4.80	0.0404	0.0402	0.0336	0.0432	0.0451	0.0620
4.85	0.0408	0.0398	0.0411	0.0398	0.0449	0.0619
4.90	0.0352	0.0318	0.0362	0.0419	0.0438	0.0530
4.95	0.0353	0.0414	0.0403	0.0392	0.0419	0.0472
5.00	0.0351	0.0414	0.0390	0.0332	0.0402	0.0587

B 5.50 - 6.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.3485	0.4158	0.4831	0.5504	0.6177	0.6850
5.50	0.0661	0.0658	0.0408	0.0346	0.0361	0.0267
5.55	0.0648	0.0585	0.0400	0.0354	0.0304	0.0367
5.60	0.0649	0.0540	0.0359	0.0400	0.0302	0.0373
5.65	0.0652	0.0616	0.0345	0.0392	0.0259	0.0335
5.70	0.0642	0.0642	0.0265	0.0395	0.0320	0.0294
5.75	0.0651	0.0635	0.0383	0.0382	0.0365	0.0302
5.80	0.0643	0.0599	0.0390	0.0343	0.0375	0.0252
5.85	0.0660	0.0641	0.0362	0.0316	0.0386	0.0283
5.90	0.0613	0.0611	0.0397	0.0295	0.0385	0.0302
5.95	0.0622	0.0415	0.0388	0.0333	0.0362	0.0269
6.00	0.0654	0.0384	0.0385	0.0365	0.0346	0.0268

Tables 31 - 35: Run 7

B 1.50 - 2.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.3485	0.4158	0.4831	0.5504	0.6177	0.6850
1.50	0.0704	0.0710	0.0685	0.0507	0.0518	0.0431
1.55	0.0703	0.0703	0.0678	0.0622	0.0547	0.0568
1.60	0.0704	0.0703	0.0669	0.0604	0.0450	0.0453
1.65	0.0701	0.0702	0.0612	0.0579	0.0543	0.0356
1.70	0.0700	0.0678	0.0578	0.0620	0.0511	0.0482
1.75	0.0703	0.0702	0.0593	0.0632	0.0600	0.0572
1.80	0.0697	0.0682	0.0472	0.0599	0.0594	0.0451
1.85	0.0682	0.0700	0.0663	0.0583	0.0599	0.0503
1.90	0.0695	0.0638	0.0469	0.0613	0.0602	0.0488
1.95	0.0685	0.0636	0.0520	0.0565	0.0565	0.0575
2.00	0.0692	0.0625	0.0578	0.0612	0.0565	0.0504

C 2.50 - 3.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.6850	0.7523	0.8196	0.8869	0.9542	1.0215
2.50	0.0631	0.0592	0.0643	0.0536	0.0483	0.0395
2.55	0.0624	0.0609	0.0569	0.0402	0.0515	0.0528
2.60	0.0623	0.0592	0.0502	0.0424	0.0482	0.0526
2.65	0.0482	0.0399	0.0364	0.0550	0.0578	0.0610
2.70	0.0378	0.0476	0.0404	0.0482	0.0582	0.0614
2.75	0.0379	0.0491	0.0512	0.0468	0.0460	0.0605
2.80	0.0519	0.0513	0.0512	0.0523	0.0365	0.0608
2.85	0.0498	0.0442	0.0412	0.0511	0.0476	0.0606
2.90	0.0499	0.0489	0.0519	0.0502	0.0502	0.0592
2.95	0.0532	0.0528	0.0518	0.0493	0.0455	0.0590
3.00	0.0469	0.0519	0.0481	0.0455	0.0540	0.0605

B 3.50 - 4.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.3485	0.4158	0.4831	0.5504	0.6177	0.6850
3.50	0.0635	0.0462	0.0498	0.0501	0.0422	0.0336
3.55	0.0558	0.0468	0.0442	0.0458	0.0373	0.0342
3.60	0.0632	0.0449	0.0452	0.0394	0.0413	0.0426
3.65	0.0631	0.0498	0.0451	0.0459	0.0436	0.0472
6.70	0.0580	0.0466	0.0249	0.0370	0.0368	0.0448
3.75	0.0528	0.0446	0.0429	0.0418	0.0450	0.0486
3.80	0.0461	0.0392	0.0454	0.0429	0.0354	0.0475
3.85	0.0532	0.0419	0.0418	0.0378	0.0380	0.0468
3.90	0.0542	0.0451	0.0338	0.0419	0.0448	0.0462
3.95	0.0489	0.0428	0.0391	0.0418	0.0427	0.0446
4.00	0.0532	0.0468	0.0351	0.0362	0.0425	0.0414

C 4.50 - 5.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.6850	0.7523	0.8196	0.8869	0.9542	1.0215
4.50	0.0394	0.0361	0.0449	0.0435	0.0449	0.0603
4.55	0.0405	0.0418	0.0445	0.0438	0.0579	0.0550
4.60	0.0428	0.0428	0.0482	0.0435	0.0496	0.0498
4.65	0.0392	0.0410	0.0418	0.0420	0.0460	0.0628
4.70	0.0400	0.0403	0.0419	0.0409	0.0598	0.0603
4.75	0.0413	0.0405	0.0406	0.0403	0.0515	0.0562
4.80	0.0396	0.0375	0.0409	0.0432	0.0495	0.0621
4.85	0.0387	0.0350	0.0405	0.0423	0.0600	0.0565
4.90	0.0378	0.0379	0.0402	0.0429	0.0432	0.0467
4.95	0.0362	0.0371	0.0384	0.0399	0.0415	0.0545
5.00	0.0339	0.0360	0.0382	0.0383	0.0461	0.0629

B 5.50 - 6.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.3485	0.4158	0.4831	0.5504	0.6177	0.6850
5.50	0.0607	0.0534	0.0348	0.0268	0.0353	0.0349
5.55	0.0551	0.0482	0.0368	0.0362	0.0274	0.0239
5.60	0.0638	0.0551	0.0374	0.0342	0.0348	0.0294
5.65	0.0661	0.0663	0.0383	0.0265	0.0365	0.0242
5.70	0.0649	0.0651	0.0368	0.0334	0.0357	0.0347
5.75	0.0601	0.0628	0.0371	0.0349	0.0332	0.0345
5.80	0.0649	0.0438	0.0355	0.0276	0.0324	0.0221
5.85	0.0672	0.0645	0.0366	0.0275	0.0306	0.0317
5.90	0.0666	0.0621	0.0355	0.0333	0.0306	0.0302
5.95	0.0668	0.0652	0.0360	0.0322	0.0235	0.0332
6.00	0.0652	0.0562	0.0370	0.0195	0.0322	0.0338

Tables 36 - 40: Run 8

B 1.50 - 2.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.3485	0.4158	0.4831	0.5504	0.6177	0.6850
1.50	0.0694	0.0682	0.0663	0.0532	0.0564	0.0559
1.55	0.0679	0.0672	0.0668	0.0465	0.0638	0.0574
1.60	0.0665	0.0680	0.0632	0.0440	0.0628	0.0590
1.65	0.0672	0.0668	0.0652	0.0548	0.0582	0.0569
1.70	0.0664	0.0672	0.0514	0.0502	0.0558	0.0620
1.75	0.0685	0.0680	0.0614	0.0574	0.0590	0.0618
1.80	0.0669	0.0678	0.0575	0.0512	0.0459	0.0602
1.85	0.0680	0.0670	0.0552	0.0582	0.0510	0.0500
1.90	0.0678	0.0662	0.0532	0.0582	0.0573	0.0521
1.95	0.0669	0.0650	0.0451	0.0504	0.0585	0.0540
2.00	0.0676	0.0648	0.0570	0.0572	0.0536	0.0564

C 2.50 - 3.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.6850	0.7523	0.8196	0.8869	0.9542	1.0215
2.50	0.0431	0.0455	0.0524	0.0610	0.0639	0.0638
2.55	0.0348	0.0490	0.0553	0.0622	0.0641	0.0640
2.60	0.0440	0.0448	0.0549	0.0624	0.0622	0.0632
2.65	0.0375	0.0423	0.0524	0.0585	0.0609	0.0638
2.70	0.0481	0.0458	0.0523	0.0610	0.0608	0.0640
2.75	0.0430	0.0500	0.0469	0.0568	0.0595	0.0630
2.80	0.0460	0.0493	0.0473	0.0586	0.0641	0.0625
2.85	0.0392	0.0493	0.0526	0.0592	0.0619	0.0579
2.90	0.0428	0.0582	0.0493	0.0562	0.0584	0.0579
2.95	0.0422	0.0494	0.0458	0.0549	0.0520	0.0620
3.00	0.0500	0.0508	0.0480	0.0490	0.0629	0.0630

B 3.50 - 4.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.3485	0.4158	0.4831	0.5504	0.6177	0.6850
3.50	0.0562	0.0558	0.0339	0.0352	0.0344	0.0338
3.55	0.0535	0.0481	0.0408	0.0340	0.0414	0.0373
3.60	0.0596	0.0508	0.0439	0.0408	0.0385	0.0371
3.65	0.0569	0.0513	0.0338	0.0468	0.0458	0.0309
6.70	0.0574	0.0546	0.0375	0.0456	0.0474	0.0265
3.75	0.0553	0.0535	0.0440	0.0414	0.0412	0.0380
3.80	0.0651	0.0523	0.0416	0.0433	0.0298	0.0394
3.85	0.0503	0.0585	0.0424	0.0422	0.0355	0.0433
3.90	0.0642	0.0639	0.0505	0.0412	0.0310	0.0375
3.95	0.0618	0.0646	0.0503	0.0440	0.0245	0.0422
4.00	0.0602	0.0622	0.0478	0.0418	0.0410	0.0472

C 4.50 - 5.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.6850	0.7523	0.8196	0.8869	0.9542	1.0215
4.50	0.0422	0.0459	0.0455	0.0295	0.0622	0.0639
4.55	0.0345	0.0435	0.0442	0.0398	0.0611	0.0635
4.60	0.0450	0.0374	0.0389	0.0410	0.0631	0.0629
4.65	0.0448	0.0378	0.0348	0.0458	0.0500	0.0554
4.70	0.0440	0.0443	0.0330	0.0455	0.0573	0.0540
4.75	0.0370	0.0419	0.0255	0.0430	0.0628	0.0592
4.80	0.0405	0.0381	0.0382	0.0430	0.0542	0.0592
4.85	0.0430	0.0362	0.0409	0.0378	0.0610	0.0588
4.90	0.0402	0.0358	0.0398	0.0409	0.0482	0.0510
4.95	0.0388	0.0329	0.0372	0.0428	0.0448	0.0495
5.00	0.0361	0.0339	0.0355	0.0473	0.0482	0.0508

B 5.50 - 6.00

Distance From Source (m)	Distance Cross-stream (m)					
	0.3485	0.4158	0.4831	0.5504	0.6177	0.6850
5.50	0.0632	0.0422	0.0282	0.0351	0.0164	0.0352
5.55	0.0528	0.0403	0.0394	0.0304	0.0351	0.0344
5.60	0.0641	0.0428	0.0355	0.0265	0.0333	0.0323
5.65	0.0338	0.0415	0.0238	0.0361	0.0378	0.0250
5.70	0.0600	0.0380	0.0359	0.0340	0.0372	0.0273
5.75	0.0622	0.0401	0.0378	0.0346	0.0384	0.0245
5.80	0.0639	0.0410	0.0322	0.0314	0.0298	0.0311
5.85	0.0645	0.0395	0.0366	0.0268	0.0328	0.0272
5.90	0.0641	0.0382	0.0353	0.0236	0.0344	0.0332
5.95	0.0634	0.0369	0.0355	0.0225	0.0330	0.0297
6.00	0.0542	0.0356	0.0355	0.0322	0.0303	0.0363

Section C: Initial Height of Drainage Surface

Tables 1 - 5: Run 1

B 1.50 - 2.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.3485	0.4831	0.6177
1.50	0.0660	0.0661	0.0638
1.60	0.0659	0.0651	0.0645
1.70	0.0649	0.0650	0.0641
1.80	0.0651	0.0652	0.0648
1.90	0.0652	0.0645	0.0638
2.00	0.0641	0.0646	0.0630

C 2.50 - 3.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.6850	0.8196	0.9542
2.50	0.0612	0.0618	0.0611
2.60	0.0635	0.0602	0.0605
2.70	0.0628	0.0612	0.0615
2.80	0.0630	0.0618	0.0613
2.90	0.0630	0.0621	0.0608
3.00	0.0639	0.0612	0.0605

B 3.50 - 4.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.3485	0.4831	0.6177
3.50	0.0621	0.0619	0.0613
3.60	0.0622	0.0621	0.0619
3.70	0.0631	0.0620	0.0619
3.80	0.0631	0.0621	0.0601
3.90	0.0614	0.0613	0.0604
4.00	0.0614	0.0615	0.0602

C 4.50 - 5.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.6850	0.8196	0.9542
4.50	0.0619	0.0620	0.0615
4.60	0.0628	0.0611	0.0618
4.70	0.0619	0.0612	0.0615
4.80	0.0625	0.0601	0.0600
4.90	0.0631	0.0612	0.0613
5.00	0.0610	0.0618	0.0609

B 5.50 - 6.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.3485	0.4831	0.6177
5.50	0.0621	0.0619	0.0628
5.60	0.0629	0.0625	0.0610
5.70	0.0641	0.0619	0.0630
5.80	0.0635	0.0615	0.0612
5.90	0.0635	0.0621	0.0612
6.00	0.0629	0.0618	0.0608

Tables 6 - 10: Run 2

B 1.50 - 2.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.3485	0.4831	0.6177
1.50	0.0690	0.0668	0.0672
1.60	0.0685	0.0680	0.0665
1.70	0.0679	0.0673	0.0669
1.80	0.0675	0.0672	0.0638
1.90	0.0685	0.0682	0.0649
2.00	0.0679	0.0672	0.0662

C 2.50 - 3.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.6850	0.8196	0.9542
2.50	0.0600	0.0645	0.0642
2.60	0.0650	0.0642	0.0641
2.70	0.0634	0.0650	0.0645
2.80	0.0625	0.0632	0.0634
2.90	0.0618	0.0632	0.0633
3.00	0.0624	0.0645	0.0640

B 3.50 - 4.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.3485	0.4831	0.6177
3.50	0.0630	0.0642	0.0631
3.60	0.0634	0.0638	0.0604
3.70	0.0639	0.0640	0.0618
3.80	0.0649	0.0650	0.0609
3.90	0.0650	0.0652	0.0629
4.00	0.0649	0.0650	0.0630

C 4.50 - 5.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.6850	0.8196	0.9542
4.50	0.0621	0.0643	0.0642
4.60	0.0621	0.0651	0.0641
4.70	0.0612	0.0648	0.0642
4.80	0.0619	0.0638	0.0638
4.90	0.0605	0.0635	0.0632
5.00	0.0619	0.0642	0.0644

B 5.50 - 6.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.3485	0.4831	0.6177
5.50	0.0645	0.0646	0.0628
5.60	0.0645	0.0639	0.0618
5.70	0.0645	0.0648	0.0602
5.80	0.0648	0.0621	0.0595
5.90	0.0648	0.0631	0.0589
6.00	0.0654	0.0645	0.0595

Tables 11 - 15: Run 3

B 1.50 - 2.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.3485	0.4831	0.6177
1.50	0.0689	0.0692	0.0661
1.60	0.0689	0.0689	0.0668
1.70	0.0678	0.0689	0.0661
1.80	0.0678	0.0671	0.0663
1.90	0.0670	0.0672	0.0648
2.00	0.0681	0.0688	0.0642

C 2.50 - 3.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.6850	0.8196	0.9542
2.50	0.0618	0.0648	0.0651
2.60	0.0642	0.0645	0.0645
2.70	0.0635	0.0652	0.0651
2.80	0.0632	0.0651	0.0653
2.90	0.0615	0.0650	0.0645
3.00	0.0619	0.0658	0.0651

B 3.50 - 4.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.3485	0.4831	0.6177
3.50	0.0661	0.0652	0.0651
3.60	0.0649	0.0655	0.0642
3.70	0.0651	0.0655	0.0645
3.80	0.0658	0.0661	0.0659
3.90	0.0655	0.0662	0.0668
4.00	0.0658	0.0658	0.0658

C 4.50 - 5.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.6850	0.8196	0.9542
4.50	0.0665	0.0651	0.0645
4.60	0.0681	0.0652	0.0652
4.70	0.0688	0.0654	0.0652
4.80	0.0672	0.0655	0.0651
4.90	0.0670	0.0668	0.0652
5.00	0.0658	0.0658	0.0654

B 5.50 - 6.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.3485	0.4831	0.6177
5.50	0.0652	0.0648	0.0643
5.60	0.0646	0.0645	0.0630
5.70	0.0654	0.0648	0.0624
5.80	0.0645	0.0645	0.0618
5.90	0.0654	0.0652	0.0622
6.00	0.0652	0.0646	0.0618

Tables 16 - 20: Run 4

B 1.50 - 2.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.3485	0.4831	0.6177
1.50	0.0701	0.0695	0.0681
1.60	0.0699	0.0696	0.0676
1.70	0.0695	0.0688	0.0678
1.80	0.0685	0.0690	0.0688
1.90	0.0688	0.0689	0.0683
2.00	0.0691	0.0690	0.0675

C 2.50 - 3.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.6850	0.8196	0.9542
2.50	0.0670	0.0660	0.0653
2.60	0.0651	0.0645	0.0651
2.70	0.0635	0.0651	0.0648
2.80	0.0645	0.0657	0.0656
2.90	0.0629	0.0658	0.0645
3.00	0.0619	0.0651	0.0645

B 3.50 - 4.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.3485	0.4831	0.6177
3.50	0.0656	0.0654	0.0629
3.60	0.0649	0.0651	0.0602
3.70	0.0649	0.0651	0.0600
3.80	0.0652	0.0654	0.0585
3.90	0.0648	0.0642	0.0572
4.00	0.0656	0.0654	0.0613

C 4.50 - 5.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.6850	0.8196	0.9542
4.50	0.0671	0.0662	0.0651
4.60	0.0672	0.0652	0.0645
4.70	0.0682	0.0645	0.0648
4.80	0.0688	0.0652	0.0645
4.90	0.0661	0.0580	0.0639
5.00	0.0671	0.0652	0.0652

B 5.50 - 6.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.3485	0.4831	0.6177
5.50	0.0661	0.0660	0.0659
5.60	0.0668	0.0669	0.0655
5.70	0.0658	0.0661	0.0655
5.80	0.0658	0.0659	0.0649
5.90	0.0655	0.0653	0.0629
6.00	0.0649	0.0645	0.0632

Tables 21 - 25: Run 5

B 1.50 - 2.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.3485	0.4831	0.6177
1.50	0.0692	0.0685	0.0660
1.60	0.0692	0.0690	0.0662
1.70	0.0685	0.0678	0.6550
1.80	0.0685	0.0685	0.6480
1.90	0.0681	0.0683	0.0658
2.00	0.0680	0.0684	0.0659

C 2.50 - 3.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.6850	0.8196	0.9542
2.50	0.0670	0.0651	0.0654
2.60	0.0668	0.0648	0.0649
2.70	0.0668	0.0650	0.0642
2.80	0.0660	0.0649	0.0648
2.90	0.0648	0.0640	0.0639
3.00	0.0640	0.0642	0.0638

B 3.50 - 4.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.3485	0.4831	0.6177
3.50	0.0658	0.0668	0.0659
3.60	0.0660	0.0655	0.0648
3.70	0.0650	0.0652	0.0644
3.80	0.0652	0.0653	0.0642
3.90	0.0652	0.0653	0.0653
4.00	0.0660	0.0665	0.0669

C 4.50 - 5.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.6850	0.8196	0.9542
4.50	0.0653	0.0637	0.0632
4.60	0.0656	0.0650	0.0642
4.70	0.0655	0.0652	0.0650
4.80	0.0661	0.0659	0.0654
4.90	0.0654	0.0661	0.0651
5.00	0.0639	0.0651	0.0644

B 5.50 - 6.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.3485	0.4831	0.6177
5.50	0.0662	0.0663	0.0638
5.60	0.0680	0.0672	0.0648
5.70	0.0665	0.0654	0.0629
5.80	0.0655	0.0645	0.0634
5.90	0.0661	0.0652	0.0634
6.00	0.0662	0.0651	0.0632

Tables 26 - 30: Run 6

B 1.50 - 2.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.3485	0.4831	0.6177
1.50	0.0689	0.0690	0.0668
1.60	0.0694	0.0696	0.0661
1.70	0.0703	0.0695	0.0654
1.80	0.0693	0.0684	0.0655
1.90	0.0683	0.0684	0.0645
2.00	0.0673	0.0680	0.0638

C 2.50 - 3.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.6850	0.8196	0.9542
2.50	0.0629	0.0652	0.6510
2.60	0.0632	0.0646	0.0645
2.70	0.0635	0.0644	0.0643
2.80	0.0638	0.0642	0.0633
2.90	0.0635	0.0642	0.0626
3.00	0.0643	0.0645	0.0638

B 3.50 - 4.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.3485	0.4831	0.6177
3.50	0.0658	0.0659	0.0657
3.60	0.0649	0.0650	0.0640
3.70	0.0654	0.0647	0.0642
3.80	0.0648	0.0643	0.0627
3.90	0.0643	0.0652	0.0638
4.00	0.0657	0.0650	0.0638

C 4.50 - 5.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.6850	0.8196	0.9542
4.50	0.0646	0.0642	0.0641
4.60	0.0652	0.0649	0.0639
4.70	0.0668	0.0660	0.0649
4.80	0.0655	0.0640	0.0636
4.90	0.0642	0.0648	0.0633
5.00	0.0636	0.0652	0.0634

B 5.50 - 6.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.3485	0.4831	0.6177
5.50	0.0658	0.0661	0.0638
5.60	0.0663	0.0652	0.0636
5.70	0.0654	0.0653	0.0634
5.80	0.0658	0.0651	0.0624
5.90	0.0656	0.0653	0.0620
6.00	0.0664	0.0645	0.0618

Tables 31 - 35: Run 7

B 1.50 - 2.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.3485	0.4831	0.6177
1.50	0.0720	0.0709	0.0702
1.60	0.0704	0.0708	0.0695
1.70	0.0700	0.0702	0.0675
1.80	0.0712	0.0708	0.0692
1.90	0.0694	0.0685	0.0700
2.00	0.0699	0.0692	0.0688

C 2.50 - 3.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.6850	0.8196	0.9542
2.50	0.0638	0.0652	0.0654
2.60	0.0650	0.0649	0.0641
2.70	0.0644	0.0642	0.0639
2.80	0.0631	0.0632	0.0641
2.90	0.0634	0.0639	0.0634
3.00	0.0631	0.0646	0.0648

B 3.50 - 4.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.3485	0.4831	0.6177
3.50	0.0646	0.0655	0.0629
3.60	0.0649	0.0632	0.0615
3.70	0.0652	0.0643	0.0606
3.80	0.0652	0.0640	0.0608
3.90	0.0645	0.0632	0.0634
4.00	0.0650	0.0642	0.0655

C 4.50 - 5.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.6850	0.8196	0.9542
4.50	0.0649	0.0643	0.0638
4.60	0.0628	0.0647	0.0639
4.70	0.0632	0.0647	0.0638
4.80	0.0645	0.0654	0.0632
4.90	0.0638	0.0638	0.0642
5.00	0.0648	0.0650	0.0650

B 5.50 - 6.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.3485	0.4831	0.6177
5.50	0.0662	0.0648	0.0642
5.60	0.0662	0.0651	0.0645
5.70	0.0662	0.0653	0.0658
5.80	0.0634	0.0645	0.0640
5.90	0.0672	0.0655	0.0662
6.00	0.0651	0.0652	0.0646

Tables 36 - 40: Run 8

B 1.50 - 2.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.3485	0.4831	0.6177
1.50	0.0700	0.0683	0.0652
1.60	0.0685	0.0684	0.0647
1.70	0.0669	0.0673	0.0668
1.80	0.0680	0.0673	0.0654
1.90	0.0679	0.0667	0.0633
2.00	0.0680	0.0678	0.0655

C 2.50 - 3.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.6850	0.8196	0.9542
2.50	0.0581	0.0645	0.0633
2.60	0.0612	0.0629	0.0624
2.70	0.0608	0.0630	0.0625
2.80	0.0612	0.0643	0.0638
2.90	0.0612	0.0647	0.0627
3.00	0.0617	0.0648	0.0638

B 3.50 - 4.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.3485	0.4831	0.6177
3.50	0.0645	0.0641	0.0658
3.60	0.0643	0.0648	0.0618
3.70	0.0658	0.0638	0.0602
3.80	0.0668	0.0663	0.0594
3.90	0.0658	0.0662	0.0600
4.00	0.0639	0.0649	0.0595

C 4.50 - 5.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.6850	0.8196	0.9542
4.50	0.0634	0.0642	6.4400
4.60	0.0633	0.0649	6.3200
4.70	0.0639	0.0641	6.2200
4.80	0.0642	0.0633	6.4100
4.90	0.0653	0.0642	6.4100
5.00	0.0632	0.0649	6.3300

B 5.50 - 6.00

Distance From Source (m)	Distance Cross-stream (m)		
	0.3485	0.4831	0.6177
5.50	6.5400	6.3000	6.0500
5.60	6.4500	6.4200	5.8100
5.70	6.4300	6.5100	5.5200
5.80	6.4600	6.4500	5.5200
5.90	6.4800	6.3900	5.3800
6.00	6.5300	6.4800	5.7800

Section D: Percentage Sediment Saturation

Run No.	Section				
	b1	c2	b3	c4	b5
1	18.34	19.58	18.24	18.81	19.60
2	18.98	19.19	18.13	19.85	19.78
3	19.14	19.60	18.38	19.13	18.42
4	18.75	18.57	18.86	18.56	19.78
5	19.35	19.72	18.81	19.00	19.07
6	19.33	19.34	18.82	18.54	19.11
7	18.01	19.10	18.45	17.71	17.71
8	16.00	17.22	16.06	17.32	17.26

Section E: Temperature

Run No.	Temp. (°C)
1	13.80
2	13.80
3	13.40
4	13.40
5	14.00
6	13.80
7	13.00
8	13.20

Section F: Weight of Eroded Sediment

Run No.	Weight (kg)
1	178.49
2	186.88
3	204.57
4	205.48
5	200.49
6	219.54
7	194.37
8	210.47

Section G: Table of All Runs Performed

Run No.	Discharge (l/s)			Sediment	Slope
	Hour 1	Hour 2	Hour 3		
1	1.000	1.000	1.000	standard	1:100
2	1.000	1.000	1.000	standard	1:100
3	1.000	1.000	1.000	standard	1:100
4	1.000	1.000	1.000	standard	1:100
5	1.000	1.000	1.000	standard	1:100
6	1.000	1.000	1.000	standard	1:100
7	1.000	1.000	1.000	standard	1:100
8	1.000	1.000	1.000	standard	1:100
9	0.800	0.900	1.300	standard	1:100
10	0.900	0.900	1.200	standard	1:100
11	0.900	1.300	0.800	standard	1:100
12	0.500	1.500	1.000	standard	1:100
13	1.500	1.000	0.500	standard	1:100
14	1.000	1.000	1.000	5% kaolinite	1:100
15	1.000	1.000	1.000	10% kaolinite	1:100

See **chapter six** for standard sediment conditions. Percentage kaolinite is in terms of total sediment weight.

References

Abbott, J. E. and **Francis, J. R. D. 1977.** Saltation and Suspension Trajectories of Solid Grains in a Water Stream. Philosophical Transactions of the Royal Society, vol. 284A, pp. 225 - 254.

Ackers, P. 1964. Experiments on Small Streams in Alluvium. Journal of the Hydraulics Division of the American Society of Civil Engineers, vol. 90, 4, pp. 1 - 37.

Ackers, P. and **Charlton, F. G. 1970.** Dimensional Analysis of Alluvial Channels With Special Reference to Meander Length. Journal of Hydraulic Research, vol 8, pp. 287 - 315.

Alabyan, A. M. and **Chalov, R. S. 1998.** Types of River Channel Pattern and Their Natural Controls. Earth Surface Processes and Landforms,vol. 23, pp. 467 - 474.

Albritton, Jr. C. C. (Ed.) **1963.** The Fabric of Geology. Addison-Wesley Publishing Company, U. S. A.

Albritton, Jr. C. C. 1963b. Introduction. In: **Albritton, Jr. C. C.** (Ed.) **1963.** The Fabric of Geology, pp. v - vii. Addison-Wesley Publishing Company, U. S. A.

Albritton, Jr. C. C. (Ed.) **1975.** Introduction. In: **Albritton, Jr. C. C.** (Ed.) **1975.** Philosophy of Geohistory 1785 - 1970: Benchmark Papers in Geology, vol. 13, pp. 1 - 10. Dowden, Hutchinson & Ross, Stroudsburg, Pensylvania, U. S. A.

Allaby, A. and **Allaby, M. 1999.** Oxford Dictionary of Earth Sciences, Second Edition. Oxford University Press, Oxford, England.

Andrle, R. and **Abrahams, A. D. 1989.** Fractal Techniques and the Surface Roughness of Talus Slopes. Earth Surface Processes and Landforms, vol. 14, pp. 197 - 209.

Alligood, K. T., Sauer, T. D. and **Yorke, J. A. 1996.** Chaos: an Introduction to Dynamical Systems. Springer-Verlag, New York, U. S. A.

Andreev, O.V. and **Yaroslavtsev, I. A. 1958.** Modelling of Channel Deformations (basics). In: **Kondrat'ev, N. E.** (Ed.) **1958.** Channel processes, pp. 173 - 181. Academy of Sciences, Moscow, (in Russian).

Ashworth, P. J., Best, J. L., Leddy, J. O. and **Geehan, G. W. 1994.** The Physical Modelling of Braided Rivers and Deposition of Fine-Grained Sediment. In: **Kirkby, M. J.** (Ed.) **1994.** Process Models and Theoretical Geomorphology, pp. 115 - 139. John Wiley & Sons, Chichester, England.

Baeyer, Hans Christian, von 1999. Warmth Disperses and Time Passes: The History of Heat. The Modern Library, New York, U. S. A..

Bagnold, R. A. 1941. The Physics of Blown Sand and Desert Dunes. Methuen, London, England.

Bagnold, R. A. 1990. Sand, Wind and War: Memoirs of a Desert Explorer. The University of Arizona Press, Tuscon, U. S. A.

Bailey, C. 1928. The Greek Atomists and Epicurus, A Study. Oxford University Press, Oxford, England.

Bailey, C. 1947. Lucretius, De Rerum Natura, vol. 2, commentary, books I - III. Oxford University Press, Oxford, England.

Baker, G. L. and **Gollub, J. P. 1990.** Chaotic Dynamics: an Introduction. Cambridge University Press, Cambridge, England.

Baker, V. R. 1993. Extraterrestrial Geomorphology: Science and Philosophy of Earthlike Planetary Landscapes. Geomorphology, vol. 7, pp. 9 - 36.

Baker, V. R. 1996. Hypotheses and Geomorphological Reasoning. In: **Rhoads, B. L.** and **Thorn, C. E.** (Eds.) **1996.** The Scientific Nature of Geomorphology: Proceedings of the 27th Binghampton Symposium in Geomorphology, pp. 57 - 86.

Baker, V. R. and **Pyne, S. 1978.** G. K. Gilbert and Modern Geomorphology. American Journal of Science, vol. 278, pp. 97 - 123.

Baker, V. R. and **Twidale, C. R. 1991.** The Re-enchantment of Geomorphology. Geomorphology, vol. 4, pp. 73 - 100.

Bassett, K. 1994. Short Communication: Comments on Richards: the Problems of 'Real' Geomorphology. Earth Surface Processes and Landforms, vol. 19, pp. 273 - 276.

Bauer, B. O. 1996. Geomorphology, Geography and Science. In: **Rhoads, B. L.** and **Thorn, C. E.** (Eds.) **1996.** The Scientific Nature of Geomorphology: Proceedings of the 27th Binghampton Symposium in Geomorphology, pp. 381 - 413.

Bell, E. T. 1951. Introductory Note. In: **Laplace, P. S. 1814.** A Philosophical Essay on Probabilities, Dover Edition **1995.** Dover Publications, New York, U. S. A.

Bennett R. J. and **Chorley R. J. 1978.** Environmental Systems: Philosophy, Analysis and Control. Methuen, London, England.

Bergé, P., Pomeau, Y., and **Vidal, C., 1984.** Order Within Chaos: Towards a Deterministic Approach to Turbulence. John Wiley & Sons, New York, U. S. A.

Bernoulli, James 1713. Ars Conjectandi. In: **Sung, Bing, 1966.** Translations from James Bernoulli. Department of Statistics, Harvard University, Technical Report No. 2.

Bertalanffy, L. Von. 1950. An Outline of General System Theory. British Journal, Philosophy of Science, vol. 1, pp. 134 - 165.

Bertalanffy, L. von 1969. General Systems Theory: Foundations, Development, Applications. George Braziller, New York, U. S. A.

Beven. K. 1996. Equifinality and Uncertainty in Geomorphological Modelling. In: **Rhoads, B. L.** and **Thorn, C. E.** (Eds.) **1996.** The Scientific Nature of Geomorphology: Proceedings of the 27th Binghampton Symposium in Geomorphology, pp. 289 - 313.

Bishop, P. 1980. Popper's Principle of Falsifiability and the Irrefutability of the Davisian Cycle. Professional Geographer, vol. 32, pp. 310 - 315.

Born, M. 1934. Experiment and Theory in Physics. Cambridge University Press, Cambridge, England.

Born, M. 1971. The Born - Einstein Letters: Correspondence between Albert Einstein and Max and Hedwig Born from 1916 to 1955 with commentaries by Max Born. Macmillan Press, London and Basingstoke, England.

Boyd, R. 1983. On the Current Status of Scientific Realism. In: **Boyd, R., Gasper, P.** and **Trout, J. D.** (Eds.) **1991.** The Philosophy of Science, pp. 223 - 246. The M. I. T. Press, Cambridge, Massachusetts U. S. A.

Bradley, W. H. 1963. Geologic Laws. In: **Albritton, Jr. C. C.** (Ed.) **1963.** The Fabric of Geology, pp. 12 - 23. Freeman, Cooper & Company, San Francisco, U. S. A.

Bray, D. I. 1982. Regime Equations for Gravel-Bed Rivers. In: **Hey, R. D, Bathurst, J. C.** and **Thorne, C. R.** (Eds.) **1982.** Gravel-Bed Rivers: Fluvial Processes, Engineering and Management, pp. 517 - 541. John Wiley & Sons, Chichester, England.

Brennan, R. P. 1997. Heisenberg Probably Slept Here: The Lives, Times, and Ideas of the Great Physicists of the 20th Century. John Wiley & Sons, New York, U. S. A.

Brewer, P. A. and **Lewin. J. 1998.** Planform Cyclicity in an Unstable Reach: Complex Fluvial Response to Environmental Change. Earth Surface Processes and Landforms, vol. 23, pp. 989 - 1008.

Brice, J. C. 1960. Index for Description of Channel Braiding. Geological Society of America Bulletin Abstracts., vol. 71, p. 1833.

Brice, J. C. 1964. Channel Patterns and Terraces of the Loup Rivers in Nebraska. United States Geological Survey Professional Paper 422 -D.

Brookes, A. 1996. River Restoration Experience in Northern Europe. In: **Brookes. A.** and **Shields, Jr. F. D.** (Eds.) **1996.** River Channel Restoration: Guiding Principles for Sustainable Projects, pp. 233 - 267. John Wiley & Sons, Chichester, England.

Brown, H. I. 1977. Perception, Theory and Commitment: The New Philosophy of Science. The University of Chicago Press, Chicago, U. S. A. and London, England.

Brown, H. I. 1996. The Methodological Roles of Theory in Science. In: **Rhoads, B. L.** and **Thorn, C. E.** (Eds.) **1996.** The Scientific Nature of Geomorphology: Proceedings of the 27th Binghampton Symposium in Geomorphology, pp. 3 - 20.

Brunsden, D. and **Thornes, J. B. 1979.** Landscape Sensitivity and Change. Institute of British Geographers, Transactions, New Series, vol. 4, pp. 463 - 483.

Bunge, M. 1967. Scientific Research 1: The Search for System. Springer-Verlag, New York, U. S. A.

Bunge, M. 1979. Causality and Modern Science, Third Revised Edition. Dover Publications, New York, U. S. A.

Calaprice, A. (Ed.) **1996.** The Quotable Einstein. Princeton University Press, Princeton, New Jersey, U. S. A.

Carlston, C. W. 1965. The Relation of Free Meander Geometry to Stream Discharge and its Geomorphic Implications. American Journal of Science, vol. 263, pp. 864 - 885.

Carnap, R. 1950. Logical Foundations of Probability. Chicago.

Carson, M. A. 1984. The Meandering-Braided River Threshold: a Reappraisal. Journal of Hydrology, vol. 73, pp. 315 - 334.

Cartwright, N. 1981. The Reality of Causes in a World of Instrumental Laws. In: **Boyd, R., Gasper, P.** and **Trout, J. D.** (Eds.) **1991.** The Philosophy of Science, pp. 379 - 386. The M. I. T. Press, Cambridge, Massachusetts, U. S. A.

Casti, J. L. 1991. Searching for Certainty: What Science Can Know About the Future. Scribners, London, England.

Castillo, V. M. 1998. Calculating the Box Dimension. In: **Lam, L. 1998.** Nonlinear Physics for Beginners: Fractals, Chaos, Solitons, Pattern Formation, Cellular Automa and Complex Systems, pp. 237 - 239. World Scientific Publishing. Singapore.

Chaitin, G. 1988. Randomness in Arithmetic. Scientific American, vol. 259, pp. 80 - 85.

Chaitin, G. 1992. A Random Walk in Arithmetic. In: **Hall, N.** (Ed.) **1992.** The New Scientist Guide to Chaos, pp. 196 - 202. Penguin Books. Middlesex, England.

Chalmers, A. F. 1999. What is this Thing Called Science? Third Edition. Open University Press, Buckingham, England.

Chamberlin, T. C. 1890. The Method of Multiple Working Hypotheses. Science, vol. 15, pp. 92 - 96. In: **Albritton, Jr. C. C.** (Ed.) **1963**. Philosophy of Geohistory 1785 - 1970, Benchmark Papers in Geology, vol. 13, pp. 126 - 131. Dowden, Hutchinson & Ross, Stroudsburg, Pensylvania, U. S. A.

Chamberlin, T. C. 1897. The Method of Multiple Working Hypotheses. Journal of Geology, vol. 5, pp. 837 - 848.

Chamberlin, T. C. 1904. The Methods of the Earth Sciences. Popular Science Monthly, vol. 66, pp. 66 -75.

Chandrasekhar, S. 1995. Newton's Principia for the Common Reader. Oxford University Press, Oxford, England.

Chapman, G. P. 1977. Human and Environmental Systems: a Geographer's Appraisal. Methuen, London, England.

Chisholm, M. A. 1967. General Systems Theory and Geography. Transactions of the Institute of British Geographers,vol. 42, pp.. 45 - 52.

Chorley, R. J. 1962. Geomorphology and General Systems Theory. United States Geological Survey Professional Paper, No. 555-B.

Chorley, R. J. 1965a. A Re-evaluation of the Geomorphic System of W. M. Davis. In: **Chorley, R. J.** and **Hagget, P.** (Eds.) **1965.** Frontiers in Geographical Teaching, pp. 21 - 38. Methuen, London, England.

Chorley, R. J. 1965b. The Application of Quantitative Methods to Geomorphology. In: **Chorley, R. J.** and **Hagget, P.** (Eds.) **1965.** Frontiers in Geographical Teaching, pp. 147 - 163. Methuen, London, England.

Chorley, R. J. 1966. The Application of Statistical Methods to Geomorphology. In: **G. H. Dury** (Ed.)**1966.** Essays in Geomorphology, pp. 275 - 387. Heinemman Eductational Books, London, England.

Chorley, R. J. (Ed.) **1969.** Introduction to Fluvial Processes. University Paperbacks, Methuen, London, England.

Chorley, R. J. 1978. Bases for Theory in Geomorphology. In: **Embleton, C., Brunsden, D.** and **Jones D. K. C** (Eds.) **1978**. Geomorphology: Present Problems and Future Prospects, pp. 1 - 13. Oxford University Press, Oxford, England.

Chorley, R. J., Beckinsdale, R. P. and **Dunn, A. J. 1973.** The History of the Study of Landforms or the Development of Geomorphology: volume 2, the Life and Work of William Morris Davis. Methuen, London, England.

Chorley, R. J. and **Kennedy, B. A. 1971.** Physical Geography: a Systems Approach. Prentice-Hall International, London, England.

Church. M. 1996. Space, Time and the Mountain - How Do We Order What We See? In: **Rhoads, B. L.** and **Thorn, C. E.** (Eds.) **1996.** The Scientific Nature of Geomorphology: Proceedings of the 27th Binghampton Symposium in Geomorphology, pp. 147 - 170.

Clagett, M. 1959. The Science of Mechanics in the Middle Ages. The University of Winconsin Press, Winconsin, U. S. A.

Claps, P. and **Oliveto, G. 1996**. Re-examining the Determination of the Fractal Dimension of River Networks. Water Resources Research, vol. 32, pp. 3123 - 3135.

Clifford, N. J. 1996. Classics in Physical Geography Revisited: **Leopold, L. B.** and **Maddock, T. M. Jr. 1953.** The Hydraulic Geometry of Stream Channels and Some Physiographic Implications. United States Geological Survey Professional Paper 252. Progress in Physical Geography, vol. 20, No. 1, pp. 81 - 87.

Cohen, J. and **Stewart, I. 1994**. The Collapse of Chaos. Penguin Books, London, England.

Cole, J. P. and **King, C. A. M. 1968.** Quantitative Geography: Techniques and Theories in Geography. John Wiley & Sons, London, England.

Collins English Dictionary, 1998. Fourth Edition. Harper Collins Publishers, Glasgow, Scotland.

Concacher, A. J. 1969. Open Systems and Dynamic Equilibrium in Geomorphology: a Comment. Australian Geographical Studies, vol.7, pp. 153 - 158.

Cook, R. D. and **Weisberg, S. 1982.** Residuals and Influence in Regression. Chapman and Hall, London, England.

Cournot, A. A. 1843. Exposition de la théorie des chances et des probabilitiés. Paris.

Coveney, P. 1992. Chaos, Entropy and the Arrow of Time. In: **Hall, N.** (Ed.) **1992**. The New Scientist Guide to Chaos, pp. 203 - 212. Penguin Books, Middlesex, England.

Cox, N. J. 1985. The David Linton Award 1984: Richard Chorley. Earth Surface Processes and Landforms, vol. 10, pp. 425 - 427.

Crutchfield, J. P., Farmer, J. C., Packard, H. N. and **Shaw, R. S. 1986.** Chaos. Scientific American, vol. 254 (12), pp. 46 - 58.

Crystal. D. (Ed.) **1990.** The Cambridge Encyclopaedia. Cambridge University Press, Cambridge, England.

Culling, W. E. H. 1957. Multicyclic Streams and the Equilibrium Theory of Grade. Journal of Geology, vol. 65, pp. 259 - 274.

Culling, W. E. H. 1986a. Highly Erratic SpatiaL Variability of Soil Ph on Iping Common, West Sussex. Catena, vol. 13, pp. 81 - 98.

Culling, W. E. H. 1986b. On Hurst Phenomena in the Landscape. Transactions of the Japanese Geomorphological Union, vol. 7 (4), pp. 221 - 243.

Culling. W. E. H. 1987a. A New View of the Landscape. Transaction of the Institute of British Geographers, NS 13, pp. 345 - 360.

Culling, W. E. H. 1987b. Equifinality: Modern Approaches to Dynamical Systems and Their Potential for Geographical Thought. Transactions of the Institute of British Geographers, N. S. vol. 12, pp. 57 - 72.

Culling, W. E. H. (1988), Dimension and Entropy in the Soil-Covered Landscape. Earth Surface Processes and Landforms, vol. 13, pp. 619 - 648.

Culling, W. E. H. and **Dakto, M. (1987)**, The Fractal Dimension of the Soil-Covered Landscape. Earth Surface Processes and Landforms, vol. 12, pp. 369 - 385.

Cvitanovic, P. 1984. Universality in Chaos. In: **Cvitanovic, P.** (Ed.) **1984**. Universality in Chaos, 2nd Edition, Neils Bohr Institute - Copenhagen. Adam Hilger, Bristol, England.

David, F. N. 1962. Games, Gods and Gambling: A History of Probability and Statistical Ideas: Dover Edition **1998.** Dover Publications, New York, U. S. A.

Davies, P. 1992. Is the Universe a Machine? In: **Hall, N.** (Ed.)**1992.** The New Scientist Guide to Chaos, pp. 213 - 221. Penguin Books. Middlesex, England.

Davis, W. M 1899. The Geographical Cycle. Geographical Journal, vol. 14, pp.. 481 - 504.

Davis, W. M. 1900. The Physical Geography of the Lands. Popular Science Monthly, vol. 57, pp. 157 - 170.

Davis, W. M. 1905. Complications of the Geographic Cycle. Report of the Eighth International Geographical Congress, Washington, pp. 150 - 163. In: **Chorley, R. J., Beckinsdale, R. P.** and **Dunn, A. J. 1973.** The History of the Study of Landforms or the Development of Geomorphology: volume 2, the Life and Work of William Morris Davis. Methuen , London, England.

Davis, W. M. 1909. The Systematic Description of Land Forms. Geographical Journal, vol 34, pp. 300 - 318 with subsequent discussion, pp. 318 -326. In: **Chorley, R. J., Beckinsdale, R. P.** and **Dunn, A. J. 1973.** The History of the Study of Landforms or the Development of Geomorphology: volume 2, the Life and Work of William Morris Davis. Methuen, London, England.

Davis, W. M. 1911. The Disciplinary Value of Geography. Popular Science Monthly, vol. 78, pp. 105 - 119 and 223 -240.

Davis, W. M. 1915. The Principles of Geographic Description. Annals of the Association of American Geographers, vol. 5, pp. 61 - 105.

Davis, W. M. 1926. The Value of Outrageous Geological Hypotheses. Science, vol. 63, pp. 463 -468. In: **Albritton, Jr. C. C.** (Ed.) **1975.** Philosophy of Geohistory 1785 - 1970. Benchmark Papers in Geology, vol. 13, pp. 147 - 152. Dowden, Hutchinson & Ross, Stroudsburg, Pensylvania, U. S. A.

Dorn, R. I. 1996. Climatic Hypotheses of Alluvial-Fan Evolution in Death Valley are Not Testable. In: **Rhoads, B. L.** and **Thorn, C. E.** (Eds.) **1996.** The Scientific Nature of Geomorphology: Proceedings of the 27th Binghampton Symposium in Geomorphology, pp. 191 - 220.

Drake, T. G., Shreve, R. L., Dietrich, W. E., Whiting, P. J, and **Leopold, L. B. 1988.** Bedload Transport of Fine Gravel Observed by Motion Picture Photography. Journal of Fluid Mechanics, vol. 192, pp. 193 - 217.

Dury, G. H. 1969. Hydraulic Geometry. In: **Chorley, R. J.** (Ed.) **1969.** Introduction to Fluvial Processes, pp. 146 - 156. Methuen, London, England.

Dury, G. H. 1970. General Theory of Meandering Valleys and Underfit Streams. In: **Dury, G. H.** (Ed.) **1970.** Rivers and River Terraces, pp. 264 - 275. Macmillan, London, England.

Dury. G. H. 1973. Magnitude - Frequency Analysis and Channel Morphometry. In: **Morisawa. M.** (Ed.) **1973.** Fluvial Geomorphology, pp. 91 - 121. Proceedings of the 4th Binghampton Symposium in Geomorphology. George Allen & Unwin, London, England.

Dury, G. H. 1976. Discharge Prediction, Present and Former from Channel Dimensions. Journal of Hydrology, vol. 30, pp. 219 - 245.

Eckmann, J. P. 1981. Roads to Turbulence in Dissipative Dynamical Systems. Reviews of Modern Physics, vol. 53. pp. 643 - 654. In: **Cvitanovic. P.** (Ed.) **1984.** Universality in Chaos, pp. 94 - 105. Adam Hilger, Bristol, England.

Edgar, D. E. 1973. Geomorphic and Hydraulic Properties of Laboratory Rivers. Unpublished M. S. Thesis Colorado State University, Fort Collins. In: **Schumm S. A., Mosley M. P** and **Weaver W. E. 1987.** Experimental Fluvial Geomorphology. Wiley Interscience Publication, John Wiley & Sons, New York, U. S. A.

Feigenbaum. M. J. 1980. Universal Behaviour in Non-Linear Systems. Los Alamos Science, vol. 1, pp. 4 - 27. In: **Cvitanovic, P.** (Ed.) **1984.** Universality in Chaos, 2nd Edition, Neils Bohr Institute - Copenhagen. Adam Hilger, Bristol, England.

Ferguson, R. I. 1973. Channel Pattern and Sediment Type. Area, vol. 5, pp. 38 - 41.

Ferguson. R. I. 1977. Meander Migration: Equilibrium and Change. In: **Gregory, K. J.** (Ed.) **1977.** River Channel Changes, pp. 235 - 248. Wiley Interscience, Chichester, England.

Ferguson, R. I. 1986. Hydraulics and Hydraulic Geometry. Progress in Physical Geography, vol. 10, pp. 1 - 31.

Feynman, R. P. 1985. Q.E.D; the Strange Theory of Light and Matter. Penguin Books, London, England.

Fracastoro, Giralmo 1546. De Sympathia et Antipatha Rerum Libers Unus: De Contagione et Contagiosis Morbis et Eorum Curatione, Libri iii. Translated by: **Wright, C. 1930.** Fracastoro's de Contagione. New York, U. S. A.

Freund, J. E. 1984. Modern Elementary Statistics, Sixth Edition. Prentice-Hall, New Jersey, U. S. A.

Freund, J. E. 1992. Mathematical Statistics, Fifth Edition. Prentice Hall International, London, England.

Friedkin. J. F. 1945. A Laboratory Study of the Meandering of Alluvial Rivers. U. S. Waterways Experiment Station, Vicksburg, Mississippi In: **Schumm, S. A.** (Ed.) **1972.** River Morphology - Benchmark Papers in Geology, pp. 237 - 282. Dowden, Hutchinson & Ross, Stroudsburg, Pennsylvania, U. S. A.

Gao, J. and **Xia, Zong-Gua 1996.** Fractals in Physical Geography. Progress in Physical Geography, vol. 20 2., pp. 178 - 191.

Gasper, P. 1991. Introductory Essay. In: **Boyd, R., Gasper, P.** and **Trout, J. D.** (Eds.) **1991.** The Philosophy of Science, pp. 289 - 298. The M. I. T. Press. Cambridge, Massachusetts, U. S. A.

Gieryn, T. F. 1995. Boundaries of Science. In: **Tauber, A. I.** (Ed.) **1997.** Science and the Quest for Reality: Main Trends of the Modern World, pp. 293 - 332. Macmillan Press, London and Basingstoke, England.

Gilbert. G. K. 1880. Report on the Geology of the Henry Mountains. Department of the Interior, U.S. Geographical and Geological Survey of the Rocky Mountain Region. In: **Schumm, S. A.** (Ed.)**1972.** River Morphology - Benchmark Papers in Geology, pp. 43 - 77. Dowden, Hutchinson & Ross, Stroudsburg, Pensylvania, U. S. A.

Gilbert, G. K. 1886. The Inculcation of Scientific Method by Example. American Journal of Science, Vol 31, pp. 284 - 299.

Gilbert, G. K. 1896. The Origin of Hypotheses, Illustrated by the Discussion of a Topographic Problem. Science, n. s. vol. 3, pp. 1 - 13. In: **Albritton, Jr. C. C.** (Ed.) **1975.** Philosophy of Geohistory 1785 - 1970. Benchmark Papers in Geology, vol. 13, pp. 132 - 146. Dowden, Hutchinson & Ross, Stroudsburg, Pensylvania, U. S. A.

Gillispie, C. S. 1997. Pierre-Simon Laplace 1749 - 1827; A Life in Exact Science. Princeton University Press, Princeton, U. S. A.

Gilluly, J. 1963. The Scientific Philosophy of G. K. Gilbert. In: **Albritton, Jr. C. C.** (Ed.) **1963.** The Fabric of Geology, pp. 218 - 224. Freeman, Cooper & Company, San Francisco, U. S. A.

Gjertsen, D. 1986. The Newton Handbook. Routledge & Kegan Paul, London, England.

Gleick. J. 1997. Chaos: Making a New Science, Minerva Edition. Minerva, London, England.

Goldstein, M. and **Goldstein, I. F. 1978.** How We Know: an Exploration of the Scientific Process. Plenum Press, New York, U. S. A.

Goldstein, M. and **Goldstein, I. F. 1995.** The Refrigerator and the Universe: Understanding the Laws of Energy. Harvard University Press, London, England.

Gollub, J. P. and **Swinney, H. L. 1975.** Onset of Turbulence in a Rotating Fluid. Physical Review Letters, vol. 35, pp. 927 - 930.

Gower, B. 1997. Scientific Method: an Historical and Philosophical Introduction. Routledge, London, England.

Graf, W. L. 1979. Catastrophe Theory as a Model for Change in Fluvial Systems. In: Rhodes, D. D. and Williams, G. P. (Eds.) 1979. Adjustments of the Fluvial System, pp. 13 - 32.

Graf, W. L. 1996. Geomorphology and Policy for Restoration of Impounded American Rivers: What is 'Natural?' In: **Rhoads, B. L.** and **Thorn, C. E.** (Eds.) **1996.** The Scientific Nature of Geomorphology: Proceedings of the 27th Binghampton Symposium in Geomorphology, pp. 444 - 473.

Grass, A. J., Stuart, R. J., and **Mansour-Tehrani, M. 1991.** Vortical Structures and Coherent Motion in Turbulent Flow over Smooth and Rough Boundaries. Philosophical Transaction of the Royal Society of London A, vol. 336, pp. 35 - 65.

Grassberger, P. 1986. Estimating the Fractal Dimensions and Entropies of Strange Attractors. In: **Holden, A. V. 1986.** Chaos, pp. 291 - 391. Manchester University Press, Manchester, England.

Gregory, K. J. 2000. The Changing Nature of Physical Geography. Arnold, London, England.

Gribbin, J. 1984. In Search of Schrödingers's Cat; Quantum Physics and Reality, Black Swan Edition. Black Swan. London, England.

Guelke, L. 1971. Problems of Scientific Explanation in Geography. Canadian Geographer, vol. XV, pp. 38 - 53.

Gull. S. F. 1991. Some Misconceptions About Entropy. In: **Buck. B.** and **Macaulay. V. A. 1991.** Maximum Entropy in Action, pp. 171 - 186. Clarendon Press, Oxford, England.

Hacking, I. 1975. The Emergence of Probability: A Philosophical Study of Early Ideas About Probability, Induction and Statistical Inference. Cambridge University Press, Cambridge, England.

Haines-Young, R. H. and **Petch, J. R. 1980.** The Challenge of Critical Rationalism for Methodology in Physical Geography. Progress in Physical Geography, vol. 4, pp. 63 - 77.

Haines-Young, R. H. and **Petch, J. R. 1983.** Multiple Working Hypotheses: Equifinality and the Study of Landforms. Transactions of the Institute of British Geographers, n. s., vol. 8, pp. 458 - 466.

Haines-Young, R. H. and **Petch, J. R 1984.** Multiple Working Hypotheses and Equifinality: a Reply. Transactions of the Institute of British Geographers, n. s., vol. 9, pp. 367 - 371.

Haines-Young, R. H. and **Petch, J. R. 1986.** Physical Geography: Its nature and Methods. Harper and Row, London, England.

Hald, A. 1998. A History of Mathematical Statistics From 1750 - 1930. John Wiley & Sons, New York, U. S. A.

Hall, N. (Ed.) **1992.** The New Scientist Guide to Chaos. Penguin Books, Middlesex, England.

Harvey, D. 1969. Explanation in Geography. Edward Arnold, London, England.

Heisenberg, W. 1958. The Development of Philosophical Ideas since Descartes in Comparison with the New Situation in Quantum Theory. In: **Tauber, A. I.** (Ed.) **1997.** Science and the Quest for Reality: Main Trends of the Modern World, pp. 125 - 136. Macmillan Press, Basingstoke and London, England.

Hempel, C. G. 1965. Aspects of Scientific Explanation and Other Essays in the Philosophy of Science. The Free Press, Macmillan Publishing, New York, U. S. A.

Hempel, C. G. 1966. Philosophy of Natural Science: Foundations of Philosophy Series. Prentice Hall, New Jersey, U. S. A.

Henderson, F. M. 1961. Stability of Alluvial Channels. Proceedings of the American Society of Civil Engineers, Journal of the hydraulics Division, vol. 87, HY6, pp. 109 - 138.

Hervouet, J. M. and **Van Haren, L. 1996.** Recent Advances in Numerical Methods for Fluid Flows. In: **Anderson, M. G., Walling, D. E.** and **Bates, P. D.** (Eds.) **1996.** Floodplain Processes, pp. 183 - 214. John Wiley & Sons. Chichester, England.

Hey. R. D. 1975. Design Discharge for Natural Channels. In: **Hey, R. D.** and **Davies, T. D.** (Eds.) **1975.** Science, Technology and Environmental Management. Saxon House, U. K.

Hey, R. D. 1978. Determinate Hydraulic Geometry of River Channels. Journal of the Hydraulics Division, Proceedings of the American Society of Civil Engineers, vol. 104, HY6, pp. 869 - 885.

Hey, R. D. 1979. Dynamic Process Response Model of River Channel Development. Earth Surface Processes, vol. 4, pp. 59 - 72.

Hey, R. D. 1979b. Flow Resistance in Gravel Bed Rivers. Journal of the Hydraulics Division, Proceedings of the American Society of Civil Engineers, vol. 105, HY4, pp. 365 - 379.

Hey, R. D. 1982. Gravel Bed Rivers: Process and Form. In: **Hey, R. D., Bathurst, J. C.** and **Thorne, C. R.** (Eds.) **1982.** Gravel Bed Rivers, pp. 5 - 13. Wiley, Chichester, England.

Hey, R. D. 1997. Stable River Morphology. In: **Thorne, C. R., Hey, R.D.,** and **Newson, M. D.** (Eds.) **1997.** Applied Fluvial Geomorphology for River Engineering Management. John Wiley & Sons, New York, U. S. A.

Hey, R. D. and **Thorne, C. R. 1984.** Flow Processes and River Channel Morphology. In: **Burt, T. P.** and **Walling, D. E.** (Eds.) **1984.** Catchment Experiments in Fluvial Geomorphology. Geo Books, Cambridge, England.

Hey, R. D. and **Thorne, C. R. 1986.** Stable Channels with Mobile Gravel Beds. Journal of Hydraulic Engineering, American Society of Civil Engineers, vol. 112 (8), pp. 671 - 689.

Hickin. E. J. 1972. Pseudomeanders and Point Dunes - a Flume Study. American Journal of Science, vol. 272, pp. 762 - 799.

Hickin, E. J. 1977. The Analysis of River-Planform Responses to Changes in Discharge. In: **Gregory, K. J.** (Ed.) **1977.** River Channel Changes, pp. 249 - 263. John Wiley & Sons, Chichester, England.

Higgins, C. G. 1975. Theories of Landscape Development: a Perspective. In: **Melhorn, W. N.** and **Flemal, R. C.** (Eds.) **1975.** Theories of Landform Development, pp. 1 - 28. Allen & Unwin, Boston, U. S. A.

Hjulstrøm, F. 1935. Studies of the Morphological Activity of Rivers as Illustrated by the River Fyris. Bulletin of the Geological Institute of Uppsala, vol. 25, pp. 221 - 527.

Holland. W. N. and **Pickup. G. 1976.** Flume Study of Knickpoint Development in Stratified Sediment. Geological Society of America Bulletin, vol. 87, pp. 76 - 82.

Hooke. R. LeB, 1968. Model Geology: Prototype and Laboratory Streams: Discussion. Geological Society of America Bulletin, vol. 79, pp. 391 - 394.

Horton, R. E. 1945. Erosional Development of Streams and Their Drainage Basins: Hydrophysical Approach to Quantitative Morphology. Bulletin of the Geological Society of America, vol. 56, pp. 275 - 370.

Hoskin, M. 1997. From Geometry to Physics: Astronomy Transformed. In: **Hoskin, M.** (Ed.) **1997.** Cambridge Illustrated History of Astronomy, pp. 98 - 142. Cambridge University Press, Cambridge, England.

Hoskin, M. 1997b. The Astronomy of The Universe of Stars. In: **Hoskin, M.** (Ed.) **1997.** Cambridge Illustrated History of Astronomy, pp. 198 - 256. Cambridge University Press, Cambridge, England.

Hoskin, M. and **Gingerich, O.1997.** Medieval Latin Astronomy. In: **Hoskin, M.** (Ed.) **1997.** Cambridge Illustrated History of Astronomy, pp. 68 - 98. Cambridge University Press, Cambridge, England.

Howard, A. D. 1965. Geomorphological Systems - Equilibrium and Dynamics. American Journal of Science, vol. 263, pp. 302 - 312.

Huang, H. Q and **Warner, R. F. 1995.** The Multivariate Controls of Hydraulic Geometry - a Causal Investigation in Terms of Boundary Shear Distribution. Earth Surface Processes and Landforms, vol. 20, pp. 115 - 130.

Hume, D. 1739 -1740. A treatise of Human Nature 1739 - 1740: Oxford Philosophical Texts. Edited by **Norton, D. F.** and **Norton, M. J. 2000.** Oxford University Press, Oxford, England.

Ijjasz-Vasquez, E. J., Rodriguez-Iturbe, I. and **Bras, R. L. 1992.** On the Multifractal Characteristics of River Basins. In: **Phillips, J. D.** and **Renwick, W. H.** (Eds.)**1992.** Geomorphic Systems, pp. 297 - 310. Elsevier, Amsterdam, Holland.

Inglis, C. C. 1941. Digest of Answers to the Central Board of Irrigation Questionnaire on Meandering of Rivers with Comments on Factors Controlling Meandering and Suggestions for Future Actions. In: **Edgecombe, A. R. B.** (Ed.) **1941.** Annual Report technical, 1939 -1940, pp. 100 - 114. India Central Board of Irrigation, Publication 24.

Inglis, C. C. 1949. The Behaviour and Control of Rivers and Canals with the Aid of Models. Poona, India, Central Waterpower Irrigation Navigation Research Station, Research Publication 13, part 1.

Jeeves, M. A. and **Berry, R. J. 1998.** Science, Life and Christian Belief: a Survey and Assessment. Inter-Varsity Press, Leicester, England.

Jefferson, M. S. W. 1902. Limiting Width of Meander-Belts. National Geographic Magazine, vol. 13, pp. 373 - 384.

Johnson, D. 1933. Role of Analysis in Scientific Investigation. Bulletin of the Geological Society of America, vol. 44, pp. 461 - 493. In: **Albritton, Jr. C. C.** (Ed.) **1975.** Philosophy of Geohistory 1785 - 1970: Benchmark Papers in Geology, vol. 13, pp. 153 - 185. Dowden, Hutchinson & Ross, Stroudsburg, Pensylvania, U. S. A.

Johnson, R. 1988. Elementary Statistics, Fifth Edition. P. W. S. Publishing, Boston, U. S. A.

Kant, I. 1781 and **1787.** Critique of Pure Reason, Unified Edition (with all variants from the 1781 & 1787 Editions). Translated by **Pluhar, W. S. 1996.** Hackett Publishing Company, Indeanapolis, U. S. A.

Karcz. I. 1973. Reflections on the Origin of Small Scale Longitudinal Streambed Scours. In: **Morisawa. M.** (Ed.) **1973.** Fluvial Geomorphology, pp. 149 - 173. Proceedings of the 4th Binghampton Symposium in Geomorphology. George Allen & Unwin, London, England.

Karcz. I. 1980. Thermodynamic Approach to Geomorphic Thresholds. In: **Coates. D. R.** and **Vitek. J. D.** (Eds.) **1980.** Thresholds in Geomorphology, Binghampton Symposia on Geomorphology, International Series 9, pp. 209 - 226. George Allen & Unwin, London.

Kellerhalls, R. 1967. Stable Channels with Gravel-Paved Beds. Journal of Waterways and Harbors Division, American Society of Civil Engineers, vol. 93 ww1., pp. 63 - 84.

Kendall, M. G. 1956. Studies in the History of Probability and Statistics. II. The Beginnings of a Probability Calculus. Biometrika, vol. 43, pp. 1 - 14.

Kennedy, B. A. 1977. A Question of Scale? Progress in Physical Geography, vol. 1, pp. 154 - 157.

Kennedy, B. A. 1980. A Naughty World. Transactions of the Institute of British Geographers, N. S. vol. 4, pp. 550 - 558.

Kennedy, J. F. 1983. Reflections on Rivers, Research and Rouse. Journal of Hydraulic Engineering, vol. 109, No 10, pp. 1254 - 1271.

Kennedy, S. K. and **Lin, W. 1986**. FRACT - A Fortran Subroutine to Calculate the Variables Necessary to Determine the Fractal Dimension of Closed Forms. Computer Geoscience, vol. 12 pp. 705 - 712.

Kirchner, J. W., Dietrich, W. E., Iseysa, F., and **Ikeda, H. 1990.** The Variability of Critical Shear Stress, Friction Angle and Grain Protrusion in Water-Worked Sediments. Sedimentology, vol. 37, pp. 647 - 672.

Kitcher, P. W. 1996. Introduction. In: **Kant, I. 1781** and **1787.** Critique of Pure Reason, Unified Edition with all variants from the 1781 & 1787 editions. Translated by: **Pluhar, W. S.** Hackett Publishing Company, Indeanapolis, U. S. A.

Kitts, D. B. 1963. The Theory of Geology. In: **Albritton, Jr. C. C.** (Ed.) **1963.** The Fabric of Geology, pp. 49 - 68. Freeman, Cooper & Company, San Francisco, U. S. A.

Klinkenberg, B. and **Goodchild, M. F. 1992.** The Fractal Properties of Topography: a Comparison of Methods. Earth Surface Processes and Landforms, vol. 17, pp. 217 - 234.

Knighton, D. 1977. Short-Term Changes in Hydraulic Geometry. In: **Gregory, K. J.** (Ed.) **1977.** River Channel Changes, pp. 101 - 119. John Wiley & Sons, Chichester, England.

Knighton, D. 1984. Fluvial Forms and Processes. Arnold, London, England.

Knighton, A. D. and **Nanson, G. C. 1993.** Anastomosis and the Continuum of Channel Pattern. Earth Surface Processes and Landforms, vol. 18, pp. 613 - 625.

Kondolf. G. M. and **Downs. P. W. 1996.** Catchment Approach to Planning Channel Restoration. In: **Brookes. A.** and **Shields F. D. Jr.** (Eds.) **1996.** River Channel Restoration: Guiding Principles for Sustainable Projects, pp. 129 - 148. John Wiley & Sons, Chichester, England.

Koyré, Alexandre 1965. Newtonian Studies. Chapman & Hall, London, England.

Krumbein, W. C. 1962. The Computer in Geology. Science, vol. 136, pp. 1087 - 1092.

Krumbein, W. C. and **Graybill, F. A. 1965**. An Introduction to Statistical Models in Geology. McGraw-Hill, New York, U. S. A.

Kuhn, T. S. 1962. The Structure of Scientific Revolutions: Third Edition **1996**. The University of Chicago Press, Chicago, U. S. A.

Lakatos, I. 1970. Falsification and the Methodology of Scientific Research Programmes. In: **Lakatos, I.** and **Musgarve, A.** (Eds.) **1970.** Criticism and the Growth of Knowledge. Cambridge University Press, Cambridge, England.

Lakatos, I. 1978. History of Science and its Rational Reconstruction. In: **Worrall, J.** and **Currie, G.** (Eds.) **1978.** Imre Lakatos, Philosophical Papers, vol. I: The Methodology of Scientific Research Programmes. Cambridge University Press, Cambridge, England.

Lam, L. 1998. Nonlinear Physics for Beginners: Fractals, Chaos, Solitons, Pattern Formation, Cellular Automa and Complex Systems. World Scientific Publishing, Singapore.

Landau, l. D. and **Lifshiftz, E. M. 1959.** Fluid Mechanics: vol. 6 of 'Course of Theoretical Physics.' Translated from the Russian by: **Sykes, J. B.** and **Reid, W. H.** Pergamon Press. London, England.

Lane, E. W. 1935. Stable Channels in Erodible Material. American Society of Civil Engineers, paper No. 1957. In: **Schumm, S. A.** (Ed.) **1972.** River Morphology - Benchmark Papers in Geology, pp. 97 - 116. Dowden, Hutchinson and Ross, Stroudsburg, Pennyslyvania, U. S. A.

Lane, E. W. 1957. A Study of the Shape of Channels Formed by Natural Streams Flowing in Erodible Material: U. S. Army Engineering Division, Missouri River, M. R. D. Sediment Serial, No. 9.

Lane. S. N. and **Richards. K. S. 1997.** Linking River Channel Form and Process: Time, Space and Causality Revisited. Earth Surface Processes and Landforms, Vol. 22, pp. 249 - 260.

Lane, S. N., Richards, K. S., and **Chandler, J. H., 1995.** Within-Reach Spatial Patterns of Process and Channel Adjustment. In: **Hickin, E. J.** (Ed.) **1995.** River Geomorphology, John Wiley & Sons, London, England.

Langbein. W. B. and **Leopold L. B. 1968.** River Channel Bars and Dunes - Theory of Kinematic Waves. Geological Survey Professional Paper, 422 - L.

Laplace, P.S. 1776. Reserches, sur l'intégration des équations différentielles aux différences finies et sur leur usage dans la théorie des hasards. In: **Gillispie, C. S. 1997.** Pierre-Simon Laplace 1749 - 1827; A Life in Exact Science. Princeton University Press, Princeton, England.

Laplace, P. S. 1814. A Philosophical Essay on Probabilities, Dover Edition **1995.** Dover Publications, New York, U. S. A.

Laudan, L. 1984. Explaining the Success of Science: Beyond Epistemic Realism and Relativism. In: **Tauber, A. I.** (Ed.) **1997.** Science and the Quest for Reality: Main Trends of the Modern World, pp. 137 - 161. Macmillan Press, London and Basingstoke, England.

Leighly, J. B. 1932. Toward a Theory of the Morphologic Significance of Turbulence in the Flow of Water in Streams. University of California Publications in Geography, vol. 6, No 1, pp. 1 - 22.

Leighly, J. 1940. Walter Penck's Contribution to Geomorphology: Comments by J. Leighly. Annals of the Association of American Geographers, vol. 30, pp. 223 - 232.

Leopold, L. B. 1953. Downstream Changes of Velocity in Rivers. American Journal of Science, vol. 251, pp. 606 - 624.

Leopold, L. B. and **Langbein, W. B. 1962.** The Concept of Entropy in Landscape Evolution. United States Geological Survey Professional Paper 500 - A.

Leopold L. B. and **Langbein W. B. 1963.** Association and Indeterminacy in Geomorphology. In: **Albritton, Jr. C. C.** (Ed.) **1963.** The fabric of Geology, pp. 184 - 192. Freeman, Cooper & Company, San Francisco, U. S. A.

Leopold, L. B. and **Maddock, T. Jr. 1953.** The Hydraulic Geometry of Stream Channels and Some Physiographic Implications. United States Geological Survey, Professional Paper, 252.

Leopold. L.B. and **Wolman. M.G. 1957.** River Channel Patterns: Braided, Meandering and Straight. United States Geological Survey Professional Paper, 282 - B.

Leopold, L.B., Wolman, M. G. and **Miller, J. P. 1964.** Fluvial Processes in Geomorphology: Dover Edition, 1995. Dover Publications, New York, U. S. A.

Libchaber. A. and **Maurer. J. 1982.** A Rayleigh Bernard Experiment: Helium in a Small Box. In: **Riste. T.** (Ed.) **1982.** Non-Linear Phenomena at Phase Transitions and Instabilities, pp. 259 - 286. In: **Cvitanovic, P.** (Ed.) **1984.** Universality in Chaos, 2nd Edition, Neils Bohr Institute - Copenhagen, pp. 109 - 136. Adam Hilger, Bristol, England.

Lines, M. E. 1994. On the Shoulders of Giants. Institute of Physics Publishing, Bristol, U. K.

Lorenz, E. N. 1962. The Statistical Prediction of Solutions of Dynamic Equations. Proceedings International Symposium Numerical Weather Prediction, Tokyo: Meteorological Society of Japan, pp. 629 - 635, 647.

Lorenz, E. N. 1963. Deterministic Nonperiodic Flow. Journal of the Atmospheric Sciences, vol. 20, pp. 130 - 141.

Lorenz. E. N. 1993. The Essence of Chaos. University of Washington Press, Seattle, U. S. A.

Mackin, J. H. 1963. Rational and Empirical Methods of Investigation in Geology. In: **Albritton, Jr. C. C.** (Ed.) **1963.** The Fabric of Geology, pp. 135 - 163. Freeman, Cooper & Company, San Francisco, U. S. A.

Maddock, T. Jr. 1969. The Behaviour of Straight Open Channels with Moveable Beds. United States Geological Survey, Professional Paper, 622 - A.

Mahmood, K. and **Shen, H. W. 1971.** Regime Concept of Sediment - Transporting Canals and Rivers. In: **Shen . H. W.** (Ed.) **1971.** River Mechanics, vol I, pp. 30-1 to 30-39. Water Resources Research Publication, Fort Collins, Colorado, U. S. A.

Mandelbrot, B. B. 1982. The Fractal Geometry of Nature. W. H. Freeman, San Francisco, U. S. A.

Mandelbrot, B. B. 1992. Fractals - A Geometry of Nature. In: **Hall, N. 1992.** The New Scientist Guide to Chaos, pp. 122 - 135. Penguin Books, Middlesex, England.

Mayer. L. 1992. Some Comments on Equilibrium Concepts and Geomorphic Systems. In: **Phillips, J. D.** and **Renwick, W. H.** (Eds.) **1992.** Geomorphic Systems, pp. 277 - 295. Elsevier, Amsterdam, Holland.

McKnight, T. L. 1999. Physical Geography: A Landscape Appreciation, Sixth Edition. Prentice-Hall, Upper Saddle River, New Jersey, U. S. A.

Melton, M. A. 1958. Correlation Structure of Morphometric Properties of Drainage Systems and their Controlling Agents. Journal of Geology, vol. 66, pp. 442 - 460.

Minitab, 1995. Minitab Reference Manual, Release 10 Xtra for Windows and Macintosh. Minitab Incorporated, U. S. A.

Moivre, Abraham de 1718. The Doctrine of Chances or a Method of Calculating the Probabilities of Events in Play, Third Edition. Millar, London. Reprinted by Chelsea Publishing Company, New York, U. S. A. 1967.

Montgomery, D. C. and **Peck, E. A. 1992.** Introduction to Linear Regression Analysis, Second Edition. John Wiley & Sons, Chichester, England.

Montgomery, K. 1989. Concepts of Equilibrium and Evolution in Geomorphology: the Model of Branch Systems. Progress in Physical Geography, vol. 13, No 1, pp. 47 - 66.

Mosley. M. P. 1981. Semi-Determinate Hydraulic Geometry of River Channels, South Island, New Zealand. Earth Surface Processes and Landforms, vol. 6, pp. 127 - 137.

Mosley, M. P. and **Zimpfer, G. L. 1976.** Explanation in Geomorphology. Zeitschrift fur Geomorphologie, New Series, vol. 20, No 4, pp. 381 - 390.

Mullin, T. 1992. Turbulent Times for Fluids. In: **Hall, N. 1992.** The New Scientist Guide to Chaos, pp. 59 - 68. Penguin Books, Middlesex, England.

Nanson, G. C. and **Knighton, A. D. 1996.** Anabranching Rivers: their Cause, Character and Classification. Earth Surface Processes and Landforms, vol. 21, pp. 217 - 239.

National Rivers Authority 1996. River Habitats in England and Wales: a National Overview. River Habitat Survey, Report No 1.

Nelkon, M. and **Parker, P. 1977.** Advanced Level Physics, Fourth Edition. Heinemann Educational Books, London, England.

Nicholas, A. P., Ashworth, P. J., Kirkby, M. J, Macklin, M. G. and **Murray, T. 1995.** Sediment Slugs: Large Scale Fluctuations in Fluvial Sediment Transport Rates and Storage Volumes. Progress in Physical Geography, vol. 19 (4), pp. 500 - 519.

Nikora, V. I. 1991. Fractal Structures of River Plan Forms. Water Resources Research, vol. 27 (6) pp. 1327 - 1333.

Nixon, M. 1959. A Study of the Bank-Full Discharges of Rivers in England and Wales. Proceedings of the Institution of Civil Engineers, vol. 12, pp. 157 - 174.

Orlowski. L. A., Schumm. S. A., Mielke. P. W. Jr. 1995. Reach Classifications of the Lower Mississippi River. Geomorphology, vol. 14, pp. 221 - 234.

Osterkamp, W. R. 1978. Gradient, Discharge and Particle-Size Relations of Alluvial Channels in Kansas, With Observations on Braiding. American Journal of Science, vol. 278, pp. 1253 - 1268.

Osterkamp, W. R. and **Hedman, E. R. 1982.** Perennial-Streamflow Characteristics Related to Channel Geometry and Sediment in Missouri River Basin. United States Geological Survey Professional Paper 1242.

Osterkamp, W. R. and **Hupp, C. R. 1996.** The Evolution of Geomorphology, Ecology and Other Composite Sciences. In: **Rhoads, B. L.** and **Thorn, C. E.** (Eds.) **1996.** The Scientific Nature of Geomorphology: Proceedings of the 27th Binghampton Symposium in Geomorphology, pp. 415 - 441.

Palmer, T. 1992. A Weather Eye on Unpredictability. In: **Hall, N. 1992.** The New Scientist Guide to Chaos, pp. 69 - 81. Penguin Books, Middlesex, England.

Park, C. C. 1977. Man-Induced Changes in Stream Channel Capacity. In: **Gregory, K. J.** (Ed.) **1977.** River Channel Changes, pp. 121 - 144. John Wiley & Sons, Chichester, England.

Parker, G. 1982. Discussion of 'Chapter 19: Regime Equations for Gravel-Bed Rivers.' In: **Hey, R. D., Bathurst, J. C.** and **Thorne, C. R.** (Eds.) **1982.** Gravel-Bed Rivers: Fluvial Processes, Engineering and Management, pp. 542 -552. John Wiley & Sons, Chichester, England.

Pearson, K. 1926. Abraham de Moivre. Nature, vol. 117, pp. 551 - 552.

Percival, I. 1992. Chaos: a Science for the Real World. In: **Hall, N.** (Ed.) **1992.** The New Scientist Guide to Chaos, pp. 11 - 21. Penguin Books, Middlesex, England.

Percival, I. 1992b. Period Doubling and Feigenbaum Numbers. In: **Hall, N.** (Ed.) **1992.** The New Scientist Guide to Chaos, pp. 42 - 43. Penguin Books, Middlesex, England.

Phillips. J. D. 1990. The Instability of Hydraulic Geometry. Water Resources Research, vol. 26 (4), pp. 739 - 744.

Phillips, J. D. 1994. Deterministic Uncertainty in Landscapes. Earth Surface Processes and Landforms, vol. 19, pp. 389 - 401.

Phillips. J. D. 1995. Self Organisation and Landscape Evolution. Progress in Physical Geography, vol. 19 (3), pp. 309 - 321.

Phillips, J. D. 1999. Earth Surface Systems: Complexity, Order and Scale. Blackwell, Massachussetts, U. S. A.

Pickup, G. and **Warner, R. F. 1976.** Effects of Hydrologic Regime on Magnitude and Frequency of Dominant Discharge. Journal of Hydrology, vol. 29, pp. 51 - 75.

Pitt. J. C. 1988. The Success of Science. In: **Pitt. J. C.** (Ed.). Theories of Explanation, pp. 3 - 8. Oxford University Press, Oxford, England.

Pitty, A. F. 1979a. Introduction. In: **Pitty, A. F.** (Ed.) **1979.** Geographical Approaches to Fluvial Processes, pp. 1 - 4. Geo Abstracts Ltd, Norwich, England.

Pitty, A. F. (Ed.) **1979b.** Geographical Approaches to Fluvial Processes. Geo Abstracts, Norwich, England.

Poisson, S. D. 1837. Recherches sur la probabilité des jugements en matière criminelle et en matière cicile, precédées des règles générales du calcul des probabilités. Paris.

Polkinghorne, J. 1991. Reason and Reality: the Relationship between Science and Theology. SPCK, London, England.

Popkin, R. H. and **Stroll, A. 1993.** Philosophy Made Simple, Third Edition. Butterworth-Heinemann, Oxford, England.

Popper, K. R. 1972. The Logic of Scientific Discovery. Hutchinson, London, England.

Popper, K. R. 1989. Conjectures and Refutations: the Growth of Scientific Knowledge. Routledge, London, England.

Prigogine, I. 1997. The End of Certainty: Time, Chaos and the New Laws of Nature. The Free Press, New York, U. S. A.

Quine, W. V. O. 1969. Natural Kinds. In: **Boyd, R., Gasper, P.** and **Trout, J. D.** (Eds.) **1991.** The Philosophy of Science, pp. 159 - 170. The M. I. T. Press, Cambridge, Massachusetts U. S. A.

Rasband, S. N. 1990. Chaotic Dynamics of Nonlinear Systems. John Wiley and Sons, New York, U. S. A.

Research and Education Association (R. E. A.) 1985. The Statistics Problem Solver, Revised Edition. R. E. A., New York, U. S. A.

Renwick. W. H. 1992. Equilibrium, Disequilibrium, and Nonequilibrium Landforms in the Landscape. In: **Phillips, J. D.** and **Renwick, W. H.** (Eds.) **1992.** Geomorphic Systems, pp. 265 - 276. Elsevier, Amsterdam, Holland.

Reynolds, O. 1883. An Experimental Investigation of the Circumstances Which Determine Whether the Notion of Water Shall be Direct or Sinuous and the Law of Resistance in Parallel Channels. Philosophical Transactions of the Royal Society of London (A), Vol. 186.

Rhoads, B. L. 1994. On Being a Real Geomorphologist. Earth Surface Processes and Landforms, vol. 19, pp. 269 - 272.

Rhoads, B. L. and **Thorn, C. E. 1993.** Geomorphology as Science: the Role of Theory. Geomorphology, vol. 6, pp. 287 - 307.

Rhoads, B. L. and **Thorne, C. E. 1994.** Contemporary Philosophical Perspectives on Physical Geography with Emphasis on Geomorphology. Geographical Review, vol. 84, pp. 90 - 101.

Rhoads. B. L. and **Thorn. C. E. 1996a.** Preface. In: **Rhoads, B. L.** and **Thorn, C. E.** (Eds.) **1996.** The Scientific Nature of Geomorphology: Proceedings of the 27th Binghampton Symposium in Geomorphology, pp. ix - xi.

Rhoads. B. L. and **Thorn. C. E. 1996b.** Observation in Geomorphology. In: **Rhoads, B. L.** and **Thorn, C. E.** (Eds.) **1996.** The Scientific Nature of Geomorphology: Proceedings of the 27th Binghampton Symposium in Geomorphology, pp. 21 - 56.

Rhoads. B. L. and **Thorn. C. E. 1996c.** Toward a Philosophy of Geomorphology. In: **Rhoads, B. L.** and **Thorn, C. E.** (Eds.) **1996.** The Scientific Nature of Geomorphology: Proceedings of the 27th Binghampton Symposium in Geomorphology, pp. 115 - 143.

Rhodes, D. D. 1977. The b-f-m Diagram: Graphical Representation and Interpretation of At-A-Station Hydraulic Geometry. American Journal of Science, vol. 277, pp. 73 - 96.

Rhodes, D. D. 1978. World-Wide Variations in Hydraulic Geometry Exponents of Stream Channels; An Analysis and Some Observations - Comments. Journal of Hydrology, vol. 39, pp. 193 -197.

Richards, K. S. 1973. Hydraulic Geometry and Channel Roughness - a Non-Linear System. American Journal of Science, vol. 273, pp. 877 - 896.

Richards, K. S. 1977. Channel and Flow Geometry. Progress in Physical Geography, vol. 1, pp. 65 -102.

Richards, K. S. 1990. 'Real' Geomorphology. Earth Surface Processes and Landforms, vol. 15, pp. 195 -197.

Richards, K. S. 1994. 'Real' Geomorphology Revisited. Earth Surface Processes and Landforms, vol. 19, pp. 277 -281.

Richards, K. 1996. Samples and Cases Generalisation and Explanation in Geomorphology. In: **Rhoads, B. L.** and **Thorn, C. E.** (Eds.) **1996.** The Scientific Nature of Geomorphology: Proceedings of the 27th Binghampton Symposium in Geomorphology, pp. 171 - 190.

Richards, R. 1987. Theories of Scientific Change. In: **Tauber, A. I.** (Ed.) **1997.** Science and the Quest for Reality: Main Trends of the Modern World, pp. 203 - 230. Macmillan Press, Basingstoke and London, England.

Richardson, E. V. 1971. Sediment Properties. In: **Shen . H. W.** (Ed.) **1971.** River Mechanics, vol I. pp. 6-1 to 6-23. Water Resources Research Publication, Fort Collins, Colorado, U. S. A.

Rodriguez-Iturbe, I., Rinaldo, A., Rigon, R., Bras, R. L., Ijjasz-Vasquez, E. J., and **Marani, A. 1992.** Fractal Structures as Least Energy Patterns: the Case of River Networks. Geophysical Research Letters, vol. 19, pp. 889 - 892.

Rosgen. D. L. 1994. A Classification of Natural Rivers. Catena, vol. 22, pp. 169 - 199.

Rossinskiy, K. I. and Kuz'min, I. A. 1947. Some Questions of the Applied Theory of River Channel Formation. Problems of River Yield Regulation, Academy of Science, Moscow. (In Russian)

Rosso, R., Bacchi, B., and **La Barbera, P. 1991.** Fractal Relation of Mainstream Length to Catchment Area in River Networks. Water Resources Research, vol 27, pp. 381 - 387.

Ruelle, D. 1980. Strange Attractors. The Mathematical Intelligencer, vol. 2, pp. 126 - 137. In: **Cvitanovic, P.** (Ed.) **1984.** Universality in Chaos, 2nd Edition, Neils Bohr Institute - Copenhagen, pp. 37 - 48. Adam Hilger, Bristol, England.

Ruelle, D. and **Takens, F. 1971.** On the Nature of Turbulence. Communications in Mathematical Physics, vol. 20, pp. 167 - 192.

Russell, B. 1948. Human Knowledge, its Scope and Limits. London

Russell, B. 1961. History of Western Philosophy, Routledge Edition. Routledge, London, England. 2000 Edition.

Russell, C. A. 1985. Cross-Currents: Interactions between Science and Faith. Inter-Varsity Press, Leicester, England.

Ryan, T. P. 1997. Modern Regression Methods. John Wiley & Sons, Chichester, England.

Sack, D. 1991. The Trouble with Antithesis: the Case of G. K. Gilbert, Geographer and Educator. Professional Geographer, vol. 43, pp. 28 - 37.

Sack, D. 1992. New Wine in Old Bottles: the Historiography of a Paradigm Change. Geomorphology, vol. 5, pp. 251 - 263.

Sambursky, S. 1956. On the Possible and Probable in Ancient Greece. Osiris, vol. 12, pp. 35 - 48.

Scheidegger, A. E. 1970. Theoretical Geomorphology, Second Revised Edition. George Allen and Unwin, London, England.

Schumm, S. A. 1960. The Shape of Alluvial Channels in Relation to Sediment Type. United States Geological Survey, Professional Paper, 352 - B.

Schumm, S. A. 1963. A Tentative Classification of Alluvial River Channels. United States Geological Survey Circular, No. 477.

Schumm, S. A. 1969. River Metamorphosis. Journal of the Hydraulics Division, Proceedings of the American Society of Civil Engineers, vol. 95, pp. 255 - 273.

Schumm. S. A. 1971a. Fluvial Geomorphology: the Historical Perspective. In: **Shen H. W.** (Ed.) **1971.** River Mechanics, vol. I. pp. 4-1 to 4-30. Water Resources Research Publication, Fort Collins, Colorado, U. S. A.

Schumm. S. A. 1971b. Fluvial Geomorphology: Channel Adjustment and River Metamorphosis. In: **Shen . H. W.** (Ed.) **1971.** River Mechanics, vol. I, pp. 5-1 to 5-22. Water Resources Research Publication, Fort Collins, Colorado, U. S. A.

Schumm. S. A. 1972. Fluvial Geomorphology in River Mechanics. In: **Schumm, S. A.** (Ed.) **1972.** River Morphology, pp. 365 - 422. Dowden, Hutchinson and Ross, Stroudsburg, Pennysylvania, U. S. A. In: **Shen. H. W.** (Ed.) **1971.** River Mechanics, vol I. pp. 4-1 to 5-22. Water Resources Research Publication. Fort Collins, Colorado, U. S. A.

Schumm. S. A. 1973. Geomorphic Thresholds and Complex Response of Drainage Systems. In: **Morisawa. M.** (Ed.) **1973.** Fluvial Geomorphology, pp. 299 - 310. Proceedings of the 4th Binghampton Symposium in Geomorphology. George Allen & Unwin, London, England.

Schumm. S. A. 1979. Geomorphic Thresholds: the Concept and its Applications. Transactions of the Institute of British Geographers, N. S. vol. 4, pp. 485 - 515.

Schumm, S. A. and **Khan, H. R. 1971.** Experimental Study of Channel Patterns. Nature, vol. 233 , pp. 407 - 409.

Schumm, S. A. and **Khan, H. R. 1972.** Experimental Study of Channel Patterns. Bulletin of the Geological Society of America, vol. 83, pp. 1755 - 1770.

Schumm, S. A. and **Lichty, R. W. 1963.** Channel Widening and Flood - Plain Construction Along Cimarron River in South Western Kansas. United States Geological Survey, Professional Paper 352 - D.

Schumm, S. A. and **Lichty, R. W. 1965.** Time, Space and Causality in Geomorphology. American Journal of Science, vol. 263, pp. 110 - 119.

Schumm S. A., Mosley M. P and **Weaver W. E. 1987.** Experimental Fluvial Geomorphology. Wiley Interscience Publication, John Wiley & Sons, New York, U. S. A.

Schuster, H. G. 1995. Deterministic Chaos, an Introduction: Third Augmented Edition. Weinhem, New York, U. S. A.

Scott, S. 1992. Clocks and Chaos in Chemistry. In: **Hall, N.** (Ed.) **1992.** The New Scientist Guide to Chaos, pp. 108 - 121. Penguin Books, Middlesex, England.

Scriven, M. 1959. Explanation and Prediction in Evolutionary Theory; Satisfactory Evidence of the Past is Possible even When Prediction of the Future is Impossible: Science, vol. 130, pp. 477 - 482.

Shamos, M. H. 1995. The Myth of Scientific Literacy. Rutgers University Press, New Brunswick, U. S. A.

Shapere, D. 1987. Method in the Philosophy of Science and Epistemology. In: **Nersessian, N. J.** and **Nijhoff, M.** (Eds.) **1987.** The Process of Science, pp. 1 - 39. Dordrecht.

Shaw, R. 1981. Strange Attractors, Chaotic Behaviour and Information Flow. Zeitschrift Naturforsch, vol 36a, pp. 80 - 112. In: **Culling. W. E. H. 1987a.** A New View of the Landscape. Transactions of the Institute of British Geographers, N. S. vol. 13, pp. 345 - 360.

Shen, H. W. 1971a. Wash Load and Bed Load. In: **Shen. H. W.** (Ed.) **1971.** River Mechanics, vol I. pp. 11-1 to 11-30. Water Resources Research Publication. Fort Collins, Colorado, U. S. A.

Shen, H. W. 1971b. Stability of Alluvial Channels. In: **Shen. H. W.** (Ed.) **1971.** River Mechanics, vol I, pp. 16-1 to 16-33. Water Resources Research Publication, Fort Collins, Colorado, U. S. A.

Shenker, S. J. 1982. Scaling Behaviour in a Map of a Circle onto Itself: Empirical Results. Physica 5D, pp. 405 - 411.

Sherman. D. J. 1996. Fashion in Geomorphology. In: **Rhoads, B. L.** and **Thorn, C. E.** (Eds.) **1996.** The Scientific Nature of Geomorphology: Proceedings of the 27th Binghampton Symposium in Geomorphology, pp. 87 - 114.

Shields, A. 1936. Anwendung der Ähnlichkeitsmechanik und der Turbulenzforschung auf die Geschiebebewegung. Mitteilung der preussischen Versuchsanstalt für Wasserbau und Schiffbau, 26. Berlin.

Shields, Jr. F. D. 1996. Hydraulic and Hydrologic Stability. In: **Brookes. A.** and **Shields, Jr. F. D.** (Eds.) **1996.** River Channel Restoration: Guiding Principles for Sustainable Projects, pp. 23 -74. John Wiley & Sons, Chichester, England.

Simon, A. 1995. Adjustment and Recovery of Unstable Alluvial channels: Identification and Approaches for Engineering Management. Earth Surface Processes and Landforms, vol. 20, pp. 611 - 628.

Simons, D. B. 1969. Open Channel Flow. In: **Chorley, R. J.** (Ed.) **1969.** Introduction to Fluvial Processes, pp. 124 - 144. Methuen, London, England.

Simons, D. B. 1971. River and Canal Morphology. In: **Shen. H. W.** (Ed.) **1971.** River Mechanics, vol. I, pp. 20-1 to 20-60. Water Resources Research Publication, Fort Collins, Colorado, U. S. A.

Simons, D. B. and **Richardson, E. V. 1971.** Flow in Alluvial Sand Channels. In: **Shen. H. W.** (Ed.) **1971.** River Mechanics, vol. I, pp. 9-1 to 9-89. Water Resources Research Publication, Fort Collins, Colorado, U. S. A.

Simons, D. B., Richardson, E. V., and **Nordin, C. F. Jr. 1965.** Sedimentary Structures Generated by Flow in Alluvial Channels. In: Primary Sedimentary Structures and their Hydrodynamic Interpretation. The American Association of Petroleum Geologists, Special Publication No. 12. In: **Schumm, S. A. 1972.** River Morphology, pp. 315 - 335. Dowden, Hutchinson and Ross, Stroudburg, Pennsylvania, U. S. A.

Simpson, G. G. 1963. Historical Science. In: **Albritton, Jr. C. C.** (Ed.) **1963.** The Fabric of Geology, pp. 24 - 48. Freeman, Cooper & Company, San Francisco, U. S. A.

Simpson, T. 1756. A letter to the Right Honourable George Earl of Macclesfield, President of the Royal Society, on the advantage of taking the mean number of observations, in practical astronomy. Philosophical Transactions of the Royal Society of London, vol. 49, pp. 82 -93.

Smale, S. 1967. Differentiable Dynamical Systems. Bulletin of the American Mathematical Society, vol. 73, pp. 747 - 817.

Smith, D. G. 1993. Fluvial Geomorphology: Where Do We Go From Here? Geomorphology, vol. 7, pp. 251 - 262.

Stebbings, J. 1963. The Shapes of Self Formed Model Alluvial Channels. Proceedings of the Institute of Civil Engineers, vol. 25, pp. 485 - 510.

Stewart, I. 1997. Does God Play Dice? The New Mathematics of Chaos: Second Edition. Penguin Books, London, England.

Strahler, A. N.1950a. Davis's Concepts of Slope Development Viewed in the Light of Recent Quantitative Investigations. Annals of the Association of American Geographers, vol. 40, pp. 209 - 213.

Strahler, A. N. 1950b. Equilibrium Theory of Erosional Slopes Approached by Frequency Distribution Analysis. American Journal of Science, Vol. 248, pp. 673 - 696.

Strahler, A. N. 1952a. Dynamic Basis of Geomorphology. Bulletin of the Geological Society of America, vol. 63, pp. 923 - 938.

Strahler, A. N. 1952b. Hyposmetric (area-altitude) Analysis of Erosional Topography. Bulletin of the Geological Society of America, vol. 63, pp. 1117 - 1142.

Strahler, A. N. 1954. Statistical Analysis in Geomorphic Research. The Journal of Geology, vol. 62, No. 1, pp. 1 - 25.

Strahler, A. N. 1956. Quantitative Slope Analysis. Bulletin of the Geological Society of America, vol. 67, pp. 571 - 596.

Strahler, A. N. 1958. Dimensional Analysis Applied to Fluvially Eroded Landforms. Bulletin of the Geological Society of America, vol. 69, pp. 279 - 300.

Strahler. A. N. 1992. Quantitative/Dynamic Geomorphology at Columbia 1945 - 60: a Retrospective. Progress in Physical Geography, vol. 16 (1), pp. 65 - 84.

Strahler, A. N. 1999. Science and Earth History: the Evolution/Creation Controversy. Prometheus Books, New York, U. S. A.

Strathern, P. 1997. The Big Idea: Newton and Gravity. Arrow Books, London.

Summerfield, M. A. 1991. Global Geomorphology: an Introduction to the Study of Landforms. Longman Scientific and Technical, Harlow, England.

Swartz, N. 1985. The Concept of Physical Law. Cambridge University Press, Cambridge, England.

Tarboton, D. G., Bras E. L. and **Rodriguez-Iturbe, I. 1988.** The Fractal Nature of River Networks. Water Resources Research, vol. 24, pp. 1317 - 1322.

Task Committee for Preparation of the Sedimentation Manual, Comittee on Sedimentation of the Hydraulics Division 1971. Journal of the Hydraulics Division, Proceedings of the American Society of Civil Engineers, vol. 97, HY1, pp. 101 - 141.

Tauber, A. I. 1997. Introduction. In: **Tauber, A. I.** (Ed.) **1997.** Science and the Quest for Reality: Main Trends of the Modern World, pp. 1 - 52. Macmillan Press, Basingstoke and London, England.

Tennekes, H. and **Lumley. J. L. 1972.** A First Course in Turbulence. The M. I. T. Press, Cambridge, Massachusetts, U. S. A.

Thorn, C. E. 1988. Introduction to Theoretical Geomorphology. Unwin Hyman, Boston, U. S. A.

Thorn, C. E, and **Welford, M. R. 1994.** The Equilibrium Concept in Geomorphology. Annals of the Association of American Geographers, vol. 84 (4), pp. 666 - 696.

Thornes, J. B. 1978. The Character and Problems of Theory in Contemporary Geomorphology. In: **Embleton, C., Brunsden, D.** and **Jones D. K. C** (Eds.) **1978.** Geomorphology: Present Problems and Future Prospects, pp. 14 - 24. Oxford University Press, Oxford, England.

Thornes. J. B. 1983. Evolutionary Geomorphology. Geography, vol. 68, pp. 225 - 235.

Tinkler, K. J. 1985. A Short History of Geomorphology. Croom Helm, Sydney, Australia.

Trovimof. A. M. and **Phillips. J. D. 1992.** Theoretical and Methodological Premises of Geomorphological Forecasting. Geomorphology, vol. 5, pp. 203 - 211.

Unwin, D. J. 1977. Statistical Methods in Physical Geography. Progress in Physical Geography, vol. 1, pp. 185 - 221.

Vitek, J. D. and **Giardino, J. R. 1993b.** Preface: A Perspective on Getting to the Frontier. In: **Vitek, J. D.** and **Giardino, J. R.** (Eds.) **1993.** Geomorphology: The Research Frontier and Beyond: Proceedings of the 24th Binghampton Symposium in Geomorphology. Geomorphology, vol. 7, pp. ix - xiv.

Want, C. and **Klimowski, A. 1996.** Kant for Beginners. Icon Books. Cambridge, England.

Watson, R. A. 1966. Is geology different? A Critical Discussion of 'The Fabric of Geology.' Philosophy of Science, vol. 33, pp. 172 -185.

Weisberg. S. 1985. Applied Linear Regression, Second Edition. John Wiley and Sons, New York, U. S. A.

Whitehead, A. N. 1925. The Origins of Modern Science. In: **Tauber, A. I.** (Ed.) **1997.** Science and the Quest for Reality: Main Trends of the Modern World, pp. 53 - 69. Macmillan Press, Basingstoke and London, England.

Whittow, J. 1984. Penguin Dictionary of Physical Geography. Penguin, London, England.

Williams. J. 1996. Turbulent Flow in Rivers. In: **Carling. P. A.** and **Dawson. M. R.** (Eds.) **1996.** Advances in Fluvial Dynamics and Stratigraphy, pp. 1 - 33. John Wiley & Sons, Chichester, England.

Wilson, A. G. 1972. Theoretical Geography: Some Speculations. Transactions of the Institute of British Geographers, vol. 57, pp. 31 - 44.

Winterbottom. S. J. and **Gilvear. D. J. 1997.** Quantification of Channel Bed Morphology in Gravel Bed Rivers Using Airborne Multispectral Imagery and Aerial Photography. Regulated Rivers: Research and Management, vol. 13, pp. 489 - 499.

Wolman, M. G. and **Brush, Jr. L. M. 1961.** Factors Controlling the Size and Shape of Stream Channels in Coarse Noncohesive Sands. United States Geological Survey, Professional Paper 282 - G, pp. 180 - 210.

Wolman, M. G. and **Eiler, J. P. 1958.** Reconnaissance Study of Erosion and Deposition Produced by the Flood of 1953 in Connecticut. Transactions of the American Geophysics Union, vol. 39, pp. 1 - 14.

Wolman, M. G. and **Miller, J. P. 1960.** Magnitude and Frequency of Forces in Geomorphic Processes. Journal of Geology, vol. 68, pp. 54 -74.

Woolhouse, R. S. 1988. A History of Western Philosophy, vol. 5: The Empiricists. Oxford University Press, Oxford, England.

Xu, T., Moore, I. D. and **Gallant, J. C. 1993.** Fractals, Fractal Dimension and Landscapes - a Review. Geomorphology, vol. 8, pp. 245 - 262.

Yepez, H. N., Brito, A. L. S., Vargas, C. A. and **Vicente, L. A. 1989.** Chaos in a Dripping Faucet. In: **Lam, L. 1998.** Nonlinear Physics for Beginners: Fractals, Chaos, Solitons, Pattern Formation, Cellular Automa and Complex Systems. World Scientific Publishing. Singapore.

Yevjevich, V. 1971. Properties of River Flows of Significance to River Mechanics. In: **Shen. H. W.** (Ed.) **1971.** River Mechanics, vol. I, pp. 2-1 to 2-20. Water Resources Research Publication, Fort Collins, Colorado, U. S. A.

Younis, B. A. 1996. Progress in Turbulence Modelling for Open Channel Flows. In: **Anderson, M. G., Walling, D. E.** and **Bates, P. D.** (Eds.) **1996.** Floodplain Processes, pp. 300 - 332. John Wiley & Sons, Chichester, England.

Zumdahl, S. S. 1995. Chemical Principles, Second Edition. D. C. Heath, Lexington, Massachusetts, U. S. A.