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Les vertus des défauts:

The scientific works of the late Mr Maurice Kleman analysed, discussed and placed in historical context, with particular stress on dislocation, disclination and other manner of local material disbehaviour

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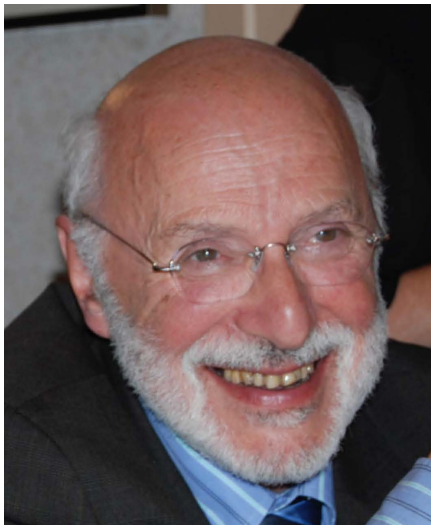
Abstract



In memory of Maurice Kleman: Over the last half century, Maurice Kleman was the High Priest of the science of defects in condensed matter. I discuss some aspects of the history of dislocations, disclinations, and defects in liquid crystals, together with some of Maurice's other work. In so doing, I combine intellectual strands coming from pure and applied mathematics, physics, material science, and biology.

Key Words: Liquid Crystals; Kleman; Defects; Disclination; Dislocation; Topology; Homotopy.
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1 Introduction

Crystals, as is now even taught to schoolchildren, consist – mainly – of arrays of regularly spaced atoms. This hypothesis was confirmed by classic X-ray scattering experiments by von Laue and collaborators [1, 2] and by the Braggs [3] in the period 1910-1915. Many crystalline properties – acoustic, electrical, thermal – depend, as one might expect, on the fact that the arrays are indeed regular. Early theoreticians of the solid state – Einstein [4], Born and von Kàrmàn [5], Debye [6], Felix Bloch [7] – were much encouraged by their success in reconciling theory and experiment. But some other crystalline properties, and in particular the ability of a crystal to withstand aggressive forces which seek to crush it, damage it or tear it apart, are not so amenable. In order to understand these properties, it turns out to be necessary to take account of the *caveat* "mainly" in the first sentence of this paragraph. *Dislocations* are defects – irregularities – in the regular arrays. Their intellectual history will be described in more detail below. The key progress here dates from the period 1930-1960.

Liquid crystals, are not, as we seem obliged to explain perennially to passing journalists and beginning graduate students, crystals at all. In the 1890s and early 1900s, the founding fathers, and preeminently Otto Lehmann (1855-1922), took evidence for anisotropy as evidence for crystallinity at a time when the "lattice theory of solids" (as it was then known, with what appears to us now as archaic charm) was merely a theory and not yet established fact. After much professional debate and not a little professional antagonism, the liquid crystals were *not* renamed as mesomorphic materials, even though *everyone* agreed that this sobriquet would have conformed more closely to scientific verity. But despite their uncrystalline status, the liquid crystals retained some regularities. Their primary optical signatures, however, were not these regularities, but rather the irregularities. Irregularities which were recognised as defects, and defects which carried many analogies to the defects in solids, defects which in some cases came to be known as *disclinations* by analogy with the much studied solid dislocations mentioned above.

It was Maurice Kleman, who died on January 29 2021, who converted the investigation of defects in various materials, and particularly in liquid crystals, from a set of accidental and unconnected studies – albeit extremly interesting studies – into a professional and coherent science of defects. His distinguished career lasted more than half a century, with at least one article in the press at the time of his death. In this article I celebrate his scientific contribution by giving some account of his personal background, briefly reviewing some of the highlights of his own work, and also, perhaps primarily, by trying to place his work in a larger scientific context. A particular feature of this context is the wide set of scientific fields in which the concept of defect either makes a significant contribution or is required in order to construct a conceptual framework. These fields range from the pure mathematical algebraic topology, through standard ideas of partial differential equations in applied mathematics, continuum mechanics in physics, material science and metallurgy, as well as physical chemistry and finally as we shall see, rather more surprisingly, into various aspects of biology.

We start by quoting verbatim an extract from the abstract of a review of the field written by Maurice in 1989 [8]:

> Defects are local breakings of symmetry in an ordered medium. The physics of defects has long been reduced to the study of dislocations in solids, and to the main physical phenomena they are responsible for, like plastic deformation. Dislocations break translational symmetries. Disclinations break rotational symmetries and are the basic defects of media with continuous symmetries, like liquid crystals. In this review, it is stressed how their study has contributed to a renewal of the physics of defects ...

To summarise, in order that there be defects, there must first be (global) broken symmetry, and then, the broken symmetry must itself (locally) be broken. In the context of condensed matter the symmetry of free space is broken because the temperature has been lowered. Analogous physics appears, as we shall see below, in cosmology and fundamental particle physics, albeit usually with a different group structure. And then because the symmetry is locally broken, the manner of the breaking can change from place to place. Under suitable circumstances, this can lead to defects, which can be points, lines, or surfaces. Like knots in a rope, *exactly* like knots in a rope (as again we shall see), they cannot be untied, without moving the offending object out of the spatial domain of interest.

Our narrative will introduce a number of *dramatis personae*, whose historical influence on Maurice and his work will become clear in the course of this essay. Jacques Friedel (1921-2014) (Fig.1, left), in his time President of the *Académie des Sciences*, was Maurice's Ph.D supervisor. Friedel and he remained close throughout Maurice's career. Maurice's first paper on liquid crystals (although subsequent to his Ph.D) was a collaboration with Friedel [9]; it deals with dislocation lines in cholesteric liquid crystals, and is reviewed elsewhere in this volume. It is perhaps not as often read as it ought to be, partly because it is written in French just as the dominance of English was beginning to overcome the power of even other world languages. Their collaborative review article "Disclinations, dislocations, and continuous defects: A reappraisal" appeared (this time in English!) in the prestigious *Reviews of Modern Physics* as late as 2008 (when Maurice was 74 years old, and Friedel was 87!).

Friedel is regarded as a major figure in material science, and was showered with honours and prizes both at home and abroad. His 1956 textbook on dislocations [10], translated into English in 1964 [11] was immensely influential. He is also of interest in a more general cultural context. His partly historical, partly autobiographical *Graine de Mandarin* [12] is a fascinating memoir on his life and those of some of his illustrious forebears (see also e.g. [13]). It was this cultural hinterland which motivated him to encourage Maurice also to chronicle his own life in science [14].

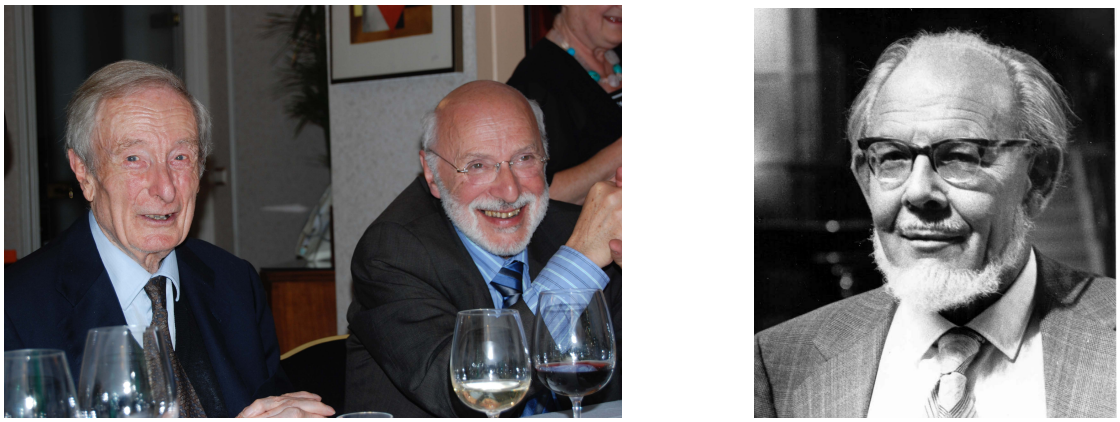


Figure 1: **Left**: Jacques Friedel (left) and Maurice Kleman (right), 2007. [Courtesy of Kleman family.] **Right**: F.C. (from 1977 Sir Charles) Frank. [Photograph by R.A. Philpott. Courtesy of University of Bristol School of Physics; ©University of Bristol].

Friedel learned his craft as a solid state physics PhD student in Bristol, England, which in the immediate postwar period had become a major centre in dislocation research. The leading figure in this school was F.C.(Charles) Frank (1911-98) (Fig.1, right). It was Frank who in 1958 at a Faraday Discussion meeting in Leeds, UK, launched the rebirth in the interest in liquid crystals which led directly to our current view. In this paper he outlined problems in nematic

liquid crystals, used methods derived from elasticity theory to derive equivalent "Frank" elastic constants, and introduced the term "disinclination" to describe the elementary nematic liquid crystal defect line. Frank and Maurice met for the first time in a conference in Montpelier in 1969, and on numerous occasions subsequently, including at conferences jointly organised. Although they have no papers in common, a measure of Frank's huge intellectual influence on Maurice is that his autobiography [14] mentions Frank 86 times in 232 pages, thus more than once in every 3 pages.

Edmond Friedel (1895-1972) (Fig. 2, left) was the father of Jacques Friedel, and Director of the *École des Mines* when Maurice was studying there in the late 1950s (I postpone a more detailed biography to a later section). It was Edmond, when faced with an enthusiastic student who had nevertheless somehow lost his academic way, who suggested that Maurice turn to his son Jacques for some professional guidance: a suggestion which in the end was professionally the foundation of his career. Of further interest in the context of liquid crystals, however, is that it was Edmond who, in 1923 in the private laboratory of Maurice de Broglie (older brother of the quantum pioneer Louis of wavelength fame), carried out the X-ray scattering *experimentum crucis* on a smectic liquid crystal sample confirming its lamellar status [15, 16].

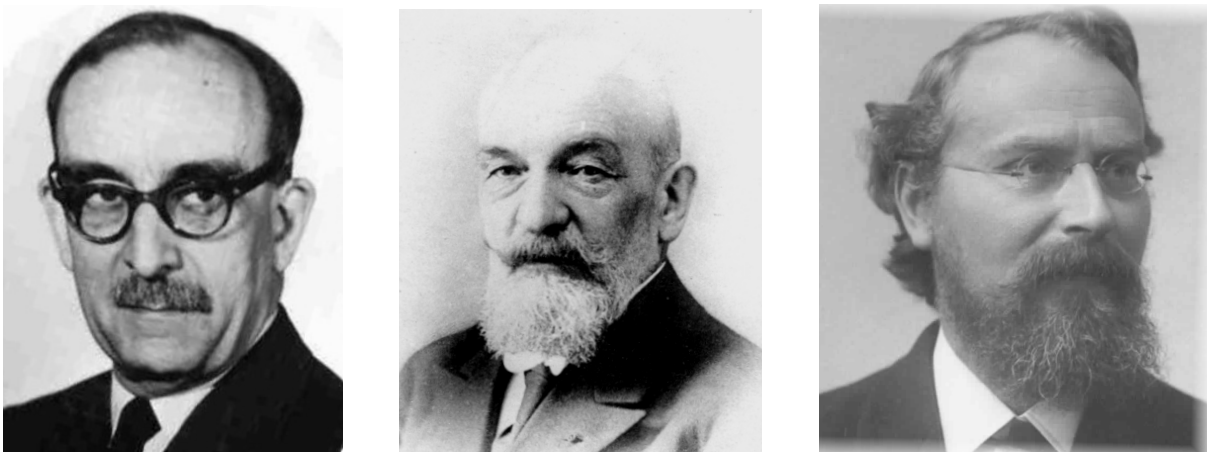


Figure 2: **Left**: Edmond Friedel. **Centre**: Georges Friedel. [Both courtesy of Friedel family]; **Right**: Otto Lehmann. [Courtesy of Deutsche Bunsen Gesellschaft]

The Friedel liquid crystal lineage, however, extends a generation further back, to Edmond's father Georges Friedel (1866-1933)(Fig. 2, centre). We note in passing that as a scientific family, the family Friedel rivals the Huxleys and Darwins in the UK in the eminence of its scions. Not only was Georges' father the distinguished chemist Charles Friedel (he of the Friedel-Crafts reaction), but according to that reliable purveyor of information, the French edition of Wikipedia, Jacques Friedel's direct ancestry extends as far as the eminent Swiss mathematician Johann Bernoulli (1667-1748).

It is to Georges Friedel that we owe the terms "nematic","smectic","cholesteric" and "mesomorphic". The last of these he advocated as "more correct" terminology, which should replace the "liquid crystals" so-named by his German rival Otto Lehmann (1855-1922) (Fig. 2, right). Lehmann was the grand pioneer of liquid crystals (see e.g. [16, 18]); his monograph of 1904 includes an enormous number of photographs and diagrams, both of his apparatus and of his observations. We show in Fig. 3 some representative photographs which show what we now understand to be defects in the nematic orienation field ("disclinations") and the focal conic structures which appear in the smectic liquid crystals. Of course, at the time, no-one was quite sure whether the

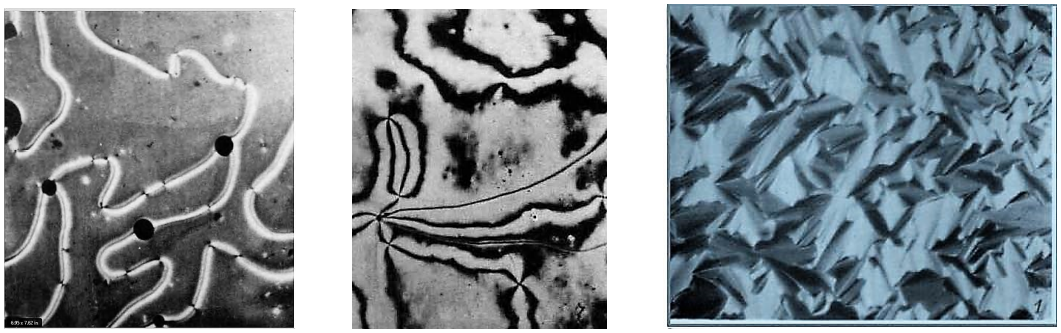


Figure 3: Photographs of liquid crystalline defects appearing in Otto Lehmann's famous 1904 monograph on liquid crystals [17]. **Left**: disclination lines in nematic liquid crystals (in Lehmann's classification, *flüssige Kristalle*). In Friedel's classification these were *structures à fils*. **Centre**: The well-known Schlieren texture (so labelled by Ludwig Gatterman as early as 1890), labelled by Friedel as *structures à noyaux*. The *Kernpunkten* (hard points) in the centre of the brushes remain unaltered when the crossed polars are rotated. **Right**: Conic structures appearing in Lehmann's *fliessende Kristalle*, later identified by Georges Friedel as conic sections associated with grain boundaries of differently oriented smectics.

structures were objects (i.e. like a colloidal aggregate, for example) or patterns, or whether they were situated in the bulk of the material or somehow attached to the sample surface.

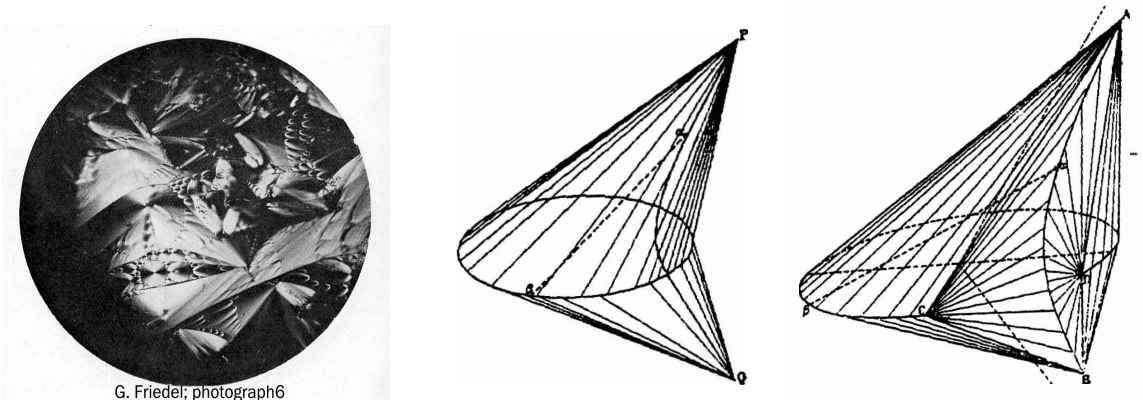


Figure 4: Extracts from Georges Friedel's magisterial 1922 paper *Les états mésomorphes de la matière* [19]. **Left**: photograph of focal conic sections. **Centre, Right**: Drawings designed to show that non-uniform smectic domains inevitably gave rise to defect lines in the form of conic sections at which there would be strong discontinuities in the extraordinary refractive index, and hence light scattering.

Georges Friedel authored a 1922 review article [19] which stamped its mark on the field in such a solid fashion that it remains one of the most cited liquid crystal papers of all time, rivalled in the present day only by standard textbooks such as that by Pierre-Gilles de Gennes [20]. This paper labelled the liquid crystal phases so convincingly that within a decade even his opponents adopted his nomenclature. His geometric arguments were persuasive that the conical structures appearing in a microscopic view of the smectic liquid crystals were large-scale signatures of a layered structure: an imperfect layered structure, to be sure, but locally layered nevertheless. We

show in Fig.4 his photographs and the geometric reasoning which led to this conclusion. The conic section texture seemed very common, implying an equal frequency of grossly imperfect samples. Thus one might expect that it would be a non-trivial task to prepare a sample sufficiently well-ordered that the layered structure would appear clearly in the X-ray scattering signature. It was therefore somewhat of a surprise to Georges and Edmond Friedel that this turned out not to be the case [21].

The point here is merely that the defects so carefully studied by Maurice can be found in the very earliest observations on liquid crystals in the optical microscope. Indeed they were used, albeit in somewhat haphazard fashion, to establish the key properties of those phases. But somehow they were not thought of as objects worthy of study in and of themselves. It was only fruitful to return to the defects themselves, once one was sure that the structures of the phases themselves were well-established and well-understood.

The structure of this essay is as follows. In section 2 we relate some brief biographical details of Maurice's life. In section 3, we give a historical discussion of the development of the ideas of dislocation and disclination in the first half of the 20^{th} century. In section 4 we present some of the academic highlights of Maurice's career. In section 5 we expand on a particular aspect of Maurice's career, that of the introduction of ideas from topology to the analysis of defects in condensed matter physics. In this section we trace some of the subsequent developments in this area.

In section 6 we glance at developments over the last two decades concerning the role and function of defects in biological systems. These were somewhat unforeseen developments mainly occurring after Maurice's main body of work. However, they do follow-up and extend the work of Maurice's close colleague Yves Bouligand. Moreover they serve to underline the broad academic extent, and at the same time coherent unity, of the set of interests that encapsulate Maurice's career.

The main conclusion we can already state here, and it will echo throughout this article. That conclusion is of Maurice's "life well-lived". But to say this will not require a separate section. Instead we finish with an Afterword, focussing on the distinguished Early Modern philosopher Benedict Spinoza – one of Maurice's amateur, but nevertheless profound, interests.

2 Biography

Maurice Kleman seemed to outsiders typically French. However, his almost parodic Maurice Chevalier accent in English – albeit hidden by a fine style when it came to the written word – belied his origins. For both his parents were born in Poland, speaking Yiddish as their first language. His father Zelman (transformed into Jules once in France) was born in 1903, while his mother Shara – frenchified into Simone (née Goldberg) – was born in Częstochowa in 1910. Both parents came to France as children with families who were seeking a better life in a more tolerant society.

Jules and Simone were married in Paris in 1930, where Maurice, their first-born, first saw the light of day on August 11 1934. Both parents were in the rag trade. Later Jules became a celebrity tailor, Jules Kleyman. Sometimes the surname was Klajman or Kleimann; spellings differ. The "Kléman/Kleman" version dates only from Jules's naturalisation in 1952 [22].

The family, mother, father, Maurice and his younger brother Roger, lived in a large apartment above the shop, opposite the famous Hôtel Drouot auction house in the 9^{th} arrondissement (his younger sister France was born after the War). Maurice was brought up speaking only French, but the language of his parents' generation remained almost exclusively Yiddish. One of his

enduring sorrows was that although this language always seemed familiar, he was never happy in it. The family was not particularly religious; in adulthood Maurice, while deeply conscious of his cultural background, regarded himself as completely secular.

When Maurice was only five years old, the family's world was suddenly upended by the Nazi invasion of France and occupation of Paris in May 1940. There followed a period of extreme peripatetism. The family fled to that part of France that remained, at least until November 1942, unoccupied. They were endeavouring, successfully as it turned out, to avoid the fate of many co-ethnics who were deported to Concentration Camps in the east. The last part of the war was spent, lying low, in the small village of Le Chambon-sur-Lignon in the mountainous central region of France, between Le Puy and Valence. In 1990 this village, which had been primarily Protestant – and hence in a French context anti-Establishment – since the 17th century, was recognised by *Yad Vashem* in Jerusalem as collectively "Righteous among Nations". During the Nazi period, in addition to the Kleman family, this commune had sheltered many thousands of fugitive Jews.

Figure 5: Left: Maurice aged about 11. Centre: Maurice aged about 18, with his childhood sweatheart (and future wife) Jacqueline Blioch. Right: Maurice in 1954, dressed in his *Polytechnicien* uniform. Courtesy of the Kleman family.

At the end of the war the family returned to Paris. At the *lycée* (High School), which he entered more or less on his return to Paris, he studied Latin, Greek, History and the Exact Sciences. He claimed that it was not German culture, nor yet the recent bad experiences of Germans, but rather the Gothic script, that put him off studying the German language. Quite evenhandedly, he disappointed the rabbi who had been entrusted with training him for his *Bar Mitzvah* by preferring the scholarship of Homer and Virgil to that of the Hebrew prophets. His successful school academic career was crowned by the *prépas*. This is an extremely academically demanding two-year targetted preparation period for the elite *Grandes Écoles*. Maurice successfully navigated this hurdle and entered the École Polytechnique in September 1954, aged just 20 (see Fig. 5 for a photograph of Maurice in his Polyechnicien uniform).

The *Grandes Écoles* carry both social status and academic power analogous to that of Ivy League universities in the US or Oxbridge in the UK. Traditionally the future leadership of the French institutions, whether in the private or the public sector, are recruited from their graduates. The École Polytechnique (or simply *l'X*) was founded by the celebrated mathematician Gaspard Monge (1746-1818), and is a military school, explaining the uniform in Fig.5. It specialised in engineering and science. There are so many distinguished graduates that we can mention here but a few, such as Sadi Carnot (1796-1832, X1812) of thermodynamics, Augustin Cauchy (1789-1857, X1805) of continuum mechanics and residues, or Henri Poincaré (1854-1912, X1873), of well, almost everything really. Traditionally the date after the X marks the year of admission.

Those who studied in it carried an obligation to provide several years of public service before embarking on an independent career.

Having graduated from *l'X* in 1956, Maurice would have liked to proceed to a research career, although at that time he had little idea of exactly what he wanted to do in research. But first he had to fulfil his duties to the state, which involved in his case an attachment with the *Corps des Mines*, a kind of state engineering service with a mining and mineralological orientation. This was an exclusive posting, for which a high classification in the final examinations at *l'X* was required. Attendance at *l'X* counted for two out of three years of required military service at the time. The third he spent in the Air Force; as the war in Algeria was raging at the time, he was lucky to avoid being put into the firing line. He then spent a year in the *École des Mines* (School of Mines) in Paris, actually a rather high status and powered school, taking more specialised courses.

The Director of the School of Mines was Edmond Friedel (X1914), scion of the famous French scientific family. This was the first time that Maurice was to meet a Friedel. As we have already noted, sensing some disaffection in this student, Edmond Friedel directed Maurice toward his son Jacques Friedel (X1942). Their later interaction was probably to be the Maurice's most important scientific influence. He wished to proceed directly to a research post. Unfortunately, high as it had been, his ranking at the Polytechnique had not been quite high enough to be permitted to follow this route (two higher-ranking students had been placed above him on the list). However, the Deputy Director of the School placed Maurice on a two-year waiting list. One year was spent carrying out X-ray analysis of rocks. In 1958 de Gaulle had just reassumed office as President of France, but not yet abandoned the idea of *Algérie française*. A by-product of this vain pursuit was that the next year found Maurice at the Algerian Bureau of Mining Research close to Algiers.

A special scheme allowed members of the *Corps des Mines* to fulfil their obligatory duties by studying toward a Ph.D. Embarking within this scheme, and after some hesitation and false starts, Maurice enrolled as a graduate student at IRSID (*Institut de recherche de la sidérurgie*, Steel industry research institute) at Saint-Germain-en-Laye, about 25km to the north-west of Paris. There he was to stay between 1961 and 1967, as a student of Jacques Friedel. His work uncharacteristically in the context of contemporary hyperspecialisation, involved both theory and experiment. He defended his thesis *Contribution á l'étude des propriétés des lames minces ferromagnétiques et à la théorie de la magnétostriction* (Contribution to the study of the properties of thin ferromagnetic films and to the theory of magnetostriction) in June 1967. It is of relevance to recall that Friedel had himself finished his PhD in Bristol in 1952, and thus knew the British "dislocation community" well.

Following his PhD, Friedel arranged for Maurice to spend a year in Peter Hirsch's laboratory (the Department of Metallurgy) in Oxford, where he continued to work on the interactions between magnetism and defects in solids. A year later, he was able to exchange his civil service Mining Engineer position for a much less lucrative post as a CNRS (the national research organisation) researcher at the Laboratoire de physique des solides (LPS; Solid state physics lab) at the Université Paris-Sud in Orsay, just south of Paris. He was 34 years old, and it was his first permanent academic post.

We postpone a more detailed discussion of Maurice's work, other than to note that academically the next few years were extremely fruitful for him. At Orsay he enjoyed a successful career, including directing the laboratory between 1982 and 1984. He left LPS in 1993, moving to the Laboratoire de Minéralogie et de Cristallographie de Paris (LMCP), where he remained until he retired in 1999 and beyond.

One measure of the success of Maurice's career were the prizes and distinctions awarded to him.

These included the Silver medal of CNRS (1975), the Jean Ricard Prize of the French Physical Society (1980), and the *Grand prix du Commissariat à l'énergie atomique* (*Académie des Sciences*) (2007). He felt particularly honoured in 2018 by his election, as an international honorary member, to AAAS (Anerican Academy of Arts and Sciences), the distinguished American learned society dating back almost to colonial times.

My impression was that he appreciated the attention and the acknowledgment. He certainly felt indignation when others were rewarded for what he felt that he had achieved himself. He was an essentially kind person, sometimes carelessly rude, but only deliberately so when he felt that he had first been slighted. But without doubt his primary motivation, throughout his career, was an insatiable curiosity and thirst for knowledge, and a genuine joy in intellectual exchange with whomever he came into contact.

He married his childhood sweetheart Jacqueline Blioch in 1958. She predeceased him in 2015. There were three children of the marriage: Agnès (1962) and Jean-Philippe (1967), who survive him, and Laure (1965), who died within a few hours of birth, and whom he remembered with sadness the whole of his life. Toward the end of his life his partner was Madeleine Veyssié, with whom he shared the Covid-induced confinement. In late 2020 Maurice was struck down by a rapidly-growing brain tumour. The support given to him by Madeleine during this final illness was much appreciated by the family.

The deterioration in his health was very rapid after his diagnosis. After some weeks in hospital he was transferred to a hospice, concentrating on palliative care. His indomitable and enduring academic spirit was epitomised by his request as the transfer was being organised. He knew how sick he was. His powers of concentration were much reduced as a result of his illness. But in the short time he had left, there were still ideas to be worked through, and there was still the necessity to remain in contact with the extended scientific community. Just in case, he asked, just in case I have a chance to do some work, do they have broadband, and a table where I can work? A week later he was dead.

3 Prolegomena: Defects before 1960

3.1 Dislocations and distorsions

3.1.1 Preliminaries

Maurice's work involved both dislocations in solids and disclinations in liquid crystals. But in fact of course the concept of disclinations itself, as we shall see further below, developed out of that of dislocations. It was Charles Frank who coined a specific term for angular defects in nematic liquid crystals. In so doing, he was drawing analogies with the theory of defects in solids, an area in which he was then one of the leading exponents.

Telling the story of the development of the theory of dislocations illustrates some key themes in the history of scientific ideas. Firstly we see how concepts in macroscopic physics are extended to phenomena on scales on which they were not originally intended to apply. Secondly we see an interplay of theory and experiment which enables the original ideas to be refined and transformed to a point at which in their new articulation they seem almost unrecognisable. Thirdly we see how the transformed ideas can then be applied in related areas of physics. And fourthly, although we shall not dwell on this point, the benefit of hindsight allows experienced practitioners to realise that *if only they had known*, well back into the 19th century there had been numerous hints of the new physics in the literature.

The roots are usually traced back to work by the distinguished Italian mathematician Vito Volterra (1860-1940) in the early years of the last century. As it happens, Volterra was only the most distinguished and influential of these founding fathers. As so often is the case (and will also occur in our discussion of Maurice's contribution to the use of homotopy in the discussion of defect stability), parallel ideas were developed independently, because in some sense, the time was right.

The notion of a *Volterra process* enters Maurice's work on numerous occasions. So before continuing with the story of dislocations, we take a brief diversion to discuss Volterra, who was one of the most important Italian mathematician of the modern era. He has been the subject of numerous technical scientific biographies in Italian (see e.g. [23]) and one recent major biography concentrating on his human relationships [24] in English. His work on dislocations was but a minor part of his total mathematical *oeuvre*.

Contemporary mathematics contains frequent echos of Volterra's contributions. There are, for example, (different) *Volterra processes* both in material science and in control theory. *Volterra integral equations* and *integro-differential equations* not only crop up in the theory of elastic media but are also prominent on the pages of every textbook on integral equations. All theoretical ecologists are familiar with *Lotka-Volterra* systems, although sometimes the mathematics is disguised as the *Volterra population equation*. Elsewhere we find *Volterra systems*, *Volterra series*, *Volterra operators*, *Volterra kernels*, *Volterra filters*, *Volterra spaces*, and *Volterra functionals*. The *Volterra crater* in the northern hemisphere of the far side of the moon celebrates his astronomical work. And the founders of the *Volterra Consulting Group* may well have been unaware of his mathematical importance, for they are celebrating his influence on the early stages of the quantification of economic theory (see e.g. [25]).

3.1.2 Distorsions and dislocations

The discontinuities in crystal lattices which have come to be called *dislocations*, derive somewhat indirectly from the set of papers written by Volterra in 1905. As the reader will be aware, in 1905, neither the fact that most solids are built up from regular arrays of atoms (i.e. the lattice theory of crystals), nor the connection between the regular facets of observable crystals and the lattice theory of solids, had been established.

In 1905 the definitive experiments proving the structure of crystals, due to von Laue [2] and Bragg *père et fils* [3] were still eight years into the future. Even the atomic nature of matter required Einstein's famous insight into Brownian motion [26], and Perrin's subsequent experiment [27], rather than the mere spatial imagination of chemists. And in any case, Volterra was a mathematician with interests in continuum mechanics. There is no record of him pursuing mechanics at molecular length scales.

Nevertheless, curiously, there is an intellectual route leading directly from Volterra's purely macroscopic studies of elastic continua, to the current profound understanding of the molecular properties of materials. The observational key to the transfer of Volterra's ideas from everyday to atomic length scales lay thirty years ahead. The puzzle concerned the *flow* of elastic solids when subject to extreme tension. In that sense Volterra's most enduring legacy to this kind of elasticity lay in the field of 'soft solids'. But at what point does a *soft solid* become a *hard liquid*? This is a question impossible to answer precisely. All we can say is that, in a sense, both of these contributions have been fruitful in the interregnum where the formal theory of elasticity has given out, but that of hydrodynamics does not yet hold sway.

But all this was far from Volterra's mind when, around 1904, he began to consider the properties of elastic materials in multiply-connected bodies. The property of connectedness in a region of

space is much considered by pure mathematicians and geometers. Let us briefly recapitulate the key ideas. A region is *simply connected* if any closed curve or surface inside it can be smoothly shrunk to a point. An example would be the inside of a sphere. A *multiply connected region* does not share this property. The simplest example is the inside of a donut (known as a *torus* to mathematicians). In this case a closed curve stretching around the axis of the donut is unshrinkable. Examples are shown in Fig. 6 below.

The study and classification of such objects is now known (although not yet really in Volterra's day) as *topology*. Topologists can divide all closed regions into classes, depending, roughly speaking, on how many holes or handles the region encompasses. We shall return to a more comprehensive study of topology below, as it will turn out to be important, in a rather different way, in Maurice's work also.

The initiating step in Volterra's study seems to have been a 1901 paper in the Proceedings of the Accademia Lincei by the prominent German differential geometer Julius Weingarten (1836-1908) entitled "On discontinuity surfaces in solid body elasticity theory" [28]. Weingarten had been elected in 1899 as a Foreign Member of the *Accademia Lincei*, the Italian equivalent of the Royal Society. This paper, published under the moniker *Giulio* Weingarten (and in Italian) was read in Rome when the award was bestowed upon him. He was a specialist in the geometric study of surfaces and his (rather short) paper was concerned with surfaces of discontinuity in elastic theory. Weingarten spotted that if the region was multiply connected something peculiar happens to elastic theory.

The peculiar elastic effect noticed by Weingarten concerned the internal state of stress and strain within a body. Suppose first of all that forces are imposed on the *exterior* of a body (i.e. *external* forces). Then the material elasticity transmits the forces to the *interior* of the body. The existence of these interior forces is expressed mathematically in terms of a non-zero *stress tensor*. But Hooke's law requires that non-zero stresses imply non-zero strains. These non-zero strains should be integrable, yielding the local (unique) local displacement. The story is clear and unambiguous, and indeed apparently not only obvious but unassailable. External forces imply local internal displacements. Their absence, by contrast, implies no displacements, thus no internal strains, and hence no internal stresses.

Weingarten's theorem states that this is no longer true if the body is multiply-connected. Volterra's attention was drawn to Weingarten's paper, and indeed they exchanged considerable correspondence on the subject. We know that he had some doubts over some of Weingarten's points. Moreover, Volterra realized that the four pages devoted to the subject by Weingarten were insufficient to do the subject justice. And thus started his researches in this area. The key point was that a multiply connected body could sustain internal stresses and strains *without any external force being applied*.

With the benefit of hindsight this is not so surprising. Volterra's clear explanation is as follows. Consider a simply connected cylinder. All material points start in their "proper" places. Now remove a minuscule cylinder along the axis, just large enough to ensure that the body is no longer simply connected. He is introducing a *short-range cut-off*, which is common contemporary trick to avoid singularities in continuum theory which cannot exist on a molecular scale. Here the motivation is not exactly the same. But in Volterra's time, it was new. In any case, the hole did not have to be microscopic, only relatively small.

At this stage there are no stresses, strains, or displacements, only a hole in the middle. Now Volterra cuts out a small slice, all the way from the hole in the middle to the outside of the cylinder. Then he throws it away. Still no stresses, still no strains, still no displacement. Then (and this is the key step) he takes what is left, pulls it so that the cylinder is closed again (apart from the hole in the middle). And then he *glues it together* (see Fig. 6). The join is

so good that the position of the original cut can no longer be determined. Note that this is a thought experiment, which establishes a principle, rather than a practical suggestion. But as with Einstein's trains, all that is required is a thought experiment.

Now what? The surface forces which we imposed when we were pulling the cylinder closed have disappeared. Now there are only internal forces. But in order to close the cylinder, we had to move some points – in fact we probably had to move them all. So now we have a body with no external forces, but still it has internal stresses, internal strains and internal displacements, which depend, at least partly, on the way in which we glued the cylinder together.

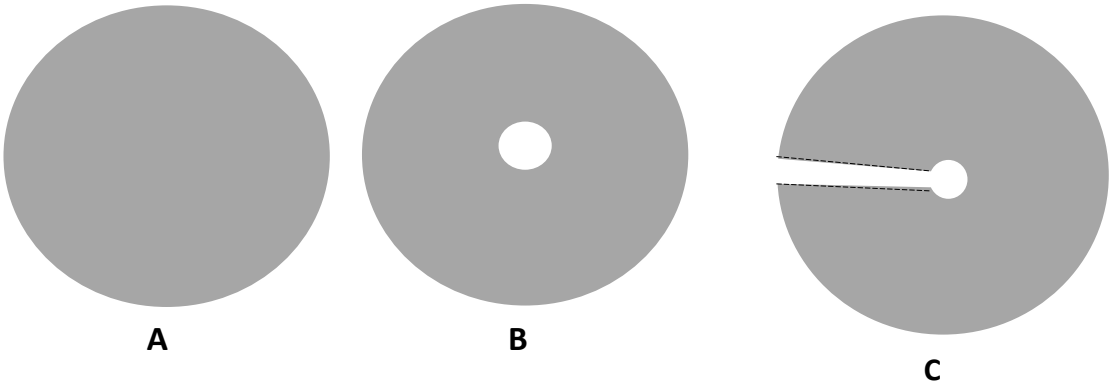


Figure 6: **A**: A simply connected region. In two dimensions, a closed loop inside this region can be shrunk to a point. In three dimensions, the analogous requirement is that a closed surface be so shrinkable. **B**: A multiply connected region in two dimensions. A closed loop which circumnavigates the hole in the middle cannot be shrunk to a point. Multiply connected regions contain either holes, handles, or both. **C**: Example of a Volterra cut. The centre of the circular region is excised, and a cut made from the centre to the outside. The dotted lines (just visible here along the cut) the cut are then glued together, leaving a multiply connected region.

This construction will in material science forever be associated with Volterra's name: it is the *Volterra process*; making a *Volterra cut* with a *Volterra knife*.

The paradoxical nature of the resulting strains is as follows. In usual elastic problems, integrating the strain from one point to another yields the relative displacement of the points as a result of the stress field. Integrate the strain round a closed circuit returns one to the same point. The relative displacement of a point with respect to itself is of course zero. Here, by contrast, because of the way that the strain fields have been set up, integrating around a circuit including the hole, does not return one to the beginning, but rather requires a subtraction of the initial displacement made by the "Volterra cut". Thus the internal displacements are no longer well-defined. The multiple-connectedness, the internal stress and the impossibility of defining uniquely the material displacements are all connected.

More formally, *either* the displacement is not a unique solution of the equations (*polydrome* in Volterra's language), *or* the displacement field is discontinuous across a cut made somewhere from the hole to the outside. Either one, or the other (you can choose whichever you prefer; they are mathematically equivalent). But neither is quite what one expects in a well-behaved theory. Whenever you get this phenomenon, Volterra says you have *una distorsione* (in Italian) or *une distorsion* (in French).

It turned out that there are mathematical analogies between field theories for fluids, solids and electromagnetism. For example, an attempt to construct a magnetic scalar potential for a field

induced by a current leads to the same ambiguity. Likewise in a fluid, a state of constant flow can exits in an annulus, even in the absence of a body force, as pointed out by Lamb [29] in his classic book on hydrodynamics.

Volterra presented this work originally in Italian in the Proceedings of *Accademia Lincei* in a series of papers appearing in rapid succession in 1905 [30]. They were translated with little change into French [31, 32] (and therefore accessible to a much wider readership) in 1907, and published, together with some new material, in the Annals of the École Normale Supérieure. The exposition is long and detailed. It is not helped by the fact that Volterra does not always use the modern suffix notation; this notation, employed by Einstein in his work on General Relativity, and by all modern workers in continuum mechanics, allows the genius of Volterra (or Rayleigh, Maxwell, etc.) to be reproduced by the merely talented.

The length of the exposition is also partly explained by the need to consider systematically different kinds of distortion, i.e. different ways of gluing together the cut after the material has been cut out. Each involves a different kind of solution for the internal stresses and strains. A diagram showing the different types of distortion (Volterra called them 'distortions of the n^{th} order') can be found in Fig. 7, taken from the book on the subject started by Vito Volterra in 1938 [33], but only finished by his son Enrico in 1960. There were originally six non-trivially different types of distortion, though by clever argument, Volterra was able to prove the equivalence of some of these types.

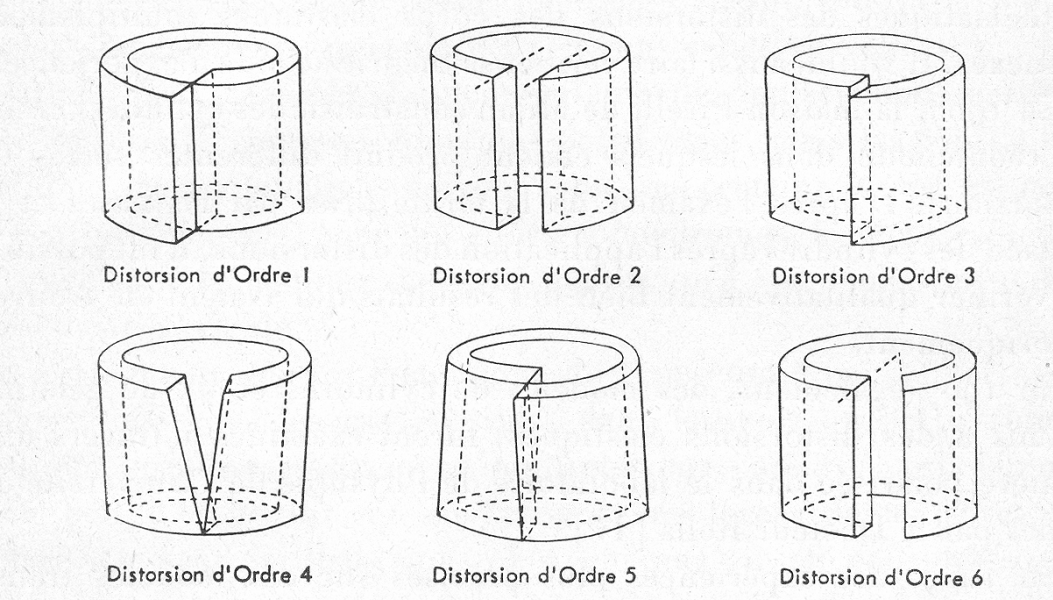


Figure 7: *Distorsioni* of orders 1 to 6 (reproduced from Volterra and Volterra [33]). The different "orders" (really classes, in modern language) correspond to qualitatively different relative positional or angular displacements after following the circumnavigation of a closed loop, and hence different types of Volterra process. For a recent sophisticated discussion, see [34].
Figure reproduced courtesy of the Volterra family

Volterra then encouraged a number of experimentalists to build apparatus to test his results. In the 1907 paper in French, he reports work by a Dr. Rolla, from the Physics Department at the University of Genoa. The internal strains could be seen because of the resulting optical anisotropy; a birefringence experiment should be able to detect them. Photographs of Rolla's apparatus (6cm tall, 5cm outer radius, 2 cm inner radius) are presented in the paper, and the theory is declared confirmed.

Volterra's long articles aroused considerable interest, leading to several more experimental and theoretical papers in the Italian scientific literature, by Cesàro, Maggi, Corbino, Trabacchi, as well as several by Colonnetti and later, by Volterra's distinguished close colleague Carlo Somigliana [35].

The 1907 paper in French is punctilious in reporting parallel work on the same subject in a 1905 Göttingen thesis by a Dr Timpe, about which Volterra had originally been unaware. The background to this is as follows. There had been a previous contribution in 1899 from the Australian mathematician John Henry Michell (1863-1940) from the University of Melbourne. He had formerly been a student in Cambridge, England, and very much a part of the English applied mathematics tradition) had published a paper [36] using the Airy Stress function to derive stress fields in a bulk material given some boundary conditions. This is essentially a potential obeying a simple differential equation from which the stress can be derived. He had already noted that problems arose when the body in question was multiply connected.

Anton Aloys Timpe (1882-1959) was a graduate student of Felix Klein in Göttingen; his thesis [37] was entitled "Stress distribution problems in planar systems, easily resolved using Airy functions". The resulting academic paper appeared in the Zeitschrift für Mathematik und Physik [38] is influenced by Michell's methods. He notes that ...

> (this method)... which oddly still seems to be rather unfamiliar, and which my distinguished teacher Herr Professor F. Klein drew to my attention, turns out to be extremely fruitful.

The last section of this paper is entitled "Artificial self-tension", and is concerned with the stress in an annular ring as it is progressively closed. Unsurprisingly he encounters difficulties. There is a rather sad footnote, recording a nightmare scenario for any finishing graduate student:

> My dissertation, on which this work is based, was submitted on 14 December 1904. Subsequently an in-depth study of self-tension by V. Volterra, Atti Acc. Linc. Rend. (5), vol 14 (1905) was published.

It may have been the Italian papers on *distorsioni* that attracted the interest of the English mathematician A.E.H. Love (1863-1940). More likely, it was Volterra's visit to England in the course of war work in 1917-18, during which time they interacted strongly. In 1892 Love had written a monograph on elasticity [39]. Subsequent editions appeared in 1906, 1920 and 1927, by which time it had become (as it remains!) the standard work. He also had been the Secretary of the London Mathematical Society when Michell's paper had been communicated twenty years earlier, and no doubt the interaction with Volterra jogged the fading memory of these peculiar multiply-connected effects.

By the time 4th edition of "Love" appeared in 1927, it had acquired an entirely new section entitled "Volterra's theory of *dislocations*". Love is generous in also giving credit in the text to Michell, Timpe and Weingarten. He even notes that the great J.C. Maxwell had addressed the problem of multiple-connectedness in the (posthumously published) second edition of his Treatise on Electricity and Magnetism as long ago as 1881 (it is also actually in the previous 1873 edition).

But apparently arbitrarily, and with little excuse, Love, in his own words, "rendered (*distorsioni*) into English as dislocations". Why this apparently deliberate mistranslation? Perhaps it was that "distorsione" is the opposite of 'torsione", and although there is a tor*s*ion in English, there is only a distor*t*ion: not quite the opposite concept. In any case, any book on elastic theory is necessarily awash with all manner of distortion. Love may have thought that an extra Volterra distortion would be a twist too far. And, of course, for us in the liquid crystal world, the

linguistic dislocation is a happy one; we draw strong distinction to be drawn between dislocation and distortion.

The revised English term, whatever the reason for inventing it, became the standard. From then on, *dislocations* it was. The presence of a section on this subject in Love's book elevated the study from a scientific bywater to the mainstream. Specifically, when phenomena elsewhere required new analyses, there was a ready-made technology and language to describe them.

We have now covered the antiquity of dislocations. The problem to be solved was a continuum problem: that of determining stress and hence strain inside a body, given some boundary conditions. Usually the further question as to whether there is a set of displacements consistent with the strain involves some further compatibility equations. For Volterra and other workers in continuum mechanics, complications arose when the body was no longer simply connected (incidentally a topological concept, although it took some time for the term "topology" to become established). Experimentally the existence of *distorsioni* had been confirmed.

But as yet, there is no hint of a crystalline lattice, still less of imperfections in said lattice requiring explanation. No connection, in fact, to what we understand nowadays as dislocation physics. For this connection to emerge, however, only a short wait would be required.

3.1.3 Dislocation physics

Dislocation physics, as we now understand it, now almost a hundred years on, involves microscopic scales and irregularities in the crystal lattice. The *evidence*, as opposed merely to the *suspicion*, that the fundamental particles in a crystal are arranged in a regular lattice, comes from X-ray scattering experiments [2, 3] carried out in 1912-13.

The "lattice picture of solids", however, considered as a *theory*, rather than as established *fact* is much older. If we ignore speculations of ancient philosophers such as Leucippus and Democritus, whose beliefs were based more on metaphysics than on what modern scientists would regard as proper empirical evidence, then we can trace the atomic lattice to the French crystallographer René-Just Haüy (1743-1822)]. He developed the idea of a fundamental crystal shape and "atomic" crystalline unit which cannot be further broken down. His student Gabriel Delafosse (1796-1878) was the first to suggest the idea of a unit cell and lattice spacing, which in turn influenced Auguste Bravais (1811-63) to investigate his eponymous fundamental set of lattices. In a parallel narrative, the English chemist John Dalton (1766-1844) proposed an atomic theory using evidence based on chemical reactions. Later chemists then proposed molecular properties requiring real molecular shapes. The two strands together provided further circumstantial evidence for the lattice theory of solids. Given its importance for current world views, the subject has been intensively discussed by historians of science; for further discussion of these issues, see e.g. [40–42]).

The physics of crystalline solids is a big topic, still, we need not even mention, the subject of much research. It is not surprising that it took some time to get going. A theory should connect the interparticle forces to (a) the (melting) temperature at which the forces between the atoms are no longer strong enough to hold the lattice together, (b) the elastic moduli (i.e. the elastic response to forces imposed upon the crystal), and (c) the yield strength (i.e. how hard you have to pull in order that the crystalline lattice break apart).

The Russian physicist Yakov Frenkel (1894-1952) [43], estimated that the yield strength should be of the order of the elastic modulus. For a strain of order unity, the atoms are roughly twice as far apart as they should be. If they are this far apart, more or less, Frenkel suggests, this will be a trigger for the crystal no longer to hold together. The argument is approximate, because at this strain the stress-strain relation will no longer be. Nevertheless for an order of magnitude

estimate it should be OK.

Frenkel actually considered shear stresses, which in a fluid would cause shear flow, but in a solid usually only cause shear distortions, in which atoms in neighbouring layers are pulled in opposite directions within the layers. Crystal failure, at a critical stress known as the *shear strength*, occurs when neighbouring layers slide rather than merely distort. Frenkel's intuition was sensible and should have worked. But this quantitative estimate for the shear strength in metals was more than an order of magnitude too high.

One cannot place too much trust in calculations of this kind. There is no detailed mechanical model and at the end of the day, they are indeed merely estimates. However, a disparity of two orders of magnitude between theory and experiment was strong circumstantial evidence that something was awry with the physical picture. The naïve picture of crystal breakdown, in which the lattice is literally torn apart, must be wrong. This observation was coupled with a further observation that when the crystal did slide, it did so not smoothly, but in a series of little jumps.

But why and how? The first attempt at an explanation was made in 1929 by the German theoretical physicist Ulrich Dehlinger (1901-83) [44]. In a shear field, he reasoned, one layer of the crystal must slide by the next, periodically getting stuck when the crystal is well-ordered. His picture involved little hooks (*Verhakungen*) on one crystal plane which were grasping the atoms in the next plane, only to release their hold when the sliding force became large enough again. Qualitatively, but only phenomenologically, this would explain the jerky sliding motion. But why did the shear force have to be *concentrated* in some places, in just such a way that the local, and hence the global, failure mechanism involved the little jump?

The key idea was presented in three almost simultaneous papers in 1934 by the English physicist G.I. Taylor (1886-1975) [45], and the Hungarian chemists Egon Orowan (1901-89) [46] and Michael Polanyi (1891-1976) [47]. In Material Science, the story so far is regarded as the prehistory of dislocations. The history, as properly understood, starts with Taylor (later *Sir* Geoffrey, and the *doyen* of British fluid mechanics), Orowan (who later emigrated to the U.K., and then in 1950 to the U.S., subsequently finishing his career at M.I.T.) and Polanyi (who in 1934 had just arrived in Manchester, and later had a distinguished career as a philosopher of science). All three contributions identified irregularities in the crystal lattice as the fundamental elements of the solution.

The idea was that inside a crystal there are regions where the lattice changes its point of reference. All three papers identify lines along which an extra row of atoms is introduced into the lattice, shown in Fig. 8.

So now, in most of the crystal the lattice is regular and well-behaved, apart, maybe, from a little strain, which you detect by a departure of the regular crystal cell from its proper shape. In this case you take a circuit in the lattice: n atoms to the left, m atoms up, n atoms to the right, and finally m atoms down again. Where do you finish up? Back where you started, of course.

But if your circuit contains the line where you have added the extra row of atoms, you don't finish up where you started. You finish up one atom to the right. G.I. Taylor recognized what was going on; he knew his *Love*, he was familiar with the new section on Volterra's dislocations, and in any case he had met Volterra during the first world war. Taylor, unlike Polanyi and Orowan, cited Volterra, explaining that Volterra's mathematics held the key to understanding these new objects.

The full microscopic mathematical formalism was provided by the Dutch theoretical physicist Johannes Martinus Burgers (1895-1981) [48, 49]. Burgers borrowed copiously (as he openly recorded in later life to those who might have mistaken – indeed, did mistake – the required mathematical virtuosity for his own). The key element in mapping the Volterra theory onto

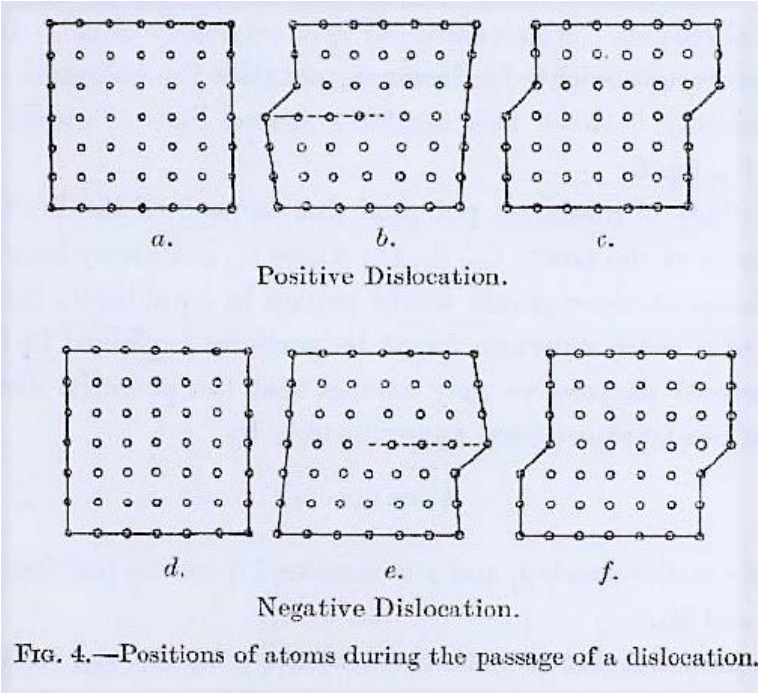


FIG. 4.—Positions of atoms during the passage of a dislocation.

Figure 8: G.I. Taylor's picture of slip along the glide plane, showing how planes slip against each other through dislocation movement/. Reproduced from Taylor's paper [45].

the molecular problem is the cutting-out process. Because the lattice must match everywhere away from the *dislocation line*, only certain kinds of cuts can be made before the Volterra gluing begins.

There are different kinds of possible processes, corresponding to Volterra's *distorsioni* of different order, or associated with different vectors which occur on making a circuit. Thus we get different kinds of dislocation – the *edge* dislocation is the one appearing in Taylor's seminal 1934 paper, whereas Burgers also introduced the *screw* dislocation, in which the vector shift on one circuit is parallel to the dislocation line.

The idea was so plausible that even absent direct experimental evidence, dislocations almost immediately entered the material science canon. Already in 1938 Frenkel and his student Tatiana Kontorova (1911-1977) produced an impressive set of papers with a detailed mathematical model of the sliding jumps [50, 51]. The resulting equation was a difference equation in space and a second order differential equation in time, of sufficient mathematical complexity that it is still attracting the attention of contemporary mathematicians (see e.g. [52]). Prompted by his lodger Orowan [53], Rudolf Peierls (1907-1995) took time off from building a nuclear bomb in 1940 to calculate the size of the dislocation-induced deformed region within the lattice, and the critical stress required to move the dislocation [54]. Peierls emphasised that his quantitative estimates should not be taken too seriously. It was just as well. His calculation of the required critical stress contained an error of a factor of 2, but the effect of the error on the critical stress was an overestimate by a factor of approximately 1000. The error was corrected by Frank Nabarro (1916-2006) [53, 55] in 1947 (people were too busy with other matters during the 1939-45 war to worry about attempting to move dislocations), which is why it has come to be called the Peierls-Nabarro stress.

The phenomenon of *work-hardening* in which the yield strength increases after bashing the material around a bit, because (roughly speaking) the dislocations become entangled, locking

up some further energy [56]. The Peach-Koehler force [57] (1950) is regarded as a force on a dislocation thought of as an object in itself, rather than as a pattern superimposed on an underlying atomic or molecular structure.

By 1947 Nabarro was a research fellow in Bristol, which had become a major centre for dislocation research. The central figures, apart from Nabarro, were J.F. Nye (1923-2019), J.D. Eshelby (1916-1981), but in particular and more relevant to this essay, Jacques Friedel and Charles Frank. Frank and collaborators were particularly fruitful in the postwar period, producing a stream of fundamental papers. These papers were not only extraordinarily imaginative from a physical point of view, but also mathematically sophisticated, using ideas ranging from functional analysis to differential geometry. A continuum simplification of the Frenkel-Kontorova picture of slip [58], led to the *Frank-van der Merwe equation*, better known nowadays as the Sine-Gordon equation [59]. An explanation of crystal growth from the melt [60–62] depended on a spare misaligned layer at the crystal-melt interface for the new atoms to attach to. Work hardening was explained in terms of the *Frank net* of dislocations, which had multiplied in a material under stress as a result of the *Frank-Read mechanism* [63,64]. It was above all Frank, who in 1951 in an article entitled "Crystal dislocations: Elementary concepts and definitions" [65] drew the various ideas together. Fig.9 shows a circuit around the crystal defect lines consisting of n steps up and down, and m steps to the left and right. The anomalous nature of the crystal defect is articulated in the fact that one does not return to the original position, and an extra vector b is required to close the circuit. It was this vector (which had rather been defined by a triad of lattice steps by Burgers) which Frank labelled as the *Burgers vector* [66]. We note also a fundamental paper from Nye [67], which introduced the idea of dislocation *density*, acting a source of stress and of curvature.

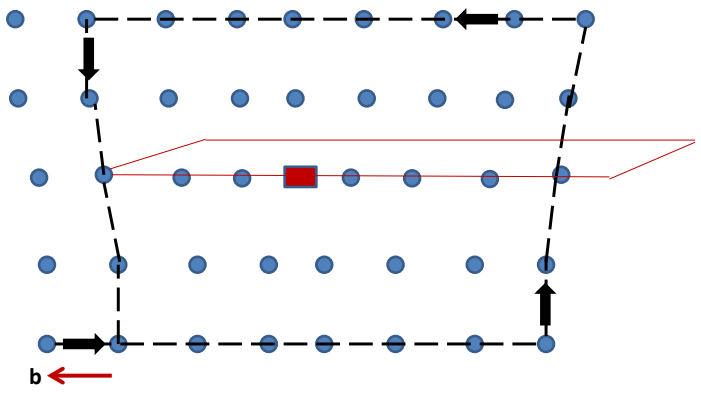


Figure 9: An edge dislocation, showing the Burgers circuit and Burgers vector **b**, the *glide plane*, and the extra plane of atoms above the dislocation. Atoms are blue circles, and the dislocation itself is shown as a red square. The edge dislocation corresponds to a *distorsione* of orders 1 or 2 in Fig. 7. The Burgers vector and circuit discussed in the text were essentially topological ideas.

3.1.4 Creep

The initial clue to the existence of microscopic dislocations in solid bodies, we have seen in the previous subsection, is the rather low value of the yield stress, which can only be interpreted in terms of layer slip and the Peierls-Nabarro phenomenon. What happens if that stress is exceeded? Sometimes the solid *cracks* before breaking, but sometimes it *flows* like a liquid. This kind of

situation occurs in moving glaciers, or when rocks distort in geophysical contexts, or when, as during the collapse of the World Trade Centre Towers on 9/11, the heat from fires cause the yield strength of the steel in reinforced concrete to reduce dramatically. This is termed plastic flow or more prosaically, simply simply *creep*.

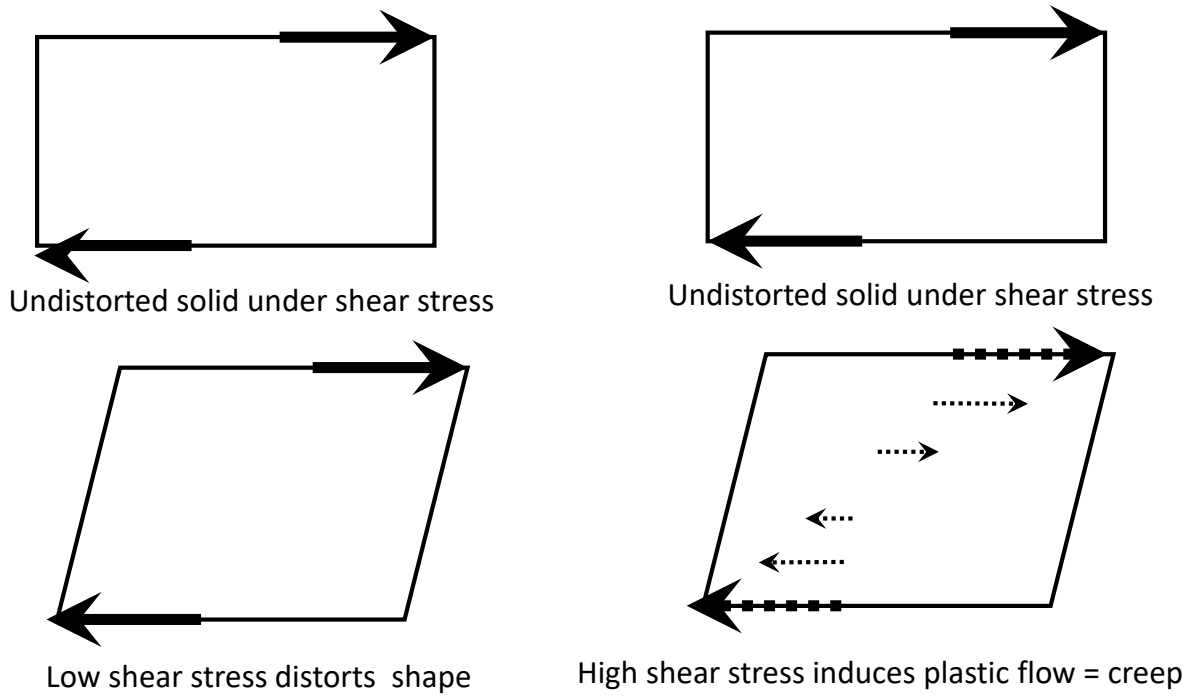


Figure 10: Low shear stress σ induces a static distortion of a solid body, i.e. a *strain* $\gamma(\sigma)$, as shown bottom left. Above a critical *yield stress* σ_c, the distorted body begins to flow, i.e. $\dot\gamma(\sigma)$.

Let us consider, for definiteness, a case in which the solid is subject to a shear stress, as caricatured in Fig. 10. The objects of interest are the *stress* σ, the *strain* $\gamma = \nabla u$, where u is the solid displacement from equilibrium, and the *rate of strain* $\dot\gamma = \partial\gamma/\partial t = \nabla v$, where v is the local velocity. In the general case these quantities are tensors, but in the shear case under consideration, they can be considered as scalars. In an elastic solid $\gamma \propto \sigma$, so long as $\sigma < \sigma_c$, the yield stress. In a simple liquid, on the other hand, $\dot\gamma \propto \sigma$, although the proportionality law no longer holds in so-called *viscoelastic* fluids. Beyond σ_c $\dot\gamma$ is a function of $(\sigma - \sigma_c)$, although sufficiently close to the solid melting temperature, there is a large viscoelastic region in which we can effectively ignore σ_c.

There are several different kinds of creep. *Primary* creep occurs because at low stress, the solid response is not immediate. Once the solid particle displacements have settled down to their equilibrium values the dynamic response stops. In *tertiary* creep the strain grows without bound, and ends only with the failure of the solid. Only in *secondary* creep is there a viscoelastic liquid-like reponse; this is the case on which I want to concentrate.

A more detailed background discussion, although fascinating, will take us too far from our primary topic. All textbooks of material science contain extensive sections on creep, including that of Jacques Friedel [10,11]. This author is unable to identify the origin of the technical term, other than to note its obvious transfer from ordinary language. It was already used in 1924 for beyond-yield-strength flow in a review by E.C. Bingham [68] (whom we shall meet again), whereas other contemporary workers use "creep" (with the quotation marks) [69]. But in 1910 E.N. da C. Andrade [70] merely talked of "plastic flow" of solid metals under stress, even though

later workers when citing this paper explicitly mentioned Andrade's studies of creep.

Sir Neville Mott [71] speculated in 1953 that some creep features might be explicable in terms of dislocation motion. Characteristic properties of the secondary creep in Aluminium were elucidated by John Dorn (1909-71) in 1955 [72]. His key result was that under suitable circumstances (intermediate stresses) $\dot{\gamma} \propto \sigma^m \exp(E_0/k_B T)$, for temperatures close to the melting temperature, with k_B the Boltzmann constant, E_0 some material-dependent property, and m some more or less constant parameter in the range 3-4. The proportionality of $\dot{\gamma}$ with a power of σ, together with its relationship with dislocation motion, is often known as the *Orowan Law*, though its genesis, as we see here, involved several other researchers in addition.

The following year Johannes Weertman (1925-2018), building on Mott's intuition, built a theory of creep in metals and simple alloys [73]. This yielded the Dorn rule for stresses which which were not too large. The details are too complicated to discuss here, save to remark that the key idea involved dislocation pile up, energy barriers which needed overcoming (hence the exponential term) and the creation of "Frank-Read" sources [63], as discussed in the previous subsection. The theory was subsequently refined [74]; the mechanism is consequently usually identified in the literature as *Harper-Dorn* creep. There has been much discussion in the literature as to its range of validity, the materials in which it might prevail over other creep modes, the accuracy or otherwise of numerical estimates of quantities entering the theory, and so forth (see e.g. [75, 76]).

Finally we note that other modes of creep, not involving dislocation motion, such as sliding of grain boundaries or diffusion of vacancies do exist and can dominate. The relevance of dislocation-induced creep to our story will become clear in Section 4.5.

3.1.5 Final remarks on dislocations

We leave the early story of dislocations here, for it will be the influence of Frank and Friedel, educated as they were by studies of dislocations, on defects in liquid crystals that primarily interests us. However, before we turn to disclinations, a few final remarks are in order.

Experiment: An enormous amount of early theory was developed based in indirect or circumstantial evidence of the existence of dislocations. There was clearly some pressure to produce some experimental confirmation. John Nye (Section 3.1.3 had been a research student of Orowan's in Cambridge. With Sir Lawrence Bragg, the head of the laboratory, he carried out an experiment on arrays of bubbles which exhibited wedge dislocations [77]. These are shown in Fig. 11. Technically, of course, although this experiment is rather persuasive, these are not atomic crystals but merely colloidal analogues.However, optical observations in the transparent AgBr by Hedges and Mitchell (1953) [78] exhibited crystallite grain boundaries which could be interpreted as a dislocation lattice, while Hirsch and coworkers (1956) [79] created electron micrographs of stressed aluminium with features only interpretable in terms of dislocation structures.

Antecedents: Hirth [80] reports a number of old observations, dating as far back as the 19th century, which would nowadays be interpreted in terms of dislocaiton-mediated grain boundaries. And Frank himself [81] interpreted some old puzzling micrographs by Menzies and Sloat [82], which could also easily be understood in terms of dislocations.

Material Science: Nowadays, the study of dislocations is a central part of all undergraduate materials science courses, leaking out into physics as well. All the standard textbooks, at least all those who dare subject their readers to more than Mickey Mouse mathematics, lean heavily on the treatment provided by Volterra to the readers of the *Annales de l'École Normale Supérieure*. In geophysics too, the study of dislocation stress fields is central to the understanding of earthquake genesis and dynamics.

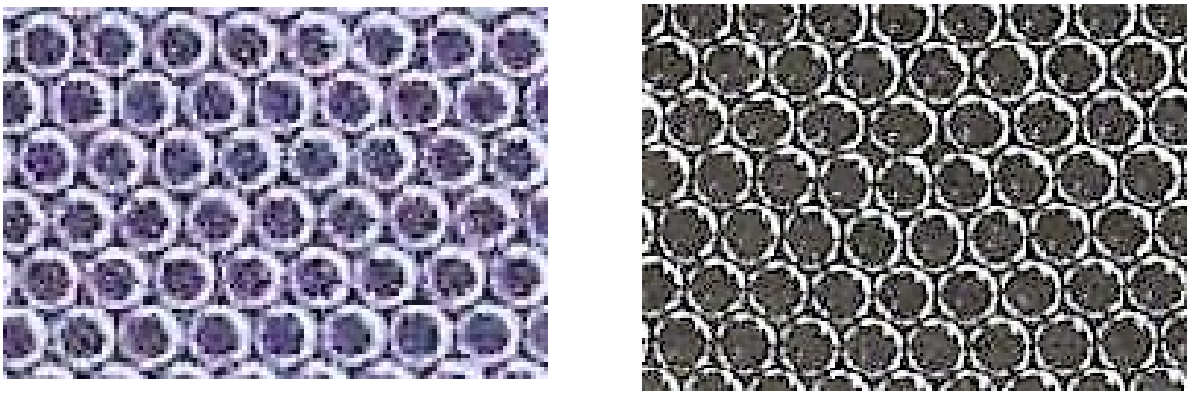


Figure 11: Photographs reproduced from Bragg and Nye [77]. Left: Perfect crystal. Right: Crystal with wedge dislocation. The dislocation is three bubbles down in the middle of the photograph. In each cases, only the central part of the photographs appearing in Ref. [77] has been reproduced, in order to identify the key features more easily.

Relationship with Volterra's original *distorsioni*: Burgers's application and extension of the Volterra theory came too late in Volterra's life for him to respond in the literature, so we do not know how he would have reacted to this intellectual distortion of his work. In their joint book on the subject [33], his son Enrico Volterra (1905-1973) is ambivalent. Partly he is proud on his father's behalf. And partly, he regrets that these are no longer *their* distortions. However, in any case, there remain macrosopic applications, particularly in his own speciality of civil engineering. Arches, he points out, are primitive examples of non-simply connected structures which are maintained in a state of stress without external forces. The well-known stability of arches is a consequence just of distortion theory.

3.2 Sir Charles Frank and disclinations

We have seen in the previous section the influential role played by Charles Frank in the development of the theory of dislocations in the immediate postwar period. So influential were his contributions that Jacques Friedel in his memoirs [12] went so far as to claim that so high was the quality of his work that he had been unjustly denied a Nobel Prize. From his interest in hard solids came also an interest in softer solids (the Bristol polymer group was a fruit of this interest), and thus, eventually, in liquid crystals.

In 1958 Frank was invited to deliver the keynote lecture at a Faraday Discussion Meeting in Leeds on liquid crystals and macromolecular systems. The resulting article [83] recapitulated points made by the Swedish theoretical physicist Carl Wilhelm Oseen (1879-1944) in a similar Discussion Meeting in London in 1933 [84]. The article sets out, with Frank's usual clarity, his view of the continuum theory of liquid crystals. This was pioneered, though not entirely completed, by Oseen in the 1920s (see e.g. [85]), but not completed until Frank Leslie's formulation of the Ericksen-Leslie theory in 1968 [86]. It also, significantly for our purpose here, enumerrated types of nematic defect, as shown in Fig. 12.

Oseen had already studied Lehmann's *Kernpunkte* and *Konvergenzpunkte* in his 1929 monograph [85]. If the elastic constants were equal, all that was required was to solve Laplace's equation for ϕ, the director orientation in the $x-y$ plane (but note that the term "director" in a liquid crystal context was only introduced by Frank Leslie in the 1960s). The solution $\psi = c\psi + \alpha z + \beta$ is readily derived, where $c = n/2$, with n an integer. The αz term describes possible torsion of

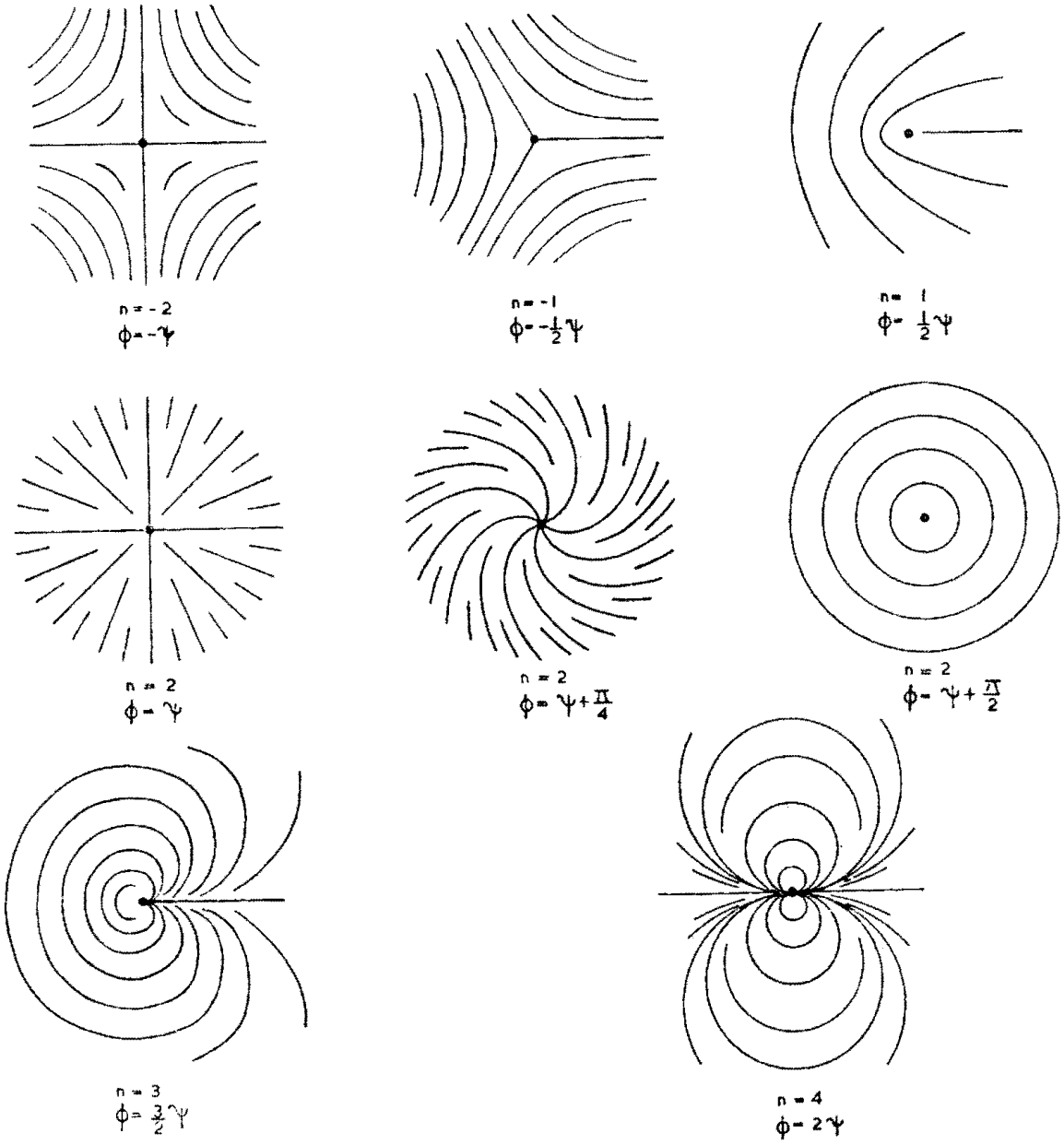


Figure 12: Charles Frank's sketches [83] of the configurations around a defect line in a nematic liquid crystal. The defect line is perpendicular to the page; the joined-up lines are 'lines of force', i.e. everywhere parallel to the director. Each subfigure is labelled by index n, and configuration, in which ϕ is the local director orientation and ψ the azimuthal angle. Frank drew an immediate analogy between what he called 'disinclinations' and the dislocation theories which he had been developing in the decade preceding his famous 1958 paper. .
Note that in the literature the definitions and nomenclature for ψ, ϕ, n are not consistent. Often the index n is replaced by winding number $S = n/2$, and the meanings of ϕ and ψ are reversed.

the defect line.

Frank in 1958 cites and borrows Oseen's solutions, although as far as I can see the pictures are new, and he does not discuss torsion. His paper does stress that his main goal is to rekindle interest in liquid crystals, which has fallen by the wayside since the War. We remark that this is not Frank's first paper on liquid crystals; he already written a short note [87] on the subject when a postdoc in Debye's lab in Berlin in 1938, so he was returning to a favourite topic.

Probably Frank's most imaginative contribution was his analogy with with the dislocations on which he was by now one of the world's leading experts, which led him to introduce the term "disinclination". The existence of a a dislocation on a line involved going round a circuit and finding oneself, as it were, somewhere else. Likewise Frank thought of a disinclination as going round a circuit and finding oneself pointing, at least in some sense, "somewhere else".

With the benefit of hindsight, we can notice what we might think of as 'folk topology'. Frank's classification is through a winding or turning number (the idea goes back to Möbius in 1836), i.e. the number of times the director rotates as a one traverses a circuit around the defect line. It is implicit in Frank's work that all configurations with the same n can be distorted into each other, are therefore in some sense equivalent, and choosing the equilibrium texture will involve detailed elastic energy considerations. More modern homotopic classifications, of course, identify but two possibilities, with the original n even and n odd classes mapping into "not-defect", and "defect" classes. But the naïve classification would be correct if the director were constrained to lie in the plane, and may be of help under some circumstances.

I repeat here (see also [88, 89]) the curious and amusing tale whereby Frank's "disinclinations" became merely "disclinations". I do so because the story actively involves Maurice, and also because in a relaxed moment, he and I discussed it at some length. In 1969, the powerful combination of Jacques Friedel and Pierre-Gilles de Gennes published a paper in the *Comptes Rendus de I'Académie des Sciences* entitled *Boucles de Disclination dans les Cristaux Liquides'* (Disclination loops in liquid crystals) [90]. With a Bristol PhD, Friedel was a close colleague of Frank's, and also had a very successful textbook on dislocations under his belt [10]. No doubt he would have wanted to use the Frank nomenclature, but this article was in French and a translation of the neologism was required. "Disclination" was a piece of imaginative translation *from* English *into* French. However, at roughly the same time an article in the *Journal de Physique* from the Orsay Liquid Crystal Group (in those revolutionary times, in order to avoid the cult of personality, the Orsay group published under a group pseudonym) [91], was referring to *dèsinclinaisons*, which is another possible translation. And in the very next paper in the same journal Maurice himself, in a paper jointly authored with Jacques Friedel [9] on cholesteric liquid crystals, discussed at some length by Pieranski in this volume, were referring to *disinclinaisons*, another possible natural translation.

By the following year, however Maurice [92], seems to have converged upon the standard French terminology of *disinclinaisons*. But the journal now required him to provide an abstract also in English, and his English translation is.... 'disclination'. And disclination it seems to have remained. Maurice and I discussed this important lexicographic question. He thought for a moment, searched back long in his memory, hummed and hawed a bit, stroked his beard several times, and finally pronounced, somewhat uncertainly:

> I think it was de Gennes who said he was disinclined to be disinclined.

Before proceeding further let us note a historical irony. Dislocations, at least as as we now understand them, are primarily microscopic phenomena (see e.g. the pictures in Figs. 9,11). In real solids it is now possible to obtain direct images of the dislocations, using, e.g. atomic force

microscopy. But these pictures are recent; the development of the idea proceeded from theory to experiment, in evidential terms, from the circumstantial to the direct. Experiment *follows* theory.

On the other hand disclinations, indeed all liquid crystalline defects (see e.g. Fig. 3) cry out to the eye immediately. Of their existence there is absolutely no doubt. Experiment *precedes* theory. The problem is the interpretation.

However, despite this contrast, the theoretical interpretive route travels *from* dislocations *to* disclinations. This is probably because solids are ubiquitous, while liquid crystals are (relatively) rare. As such the greater theoretical effort on solids meant that that when the time was ready to prepare detailed theories of liquid crystals, some preparatory work on crystals proper was already in place.

4 Career

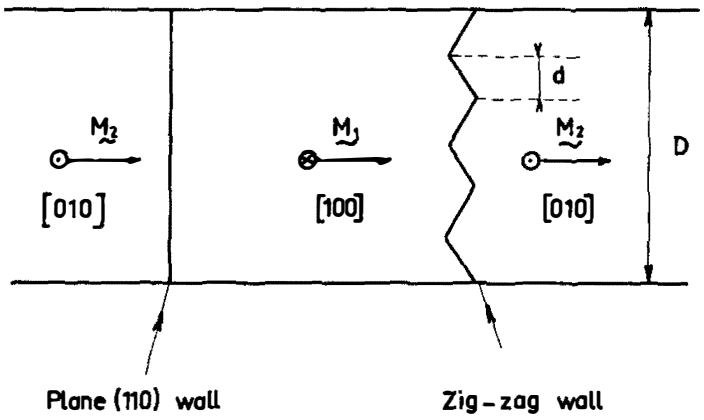
Physicists in our day are usually divided into hard-core theoreticians and confirmed experimentalists. A striking feature of Maurice's career was that he defied this simple demarcation. Aside from his theoretical work, he always kept a laboratory where collaborators were able to pursue experimental studies of one kind or another. In this section I shall, not very scientifically, but with a sly glance at the Citation Index (which, of course, we have learnt not to take *too* seriously), select some highlights from Maurice's career.

4.1 Magnetism

A considerable part of Maurice's early career, following his PhD in the same area, concerned the interaction of defects in magnetic structures and the underlying lattice. There are twelve such papers, between 1966 and 1981. We choose Maurice's collaboration with Schlenker [93] "The use of dislocation theory in magnetoelasticity" as an example. The idea is as follows. Evidently the spins in ferromagnetic solids affect the interatomic interaction, causing dilation or compression in the atomic lattice. This phenomenon is known as *magnetostriction*, and there are magnetistriction constants which link lattice strains to the local magnetisation direction. In the presence of an inhomogeneous magnetisation (e.g. because of a domain wall) these strains will change from place to place. The insight here, originally due to Kröner [94], is that the magnetoelastic strains, together with some fictitious effective stresses, can be replaced for calculational purposes by a density of infinitesimal quasidislocations of the type introduced by Nye [67] to account for curvature in solids. This is represented by a tensor α^0_{ij} which generates the closure failure b_j of a Burgers loop, by the relation $b_j = \oint_C du^0_j = \int \alpha^0_{ij} dS_i$. To this author, at least, the mathematics seems fearsome, and worthy of considerable admiration. The final result, see Fig.13, is that Bloch walls (magnetic domain walls in which the magnetisation within the wall is in the plane of the wall) sometimes distort to a zig-zag form.

4.2 Escape in the third dimension

The classification of possible disclination lines in nematic liquid crystals is clearly an important problem. Consider a configuration winding around a disclination line in the $\pm z$ direction, and which which far from the line lies in the $x-y$ plane. Frank (see Section 3.2) had examined this in 1958 [83], and generated a set of configurations (see Fig.12). The configurations are classified by



Zigzag 90° wall and [110] 90° wall in a plate of thickness D.

Figure 13: Figure from [93], showing the development of a kink structure in a magnetic domain wall as a result of magnetostriction.

their "strength" , or winding number S, where the director far from the disclination line rotates S times over a trajectory which surrounds the line. Given that the polarity of the nematic director is indefinite, S can be half-integer or integer. There is also an extra degree of freedom, in that $\phi = S\psi + \beta$, where ϕ gives the local director orientation, $\psi = \tan^{-1}(y/x)$ is the azimuthal direction, and β parameterises the extra degree of freedom.

Frank's calculation made some simplifying assumptions, however, neither of which might be expected to affect the qualitative status of the solution. One assumption was to suppose that the ("Frank-Oseen") elastic constants K_{11}, K_{22}, K_{33} were equal. The other was to suppose that if the director far from the disclination line was confined to the xy plane, then this condition would apply everywhere. In 1970 the distinguished Russian physicist Igor Dzyaloshinskii (1931-2021) had extended Frank's calculation, now allowing the elastic constants to differ from each other [95]. Frank's solution with arbitrary β is not robust; for $S = 1$ only the solutions with $\beta = 0, \pi/2$ survive.

However it turned out that the other assumption, in which the director remained in-plane, while extremely plausible, was not correct. There *is* indeed an in-plane solution to the relevant equations, with a singularity along the disclination line. But unless the radial boundary conditions are imposed in a cylinder with a very low radius, this solution is a saddle-point and not a free energy minimum. Of course within the Frank-Oseen theory the in-plane condition and consequent logarithmic energy singularity along the defect line requires a cutoff minimum radius in order that energies may sensibly be compared. However a solution discovered by Patricia Cladis (1938-2017) and Maurice [96], which bends out of plane, has no singularity at all, and simply points along the direction of the (vanished) disclination line. The theory was immediately experimentally verified in the same laboratory by Claudine Williams et al [97].

The solution is mathematically interesting in that, being out-of-plane, it does not retain the in-plane *symmetry* of the boundary conditions. It is a broken symmetry solution, with an associated polarity. Necessarily there are two equivalent solutions with opposite polarity, separated by another ("saddle-point") solution to the relevant equations with unbroken symmetry, which, however, is not a free energy minimum.

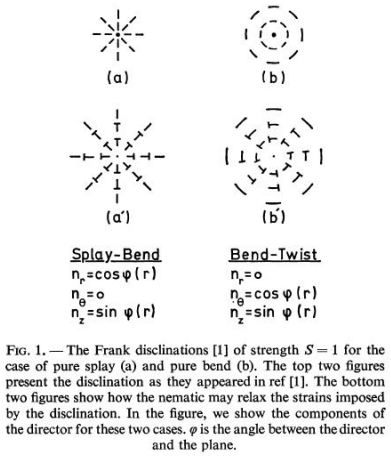


FIG. 1. — The Frank disclinations [1] of strength $S = 1$ for the case of pure splay (a) and pure bend (b). The top two figures present the disclination as they appeared in ref [1]. The bottom two figures show how the nematic may relax the strains imposed by the disclination. In the figure, we show the components of the director for these two cases. φ is the angle between the director and the plane.

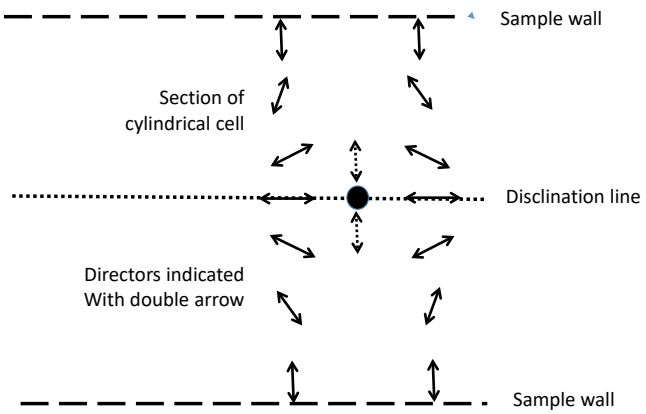


Figure 14: **Left:** The key figure in ref. [96], comparing the non-singular Cladis-Kleman disclination director configuration with that of Frank [83], Fig.12. Note that $n = 2$ in Fig.12 corresponds to $S = 1$ in this figure. [Reproduced from ref. [96] with permission]. **Right:** Director configurations shown in cross-section across the cylindrical sample. The two configurations shown possess opposite polarities; they escape into the third dimension by bending in opposite directions. If they exist in different sections of the same sample, they will be separated spatially by a *point singularity*, shown in the figure by the black dot.

In Fig. 14, we show the key figures in the Cladis-Kleman paper [96]. The paper is exhaustive in its continuum treatment of the liquid crystal director, and also addresses, somewhat inconclusively, how to manage the problem of the disclination core where the continuum treatment fails. Cladis and Kleman remark that they are unable to construct a "coreless" solution for the $S = 1/2$ disclinations, and also remark that they have observed (experimentally) two (coreless) $S = 1/2$ disclination lines combine, which then subsequently decay into a singularity-free regime. The point was emphasised in parallel work by Bob Meyer, then at Harvard University, who noted that in fact *all* integer winding number disclinations could similarly be conjured into disappearance by out-of-plane distortion [98]. It was Meyer who coined the visually evocative term "escape into the third dimension" to describe this phenomenon. .

The failure to find a coreless half-integer disclination could, of course, be ascribed to a lack of imagination. The authors suspect not, as they present a figure in which they depict a "topological scheme to demonstrate the necessity of a core" in this situation. But as pure mathematicians and lawyers are wont to emphasise (and politicians sadly seem blissfully unaware), absence of evidence is not the same thing as evidence of absence. Meyer [98] was less bashful. His paper mentions "topology" on numerous occasions, and speculates that a proof can be found, without finding (or even really seeking) a route toward any rigorous demonstration.

4.3 Topology

The search for a more profound reason underlying the "existence" of the half-integral defects, and the "non-existence" of their integral homologues, led in unexpected directions. Let us first summarise the implications of escape into the third dimension. It is reasonably obvious from Frank's pictures in Fig.12 that nearby $+1/2$ and a $-1/2$ disclinations can cancel each other out, in the sense that (a) far from a pair of such disclination lines, the director configuration tends to uniformity, and (b) the director travelling around such a line does not rotate at all overall. Frank identifies these disclinations lines as being topologically distinct, and classifies them according

to the number of half-integer rotations. But magically, the escape in the third dimension has conjured away the $S = 1$ disclination, and Meyer has shown that this applies to *any* integer S. Intuitively if $1/2 - 1/2 = 0$ and $1/2 + 1/2$ also $= 0$, it must follow that *really*, so to speak, in disclination algebra, if there were to exist such a thing, $1/2 = -1/2$. Maurice teamed up with his Orsay colleague Gérard Toulouse and tried to construct an argument.

The celebrated mathematician René Thom (1923-2002) is known to the world for his Catastrophe Theory. To his fellow pure mathematicians, by contrast, he is distinguished (*inter alia*) for the Dold-Thom Theorem, the Thom Conjecture, the Pontryagin-Thom Construction (etc. etc.). It was a lucky break that Thom worked nearby in Bures-sur-Yvette, sufficiently close to Orsay that their intellectual-geographical clusters overlapped. For it was Thom who explained to Toulouse that what he and Maurice were doing was *algebraic topology*, and specifically *homotopy*, but unfortunately without realising it.

The ensuing realisation brought the mathematicians Louis Michel (1923-99) and Valentin Poénaru into the collaboration. Poénaru, like Thom, was a coincidental collaborator, for he was a neighbour of Claudine Williams, who was a key experimental associate in the paper which confirmed the escape into the third dimension [97]. The consequence was a series of papers on the homotopic classification of defect structures. The first paper, authored by Kléman and Toulouse, submitted on New Year's Day 1976 and published in June, was entitled *Principles of a classification of defects in ordered media* [99]. A second more detailed paper, adding Louis Michel to the author list, maintained its general focus [100]. Later papers dealt with dislocations in solids [101], and then smectic liquid crystals [102]. There followed further papers on the foundations of the theory from other members of the team [103, 104].

The topological defect classification stands on two key foundation stones. The first pillar is the *manifold* V on which the relevant order parameter lives. This is related to Landau's idea of symmetry-breaking at phase transitions, introduced in classic papers in 1938 [105–107] (for a more comprehensive review see the textbook by Toledano and Toledano [108]). This much was already part of the toolbox of all condensed matter theorists in the 1970s.

The second pillar concerns the *connectedness* of the manifold V. Conceptually, this is the same thing as the physical connectedness which Volterra considered when discussing the physical space which his solid inhabited, and whose lack of simple connection gave rise to dislocations. But in this case it is the abstract space in which the order parameter lives which is the focus of interest. We postpone to the next section a longer (but still incomplete) historical discussion. The important point here is that algebraic topologists – pure mathematicians – had ready-made tools to consider just this kind of problem.

Theoretical physicists and engineers learn much mathematics in order to practice their trade. The École Polytechnique, for example, taught generations of students classical geometry – not only Euclid, but also developments of more modern practitioners from the 17th to the 19th century. The liquid crystal pioneer Georges Friedel (X1885) had learnt it at *l'X* because, well, because engineers need to know geometry. It was this geometrical training that enabled him to know that the collision of sets of parallel planes in space would give rise to conic sections and Dupin cyclides. So if he spotted conic sections under the microscope in a material with unknown structure, Georges Friedel could reasonably, although of course not absolutely conclusively, infer that spatially colliding sets of parallel surfaces were the responsible culprits. And hence deduce the likely nature of his smectic phase.

But algebraic topology and homotopy theory were not (at least not necessarily) part of the *X* syllabus. This mathematics was *pure et dure*; no-one could have anticipated how useful it would be. As we shall see, this statement is an exaggeration, but it is at least a decent excuse for Maurice's lack of relevant professional *formation*.

A full treatment of this mathematics is long and complex, with many subtle arguments and fine points. David Mermin's much-cited 1979 tutorial article in Reviews of Modern Physics [109] is 58 pages long, and that article largely avoids the lemmas and proofs so beloved by pure mathematicians (and so anathema to physicists!). Let us try to caricature the argument, concentrating on line defects in nematics, as produced by Toulouse and Kléman in 1976 [99].

For a nematic liquid crystal, the manifold $V = \mathbb{R}P^2$, the 2-sphere (i.e. the surface of a three-dimensional sphere) with antipodal (mathematese for opposite) points identified. Mathematically equivalent chracterisations are either (a) the projective plane (from which comes the $\mathbb{R}P^2$), or (b) the set of lines through the origin. Recall that we identify a disclination by taking a trip around a loop in real space, and finding that along that loop, the director has rotated by some integer number of half turns. At each point the director $\hat{n}(\mathbf{r})$ is a function of position. The mathematical way of saying this is that there is a *mapping* from $\mathbb{R}^3 \to \mathbb{R}P^2$; from any point one can construct loops taking routes through real space.

Now actually both $\mathbb{R}^3$ and $\mathbb{R}P^2$ have a metric structure (i.e. you can move continuously around them from point to point, and you know how far you've gone; strictly speaking only the continuity is necessary), and in general so is the function ("mapping") $\hat{n}(\mathbf{r})$. So one can continuously deform one loop into another, and also change the start/end point. If one does this, one gets an "equivalent" loop, and thus classes of equivalent loops, known as *homotopy classes*. Then one can combine loops, and do them backwards, and keep the loop as it was. At the end of the day, the classes of loops can be thought of as elements of a group, with the unit element consisting of "staying where you are". The group element "staying where you are" consists of all loops which can be continuously shrunk to a point.

And it turns out this group has already been given a name by the topologists – actually two names, either the *fundamental group* or the *first homotopic group* – as well as a notation by which it is recognised: $\pi_1(V)$. And in addition, for a given V, there are standard ways of calculating $\pi_1(V)$. Using this technology, one can read off the number of "homotopically distinct" defect lines for particular types of order parameter. For statistical mechanicians, some other frequently occurring systems are the XY magnet (with two independent degrees of freedom orientationally uncoupled to the underlying lattice) ($V = \mathbb{S}^1$, a circle), and the Heisenberg ferromagnet, with three degrees of freedom ($V = \mathbb{S}^2$, the surface of a sphere).

Then reading off the fundamental groups, one finds that:

$$\pi_1(\mathbb{S}^1) = \mathbb{Z}; \qquad \pi_1(\mathbb{S}^2) = 0; \qquad \pi_1(\mathbb{R}P^2) = \mathbb{Z}_2.$$

This is not quite the whole of the paper, but is the key easily explainable result. Let us briefly discuss it. The homotopic classification links different defect line configurations which can be deformed into each other. On the other hand, it makes no statement about which configuration within a class is the most stable. The interpretation is as follows:

(a) For XY spins with two components, the manifold of possible spin orientations is the circle $\mathbb{S}^1$. The class of defect lines then corresponds to the integers $\mathbb{Z}$. Each integer is the *winding number*: i.e. the number of times the spin direction rotates along a closed loop surrounding the offending line. For each class, the winding number is a *conserved variable*.

(b) For Heisenberg spins with three components, the manifold of possible spin orientations is the surface of the sphere $\mathbb{S}^2$. Now the class of defect lines is *trivial*; this is the meaning of $\pi_1(\mathbb{S}^2) = 0$. In ordinary language, there are *no* homotopically conserved defect lines.

One might think that one had identified a defect line. But one would in some sense be mistaken; it can be conjured out of existence by suitably deforming the configurations locally.

Of course, the Cladis-Kleman-Meyer argument about escape into the third dimension applies in this case also, and it is just this escape which permits the apparent defect to disappear into thin air.

(c) As we have seen above, the manifold $\mathbb{RP}^2$ corresponds to the case of nematic liquid crystals. The defects are described by elements of the group $\mathbb{Z}_2$, the so-called cyclic group of order 2. There are two elements;$\{0, 1\}$. The unit element is 0, the other element is 1, with a combination rule that says that $1 + 1 = 0$, or equivalently $1 = -1$. Just, in fact, as we expected from the continuum mechanics argument employed by Cladis and Maurice a few years earlier and discussed above in section 4.2.

The "error" Charles Frank [83] had made in 1958 was to classify the nematic defect lines supposing that the director remained "in-plane", i.e. that the order parameter manifold had been S_1, rather than P_2. This leads to an incorrect fundamental group and hence a different homotopic classification. The 1958 paper mention "topology" on three occasions, but the use of the term is informal. There is no appeal to topology as a mathematical discipline. Frank is closing his eyes and using his "common sense". Frank's common sense, of course, was uncommonly reliable – almost to the extent of genius – in general; he had a spectacular three-dimensional intuition. It half-fails him here; half, because one could use a Volterra process to define the defect line, rather than the homotopic classification.

As we have seen, Maurice and Patricia Cladis had discovered a new continuuum configuration (which as it happens, breaks the planar symmetry) and hence are able to infer the correct classification. But the deep reason comes from the homotopy, and not, say, from the broken planar symmetry. It is possible that a $S = 1/2$ defect would also break the planar symmetry. But even if this is possible, unlike the $S = 1$ line, the $S = 1/2$ defect cannot be wished away by reconfiguration, exactly as Maurice and Pat had suspected in 1972. The corollary is that there must be a smooth distortion of Frank's $+1/2$ disclination into the opposite $-1/2$ disclination. And so there is, although it takes rather more geometrical intuition to "see" it than to see the escape into the third dimension.

There is a final important social point. Solving the continuum mechanics was a feasible task for a theoretical physicist. But the task of determining the fundamental group has been subcontracted to mathematicians, and for the everyday physicist the formal reasoning (as opposed to the intuition) becomes a task for the specialist.

To finish this section on Maurice's work, we remark that the idea that homotopy and topological methods drawn from pure mathematics can be used to describe defects in ordered phases emerged in several places – Moscow, Paris, Warsaw, London (at least) – at more or less the same time. We here continue with our discussion of Maurice's scientific contributions. We postpone to Section 5 mention of other parallel contributors to the "Homotopic Revolution", as well as some discussion of its consequences and antecedents.

4.4 Focal Conics

Maurice devoted almost a score of papers to the study of focal conics, beginning with a collaboration with Claudine Williams in 1976, and ending almost in the last year of his life. Among collaborators were Claire Meyer, Iannis Lelidis, Christophe Blanc and perhaps most importantly Oleg Lavrentovich, who has developed this theme in more detail in another article in this issue.

Some historical background: As we have seen in Fig. 3, what we now recognise as focal conics were recorded by Lehmann in his 1904 book [17] what he called *fliessende Kristalle* (flowing

crystals). But he does not draw attention to them, and is distracted by what he takes to be a veritable menagerie of different types of crystallite. A propaganda visit by Lehmann to Paris in 1909 excited interest in "Lehmann's liquid crystals" amongst several leading French mineralogists (see e.g. Ref. [16,18,110] for more details), including Frédéric Wallérant (1858-1936) and Charles Mauguin (1878-1958) at the Sorbonne, as well as by Georges Friedel and his assistant Jean Grandjean (1882-1975) at the St. Étienne School of Mines.

The "focal conic liquids," as Friedel and Grandjean called them to begin with, first saw the light of day in 1910. A short article in the proceedings of the Academy of Sciences [111], was followed up by a long and exhaustive study in the Bulletin of the French Mineralogical Society [112]. We might add that there were several other liquid crystal papers with a different focus that year from the same stable. Indeed, it seems surprising that they found time to sleep, so productive were they in this period. Some of the most pictorial results from this article are shown in Fig. 15. The tag "focal" indicates that the authors realised that what they were seeing was an optical phenomenon. But the inference of a non-uniform layered system, with the lines associated with layering singularities, had to wait until Friedel's famous 1922 review [19].

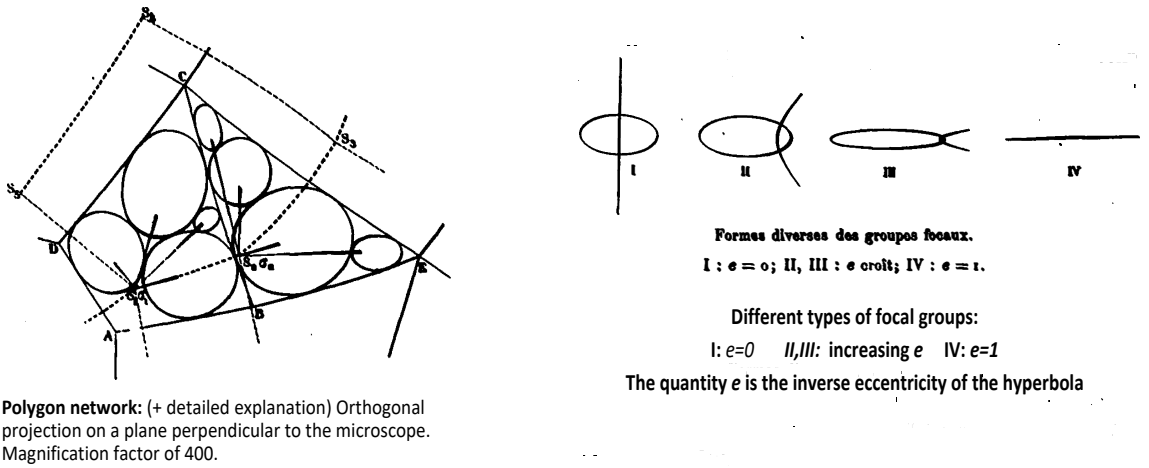


Figure 15: Figures from Friedel and Grandjean [112]. **A, Left:** The space is apparently filled with ellipses of different sizes. **B, Right:** Each ellipse is associated with an interlinking conjugate hyperbola in a perpendicular plane. Each of these cuts the plane of the other at one of its foci. Note that the present author has edited the legend, so that it partly appears in English.

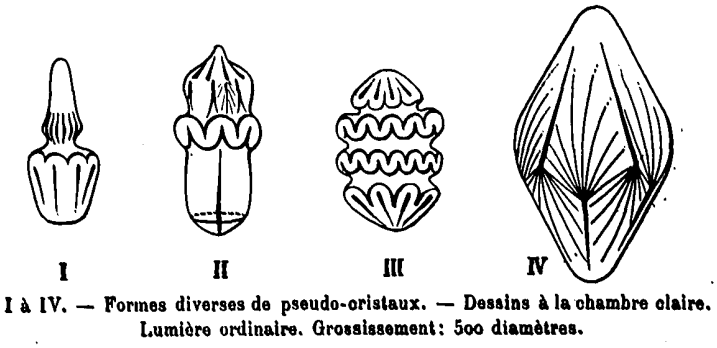


Figure 16: Another figure from Friedel and Grandjean [112], apparently demonstrating the existence of crystallites. Friedel labelled them as "pseudocrystals".

Digression: We take a brief diversion at this point, because this Friedel-Grandjean paper [112] was part of a not-so-friendly exchange between them and Lehmann about the fundamental nature of liquid crystals. Just after it [113] we find them replying to a previous paper by Lehmann as follows [my, rather free, translation]:

> Monsieur Lehmann wrongly believes that we are defining crystals as bodies which are both birefringent and uniform. ... We define *crystalline matter* as that which possesses discontinuous vector properties governed by Ha uy's Law. A *crystal* is a uniform piece of crystalline matter. ... If one doesn't use (this definition) of crystalline matter, then there is no satisfactory definition of the crystalline state.
>
> Monsieur Lehmann thinks that we regard the concept of a liquid crystal as absurd. We don't look at it that way. The idea of liquid crystals seems to us perfectly admissible. However ..... it is simply that this theoretical concept does not apply to these liquids ...

By 1922, however, Friedel was taking a much harder line. "Liquid crystals", as reasonably understood, had indeed become ridiculous. Professionally, the *experimenta cruces* of von Laue and the Braggs had pinpointed the microscopic difference between liquid and crystals. And personally, Lehmann was dead, so the pretence of politeness was no longer necessary. Now, having understood why the focal conics were occurring, he abandoned "focal conic liquids" for the "smectic phase".

Back to Georges Friedel: There were other unexplained signatures of the *fliessende Kristalle*. The exotic shapes in Fig. 16 are reminiscent of architectural spandrels, or alternatively like three-dimensional snowflakes. In the absence of alternative evidence, observers are easily persuaded that what they see in their microscope objective are elaborate crystals. And then there are the mysterious "oily streaks" already found by Lehmann in these materials. Neither are immediately explicable in terms of a layered material. However, once one is committed to the idea of a layered structure, it is possible to construct arguments that these other textures are explicable in terms of the "layer paradigm" According to Friedel, the pseudocrystals are elaborate complex multi-focal-conic edifices, while (in modern language) the oily streaks are disclination pairs or combinations thereof.

Some general physical speculations: So much for the *development* of the paradigm in the first place. We can also think about the problem starting with modern understanding. Crystalline solids, as we have seen in Section 3, would normally be divided into (three dimensional) *domains* in each of which the atomic structure is regular, but between which the regular pattern breaks. They are normally separated from each other by (two-dimensional) *grain boundaries*. There are also (one dimensional) dislocations, and occasionally coherent dislocation patterns can so-to-speak condense into a grain boundary.

Nematic liquid crystals possess no positional order whatsoever, but can reorient as one moves from place to place. The sharp grain boundaries which occur in solids do not occur, as the director changes can spread themselves over whatever space is available. But disclinations (defect lines) and hedgehogs (defect points) can occur, are localised, and do stabilise spatial director reorientation.

Smectic (A) liquid crystals are in between. They possess one-dimensional crystalline order (another way of saying that they are layered), and of course, retain the nematic orientational order. It would be a sensible question to ask, do the domains and domain boundaries behave more like the nematic, or more like the crystal? What do grain boundaries look like in smectic liquid crystals? And the answer is, well, as one might expect, a bit of one and a bit of the other. (Two-

dimensional) grain boundaries between smectic regions with layer normals in different directions can exist, for example when strong boundary conditions legislate this.

However, more usually, the freedom of smectic layer normals to wander in space is limited, because they are constrained by the obligation to remain (more or less) an equal distance apart. But nevertheless what freedom remains is sufficient almost to eliminate the grain boundaries. What remains are the (line, rather than plane) disclinations from the nematics, the focal conics (which are new), and dislocations of various types which can be carried over from solids. A problem is that space is often not filled by the domains generated by the focal conics. This is what is happening in Fig. 15 with the large and small ellipses. The extra space is filled by smaller (and then even smaller, and smaller than that, and so on ...) focal conic domains, with grain boundaries separating the different regions.

Forward to the 1970s: After Georges Friedel's 1922 paper, these problems were largely neglected until the liquid crystal renaissance in the 1970s. Two early papers from the Orsay group are worthy of mention. One was by the distinguished biological physicist (or, originally, physical zoologist) Yves Bouligand (1935-2011) (of whom more later) [114]. This paper harks back to Friedel and Grandjean, noting that not all problems were solved by them: not all conics are quite focal, requiring, inter alia, a density of screw dislocations in the smectic bulk. The other is is by Pierre-Gilles de Gennes and collaborators [115], and is concerned precisely with a statistical treatment of the nesting of ellipses shown in Fig.15.

Work by Maurice and collaborators: Here we give a brief taster, and for a more comprehensive overview refer the reader to the beautifully illustrated review by Maurice and Oleg Lavrentovich in 2009 [116]. An important early paper [117] concerned the energy of a focal conic domain. Using curvilinear coordinates, Maurice finds it to be logarithmic in the semi-major axis of the ellipse. The mathematics is impressive, although once standard and now rather out-of-fashion – presumably replaced in the present day by some engineering finite element code. The main reference was a textbook by the distinguished differential geometer Gaston Darboux (1843-1917), published in 1888 and by chance, just not available in English. This was the main tool for a number of other studies. An important theoretical conclusion is that most focal conic domains are *toric* (TFCD), i.e.the smectic layers forming the TFCD are curved in the shape of half-tori. The half-tori layers possess one positive and one negative curvature, such that their sum is almost zero. This is not a geometric but rather an energetic property; one of the terms in the local free energy depends on the mean curvature (that is, the sum of the two inverse radii of curvature).

Another important point made by Maurice and collaborators [116] is that indeed the "liquids with conics" possess them because of their layered structure. However, the conics are the result of fitting layers of equal separation, and implicitly suppose long-scale properties. On shorter scales, we can allow layer mismatch (i.e. dislocations), or layer dilation/expansion. And of course, the dislocations and focal conics interact. So other textures which appear reflect this complex reality. Among the most obvious are the *oily streaks* first identified by Lehmann. In in L_α lyotropic liquid crystals, joint work with Lavrentovich and Boltenhagen [118] showed that these were clusters of parallel edge dislocations with giant Burgers vectors. In collaboration with Jim Sethna of Cornell University, Maurice showed that objects like Friedel's *bâtonnets* can be built up from densely packed clusters of focal conic domains [119]. But the focal conic domains do not fill ("tile") space, and the gaps that remain between the domains are filled with layers of spherical curvature. Lavrentovich [120] showed that an important factor in focal conic formation is the anisotropy of the surface tension. Later Fournier and Durand [121] showed also that this factor both stabilises the bâtonnets into rod-like shapes and leads to faceting of the focal conic domains. This is the underlying cause of the beautiful structures which Friedel first found long

ago, which are shown in Fig.16.

One focus was to deepen understanding of focal conic domains in general Smectic A systems. Another was to investigate the interaction between the focal conic defects and smectic dislocations, disclinations and grain boundaries. In nematic terms, the focal conic structure consists of an $S = 1$ defect associated with the hyperbola, and an $S = 1/2$ defect associated with the ellipse. Above the smectic-nematic transition the former is unstable, and the latter reduces to a wedge disclination loop. Furthermore, as noted above, Bouligand had suggested that screw dislocations could cluster around the hyperbolic axis of a focal conic domain. A early article by Claudine Williams and Maurice had confirmed this [122]. Much later papers with Nastishin and Claire Meyer [123, 124] addressed *pre-transitional phenomena* close to the nematic-smecticA transition. One observation was that now the focal conic lines were bent (sometimes out of plane, sometimes in plane) or broken. There is confirmation of work by others that in the immediate neighbourhood of the transition to the nematic phase, dislocations multiply, and the focal conics decrease in size, eventually disappearing, with the smallest disappearing first. These experiments also make contact with influential theoretical work [125]

Other pieces of work included a paper on the smectic C phase (which necessarily has a more complex defect structure) (e.g. [126]), as well as a discussion of grain boundaries between smectic domains tilted relative to each other. These are sometimes known as *chevron walls* (see e.g the review [127]), in which the director may curve if the angle between the normals is low. Alternatively, the wall may split into a dislocation lattice, if the angle between the layers is low.

We have only mentioned a selection of Maurice's work on focal conics here. The extent of these studies was very wide, involving both experiment and theory, and also collaboration with a large number of colleagues from across the world. A particularly interesting feature is that during the late 1970s, a particularly fruitful period, he was producing more or less contemporaneously, influential studies of focal conics, topology, quasicrystals and magnetism.

4.5 Dynamic Phenomena and Rheology

4.5.1 Background

In a set of papers between the late 1970s and the early 2000s, Maurice and collaborators carried out experiments on, and provided theories for, dynamic phenomena in smectic and structurally similar lyotropic layered (= lamellar) phases . We recall here important work in two areas. A first concerns the manner in which the spatial velocity gradients respond to an applied force, and the relationship between this response and that of solids; this work may properly be called rheology [128–135]. The second involves phase and textural changes induced by shear fields [136–140]. In fact, chronologically speaking, the second of these precedes the first, but we prefer this order from a conceptual viewpoint. But in all this work, a central theme is the role played by defects of one sort or another in the phenomenon in question.

To put Maurice's work in its proper rheological context we have first to go back to the 19^{th} century. The first articulation of continuum theories of both liquids and solids can be traced back to Claude-Louis Navier (1785-1836) [141] in the early 1820s; see the book on the history of hydrodynamics by by Olivier Darrigol [142] for a more detailed discussion. Most contemporary description of Navier's papers rewrites his cumbersome longhand equations in the much more compact tensor form. The key advance over the earlier theories of Euler and D'Alembert was his introduction of *viscosity*, although on the one occasion when the term is used here by Navier, it does not have its modern meaning. To modern eyes the development looks rather forced and makes use of a microscopic particle model which at that time would have been grossly

speculative. The stress is proportional to the velocity gradient, and their ratio is (or rather will be) the viscosity.

Essentially the same equations were also derived by the Irish (but removed to Cambridge!) mathematician George Gabriel Stokes (1819-1903) [143]. Stokes's development looks more recognisable to us, although he uses the term "friction" for what soon came to be called the viscosity. Experiments showed that it applied for "low" velocities. In this regime, the viscosity equation is $\sigma = \eta\dot{\gamma}$, where σ is a force (per unit area) or stress, η is the viscosity and $\dot{\gamma}$ is the rate of strain field (i.e. the gradient of the velocity).

Stigler's law of eponymy (that if a law is named after someone, then that someone was *not* the discoverer) only partly applies here. For somehow in the telling, the tale of the Navier-Stokes equation has lost (at least!) Cauchy, Poisson and St-Venant (see [142] chapter 3). For the record, there was no Dr Navier-Stokes. By contrast, when it comes to the Levi-Civita symbol ϵ_{ijk} you can search in vain for Levi and Civita separately in the annals of applied mathematics. The famed antisymmetric object was due to but a single double-barrelled tensor pioneer. There was no team, no disputed discoverer, just Tullio (1873-1941) himself.

More complex materials are *non-Newtonian* (why, will divert us too far!), and the stress is no longer proportional to the strain. Often in this case in practice the force is the independent variable, and the relation is written $\dot{\gamma} = C\sigma^m$. We can define an effective viscosity by $\eta = \partial\dot{\gamma}/\partial\sigma$. In this case, there will also be a power law relation: $\eta \sim \dot{\gamma}^{-\alpha}$. Then by comparing powers, $\alpha = 1 - 1/m \Rightarrow (m > 1 \Leftrightarrow \alpha > 0)$. Note that this dependence is the same as the so-called Orowan Law in solid creep discussed in Section 3.1.4.

Now (by definition) if $\alpha > 0$ – and hence if $m > 1$ – the material is *shear-thinning*. Informally, it becomes easier to move the harder one pushes. The general subject – that of flow in complex fluids – is known as *rheology*, a term coined in 1920 by the American physical chemist E.C. Bingham (1878-1945) from the Ancient Greek word for flowing.

Liquid crystals have long presented rheological puzzles. Reinitzer's and Lehmann's first observations in the late 1880s were optical: a cloudy fluid phase interposed itself as one heated a crystal, before a clear liquid appeared. But Lehmann soon subdivided his new materials into *flüssigen Kristalle* (fluid/liquid crystals) and *fließenden Kristalle* (flowing crystals) [16]. The flowing crystals were more viscous, and produced the textures that were later identified with the smectic phase. Indeed Lehmann was tempted to distinguish the two types of liquid crystal using the viscosity as the more important criterion, "foolishness" for which he was later chided in insulting terms by Georges Friedel [19], who saw (correctly) phase change as the key driver. Lehmann, who in earlier exchanges with Friedel and Grandjean had been just as intemperate, would no doubt have given as good as he received, had he not been called to meet his Maker a few months earlier.

In the early years of the 20^{th} century it was no surprise to Otto Schenck [144] that the liquid viscosity underwent a discontinuity at the transition from ordinary liquid to liquid crystal. But by contrast it was a big surprise – indeed it seemed rather counter-intuitive – that in several materials, the liquid crystal just below the transition flowed more easily (i.e. the viscosity jump was positive on increasing temperature). In the 1930s Mięsowicz [145, 146] reported inconsistent viscosity results from other workers as compared to his own experiments (see also the essay by Carlsson and Leslie [147] on the development of theories for flow in nematic liquid crystals). Mięsowicz was able to produce consistent linear rheological behaviour in the presence a strong fields which compel the liquid crystal director to remain constant. This enabled him to identify three different viscosities, depending on the director orientation relative to a shear direction. The final "'settled" nematodynamic theory of Ericksen and Leslie (see e.g. [148]) – with five independent viscosities – grew out of work by Oseen [84, 85]. Apparently non-Newtonian behaviour was

the consequence of interaction between director and velocity gradients. But even this theory fails to include disclination lines endogenously; when boundary conditions and flow field are antagonistic, the consequence is "low Reynolds number" turbulence, as defects continuously peel off boundaries.

So even the rheology of nematic liquid crystals is hard. Smectic liquid crystals present further difficulties because they are layered, and hence something between a liquid and a crystal. De Gennes [20, 149] developed a Newtonian theory for smectic A, which works if the smectic layers maintain their integrity in a single domain with flat surfaces. But as the presence of exotic textures tells us that they often do not, the question of the actual rheological behaviour remained open.

4.5.2 Non-linear response

This series of papers [128–135] examined the non-linear stress-strain relation in smectics and lyotropic lamellar systems which might be expected to be structurally similar. The lyotropic system used in these studies is a mixture of soap, brine and two complex hydrocarbons. This 4-component system (other workers, see e.g. [150], showed that the precise makeup was not so important) has a very complicated phase diagram. By varying the hydrocarbon concentrations one can produce a set of phases, only some of which are the layered phases under consideration here.

The experiments yielded results with $\dot{\gamma} \propto \sigma^m$, with two different values of m, one with $m \approx 1.7$, and the other with $m \approx 4.8$. Which value obtained depended on the observed texture. The $m = 1.7$ case corresponded to "oily streak" textures, aligned in the direction of flow. These occurred in both the smectic and the lyotropic lamellar systems under shear; in the lyotropic case, this sometimes occurred only at low shear. The $m = 4.8$ case was associated with the other observed texture. This was the so-called "onion state", which occurred only in the lyotropic system, and then only in a limited concentration regime. More formally the onion state consists of a close-packed, space-filling, aggregate of spherical supramolecular particles. Each particle consists of a set of spherical layers, packed in a ball, like the layers in an onion. The technical term is *multilamellar vesicles* (MLV's); vesicles because they are closed droplets surrounded by an interface, and multilamellar because there are many layers.

These power law results for the dynamic response to a shear stress are reminiscent of the Harper-Dorn creep mode in metals and other materials, discussed in Section 3.1.4, although the exact power laws themselves differ. However, the resemblance is sufficiently strong that in both cases, a dislocation mechanism seemed a sensible place to start. The Orowan Law in smectics connecting dislocation velocity to stress rate was demonstrated experimentally by Lelidis, Kleman and Martin [130]. The $m \approx 1.7$ case could be explained in terms of dislocation climb within smectic layers; a calculation [129] yielded $m = 5/3 \approx 1.67$, which is rather close to 1.7. The $m = 5$ case was a rather tougher nut to crack. Ref. [129] speculates that here the key dislocations no longer lie within the smectic phase itself, but rather within the superlattice created by the MLV's. The presence of screw dislocations in the stressed smectic-A phase, and the yield stress as a function of screw dislocation density was observed by Lelidis, Blanc and Kleman [135].

4.5.3 Instabilities and Phase Shifts

It is well-known, of course, that in statistical mechanics, external fields can induce instabilities, and even move phase boundaries. In the smectic A phase, a dilatation, tending to increase the layer thickness provides an example of this general phenomenon. In a completely free system,

eventually the material would permeate through the layers in such a way that the "correct" layer thickness would be reestablished. But systems are not completely free. Is there some way for the system to maintain the equilibrium layer width without such a radical layer rearrangement? One answer is very easy to see: an undulation instability, with the end-result a zig-zag layer arrangement, as shown in Fig. 17. The layer tilt compensates for the "wrong" layer width. However, the price to be paid is the existence of so-called "herring-bone" grain boundaries which cost free energy.

In the figure we see a cross-section, so the grain boundaries form planes. To summarise: a free energy problem is caused by the incorrect layer width. It is resolved by the zig-zag layer tilt, which gives rise to two-dimensional defects: grain boundaries at the herring bone tips. However, the existence of focal conics in smectics bears witness to an alternative resolution. The focal conics curve the smectic layers in such a way as to reduce the dimension of the layer mismatches: the defects become lines rather than planes. Here too this option is available. Now the instability may be to *focal parabolae*, as shown in Fig. 18, as observed first by Rosenblatt et al [151].

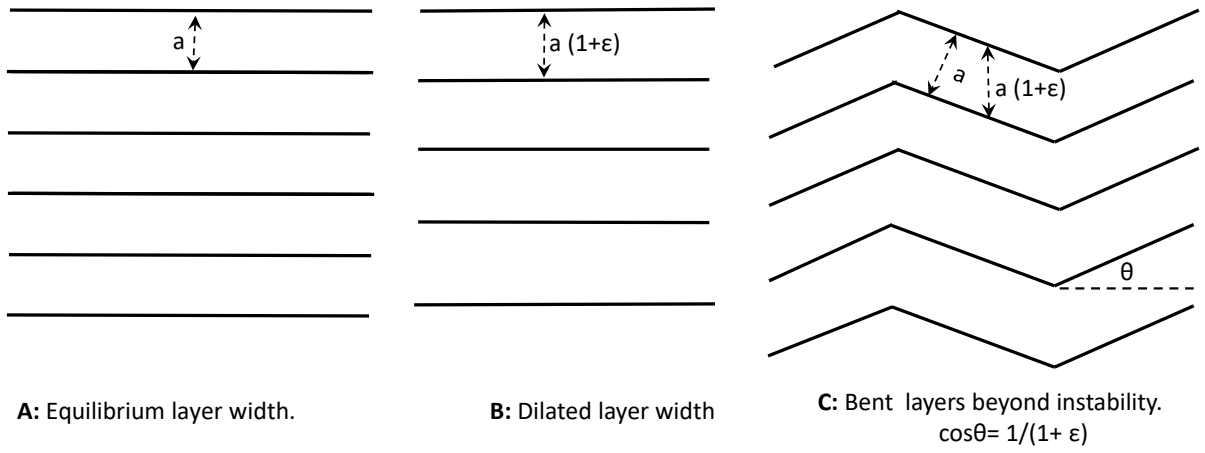


Figure 17: Possible dilatation instability smectic A liquid crystal layers.

In [136], Horn and Kleman (see also a follow-up article with Oswald and Béhar [137]) considered a small molecule smectic A liquid crystal in a shear flow field. This turns out to have a similar effect to that of dilatation. A two-dimensional superlattice is formed, with one principal lattice direction parallel to the flow, and one perpendicular to it. Some results are shown in Fig.18.

A number of theorists later addressed this problem [152–155], and were able to explain these experimental results, at least qualitatively. The question of whether there is a simple intuitive picture of the shear-induced layer instability, matching that of the layer dilatation, is more elusive. To this author, there seems at least a *prima facie* analogy with the surface instability enabling winds to generate storms on the sea. A recent review is given by Fujii et al. [156].

The next example is an interesting problem in which Maurice and colleagues became involved in a debate as to the correctness of their experiments. Lyotropic amphiphilic solutions consisting of a surfactant and co-surfactant can exhibit a rich phase diagram as the relative concentration of surfactant to co-surfactant changes (see e.g. [157–159]). Two neighbouring phases which frequently occur are the so-called L_α and L_3 phases.

L_α is an orientionally ordered layered phase, structurally analogous to the smectic A. However in the L_α phase, the orientational order can be considered to be derivative. The primary effect is an attempt by the system to phase separate between oil-like and water-like regions, which is frustrated by molecular geometry, and which is resolved by dividing the unlike regions into

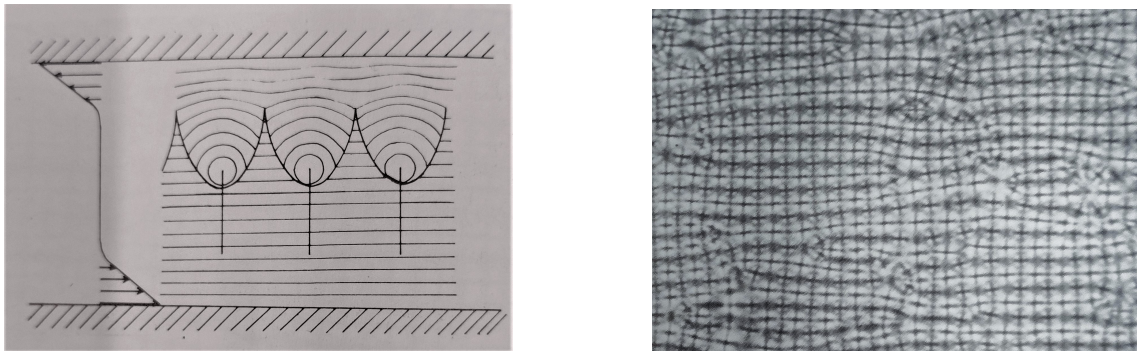


Figure 18: Results of shear flow experiments [136, 137] in the smectic A 8CB. Sample thickness: 350μ.
Left: Cross-section of sample showing fully developed focal parabolae. Flow field shown schematically on left of picture.
Right: Enhanced photograph of two-dimensional focal parabola superlattice seen through crossed polars. Note that the lattice contains several dislocations.
Figures from Oswald et al. [137], with permission ©1982 Taylor and Francis.

layers. The L_3 phase, by contrast, is a *bicontinuous* phase, often known as the *Sponge phase*. Once again the driver is a failed attempt to phase separate. In this phase space is divided, not into layers, but rather disjointly into two interpenetrating continuous regions, one water-like and the other oil-like. A caricature of the two different configurations is shown in Fig. 19.

The question was, how does a shear flow affect the phase boundary between the two phases as external conditions vary? Does the L_3 advance with respect to the L_α phase or *vice versa*? We remark that theoretically this problem is a challenge. In the presence of an extensional stress, for example, it is possible to write the force as a gradient of a potential. The consequence is that the conditions for phase equilibrium can be written in terms of the equality of thermodynamic potentials. All that one requires to figure out the phase transition shift are good approximations for the free energy of the two phases in question. If the relevant thermodynamic potentials are equal, then it is *guaranteed* that at the phase transition there will be equilibrium solutions side-by-side, with a stationary interface between them.

Not so if the forces cannot be written in terms of a potential. Then the sole condition for phase equilibrium is the existence of the stationary interface between the side-by-side phases. In principle, the bulk conditions for a stationary interface could depend on the orientational relationship between the stationary surface and the external stress. Unlike in the stationary potential case, there are in general no bulk criteria. On the other hand, one can seek a flow-induced *instability*. This does not comment directly on equilibrium, and is analogous to finding a spinodal point in equilibrium thermodynamics.

The experimental question had come to the fore, because there had been conflicting predictions concerning the effect of a shear flow on the $L_3 - L_\alpha$ transition. Calculations by Cates and Milner [162] had found that the L_α phase would be stabilised relative to the L_3 phase. Not so, said Bruinsma and Rabin [163], exactly the reverse! Experimental Studies by Maurice and collaborators starting in 1996 [138–140] suggested that the shear flow induced birefringence, i.e. that the L_α phase was preferentially stabilised. However, the Cates-Milner prediction for the critical shear rate was quantitatively an order of magnitude too great.

We do not go into the subsequent rather copious literature here, nor hold a rigorous view about the rightness (or universality) of either case (but see the review by Butler [164]). However,

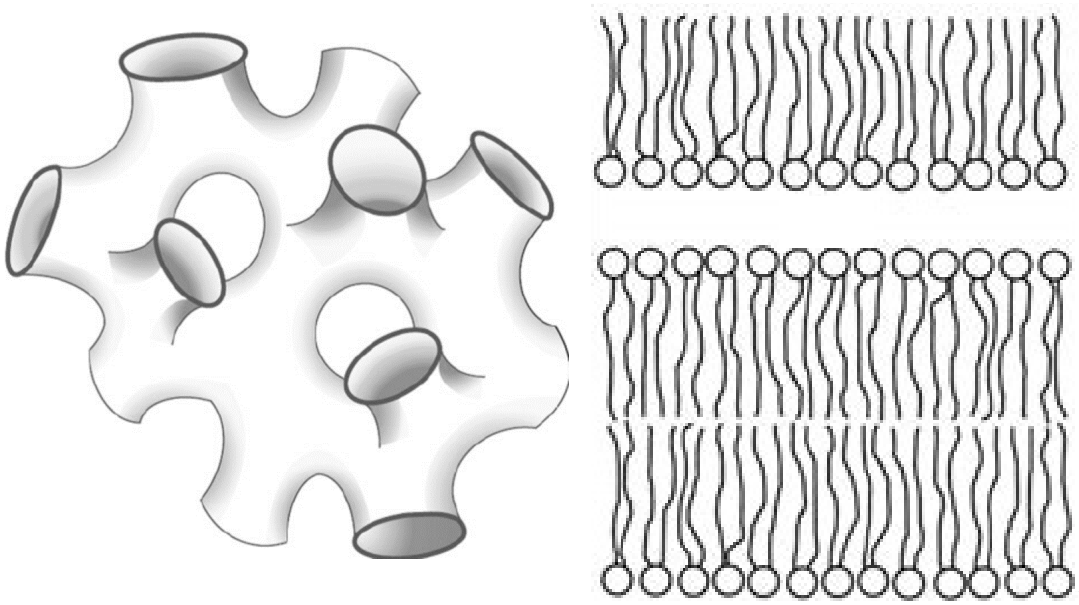


Figure 19: Schematic representation of **A**: the isotropic L_3 sponge phase, and **B:** orientationally ordered L_α bilayer lamellar phase. Sponge phase after Iñiguez-Palomares et al [160]. Lamellar phase after Garvey et al [161]. In each case, space is divided into two disjoint regions, with amphiphilic molecules straddling the interface between them. Defects in the lamellar phase can consist of holes in the layers, linking up adjacent layers. In some sense the sponge phase occurs when the lamellar bilayer defects condense, so that all layers become topologically linked.

we do remark on a comment submitted in 2002 to Physical Review Letters by Butler and colleagues [165], reporting on attempts to repeat the Kleman group experiments [139]. They were, unfortunately, (only) partially successful. Partially, in that their experiments gave irreproducible results for the critical shear rate. Indeed, a better predictor for the $L_3 - L_\alpha$ transition was the time spent in the cell. They observed a biphasic region (not unexpectedly, in the view of this author; the boundary condition at an interface is equality of shear stress, rather than of $\dot{\gamma}$.). The Butler group view was that the Kleman group transition to a biphasic region was driven primarily not by shear, but rather by cosurfactant (hexanol) evaporation, for which (they alleged) the Kleman group had not controlled. The Butler group experiments were, they averred, quantitatively consistent with the Cates-Milner picture.

Not so quick, replied immediately Maurice and friends! Butler et.al. have not specified the point in the phase diagram at which they did the experiments. They should be *embarrassed*! Our transition only occurs when the brine concentration is sufficiently high (and by implication, hlButler and his coworkers had done their experiments in the wrong concentration regime). Anyway, say Maurice and colleagues, we have done some new experiments which are consistent with our previous results. Even if there is some hexanol evaporation, it will have negligible effect on our results. But we do admit that careful temperature control is more important than we had realised, and this may account for some of the discrepancy with other authors' work. The Cates-Milner prediction is still incorrect by an order of magnitude, although in the right direction....

Regardless of who is correct, this is the scientific method at its best. Not all variables can be controlled. A change in experimental procedure (I have not discussed this in detail here) can reveal features of the problem which had previously not been understood. Changes which had been thought to be irrelevant may turn out to be relevant. And so on. We observe here Maurice as philosopher-scientist, arguing his case vigorously, but at the same time (for *we are scientists*!), always bearing in mind the possibility of having to abandon one's position in the face of new evidence.

4.6 Aperiodic solids

4.6.1 Amorphous solids

In a 1989 review article [166], summarising some of his work in this area, Maurice draws the reader's attention to the principal geometric reason for disorder in solids:

> Pentagonal symmetry is forbidden in usual crystals, which are invariant under a set of three-dimensional translational symmetries. It has been well known for a long time that supercooled liquids (Frank 1952) [167], liquids of simple spheres (Bernal 1964) [168] and probably amorphous metals (Sadoc et al. 1973) [169] are disordered because of a local tendency towards pentagonal symmetry (in the form of icosahedral dusters of atoms. [This] local symmetry, as stated by Frank as early as 1952, satisfies compacity requirements (the local density is higher than in a f.c.c, arrangement of hard spheres). [It also] provides for vibrational entropy since the 12 spheres which surround the central atom must not be in contact. [Thus it could possess] a smaller internal energy than f.c.c, or h.c.p. arrangements.
> (*Citation numbers in square brackets are references in this paper*)

In other words, the local geometry (or physics) is attempting to impose an order which cannot be repeated uniformly throughout space, leading to *frustration*. Here this frustration is manifested by a lack of consistency between global order (something, anything, that could be continued, e.g.

cubes) and local order (in the case discussed here by Maurice, it is icosahedral). The prototypical example of frustration involves spin glasses [170], in which the bonds linking neighbouring spins cannot all be satisfied at the same time, while other examples give rise to exotic magnetic structures, or complex non-uniform phases in liquid crystals.

The result is a compromise. In this compromise one of two situations obtains. Sometimes there is an *amorphous* solid, and sometimes a *quasi-crystal*, which we shall discuss further below. In amorphous solids the atoms (or more generally elementary units) in this class exhibit no positional order whatsoever. The neutron or X-ray scattering signature exhibits peaks and troughs, but no spots, and for many purposes the static positional correlations are indistinguishable from those of a liquid. The structure is frozen and the particles vibrate around their equilibrium positions, whereas in liquids they are mobile. Such solids are generally *glasses*, the understanding of the properties of which have challenged theorists for more than a century, principally because they seem not to conform to the usual Gibbs-Boltzmann statistical mechanical paradigm.

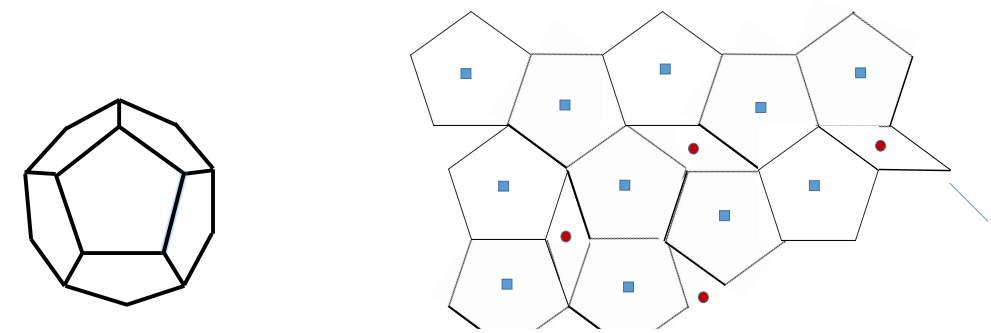


Figure 20: **LEFT:** A dodecahedron, which contains 12 pentagonal faces. Each vertex of the pentagon subtends an angle of $3\pi/5 = 108°$. In the dodecahedron, as a result of the curvature, each vertex is touched by just three faces. **RIGHT:** (Failed) attempt to tile space using regular pentagons. In a flat space, it is impossible to construct a vertex touched by an integral number of faces. Unfolding the dodecahedron, or tiling two-dimensional space with a collection of pentagons, however it is done, inevitably introduces irregularities, which can be identified with disclinations. Each pentagon contains a blue square at its centre. The red circles are at the centres of diamond shapes which must be inserted to restore the regularity.

An early collaboration with Jean-François Sadoc [171], and a number of subsequent papers (e.g. [172, 173]) dealt, using rather non-trivial geometry and group theory, with the structure of amorphous solids. A key part of these papers involved translating between curved spaces, in which it is possible to insert the local coordination, and the flat space in which we find ourselves. The idea can be seen in caricature in Fig. 20.

Fig.20 shows pentagons failing to tile a two dimensional space. The idea extends to three dimensions, with pentagons replaced by icosahedra. However, imagining it without the aid of group theory is almost impossible. The curved spaces may have positive curvature and thus be *elliptic* spaces, or negative curvature (*hyperbolic* spaces). In the curved space, there are crystallographic groups analogous to those of Euclidean space, and the difficulty involves the imperfect mapping between the space groups in the curved and flat spaces. In [172], *inter alia* Maurice wrestles with some apparently universal features of glassy materials, including the linear specific heat at low temperatures, which should be related to the existence of low lying acoustic modes which he suggested were analogous to the rotons found in liquid Helium.

4.6.2 Quasi-crystals

In later work in this general area Maurice turned his attention to *quasicrystals* [174–178], see also the reviews [166, 179]. Quasicrystals were discovered by Schechtman et al. [180]; for this work Schechtman received the 2011 Nobel Prize in Chemistry. They were so labelled by Levine and Steinhardt [181]. These materials exhibit local symmetries which appear, at least according to previous ideas in solid state physics, incompatible with the tiling of space, and thus impossible. Examples of such symmetries are pentagonal in two dimensions, and icosahedral in three dimensions. Such solids do exhibit sharp crystal-like spots in X-ray or neutron scattering. Thus, interestingly and anomalously, they are crystalline in reciprocal space, but apparently "amorphous" in real space.

As a new crystallographic paradigm, this idea had been anticipated by Mackay [182], when considering the tilings introduced in 1974 by Penrose [183], at that stage mainly as a mathematically-based aesthetic curiosity. There is some local rotational symmetry; the compromise is achieved using two basic lengths which are incommensurate with each other (i.e. the ratio is an irrational number). This is shown schematically in Fig. 21.

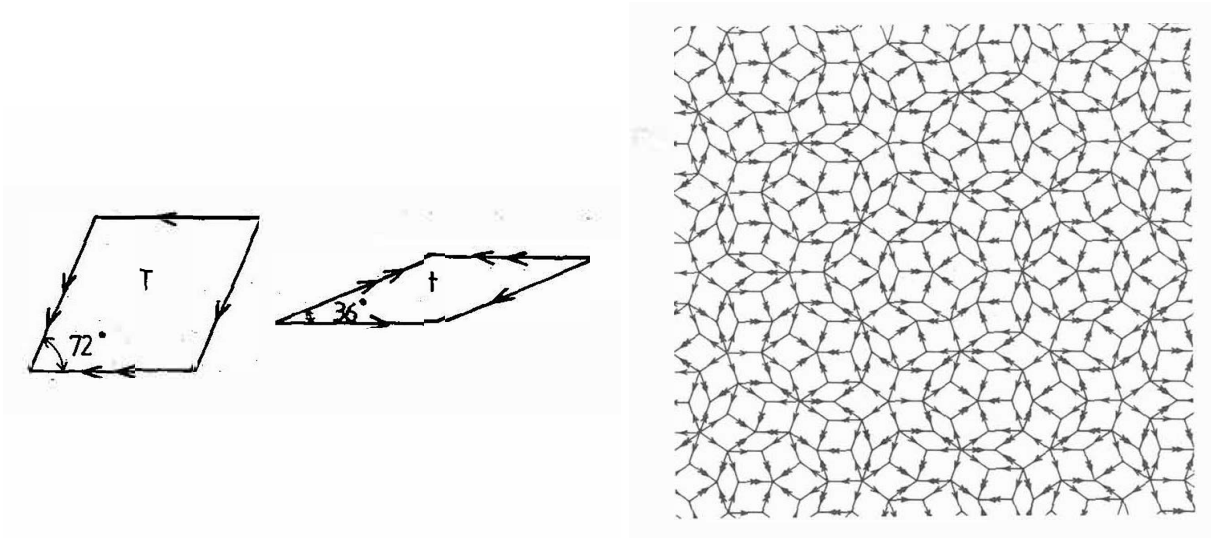


Figure 21: Example of a two-dimensional quasicrystal. **Left:** Penrose *tiles*. **Right:** Penrose *tiling*. The space-filling tiling is achieved using the two basic tiles with mutually incommensurate dimensions. This results in the observed locally pentagonal symmetry, which repeats but is nevertheless not periodic. From Kleman and Pavlovitch [174], reproduced with permission EDP.

The term "quasi-crystalline" is a transfer from the mathematical concept of *quasi-periodicity*. This occurs, for example, in studying rotations in astronomy, when bodies are undergoing several different periodic motions the periods of which are incommensurate with each other. The result is a dynamical system which never quite repeats exactly.

On the other hand, often there are non-linear effects which induce, in the long-term, a secular change in the motion toward *phase-locking*, in which the ratio between periods of the two motions tends to some simple rational number. The period of the moon's rotation around the earth and in space, for example, have phase-locked, with the result that from earth we never see the "far" side of the moon. But the period of the earth's rotation in space and around the sun have not phase-locked, causing over the years immense problems for priests, farmers and latterly metrologists.

The analogy to this phase-locking in solids is to replace the quasi-crystal by a superlattice. In

this case the system possesses periodicity on scales much longer than the interatomic distance. Maurice was initially less keen on the term "quasi-crystalline". He first (1986) refers to "aperiodic crystals" [174]. Later the same year [175], we find him referring to "non-Haüyan crystallography", a term suggested by Sir Charles Frank, and which commemorates the (French) René-Just Haüy who introduced the idea of the atomic lattice and the unit cell.

Much of the work conducted by Maurice and collaborators was concerned with the development of a theory of topological defects in quasicrystalline media, [166, 174–179], using some mathematics dev( )12) and som)ment of idea)ri [184], invc *)erlattice* at a

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1. Introduction.

The crystallographic description of an aperiodic crystal (quasicrystal) makes use of a d-dimensional Euclidean crystal $Z^d \subset E^d$ (E^d: d-dimensional Euclidean space), in which a $d_\parallel$-dimensional planar cut $P_\parallel$ is performed. This cut has some irrational orientation with respect to the lattice. A restricted set of vertices of Z^d is projected on $P_\parallel$; they constitute the quasicrystal in question. $P_\parallel$ is indeed thought of as the physical space, with $d_\parallel = 3$ or $d_\parallel = 2$ according to the case. The hyperlattice is a hypercubic lattice, with $d = 6$, $d_\parallel = 3$ (resp. $d = 5$, $d_\parallel = 2$) for the icosahedral crystal (resp. the pentagonal crystal). $P_\parallel$ is a subspace of E^d which is globally invariant under the icosahedral group, which is a subgroup of the hyperoctahedral group in $d = 6$ (resp. the pentagonal group, which is a subgroup of the hyperoctahedral group in $d = 5$). For more detail, see references [1-3].

The quasicrystal which is obtained by the above process depends of course on the restricted set of vertices of Z^d which is selected. A most usual choice is to select the vertices which belong to a d-dimensional strip parallel to $P_\parallel$ and whose breadth spans a unit cell of the high dimensional crystal as in figure 1. A more general construction employs the device of the so-called « atomic surface » S. This is a $d_\perp = d - d_\parallel$ dimensional manifold made of equal pieces (motifs) attached to all the equivalent vertices of the hyperlattice [4]. S is therefore invariant by the translations of the hyperlattice and has a global icosahedral symmetry (resp. pentagonal symmetry). The quasicrystal is the set of the zero dimensional intersections of the motifs and of $P_\parallel$. Note that the two methods are equivalent if the motif is a copy of $P_\perp$, the orthogonal complement of $P_\parallel$ in E^d ($E^d = P_\parallel \times P_\perp$).

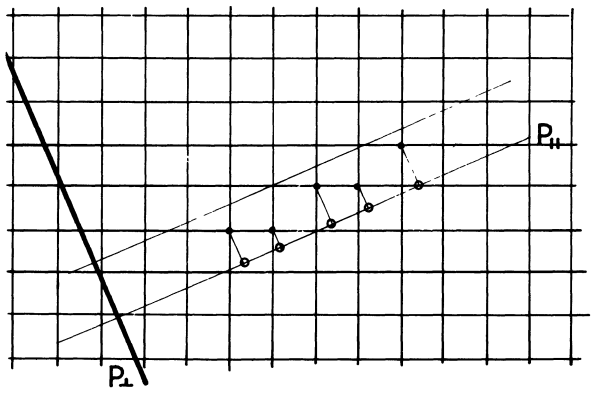

Fig. 1. — The case $d = 2$, $d_\perp = 1$ which illustrates the construction of the strip of selected vertices.

Figure 22: Schematic of the projection of a high dimensional hyperlattice into a lower dimensional space, from Kléman [177] (reproduced with permission EDP). Here for simplicity the projection is $d = 2 \rightarrow d_\parallel = 1$. The lattice points which survive the projection lie within a region of the dimension of a lattice constant in the direction $P_\parallel$. They are marked on the diagram, together with the projection direction $P_\perp$.

Whatever the method used, the structure of the quasicrystal depends globally on the exact position of $P_\parallel$ which can be moved parallel to itself in E^d. The related change of structure will be referred to as a global *phase shift* (GPS), and is easily analyzable as a sum of independent localized phase shifts (LPS), also called phasons, when the displacement of $P_\parallel$ is small.

The physical nature of phasons (an example is given in Fig. 2 for the $d_\parallel = 2$ case) is far from being understood, but it is by all means clear that they play an important role in the structural changes which affect quasicrystals [5, 6], and in the processes of plastic deformation [7, 8].

4.7 Reviews and Books

Maurice was a prolific author who contributed not only original research, but a continuing series of influential reviews and books. His first review of liquid crystal defects was in the *Bulletin de la Société française de Minéralogie et de Cristallographie*, continuing the long French tradition of considering the study of liquid crystals as a branch of mineralogy [188]. At the time the world-wide liquid crystal community was rather small, and so the article has to start by introducing elementary ideas in liquid crystals, both theoretical and experimental, before explaining the Volterra process, adapted for microscopic purposes from that described in Fig. 7. The article summarises the work of his laboratory in this area over the previous few years. It was however in French, which unfortunately limited its scientific influence. In his autobiography [14] Maurice admits, a little shamefacedly, that for his first paper in English, submitted during his postdoctoral period in Oxford, he had to wait until his 17^{th} scientific publication.

Further comprehensive reviews of the subject followed in 1989 [8], with Lavrentovich and Nastishin in 2004 [127], with Friedel in 2008 [189], and with special reference to point defects, with Lavrentovich in 2006 [190]. In addition, we have mentioned above his exhaustive reviews of aspects of the quasicrystal problem [166, 179].

Perhaps above all, the excellently reviewed 1977 *Points, Lignes, Parois* (Points, Line and Walls...) [191] was a "must-read" for all in the field. Later, when talking about the impact of this book on paradigms in the liquid crystal field, Maurice told the present author that he regretted not writing it initially in English; it took five years for the translation to appear. We also mention his 2002 Soft Matter textbook cowritten with Lavrentovich [192].

5 More about Topology

We devote a separate section to this subject. This is only partly the result of the significance of the "homotopy revolution" to the study of defects in condensed matter and, as it turned out, other physical systems as well. It is partly, implicitly, due to the more recent attention given to the importance of topology in physics in general, athough we shall not here address this aspect. This attention was highlighted by the award of the 2015 Nobel Prize for Physics to Duncan Haldane, David Thouless and Michael Kosterlitz, as well as Giorgio Parisi's share in the 2021 Nobel Prize; each of these workers used topological ideas in constructing theories of low temperature phases in condensed matter. And partly, precisely because of the intellectual separation between mathematics as applied to science and engineering and pure mathematics which has developed over the last century, many readers of this article will have no insight into the roots of the theory. It will turn out that physics and topology have surprisingly driven along parallel and to some extent intersecting paths, which are worth exploring.

Nevertheless, I emphasise that in its historical aspects, this section is little more than a sketch, containing details which will at the same time escape experimentalists and annoy serious mathematicians. Serious students of the history of mathematics are referred to, e.g. the volume edited by James [193], within which, the essay by Charles Nash [194] relating topology and physics will be of particular interest.

We start this section by discussing work on defect classification carried out more or less in parallel with that of the Paris group.

5.1 Parallel work

Original and important as the Toulouse-Kléman paper was, it turned out that the use of homotopy theory to classify defects in condensed matter was an idea whose time had come. Papers by Russian mathematical physicists interested in gauge theories of fundamental particles, using homotopic classification, had appeared in the previous year [195, 196]. The symmetry group V was different from that appearing in condensed matter theory, but the idea was the same.

The mathematical background of the Russian scientists was stronger and their explanations were briefer than those of Toulouse and Kléman. The brevity was partly due to the rather elitist attitude common in the Soviet school (*make the reader think*!), and partly, so Economic Historians tell us, due to the chronic paper shortage in the Soviet Union at the time. The latter, in particular, prohibited extended narratives, even from the naturally verbose. One of the Russian mathematicians in question, the late Misha Monastyrskii, later distinguished (see e..g. [197]) author of several important texts in the field [198, 199], was in fact often not short of words. But nevertheless, for whatever reason, even he was able to squeeze his extended argument into a single page.

Only a few months after appearance of the Toulouse-Kléman paper, and independently of it, the Monastyrsky work was picked up by Vladimir Mineev and Grigory Volovik at the Landau Institute for Theoretical Physics at the USSR Academy of Sciences in Moscow [200–202]. They were studying defects in low temperature phases of superfluid He^3. These phases (He^3-A and He^3-B) are analogous to complex superconductors. At the time there was intense interest in superfluid helium phases; the superfluid He^4 phase has been known since the late 1930s, and it has a low temperature phase whose order parameter has the symmetry of the XY model. He^3 is fermionic and hence expected at low temperatures to behave differently from the bosonic He^4. The defects in He^4 are well-studied superfluid vortices, and this is consistent with the predictions of homotopy theory. But the He^3 phases are more complex – indeed they are often described as complex liquid crystalline phases – and their defects require much more detailed study.

Further parallel and independent work on the classification of dislocations in solids was carried out by Dominik Rogula in Warsaw [203]. A particularly influential paper was written by the distinguished British cosmologist and quantum field theorist T.W.B. Kibble (1932-2016) [204]. This paper, entitled "Topology of cosmic domains and strings", dealt with cosmic strings in gauge theories, as well as domain walls and "monopoles", which we would identify as point defects. There are theoretical analogies between the gauge fields which were being developed to describe the properties of the early universe and those simpler version describing broken symmetry condensed matter phases. They are not exact, but are close enough for cosmologists to feel that experiments of orientational domain formation in liquid crystals can fruitfully be used as a model "cosmology in a lab" [205].

Before we delve into deep history, we can note that many of the homotopic ideas in fundamental physics were anticipated by an influential 1966 paper by David Finkelstein of Yeshiva University simply entitled "Kinks" [206]. Finkelstein's goal was to seek the origin of conservation rules in elementary particle physics. The paper is so exquisitely worded that extracts are worth quoting:

> The strict conservation laws of elementary particle physics correspond to exact symmetries of the underlying quantum field. In some cases the symmetries have an obvious universal significance, and the correspondence is an especially fruitful one, such as for angular momentum and charge, understood in terms of Lorentz and gauge invariance. *In other cases the correspondence serves as little more than a convenient transcription of our experimental knowledge.* ...
>
> ...In sufficiently nonlinear field theories, there are also objects whose number is strictly conserved because of continuity, (*a continuity, however, of the basic fields rather than of trajectories*). We call these conserved objects **kinks**, and seek properties that the underlying field must possess for kinks to exist ...

The italics are mine. Firstly, some conservation rules come from obvious symmetries, whereas some symmetries have, so to speak, to be invented in order that the correspondence between gauge symmetry and conservation law can be maintained. And secondly, the non-linear theories lead to other kinds of conservation rules, which later in the paper he identifies with so-called homotopic variables. Finkelstein's kinks are condensed matter's defects, and much of the technology which appears later in the paper later became part of the condensed matter defect canon.

Mainly for aesthetic reasons, I cannot avoid quoting (this time without comment) from two further paragraphs in Finkelstein's introduction:

> To avoid misunderstanding, I explicitly abjure two heresies:
> **The topological heresy:** In this theory, particles are not topological deformities of space-time. To be sure, kinks are topological in some sense. But the kinks treated here have nothing to do with any topological deformity of space-time. On the other hand, kinks still occur and indeed in greater variety, if there are admitted topological space-time deformities.
> **The quantum heresy:** This is not a classical theory of quantum effects. To be sure, kinks are "quantized" in some sense: they are discrete in number. But their behavior exhibits no trace of quantum mechanics when it is described in a completely classical field theory. On the other hand, kinks still occur if complementarity is explicitly taken into account and the field is treated quantum mechanically; then, of course, they exhibit the usual quantum effects.

However, when Maurice started to analyse the structure of liquid crystalline defects he was unaware of Finkelstein's paper and the resulting field theory literature. Finkelstein was publishing in the Journal of *Mathematical* Physics, which would not in general, we hazard a guess, be light reading for condensed matter theorists. However, the final section of Toulouse-Kléman includes this extract:

> During the course of our study, we have discovered that topological concepts have been previously used in field theory by quite a few people, the emphasis being mainly on point singularities [A] or on global configurations of the whole space [B]. *Actually, this similarity of concepts in the study of elementary particles and of defects in ordered media appears as a very promising feature.*

By the end of the study, Toulouse and Kleman had been informed of the preceding literature. Ref [A] here includes a much-cited paper [207] on magnetic monopoles in field theories by the celebrated Nobel Prize winner Gerard t'Hooft, as well as the already-cited Monastyrsky-Perelomov paper [195], while ref [B] is Finkelstein's paper [206]. The italicised sentence expresses a positive sentiment as to the mathematical relationship between the condensed matter defects and the elementary particle kinks: not as a threat but as an opportunity.

5.2 A glance in the rear mirror

A proper history of algebraic topology and homotopy is well beyond our scope. We refer the reader e.g. to the book by James [193], a shorter earlier article by him [208] and the article by Hilton [209]. The earliest mathematical relationship recognised (in hindsight) as topological is Euler's polyhedron formula, dating from 1752:

$$V - E + F = \chi = 2, \tag{1}$$

where, in the polyhedron, V is the number of vertices, E the number of edges and F the number of faces. The quantity χ is the *Euler characteristic*, and if the polyhedron has no holes or handles $\chi = 2$. Always. Or rather, almost always, for if we include some handles or holes inside it (equivalently, we make it *multiply connected*), then χ will in general no longer be equal to 2. If, for example the polyhedron takes the form of a torus, then $\chi = 0$. Gauss in later life speculated that there were many more discoveries in this field to be made.

The terminology is relatively recent; according to Google n-gram, "topology" takes off around 1920. Earlier terms are *geometria situs* (Gauss) and *analysis situs* (Poincaré). The term itself was introduced in 1848 by the German mathematician Johann Benedikt Listing (1808-1882), a student of Gauss [210] (for a historical discussion, see [211]). Other terms relevant here are homology and homotopy. The ideas of homotopy are introduced in a set of articles by the mathematical polymath Henri Poincaré (1854-1912), the most important of which appeared in 1895 [212]. In 1922 Oswald Veblen's (1880-1960) book [213] was concerned with *Analysis Situs*, but by 1935 the new textbook by Pavel Alexandroff (1896-1982) and Heinz Hopf (1894-1971)[not the Hopf of bifurcations, that was Eberhard!] was on topology.

To some extent, topology consists of the study of what remains of geometrical relationships when we distort shapes but allow parts to remain in the same relative relationships; hence Gauss's *geometria situs* = "geometry of position". One important aspect of topology is the study of knots. Alexander the Great is said to have cut the Gordian knot, instead of untying it. Clearly it was cheating, but before modern mathematicians, it was difficult to define exactly how.

It turns out that knots are of some interest in the context of the history of 19^{th} century physics, and also of relevance in our study of liquid crystal defects. By that time it was well-known that materials were associated with characteristic *spectral lines*. The atomic theory of matter was likewise accepted by chemists. But the atoms themselves (which we have discussed above) remained mysterious and inaccessible. We now know that reaching the relevant length scales were well beyond available technology. The spectral lines were supposed – sensibly, but incorrectly as it turned out – to be associated with some kind of resonance, by analogy with the acoustics of stringed musical instruments. But how?

The German physicist/physician Hermann von Helmholtz (1821-1894) had made some calculations on the hydrodynamic structure of vortices, and observed that the theoretical properties matched those observed in smoke rings. In 1867 the Scottish physicist William Thomson (1824-1907) – after 1892 Lord Kelvin, under which name he is better known – was responding to this work (Helmholtz and Thomson were personal friends as well as colleagues). Helmholtz had calculated the dynamical properties of *vortex rings* (see Fig.23, and Thomson [214] speculated that some structures in the ether along these lines, more complex vortex lines perhaps, were responsible for the different kinds of atom.

The "vortex atom" speculation, however, needed some mathematical flesh to graft onto its imaginative bones. Thomson in Glasgow was able to recruit the professor of mathematical physics at nearby Edinburgh, Peter Guthrie Tait (1831-1901) to investigate the possibility. What fol-

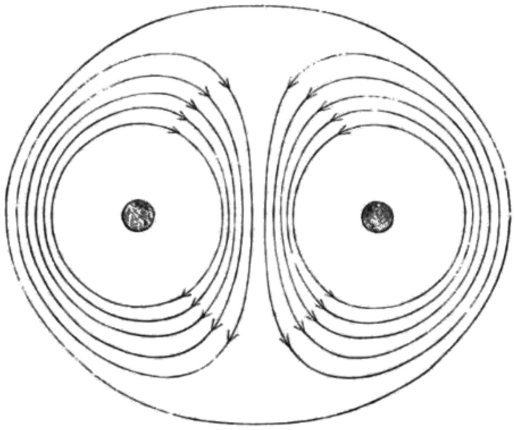

Figure 23: Fluid streamlines in the cross-section of a vortex ring, as calculated by Helmholtz. Reproduced from Thomson's paper in 1867 [214]. From a contemporary viewpoint, vortices may be regarded as defects in a hydrodynamic velocity field.

lowed, some time later – three papers appearing in 1877, 1884 and 1885 – was a painstaking enumeration of different kinds of simple knots. The enumeration involved counting the number of times the string crossed itself, and as can be seen from Fig. 24, eventually runs out of steam; the method is not practical for complicated knots. More extensive discussions of vortex atom theories, knot chemistry, the relationship between Tait, Maxwell and Kelvin, and the like, can be found in an early biography of Tait [215] and in more recent the history of science literature; see e.g. [216–219].

Both Thomson and Tait subsequently turned their attention (and achieved fame) elsewhere. These papers presented an ingenious, but premature, attempt to understand the microscopic basis of atoms. Both the mathematics and the physics was flawed. The next serious attempt to understand knots, by Max Dehn (1878-1952) in 1910 [221] (see also the commentary by Peifer [222]) used the more sophisticated group theoretical methods introduced by Poincaré [212], and indeed the subject is still active.

However, in the course of his investigation of knots, Tait discovered Listing's work on topology. The discovery was in fact due to his former school classmate, and subsequent close colleague, James Clerk Maxwell (1831-79). After Listing's death on Christmas Eve 1882, Tait wrote a very appreciative obituary in Nature [223], finishing with:

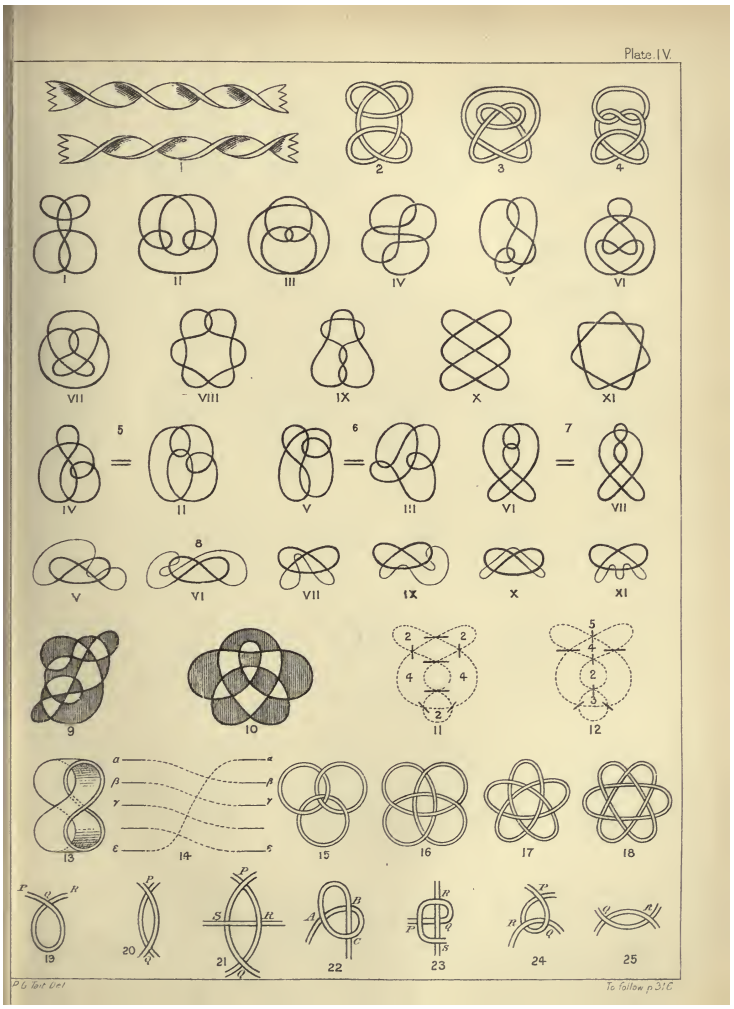

Figure 24: Different knot configurations enumerated by P.G. Tait. Reproduced from [220].

> In most works on Trigonometry there is given what is called *Euler's Theorem about polyhedra*: – viz. that if S be the number of solid angles of a polyhedron (not self-cutting), F the number of its faces, and E the number of its edges, then
>
> $$S + F = E + 2,$$
>
> The puzzle with us, when we were beginning mathematics, used to be "What is this mysterious 2, and how came it into the formula?" Listing shows that this is a mere case of a much more general theorem in which corners, edges, faces, and regions of space, have a homogeneous numerical relation. Thus the mysterious 2, in Euler's formula, belongs to the two regions of space:– the one enclosed by the polyhedron, the other (the *Amplexum*, as Listing calls it) being the rest of infinite space. The reader, who wishes to have an elementary notion of the higher forms of problems treated by Listing, is advised to investigate the modification which Euler's formula would undergo if the polyhedron were (on the whole) ring-shaped: – as, for instance, an anchor-ring, or a plane slice of a thick cylindrical tube.

His subsequent lecture on Listing's work to the Edinburgh Mathematical Society [223] includes the following extract, which draws attention to the difference between the almost immediate transmission of academic publication nowadays, with an accepted linguistic standard, and the difficulties faced by Tait in following up publications of interest (italics here copied from the original):

> I ought not to omit to say, before proceeding to our business, that it is by no means creditable to British science to find that Listing's papers on this subject – the *Vorstudien zur Topologie* (*Göttinger Studien*, 1847), and *Der Census räumlicher Complexe* (Göttingen *Abhandlungen*, 1861) – have not yet been rescued from their most undeserved obscurity, and published in an English dress, especially when so much that is comparatively worthless, or at least not so worthy, has already secured these honours. I was altogether ignorant of the existence of the *Vorstudien* till it was pointed out to me by Clerk–Maxwell, after I had sent him one of my earlier papers on *Knots*; and I had to seek, in the Cambridge University Library, what was perhaps the only then accessible copy.

5.3 Subsequent Progress

We now return to the present day, or rather to the late 1970s, the period immediately after the homotopic revolution in condensed matter physics. It is not our purpose here to provide a review, for we cannot improve on recent comprehensive professional reviews [224–226]. Rather we wish to give the naive reader some feeling for the recent research narrative in the area, and also to point to some amusing historical analogies.

It goes without saying that a new paradigm attracts a whole set of workers with different skill sets, addressing new problems. We have seen earlier that the mathematicians (loosely defined!) Poénaru and Michel were attracted to the field in order to provide some rigour. Poénaru confided to this author that after some time the attraction faded, as he felt that the mathematical problems were "trivial" and hence less interesting than his, so to speak, day job.

To the mathematical reviews [104, 227–229], we may add the general influential tutorial review by Mermin [109]. Poénaru and Toulouse [103] pointed out that the homotopy group of biaxial nematic liquid crystals was non-Abelian. The significance of this was that when two defect lines were combined, it mattered how they approached each other, whereas the combination rules for uniaxial line defects (or XY defects) involved in some sense simple addition (albeit in a cyclic group sometimes). The mathematical interest of the biaxial nematics was unfortunately

subverted by a disappointing Almighty, who seemed unhelpfully reticent in the production of useful experimental examples [230].

One of the key workers in the renaissance of interest in the experimental study has been Oleg Lavrentovich, whose 1988 review paper [231] connected experiment and homotopy. As readers will be aware, he later became Maurice's most frequent collaborator. The capacity of homotopy for classifying defects elevated interest in point defects: charges in an electric field theory, or monopoles in a magnetic field theory. In uniaxial nematic liquid crystals (and in some other fields) they have come to be known as *hedgehogs*. A hedgehog can be radial or hyperbolic [190], and in biaxial liquid crystals necessarily it sits at one end of a disclination line. Hedgehog defects were particularly evident inside spherical containers with homeotropic boundary conditions.

But if the boundary condition of the same container were to be planar degenerate, then a uniform nematic configuration over the surface would be impossible, and hence surface director defects are obligatory. This is sometimes known as the "Hairy Ball Theorem" and is due to Poincaré. Equivalently when combing one's hair there has to be a "hair whorl", or "cowlick", where one's hair sticks *up*, rather than *along*. In my case, regrettably, the hair whorl has "decayed" into a rather large bald patch; the local singularity is only avoided by placing the hair in a (global) non-simply connected manifold.

David Mermin labelled such a point as a 'boojum". The term is drawn, imaginatively or absurdly according to one's point of view, from a poem by the British Victorian mathematician and writer Lewis Carroll, author of "Alice's adventures in Wonderland". Mermin has given an entertaining account [232] of his struggles to coax an unwilling editor into accepting his lexicographic innovation (see also Fig. 27). A.M. Polyakov, who introduced the term 'hedgehog" (or rather ёж [yozh], its Russian equivalent) in 1975 when studying similar problems in quantum field theory [233], reported no such difficulties. Perhaps the editorial staff at ZhETF were more laid back than those at Physical Review ...

The confluence in interest in defects by Maurice and other condensed matter physicists on the one hand, and by fundamental physicists on the other, also turned out to be fruitful. In 1976 Kibble [204] had studied defects in fields of cosmological interest, with a view to the possible importance of "cosmic strings" in the early universe. Zurek [234] suggested that He^4 might be a suitable terrestrial model in which to look for string formation in a quench in which the temperature is suddenly lowered, by analogy with the dynamics of the early universe.

This came to be known as the "Kibble-Zurek" mechanism, and has attracted much interest in both communities. Chuang and coworkers [205] suggested that He^4 was not the most suitable material with which to work, as it requires extremely low temperatures and is available only in small quantities. But nematic liquid crystals, by contrast, were sufficiently similar to cosmological models and much easier to work with. In later papers, both by the same workers [235] and others (e.g. Bowick et al [236]), it was possible to carry through this "Cosmology in the Laboratory" programme, and make estimates of the rates of defect string formation in quenched liquid crystals.

Finally, a particularly current topic concerns aggregates of disclination loops. Let us recall that a disclination line, like a piece of string, must either end at a surface, or form a closed loop. Sometimes the loops are macroscopic. But sometimes, they are the result of an instability in a point defect. Long ago, the present author tried hard (but failed!) [237] to determine the conditions under which a hedgehog point defect would look microscopically like a $S = 1/2$ disclination ring. The problem was examined more closely by Mkaddem and Gartland [238], who found that in fact there were three possibilities, shown in Fig.25

The existence of the disclination ring in Fig.25, macroscopically but not microscopically equivalent to a point defect, begs the question of how this idea can be extended, and how the equiv-

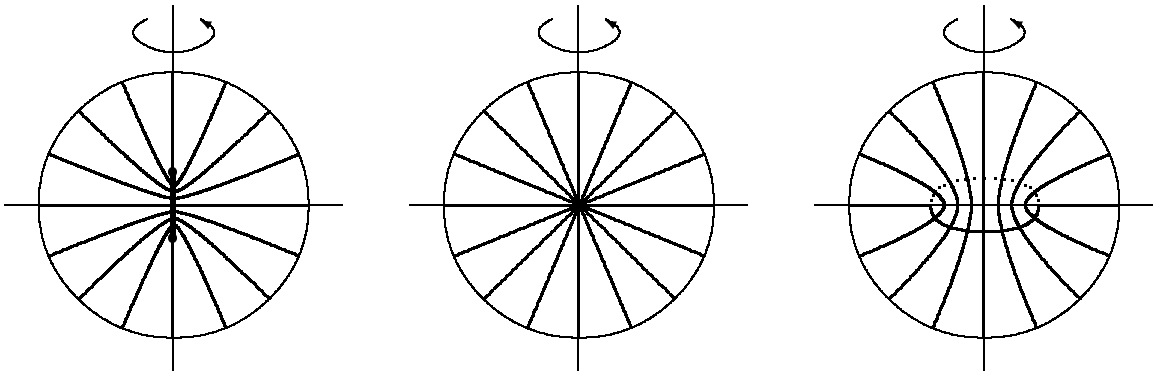

Figure 25: Different possibilities for the fine structure of a hedgehog core in a nematic liquid crystal, after Mkaddem and Gartland [238]. The pictures show a cross-section of a sphere, with director orientations indicated by lines. Depending on the ratios of the elastic constants and the temperature, the naïve hedgehog core given by **B**, may be split into the line **A** or the disclination ring **C**. However, this ring has a particularly simple structure...

alences may be established homotopically. To establish the idea, but not the details, let us first consider a single closed disclination loop. What kind of complications might crop up? In principle, all nematic disclinations are known to be homotopically equivalent, so at first sight there should be no possible complications.

However a closer look shows that this is not the case. The disclination line in Fig. 25C is a so-called S=1/2 wedge disclination line, by which we mean that the director rotates through $+\pi$ around an axis everywhere tangent to the disclination line. Furthermore, as one travels along the line the director configuration is transported parallel to the line without rotation. Now the possible complications become clearer. By the time we have circumnavigated the disclination loop, the local director structure must be reestablished...

But *on the way*, things may be different. First of all the director configuration could be rotated with respect to the dragged coordinated system. In this case, a rotation of π in the plane perpendicular to the disclination line serves to close the loop. And secondly, the direction around which the rotation occurs may change; equivalently the plane of rotation may *tumble*. If it tumbles through π, for example, then the $S = +1/2$ disclination is transformed into an $S = -1/2$ disclination. Of course, both may change at the same time. By now, the intuition of even the most imaginative geometer will be exhausted, and recourse to formal topological methods is necessary. To make a full classification of all the possibilities one needs to know which rotations are irreducible.

Furthermore, if that were not enough, even simple geometry suffices for it become clear that some of these classes require that the loop be threaded with at least one other disclination line. And if another line may thread my original line, why not the original line threading itself – equivalently a *knot*. So the general mathematical problem becomes, how do we classify a localised knotted interlinked set of possibly twisted and tumbled disclination lines, which ones are permitted, and what is the long-range signature of such a localised aggregate?

This problem was first addressed by Jänich [239]. More recently there has been significant progress; there is a review by Alexander et al [224], see also [225, 226, 240–242]. However, all this might have remained a mathematical backwater, had not experimental progress been made in constructing such assemblages. The key idea here is that the boundary condition on colloidal particles in a nematic medium are often, if not always, inconsistent with a uniform nematic director. The resulting antagonism is resolved by introducing a defect. One possibility, shown in Fig. 26, is a disclination ring, in some sense merely an inside-out version of the ring at the hedgehog core [243, 244].

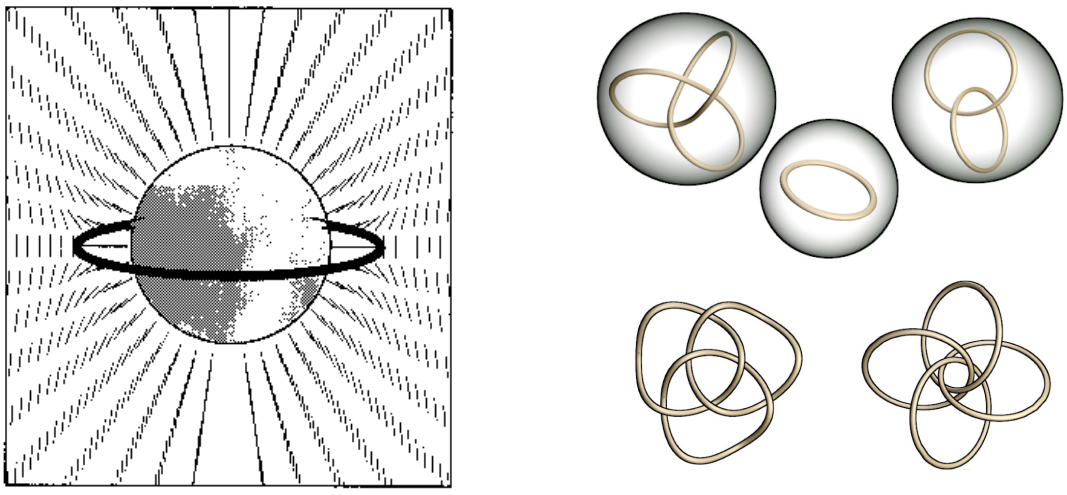

Figure 26: **Left A:** The Saturn Ring structure, one possible way of resolving the antagonism between the local homeotropic surface order on the surface of a colloid particle, and a long-range uniform texture. After Ruhwandl and Terentjev [243]. **Right B:** Complex confined knotted structures, after Machon and Alexander [240]. The original diagram includes a detailed topological commentary, which we have omitted and to which we refer the interested reader. A more detailed set of similar diagrams can be found in Tkalec et al [245] arising from colloidal aggregates.

More elaborate versions of this involve the use of entangled defect lines to tie together colloidal particles [246]; see also the review by Smalyukh [247]. Eventually, when several colloidal particles are involved, one obtains complex knots of different types, as shown in Fig. 26. The long-range structure of the nematic would be due to the defects alone.

The reader is now invited to compare Figs. 24 from 1877, and 26B from 2014. Although the necessary mathematics to analyse these knots exhaustively was not available to Tait in the 1870s and 1880s, we recall that Tait was pursuing Kelvin's idea from the 1850s that macroscopic particle structure, together with associated conservation rules, were related to an internal knotted structure of the aether. Here we have, after 170 years, a physical manifestation of that concept, and just possibly, as a result, a renewal of interest in the idea that some fundamental particle properties are related to knotted effects of the underlying field.

6 Defects in Biology

. . . liquid crystals (have) emerged as a burgeoning new frontier in cell biology . . . [248]

6.1 Biological preamble

Maurice's published work was primarily concerned with defects in material science, rather than in biology. But his close colleague Yves Bouligand started life as a zoologist, and turned to liquid crystals in order to make sense of specific sets of observations, as we shall discuss further below. Maurice long held the view that defects in liquid crystals are linked with the biological world, before it was fashionable, perhaps even before there was strong evidence to sustain his view. Indeed he and Bouligand spent many hours discussing this very subject [249], discussions which informed much of Bouligand's later work.

Out of these discussions came, as far as we are aware, one single-authored paper by Maurice, published in 1985, with the rather innocent title of "On the coexistence of cholesteric and 2-dimensional orders" [250]. Although its title is rather coy on this matter, indeed anodyne, the paper presents a model of the molecular organisation of DNA in a particular type of microorganism studied by Bouligand. The model involves molecular order on two length scales, one much longer than the other. In retrospect it resembles the so-called *supercoiling* [251] – coils within coils within coils – which is now known to govern the chromatin molecular order within a living cell.

In recent years, the domain of "soft matter physics" has steadily invaded the biological sciences. Liquid crystals have played a major part in the artillery driving that advance. This section presents a rather personal view of some of the scientific discourse which may link Maurice's studies of defects with biology. I aim for a link with *biological function*. In biology, as opposed to mere inorganic physics and chemistry, we always ask the questions: "why is there? "what does it do?"

All sorts of physical effects pop up, almost by accident, in biological materials. Nature is a promiscuous engineer, recruiting all sorts of convenient physical phenomena and materials in an apparently creative urge, formerly interpreted in terms of the Divine, and now in terms of the more prosaic but still miraculous process of Darwinian selection. When we see a link between a physical property and a biological function, then we can insert the property into some kind of contingency. Then, absent the physical property, absent the biological process under investigation.

The motivation for this section is twofold. Firstly, this subject is "in the air", so to speak, and a genuine review (we make no claim here that this essay represents such a review), as the quote at the head of the chapter [248] indicates, could not fail to include it. Secondly, our view is that, notwithstanding his few publications in this area, given a career lasting another half-century, this is the direction that Maurice would have taken. He had a great interest in philosophy; both physical form (independent of the specific material underlying that form) and biological function (independent of the specific goal of the biological process in question) represented for him related philosophical concepts.

Given the presence of liquid crystals in biological materials, we may reasonably ask a number of questions of defects. Firstly, are they there? Secondly, if there, do they play a functional role? And thirdly, if they do play a role, what is the context? For in the present day, we seek physical mechanisms in (at least) developmental biology (i.e. how does the organism come to have the form that it does) and cell/organism biology (i.e. how do the harmless and thought-free laws of

physics and chemistry combine to allow the organism to do whatever it does).

In physics, liquid crystals are examples of phases with broken symmetry, and defects, we recall, break that broken symmetry. When we search in biology for defects, we first of all seek a broken symmetry, before seeking some local disobedience to that symmetry breaking. The broken symmetry is not so hard to find. The *zygote* – the original fertilised cell which initiates the life of a multicellular individual – is, more or less, spherical. But after repeated division, the aggregate of cells forming the individual has acquired a shape. This shape contains some symmetry elements, which vary between different phyla (the groups used to classify species, which also represent ancient evolutionary branching points). The reader, the writer, his pets and the insects that bite them, more or less, possess bilateral symmetry; some jellyfish are radially symmetric, although *Aurelia marginalis* has 4-fold symmetry. And starfish (and other members of phylum *Echinodermata*) as well as the flowers of many plants are basically pentagonal.

Somewhere along the embryological pathway between zygote and adult, the original spherical symmetry has been lost. Some organisms have other symmetries associated with repetition, for example the segments of insects, or the spirals of shellfish. Bateson's Rule of Secondary Symmetry, dating from 1894 [252], stated that "extra limb(s) or parts of a limb are themselves morphologically double" made an embryological statement which biology was not at the time able to incorporate. In our discussion, therefore, we start at the level of macroscopic symmetry.

6.2 D'Arcy Thompson and biological symmetry

All, well, almost all, contemporary studies of symmetry in biology lead back to the classic *On Growth and Form* [253] by the Scottish biologist D'Arcy Wentworth Thompson (1860-1948). This was first published in 1917 to only moderate acclaim, then reprinted rather grudgingly five years later after 500 copies had been sold. But nevertheless, for the last 100 years, more or less, it has never been out of print.

D'Arcy Thompson (almost universally so named, not merely "Thompson", who could, after all, be Lord Kelvin, or J.J. Thomson or some other random Thomson/Thompson) was by trade a naturalist, from his family he was no mean classicist, and by avocation an intellectual. He would not have described himself as a mathematician or physicist, and a hundred years on would be surprised to know that he is remembered primarily for his expertise in those disciplines. In some sense, just as Francis Bacon is remembered as the High Priest of the scientific method merely for having established a programme, D'Arcy Thompson can be remembered for normalisation of physical biology. Not for Thompson the magic of vitalism. The default, the "Bayesian Prior", as we might say, was that physiology would be explicable in terms of physics and chemistry, using mathematics as the medium of argument. And he looked for "growth and form" as the basis for this understanding.

Part of D'Arcy Thompson's magic – notwithstanding his strong denial of any scientific role for magic – is his almost supernatural anticipation of future trends. I cannot resist quoting from the part of his text where he foresees the application of topology in biological applications:

> The rules and principles which we have arrived at from the point of view of surface tension have a much wider bearing than is at once suggested by the problems to which we have apphed them; for in this study of a segmenting egg we are on the verge of a subject adumbrated by Leibniz, studied more deeply by Euler, and greatly developed of recent years. It is the *Geometria Situs* of Gauss, the *Analysis Situs* of Riemann, the Theory of Partitions of Cayley, of Spatial Complexes or Topology of Johann Benedict Listing. It begins with regions, boundaries and neighbourhoods, but leads to abstruse developments in modern mathematics. Leibniz had pointed out that there was room for an analysis of mere position, apart from magnitude: "je croy qu'il nous faut encor une autre analyse, qui nous exprime directement situm, comme l'Algèbre exprime *magnitudinem*." There were many things to which the new *Geometria Situs* could be applied. Leibniz used it to explain the game of sohtaire, Euler to explain the knight's move on the chess-board, or the routes over the bridges of a town. Vandermonde created a *géometrie de tissage*, which Leibniz himself had foreseen, to describe the intricate complexity of interwoven threads in a satin or a brocade. Listing, in a famous paper, admired by Maxwell, Cayley and Tait, gave a new name to this new "algorithm," and shewed its apphcation to the curvature of a twining stem or tendril, the aestivation of a flower, the spiral of a snail-shell, the scales on a fir-cone, and many other common things. The theory of "spatial complexes," as illustrated especially by knots, is a large part of the subject.
>
> Topological analysis seems somewhat superfluous here; but it may come into use some day to describe and classify such complicated, and diagnostic, patterns as are seen in the wings of a butterfly or a fly ...

In cell biology, the so-called mitotic spindle describes the state of a eukaryotic cell during cell division. We remind the reader that *eukaryotes* are often (not always, although the reverse is the case) multi-cellular organisms with the property that the genetic material (*chromosomes* are contained inside a nucleus which is bounded by the nuclear envelope consisting a lipid bilayer. During cell division microtubules (long polymers) align between two spindle poles which act as the organising centres for the new nuclei. The separated ("single helix") chromosomes travel along the aligned microtubules to the spindle poles, eventually creating identical daughter cells.

D'Arcy Thompson includes in his tome, not only a photograph of a dividing cell, but also some carefully drawn diagrams exhibting the dividing cell at various stages. We now know that the aligned microtubules are active nematic liquid crystals.

The microtubule nematic director structure during the mitotic spindle phase closely resembles that of a director of a nematic in a spherical droplet with tangential boundary conditions. In this case, the nematic is usually more or less aligned inside the droplet. A complete tangential arrangement of the nematic director at the surface is of course forbidden on topological grounds. One resolution of this frustration is to create two so-called surface defects (each of which is *boojum*) at each pole. This analogy is demonstrated in Fig. 27.

The resemblance to liquid crystals, although not known explicitly to D'Arcy Thompson, would not have surprised him. "Our" Otto Lehmann is mentioned 26 times in the 750 pages of 'On Growth and Form"; both he and Lehmann were strong materialists seeking, albeit prematurely, physical law to explain the apparent purpose in living organisms. Here again is D'Arcy in full speculation mode:

> ..... the phenomenon of "liquid crystallisation" does not destroy the distinction between crystalline and colloidal forms , but gives added unity and continuity to the whole series of phenomena. Lehmann has also demonstrated phenomena within the crystal, known for instance as transcrystallisation, which shew us that we must not speak unguardedly of the growth of crystals as limited to deposition upon a surface, and Bütschli has already pointed out the possible great importance to the biologist of the various phenomena which Lehmann has described ...

Note to editor: *do not edit* D'Arcy! His 100-word sentences and archaic spellings (e.g. "shew")

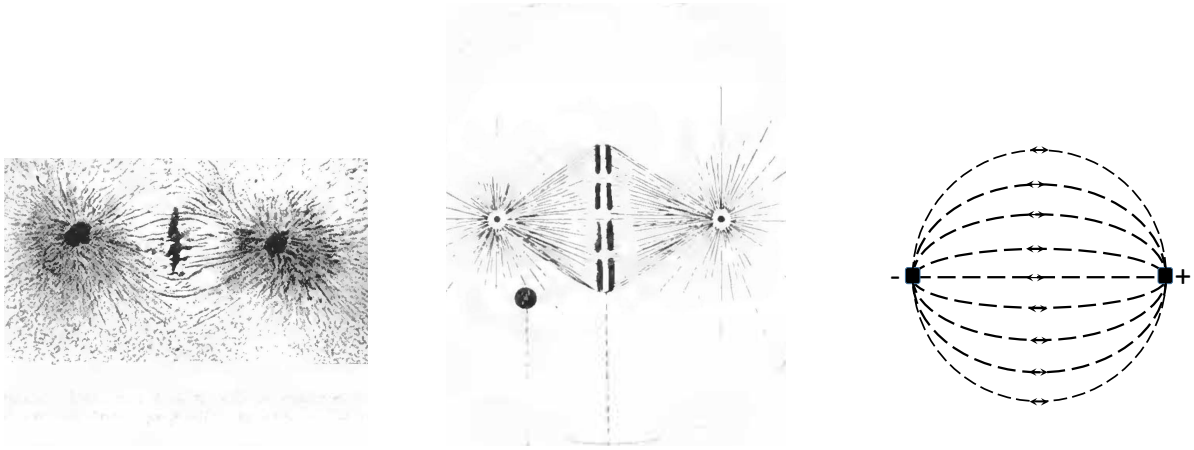

Figure 27: **Left:** Mitotic spindle: (Very!) old photograph reproduced from "On Growth and Form" [253] of the dividing cell within a trout egg. **Centre:** Idealised sketch of the mitotic spindle by Thompson reproduced [253]. The mitotic spindle is a self-organised, but non-equilibrium, bipolar array of microtubules. The chromosomes segregate and travel along the microtubules toward the centrosome organisers at each pole. See Ref. [254] for a more detailed modern picture with extensive discussion. **Right:** Cartoon of nematic droplet with a boojum at each pole. The boojum defect, is a surface director singularity of integer index. In this case, the + and - correspond to oppositely charged electric poles, causing an electric field from the negative to the positive electric pole. The arrows are double-headed because the nematic director points along the electric field lines, independent of polarity.

are there to be savoured rather than suffered, for he is at once literature, art, science, philosophy, speculation, and great learning. And if nowadays we are unfamiliar with "transcrystallisation", well, it was early days ...

More recently (e.g. [255]) the dividing cell has been examined more closely, to determine whether similar features can occur in a physicochemical system. And indeed they can, both quasi-biologically in the absence of the centrosome poles which attract the separating chromosomes, and in numerical simulations in model "active" systems.

6.3 Yves Bouligand and frozen liquid crystalline structure

It was the French zoologist and naturalist Yves Bouligand who was the first to examine in close detail the relationship between biological materials and liquid crystals. Curiously enough here we see an example of a defect which turns out to be not (at least not necessarily) a defect. The observation was that in various organic materials – the outer skeletons of various insects [256], so-called Haversian systems in human bone [257,258], chromosomes of some microplankton [259]– what seemed to be the same twisted bow-shaped fibres appeared under the microscope or electron microscope. Examples are shown in Fig.28.

Bouligand was able to interpret this structure a frozen cholesteric liquid crystalline structure. The normal to the microscope slide, however, was not identical to the direction of the cholesteric axis along which the twist was occurring. The resulting section gave rise to peculiar patterns whose interpetation was far from obvious. Developmentally, Bouligand reasoned, the material first aligned into a (polymeric) cholesteric liquid crystal, which then solidifies, retaining its frozen-in orientational signature.

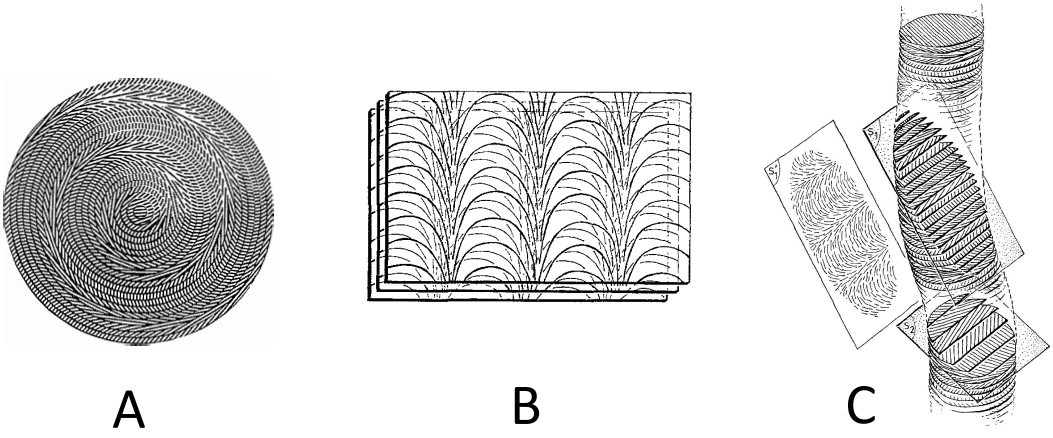

Figure 28: **A**: Drawing of the curious bow-shaped structures within fibres seen in many organic materials (Plate IIe from [258]). **B**: Same as A, but on a finer scale, reproduced from [259]. **C**: Diagrams from Ref. [259] demonstrating that the observations are explicable in terms of a material with cholesteric structure, but with the the sample at an angle to the viewing direction.
Subfigure **A** reproduced from [258] with permission Comptes Rendus.
Subfigures **B, C** reproduced from [259] with permission; ©1968 Springer-Nature.

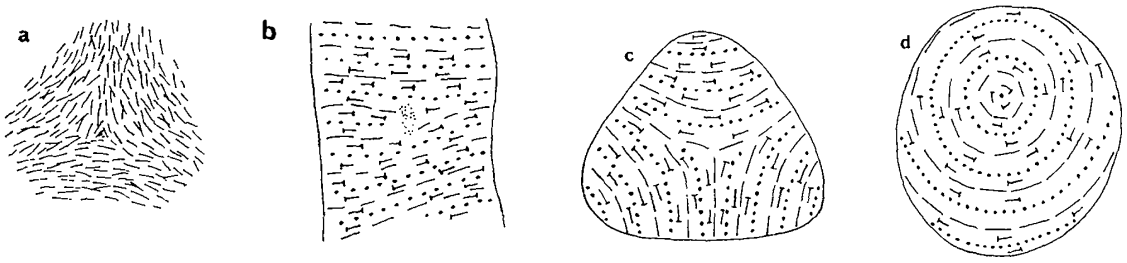

Figure 29: Defects appearing in thin section in various biological materials, from Ref. [260] (reproduced with permission EDP). More accurate details and sources given in original paper. (a) $-\pi$ disclination, equivalent to a screw dislocation, in a placental moth membrane; (b) screw dislocation in a microplankton chromosome; (c) $-\pi$ disclination in microplankton DNA; (d) So-called λ^{2+} disclination in root nodule of a *Vicia* legume.

To see this required not inconsiderable geometric intuition. What directed Bouligand toward liquid crystals, and whence came his intuition? The answer is that Bouligand was no ordinary naturalist, for he came with significant genetic academic and academic pedigree. On the one hand his father was the distinguished mathematician Georges Bouligand (1889-1979), one of whose specialities was differential geometry. And on the other, as a zoological microscopist, he was acquainted with François Grandjean, liquid crystal pioneer from the early years of the century and colleague of Georges Friedel. Bouligand would author Grandjean's obituary when he died in 1975 at the age of 93 [261]. After leaving the liquid crystal field in the 1920s, Grandjean was to become the world expert in acarology (the study of mites). Thus was Bouligand directed

also toward a purer study of liquid crystalline textures, with several collaborative papers with Maurice [92, 262], and other physicist liquid crystal colleagues; see especially the frighteningly mathematical ref. [262].

In 1975 Bouligand addressed specifically the question of defects in what he called the "cholesteric analogues" in biological systems [260]. He was able to interpret micrographs from a number of different authors in terms of specifically cholesteric liquid crystalline textures, as shown in Fig.29. For a more recent overview of the role of cholesteric liquid crystals in living material, we refer the reader to Michel Mitov's review [263].

6.4 Defects, Active Matter and Cell Biology

This is an area of an enormous amount of current research. Any attempt at a complete review would thus be out of place. The best we can do in such an active field is to swoop on recent examples which may (or may not!) demonstrate some key principles. Our purpose here is twofold. One the one hand we wish to convince the sceptical reader that ideas developed in the physical sciences and pure mathematics look like they may develop a second life in the biological sciences. Secondly, we make here a specific link between liquid crystalline defects on the one hand and biological function on the other.

For a general theoretical background on active matter, readers are referred to the review by Marchetti et al [264]. The role of liquid crystals in cell biology seems to be a particular current "hot topic". The subject was reviewed a decade ago by Rey [265]; indication of the considerable subsequent progress can be seen in the reviews by Zhao et al [266] and by Doostmohamadi and Ladoux [248].

Spatially inhomogeneous periodic chemical reactions:
We first stress that defects are not, of course, exclusive to liquid crystals. Simple multi-component chemical reaction equations of the much-cited Belousov-Zhabotinski type admit periodic solutions (e.g. [267]). These are essentially non-equilibrium models, involving constant material input and output. Such mathematical models are regarded as caricatures of periodic biological phenomena, such as the circadian rhythm, the heartbeat of many multicellular animals, brain waves, or the menstrual-oestral cycle occurring in some mammals. Mathematically the periodic solutions are limit cycles. When in its steady-state limit cycle, at any given time t the system can in principle be defined by a phase $\phi = \omega t$, where ω is the angular frequency of the oscillation.

If now we suppose the system may in principle extend in space, and add differential material diffusion to the mix then we have no guarantee that the periodic oscillations at different points in space will coincide. The local limit cycle will have an amplitude (roughly speaking, the distance in parameter space from an unstable equilibrium). There will be points at which the amplitude is zero, the phase is undefined, and round which a closed path changes the phase by multiples of $2n\pi$, where the commonest values of n are ± 1. We show in Fig.30 an experimental realisation of this phenomenon [268].

The defects at which ϕ is undefined are temporal analogues of $S = \pm 1$ defects in magnetic models or superfluid He^4. But the time aspects complicate the story significantly. At the very least, the defects are not stationary and spiral waves of advance emerge from them.

In fact, even if a chemical system would otherwise admit no particularly anomalous behaviour, the introduction of differential material diffusion can give rise to pattern formation. This idea was introduced in the classic and enormously influential paper by the renowned polymath Alan Turing (1912-54) in 1952 [269, 270], explicitly as a caricature of a model for biological morpho-

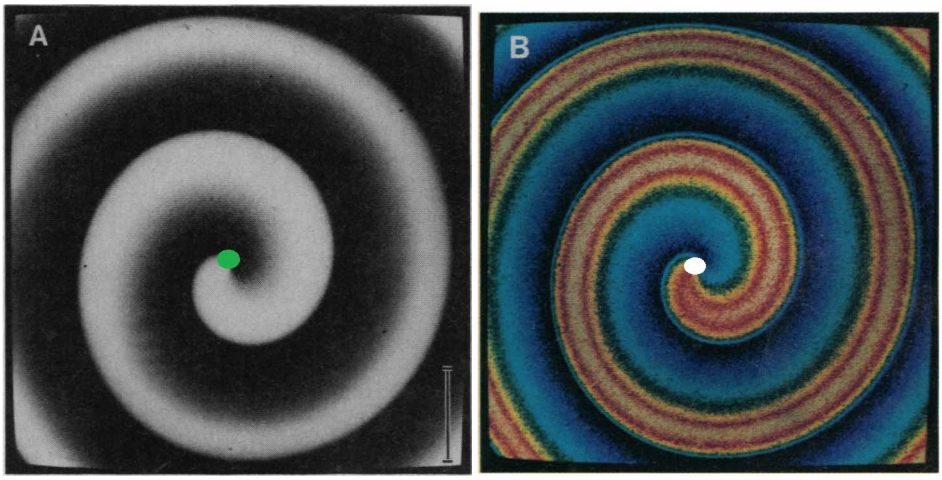

Figure 30: Digital image of defect acting as an organising centre for a spiral wave in the Belousov-Zhabotinski reaction in two dimensions. The sample is in a thin Petri dish. Left image: transmitted light intensity; Right image: the pseudocolour indicates contours of constant chemical concentrations, or equivalently the phase contours of the local Belousov-Zhabotinski oscillation. The dots (added by present author) in the centres of the diagram are locations where the phase is not defined.

genesis. Turing was strongly influenced by D'Arcy Thompson (whom he cites in the liteature, but does not otherwise mention!). More complicated systems are chaotic, with long-lasting quasi-periodicity, and this has been a subject of much interest. Mathematical approaches to e.g. epilepsy or heart failure, have speculated about the anomalous development of defects in the organ in question.

Active Matter:

From a theoretical perspective a very important innovation is the development of theories of so-called "active matter". To put this in context, recall that in usual (dissipative) fluid mechanics, turbulence causes long-wave fluctutations to dissipate into smaller and smaller eddies, before dissipation finally transforms fluid mechanical into thermal energy. This is known as the *Kolmogorov cascade* (see e.g. [271]). Although we do not pursue the analogy here, an eddy is, of course, a defect line or ring in the fluid velocity field.

But in living organisms we know that systems, although in (quasi) steady state, they are not in thermodynamic equilibrium. Energy is introduced into the organism; chemists know the mechanism, but physicists only need to know that it occurs. The energy is released through a molecular chemical process – a short scale process – and then, somehow this energy is transformed into something coherent which takes place on a longer length scale. This reversal of the Kolmogorov cascade seems to be a universal feature in living organisms.

Active hydrodynamcs is a theoretical framework which describes how this can occur. What happens when energy instead of *disappearing* locally in the form of heat (which can often be essentially ignored), instead appears locally? One can now model fluid equations simply a set of boundary conditions and some active forces or stress tensors.

This much is general to all hydrodynamic systems. Introducing polar or nematic order adds another degree of complication. The standard approach to the macroscopic dynamics of nematic liquid crystals is the Ericksen-Leslie-Parodi (ELP) theory (see e.g. [148]). This theory uses the

director $\hat{n}$ as the key state variable. The drawback when studying defects is that the defects often are points or lines of singularity of $\hat{n}$, and thus dissipation functions which include its gradients and derivatives may not capture some key features. This is particularly true close to defects, when gradients of the liquid crystal order parameter govern the dissipation.

The alternative approach is the so-called Q-tensor theory [272–274] (for an accessible introduction, see [275]). The mathematical disadvantage now is that outside any defect region, a system is constrained to lie on the equilibrium manifold, not by a hard constraint, but rather by a Landau free energy energy functional with a low valley along the "equilibrium" manifold. In the case of polar systems, likewise it is sensible to use a vector order parameter $\mathbf{p} = |\mathrm{p}|\hat{n}$.

Depending on the parameters of the system and the boundary conditions, an enormous variety of defect states can exist, and in addition there can be spontaneous defect motion. Examples, abstracted from Elgeti et al [276] are shown in Fig. 31

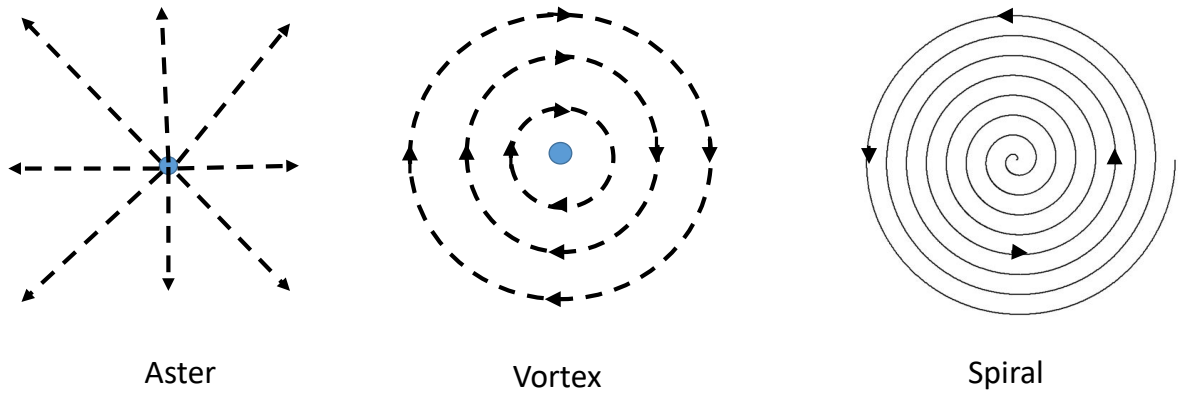

Figure 31: Polar defects in two dimensional active matter, idealised from theoretical results by Elgeti et al [276]. Arrows show field directions. If we identify field directions close to the defect with eigenvector directions at a critical point of a dynamical system, the different types of polar defect correspond to nodes, centres and stable/unstable foci respectively.

Alternatively one can examine specifically nematic models. Ravnik and Yeomans [277] studied flow patterns of an active nematic confined to a cylindrical capillary. The idea here is to postulate an active stress tensor proportional to the order paramater Q_{ij}, so that order parameter inhomogeneities drive flows. In simple non-nematic passive fluids subject to a differential pressure, this is the standard "flow in a tube" problem, studied experimentally by Poiseuille in 1838 and solved theoretically by Sir George Stokes in 1845. So it is a certainly a sensible place to start. The flow fields and nematic configurations are strongly coupled, with, for example, under some circumstances spontaneous formation of disclination rings. These are coupled but not coterminous with hydrodynamic vortices.

A related work [278], motivated by some experimental work on a fluid consisting of a dense suspension of bacteria [279], considers so-called "low Reynolds number" turbulence. In passive fluid turbulence, these workers assert, vorticity is advected in the fluid through mechanisms associated with the inertial terms in the Navier-Stokes equation at high Reynolds number. But in active nematic turbulence, they show, walls and defects may be vorticity sources; the vorticity then diffuses throughout the domain to generate active turbulence. Very recently, Aranson [280] has reviewed the specific role of defects in active liquid crystals [280].

Cell Biology and other biological Systems

We (here: physical scientists!) need to recall that biologists distinguish between experiments *in vivo* (i.e. in the living organism itself), and *in vitro* (i.e. in the "test-tube", broadly interpreted); to that has been added more recently *in silico* (i.e. simulation of a computer model). I shall here mix the first two somewhat promiscuously. The realisation that there is often some connection between liquid crystals and living organisms predates Bouligand's observations, and indeed goes back to the very foundation of the subject.

In 1888 the botanist Friedrich Reinitzer was grinding up carrots when he synthesised the first recognised liquid crystal, cholesterol benzoate [281]. Myelin is a fatty acid which surrounds nerve fibres, inflammation of which causes disabling diseases such as Multiple Sclerosis. In the early years of the last century Otto Lehmann discovered that it also turns out to be optically anisotropic [282, 283]. More than that, however, he was persuaded, albeit at that stage on inadequate evidence, of a fundamental link between liquid crystals and life. So persuaded, in fact, that he devoted two books [284, 285] specially to the subject.

In the 1940s, the Onsager theory of lyotropic liquid crystal phase behaviour was designed to explain experimental results on the the nematic ordering of the tobacco mosaic virus (TMV) [286]. Another initially unrecognised example involved (rod-shaped) human fibroblasts, an important type of cell playing a role in organ connection tissue and wound-healing [287], which order *in vitro*. Likewise we find liquid-crystalline-like structure in plant cell walls [288, 289], melanocytes (pgment cells in skin) and osteoblasts (bone cells) [290], spider silk precursor [265, 291, 292] and human granulocytes [293]; for a review see [265].

The ubiquity of liquid crystalline particles (either in the form of molecules or consequent from a high aspect ratio of the cell shape) suggests a deep functional relationship between liquid crystals and life. In the present context, we seek defects in the liquid crystalline matrices, and again ask about biological function.

Thus, for example, silk is produced as a liquid crystal polymer in the duct of a silk worm, and then extruded, at which point solidification takes place rapidly, preserving the nematic orientational structure (see e.g. [265, 292]). Deep in the duct, the nematic polymer has a homeotropic surface orientation, in principle producing an aster $S = 1$ disclination along the duct axis. The director in the centre of the cylindrical duct escapes into the third dimension, depriving the disclination line of its singularity, but the polar orientation of the escape varies in a regular fashion along the duct, thus inducing alternate hedgehog and hyperbolic point defects along the duct axis. However, as the nematic polymer is extruded, the defects disappear, so that eventually the fibres are well-aligned in the silk material. Perhaps it is the case that the point defects in the duct are mechanically necessary, but do not possess any deep biological significance? In order to answer this question, at the very least a more sophisticated modelling would be required, in which the processes of the production of the polymer, and the solidifcation of the polymer melt are both described.

A similar question can be asked of the plant walls, in which the cholesteric-like ordering also confers an elastic robustness. This is somewhat analogous to that of the artificial material plywood, also possessing a twisted structure; however, in plywood the twists are discrete, whereas the cholesteric twist is continuous. We recall that in crystalline inorganic materials, strength is often conferred through the presence of impurities which can prevent dislocations from sliding or inducing cracks, and work-hardening, which in some sense entangles dislocation lines, preventing them from spreading in catastrophic failure. Does anything like this happen in biological materials? Plant walls consisting of helicoid fibres [288, 289] do exhibit some defects. The individual fibres themselves may be defected, and in addition, there are numerous crossed fibres in a wall. Parts of this question have been addressed by Rey and coworkers [265, 294, 295]. Many of the

observed textures are consistent with a pre-existing liquid crystalline structure. Although I am not aware of specific studies, one must suppose that the presence of interlinking fibres, and the response of the fibres to the presence of impurities (e.g. plant cells, or inter-cell spaces such as "lumens" or "pit canals") do act to improve strength, and so there is some link between defect structure and biological fitness.

A particularly interesting recent study by Saw et al [296] involves a study of a monolayer of epithelial cells *in vitro*. Epithelial cells line the outer surfaces of organs, the inner surfaces of cavities. The example most familiar to readers will be skin cells. The epithelial cells can be thought of as elements in an (active) nematic medium, albeit one with high viscosities with slow time scales. Over time, epithelial cells are produced, die and migrate over the surface. A particular feature of epithelial tissue is that some cells die in programmed fashion; this is known as *apoptosis*. Apoptotic cells are extruded from the surface. There seems to be a coupling between surface shape and its local nematic state, through the medium of local stress. Specifically, cell as attracted to some defects where they bunch and are extruded. Apoptosis is a key homeostatic mechanism *in vivo*, and this work seems to provide specific role for nematic defects in this process. These authors also note that this coupling provides some hints for therapeutic strategy; placing the cells on an artifically patterned surface with defect seeds placed at particular points could be a strategy for therapeutic intervention.

Another context in which nature seems to make use of nematic defects concerns morphogenesis (i.e. development of form) in *Hydra*. This is a small freshwater organism in the phylum *Cnidaria*. It mostly reproduces by budding, and possesses the extraordinary property that it can often regenerate itself, even when deprived of 98% of its body mass. Perhaps unsurprisingly, we mere humans, who rather dramatically lack this capability, are curious as to how they do it. Maroudas-Sacks et al [297] find that there is a supracellular actin fibre matrix, which can be thought of as an active nematic. Topology (essentially the Euler characteristic in another guise) constrains the surface charge at +2, and in principle all types of surface defect may occur. The mature *Hydra* apparently only contains +1 and $-1/2$ surface defects. But the immature (or badly amputated!) *Hydra* can also possess $+1/2$ defects, and these serve as organising centres for the growth of new tissue.

A similar mechanism, but very different biological system was studied by Copenhagen et al [298]. Here the topological defects promote layer formation in bacterial clusters in an unfavourable environment. The last two studies have attracted an enormous amount of interest, and has led to speculation – indeed detailed calculation – that "defect-mediated morphogenesis" is a widespread phenomenon [299].

The last two cases are examples in which the defect physics plays an essential role in the biology of the organism. But there are many other recent papers [300, 301] in which cells which resemble nematic ("spindle-like") or polar molecules are present, where the phase properties are important. Defects are present, they may play an incidental or crucial role, but at this stage we cannot be sure.

The quotation at the top of this section comes from a paper entitled "Physics of liquid crystals in cell biology [248]. The full quotation is as follows:

> ... Physics of liquid crystals has emerged as a burgeoning new frontier in cell biology over the past few years, fuelled by an increasing identification of orientational order and topological defects in cell biology, spanning scales from subcellular filaments to individual cells and multicellular tissues...

We await future developments. ...

7 Afterword

Maurice Kleman had a love of learning, and of matters intellectual in domains well beyond his immediate area of expertise. This commitment was evident to those who knew him and those who attended his lectures. One of the ways in which he reconciled his interest in his Jewish background with his profound lack of religious belief – *Juif mécreant*, as he described himself in his autobiography – was an interest in Spinoza's philosophy. Benedict (or more correctly, *Baruch*) Spinoza (1632-1677) was a Dutch philosopher of Sephardi Jewish background who is widely appreciated in the present day for his philosophical works, some of which were only posthumously published. In 1652, Spinoza was expelled from the Amsterdam Jewish community for his heterodox beliefs; according to recent reports in the press the *cherem* remains to this day [302].

Maurice's interest in Spinoza is preserved in the professional literature by his review of a new biography by Stephen Nadler [303]. Maurice writes with approbation of Spinoza's intellectual integrity, specifically his refusal of conversion to Catholicism, despite its rather significant social advantages. These advantages were sufficient to attract many of similar background whose religious commitment of any sort was relatively weak. Spinoza did not, however – in that he was a creature of his time – reject religion (unlike, say, the eighteenth century Scottish philosopher David Hume). But he did reject the special status of the Hebrew people (no doubt one reason for his unpopularity with his home congregation!), and his views concerning the nature of the Universal Deity were decidedly unconventional in a seventeenth century context.

Spinoza moved in religiously dissenting circles, and worked as a lens-grinder. One of his most distinguished clients was Christiaan Huygens, who appreciated his skill but referred to him in letter to others as "the Jew of Voorburg" or "Our little Israelite" [304]. The labels were perhaps meant to be descriptive, rather than prejudicial, but nevertheless accidentally confirm that, expulsion or no, from some clubs one can never properly resign. In the last 15 years of his life Spinoza corresponded with some of the Great and the Good of his philosophical and scientific contemporaries, including the chemist Robert Boyle, Henry Oldenburgh, who was Secretary of the Royal Society of London, and the philosopher-mathematician Gottfried Wilhelm Leibnitz.

The letter we quote from to finish [305] formed part of a correspondence with Willem van Blijenbergh (1613-94), a grain-trader and amateur theologian from Dordrecht:

> So far as in me lies, I value, above all other things out of my own control, the joining hands of friendship with men who are sincere lovers of truth. I believe that nothing in the world, of things outside our own control, brings more peace than the possibility of affectionate intercourse with such men; it is just as impossible that the love we bear them can be disturbed (inasmuch as it is founded on the desire each feels for the knowledge of truth), as that truth, once perceived, should not be assented to.

Times have changed; to avoid the risk of public denunciation, we would nowadays replace "men" by "people"! But with this exception, the sentiment sums up Maurice's interaction with his colleagues and with the scientific truths he sought to establish.

Notes and Acknowledgments

Acknowledgements

I would like first of all to thank the editors of this issue, Oleg Lavrentovich, Randy Kamien and Corrie Imrie, for inviting me to write this paper. I had long been an admirer of Maurice Kleman. The task of summarising his work has only increased this admiration. I hope that in this essay I have been able to do him justice, both as a scientist and as a human being.

Over and above the issue editors, I first thank the late Maurice Kleman, the subject of this essay. The biographical sketch in §2 owes much to Maurice's autobiography *Chronologie d'un physicien* [14]. The autobiography, which gives insight into both Maurice's intellectual and his emotional life, well repays the read, although unfortunately it is not available in English. My biographical reading was significantly augmented by discussions with colleagues, and by my own memories of discussions with Maurice. I am particularly grateful for the significant and patient assistance from Maurice's son Jean-Philippe, and from Maurice's partner Madeleine Veyssié. In addition, I would like to single out Claire Meyer, who was kind enough to provide me with a detailed list of Maurice's publications.

Some forty years ago, before we were personally acquainted in any meaningful sense, Maurice's writings were the first to arouse in me an interest in liquid crystal defects. Over the years we enjoyed long discussions on this subject, its history, the history of science in general, and in fact other subjects of common intellectual and emotional interest. His 1977 text on *Points, Lines and Walls* [191], in the original French (rather ragged by now, but not doing so badly, all things considered) has accompanied me across Europe and back, and continues to be an inspiration. In a scientific context it was Nils Schopohl who persuaded me that he could transfer experience in superconductivity to enable us to think more fruitfully about defects in liquid crystals.

I owe my interest in Vito Volterra to the historian of science Judith Goodstein. Parts of this article have been extracted (and much edited) from a longer (eventually unpublished) article on Volterra's scientific work originally intended to accompany her magnificent biography of Volterra [24]. The present article contains reworked versions of parts of: (a) a semi-popular article in Mathematics Today [306], (b) an article celebrating Claudio Zannoni's 70th birthday celebration [89]. I apologise to readers for repetition in the pursuit of a self-contained text.

My interest in liquid crystals first emerged from a study of molecular fluids at interfaces, at which simple fluids, isotropic in the bulk, nevertheless exhibit nematic ordering. It was nurtured by discussions in Bristol with Charles Frank, and in Southampton in particular with Geoffrey Luckhurst. I owe a particular debt of gratitude to David Dunmur, who has shared much of my journey through liquid crystal history. David encouraged me to think about the history of the subject when, as editor of "Liquid Crystals Today", he asked me to write an afterword (although I now think it rather imperfect) to Charles Frank's career following his death in 1998. David savaged an earlier version of this essay. I am sure he will not be satisfied by the final version either. However, notwithstanding our differences over this particular issue, his advice has remained essential..

In writing this paper, I have needed considerable help in understanding or revising concepts in pure mathematics, theoretical physics and biology. I have also sought memories from key players in the development of important ideas in the theory of defects, as well as from members of their families. In so doing I have benefited from helpful correspondence and conversation with: Amit Acharya, Gareth Alexander, John Chad, David Chillingworth, Mark Dennis, Jean Friedel, Philip Greulich, Roger Horn, Randy Kamien, Efim Kats, Claire Meyer, Oleg Lavrentovich, Thomas Machon, Vladimir Mineev, Graham Niblo, Pawel Pieranski, Valentin Poénaru, Victor Reshetnyak, Chuck Rosenblatt, Doug Ross, Tyler Shendruk and Grigoriy Volovik. I also acknowledge (older) correspondence with distinguished colleagues no longer with us, in particular Yves Bouligand, Charles Frank, Jacques Friedel, George Gray, Frank Leslie and Misha Monastyrsky. Finally I apologise particularly to some close colleagues of Maurice Kleman, and to many others who played a role in this story, all of whom I should have consulted but was not able to as a result of time constraints.

Notes

While preparing a professional volume on the history of liquid crystal science coauthored with David Dunmur and Horst Stegemeyer (*Crystal that Flow* [16]), I consulted Maurice on various historical issues. He was kind enough to put me in contact with Jacques Friedel, who had both personal and professional expertise in this matter. I think that Maurice primed him to receive enquiries from me. The result was a correspondence with Friedel that was extremely helpful for our work.

When the volume appeared in late 2004, we received a large number of approbatory emails from around the globe. I had (*inter alia*) sent a complimentary copy to Friedel. This was the least I could do, for a copy of his autobiography, which he had kindly lent to me, had disappeared in the mail between Southampton and Paris. After some time, the Royal Mail delivered a large envelope containing a detailed, closely argued, extended, critical epistle. This latter, ten pages, written with a fountain pen in tightly spaced letters, in a slightly uncertain hand, and with many diagrams, came from Jacques Friedel.

The gist of his complaint was that we had undervalued the importance of defects in the history of liquid crystals. His colleague Kléman, who had *almost single-handedly* developed this theory, was not receiving the acclaim that was his due. He was, of course, partly right, but there were reasons for the under-emphasis: too many parallel narratives ruin a good story. I hope, however, that this essay goes some way to compensating our sin, even though Friedel himself is unfortunately no longer around to appreciate the effort.

Readers are warned that this essay is only a very rough draft of history. Given that it is written in the shadow of Maurice Kleman's passing, it is (deliberately) very Kleman-centred. Out of a very large number of papers on multiple themes, I have, at least to some extent arbitrarily, selected areas which I know about, and omitted work which, (who knows?) may become important (e.g. his work with Jonathan Robbins on the solar corona [307]). Likewise,the contributions of many of his collaborators have probably been undervalued. In the development of what I have called the "Homotopic Revolution", the contribution of the Russian school, and particularly that of A.M. Polyakov, has almost certainly not been given its proper due.

Also, I expect a heterogeneous readership. My exposition has probably drifted stochastically between undergraduate and the expert levels. My primary goal has been to discuss Maurice's work in the context of changing ideas of condensed matter defects, rather than to give a detailed account of the current state of knowledge in any of the fields that I have touched upon. For proper state of the art reviews, readers should search in the professional literature. In this context, I particularly recommend the recent "Liquid Crystals: New Perspectives", edited by Pieranski and Godinho [308], which addresses a number of defect questions raised during Maurice Kleman's career, as well as other papers in this memorial issue.

For this essay, I have borrowed liberally from a number of previous essays on the history of liquid crystals, both by myself and collaborators, and by others [16, 18, 88, 89, 110, 306]. I ask that readers forgive me for repeating ideas expressed elsewhere in the interest of producing a self-contained text. Likewise I request tolerance for omissions. These partly reflect my ignorance; I welcome emails filling me in on historical details I am missing and obvious conceptual misunderstanding that I betray. But clearly some things have been left out in order to restrict the length of this essay to manageable proportions.

Notwithstanding the copious professional interactions mentioned above, I stress that errors are due to me alone. I declare no conflicts of interest, other than those implicit in the paragraphs above.

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[66] The present author recalls a theoretical physics seminar in Bristol in the late 1970s or early 1980s, in which A.P. Young was presenting work (I think) on the two-dimensional XY model. Sir Charles, who had recently retired, was in the audience, on the front row (as ever, sucking on his pipe). It was necessary somehow to explain the concept of the Burgers vector. The speaker, at that time relatively early in his career, did not know that he was teaching the grandmother how to suck eggs. He got as far as explaining the concept, and then realised that he did not know why the Burgers vector as so named. His sentence petered out, as he guessed wildly, but in hindsight obviously incorrectly, that the Burgers vector had been so named by Burgers himself. At which point, Frank butted in at full volume: "not by Burgers, but by me, actually", with a strongly emphasised and megaphonic "me". At the time, I thought that the early 1950s were impossibly far off, but now at the other end of my career, I can see that early days of dislocation theory might well have seemed very recent to Frank.

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