**“When A Research Problem is Solved, Others are (Re)Introduced”. Review of G. Schurz (2019), *Hume’s Problem Solved: The Optimality of Meta-Induction*, MIT Press.**

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Mathematical psychology often adopts models, techniques and ideas from the social and natural sciences. What about philosophy? Yes, after all, Thomas Bayes was a philosopher. Epistemology? Well… One can think of Bishop and Trout’s (2005) *Epistemology and the Psychology of Human Judgment*, which synthesized findings from the field of judgment and decision making, including simulation and analytical studies of psychological models, in order to make epistemology more aligned with what was known from psychology. But the point of this interesting exercise was for epistemology to learn from psychology, not the other way around.

Enter Gerhard Schurz. He is a philosopher of science, with expertise in formal epistemology. Schurz’s book targets the problem of induction, and more specifically David Hume’s critique that it is not possible to logically justify induction in a non-circular way (i.e., without again using induction; Chapters 1-3). Numerous verbal attempts have been made to solve Hume’s problem. Schurz takes an analytical route. Now you might be thinking that there could be something here for a mathematical modeller of cognition, as inductive inference is a key function of the mind. There surely is, as Schurz studies extensively a quite general model of inference, called prediction games (Chapter 5). Those games can be roughly viewed as numerical time series where multiple forecasting methods exist, feedback is given and methods can learn or be misled from each other. Cognitive modellers, especially those who favour proofs, will find the results of the book well founded (Chapter 4), interesting and rich (Chapters 6 and 7), inviting the development of extensions.

But these are not the main motivations for introducing this book here. In his study of prediction games, Schurz analyses well-known formalisms in decision research: heuristic, linear and Bayesian models. In the way he does so, I argue in this review, Gerhard Schurz (re)introduces some old, nagging questions in decision research. Schurz’s main concern is how to construct, from a set of methods the decision maker knows, a new method that would be most accurate in the future. In the book, this is the question of meta-induction. Schurz defines the optimality of meta-induction and derives conditions for it. This is when he really opens up a can of worms, all too familiar to decision researchers: What kind of optimality would a functioning mind be exhibiting? Can heuristics be optimal or near-optimal? Are these the right questions? The book provides explicit answers to some of these questions and implicit ones to others. Deconstructing these interesting answers, or the equally interesting lack thereof, might give a chance for a new start in decision research.

**1. What kind of optimality would a functioning mind be exhibiting?**

Duncan Luce (1997) justified efforts to model the mind by pointing out that human behaviour has structure, and mathematics can describe structure. The readers of this journal must agree with that. Furthermore, the majority of cognitive psychologists would agree that the human mind uses its structure to fulfil functions. How to model these cognitive functions? Some favourite answers involve *optimality* (Anderson, 1990; Chater and Oaksford, 2008; Tenenbaum et al, 2011). In this approach, a functioning mind is supposed to solve problems optimally, in some well-defined sense. For example, the mind might be aiming to make decisions that maximize subjective expected utility given one’s preferences or to combine information in order to update probabilities via the rules of Bayesian inference according to one’s priors and generative model.

The optimality approach has been criticized. Herbert Simon (1956) identified functional cognition with cognition *adapted* to its environment and evolutionary theory does not view adaptation as globally optimal. In fact, Simon and others see human rationality as satisficing rather than optimizing (Gigerenzer and Selten, 2002). Good, functioning, well-adapted thinking certainly need not be optimal, and in fact it seems absolutely daunting to even try to define a super optimization problem encompassing a person’s multitude of objectives and criteria. So, the idea of optimal human behaviour must be metaphorical, to be used as a device for generating insight about how functioning behaviour and its underlying cognitive processes could look like (Chater, Tenenbaum and Yuille, 2006). But it is unclear what has been learned—or could be learned—about processes (Jones and Love, 2011) and how accurately can behaviours be predicted based on optimality approaches (Katsikopoulos and Gigerenzer, 2008).

Despite such shortcomings, the optimality approach remains a well-worked out technique for spawning interesting hypotheses about human cognition. For example, Sanborn and Chater (2016) have been advancing the thesis that basic, probabilistic sampling can suffice for approximating Bayesian reasoning. If something of this sort turns out to be correct, it could help reconcile Bayesian/optimizing and heuristic/satisficing approaches since heuristics typically rely on basic sampling as well.

Schurz seems to also take it as a given that optimality is a good way forward for modelling the function of human induction. But he puts forth a new kind of cognitive optimality, the optimality of *meta*-*induction* (Chapter 5; pp. 82-85): Instead of trying to figure out how to choose a method that will maximize accuracy for the next time period, Schurz asks how to *construct* a meta-method that will maximize accuracy over the long run. This maximization is defined over the space of primitive methods that the decision maker knows (in Schurz’s terminology “has access to”) plus the meta-methods that are combinations of the primitive methods. The versions of prediction games and loss functions studied are plenty so that, under some conditions, each of the usual suspects of decision research turn out to be optimal—Bayesian, linear as well as heuristic models (Chapters 6 and 7).

A paradigm that addresses meta-induction in psychological decision modelling is *ecological rationality* (Todd, Gigerenzer and the ABC research group, 2012). The core question of ecological rationality is under which conditions is one decision method more predictively accurate than another decision method. This question is both a special *and* a more general case of Schurz’s meta-induction question. It is a special case because it studies a particular way of constructing a meta-method—choosing just one of the primitive methods. The ecological rationality question is a more general case of the meta-induction question in two senses. First, it does not constrain the space of decision methods only to primitive methods and their combinations. Second, the question of ecological rationality refers to predicting over a test set consisting of potentially multiple time periods, as opposed to only the current time period. In studies of ecological rationality, decision methods are calibrated once and for all in a training set and evaluated in a test set, while Schurz’s meta-induction analysis does not distinguish between training and test sets (although this distinction is discussed in the book on pp. 110-111). In principle, both set-ups of the prediction problem can be related to real-world tasks, and it would be interesting to map more specifically how.

These intriguing connections are not pointed out by Schurz, but he does dedicate the lion’s share of his comments on cognitive psychology to ecological rationality (Chapter 10; pp. 273-284). There again he does not reflect on cognitive optimality as psychologists often do. This lack of reflection is itself informative. And Schurz’s suggestion for the optimality of human meta-induction is a novel one. Could it be that a functioning mind is exhibiting some kind of meta-optimality?

**2. Can heuristics be optimal or near-optimal?**

The early days of heuristics in the psychology of judgment and decision making featured verbal definitions (Tversky and Kahneman, 1973), which could be interpreted and modelled in a number of ways. This flexibility supported a narrative suggesting that heuristics were wanting in terms of performance, despite empirical evidence to the contrary (Dawes and Corrigan, 1974). Oaksford and Chater (1994) explicitly put on the table the thesis that heuristic reasoning can be optimal. Gerd Gigerenzer and colleagues integrated these ideas with the model comparison/analysis methodology of machine learning and operational research, and built an evidence base and a theory for explaining the conditions for good heuristic performance “in the wild” (Gigerenzer, Hertwig and Pachur, 2011; Katsikopoulos et al, in press). Optimality was not a focus of this approach but it did come up along the way (Katsikopoulos and Martignon, 2006; Baucells, Hogarth and Carrasco, 2008). Today, asking if and when are heuristics optimal or near-optimal is a valid question, as much as it is open.

Schurz’s first result on meta-induction (Theorem 1, p. 113; see also Thorn and Schurz, 2019) provides a sufficient condition for the optimality of a meta-method called *imitate-the-best* that selects the most accurate primitive method so far, in analogy to the take-the-best heuristic that uses the most valid cue in the training set. Further optimality conditions for heuristics exist too but the bulk of optimality conditions in the book refer to linear combinations of primitive methods (Chapter 6), which can also have Bayesian interpretations (Chapter 7). As such, Schurz’s contribution would seem to suggest that heuristic meta-induction need not be optimal very often. On the other hand, we know that there is identifiability between heuristic, linear and Bayesian decision models (Lee and Cummins, 2004; Katsikopoulos and Martignon, 2006; Martignon, Katsikopoulos and Woike, 2008). A promising future direction would then be to investigate such formal relationships at a meta-level. Such work could also fruitfully interact with theories and experiments on mental architectures as, presumably, different decision models would be implemented differently in the mind or the brain.

So far, this review has somewhat indulged in questions of optimal cognition, with predictable touches on simple heuristics. Indeed, the connections and interactions between the research programs of Bayesian cognitive science (Chater and Oaksford, 2008) and ecological rationality (Todd et al, 2012) can be greatly strengthened, using as a leitmotiv the questions asked in Schurz’s book and here. But, beyond such low-hanging fruit, one can also take a step back and rethink things more generally.

**3. Are these the right questions? or, a chance for a new start in decision research**

The optimality questions discussed above are right of course, even only because of the fact that they have stood the test of time. But I am not sure that they are the only right questions, and, in this sense, they might not necessarily be the most useful or interesting ones.

Assuming that cognition is structured and functional, we, as psychologists, should strive to uncover those models people use that produce judgments and decisions that are in some appropriate sense *improved*. Improved relative to what? Some possibilities: Over time—ontogenetic or evolutionary—compared to other models—heuristic or optimizing—and on the bundles of problems people are facing in the wild (not in the lab)—get a job, find a mate, have healthy and nice children, climb the social ladder, self-actualize… These improved decisions could be locally optimal, or near-optimal, for a single problem but it might be a stretch to expect that they are globally optimal over bundles of problems. Conceptual issues should be addressed before a notion of local optimality is fleshed out. For example, “local” with respect to what? the parameter space? the problem space? something else?

Local optimality could be a good research direction. But is moving from global to local optimality, or something of the same sort, the real innovation that decision research needs? It somehow does not seem so. We might need new ways of expressing that one decision is an improvement over another. We had a go and made progress with ideas from economics, operational research and machine learning. Following Gerhard Schurz’s proposal of meta-optimality, we might wish to dive deeper into the field of epistemology, and start thinking about our thinking. I do not have any great suggestions for you, except for recommending that you buy this book and enjoy the serendipity.

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