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REPLY TO HANDS: ON THE ROBBINS–SAMUELSON ARGUMENT PATTERN

BY

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I am gratified that Wade Hands (2008) has produced a long and carefully considered response to just one chapter of my 2005 book, *Economic Theory and Cognitive Science: Microexplanation* (henceforth *ETCS*). His critique helps me to discover respects in which my exposition might have misled some readers and to thus learn where clarification is called for. As Hands reports, *ETCS* elaborates and defends a novel conception of what I variously call 'mainstream' and 'neoclassical' microeconomics, intended to bring it into closer coherence with the surrounding behavioral and cognitive sciences. He discusses at length my association of central elements of that conception with historical contributions by Lionel Robbins and Paul Samuelson. I indeed go so far as to name the methodological implications of my view[1] 'the Robbins–Samuelson Argument Pattern' (RASP). Hands then devotes most of his paper to elaborating respects in which, according to him, Robbins and Samuelson would not or might not have endorsed my conception of microeconomics.

Hands's grounds for thinking that this represents some of sort of flaw in my conception seems to me to rest on an element of obtuseness in his reading of the text. In particular, he completely ignores the fact that in *ETCS* I *criticize* both Robbins and Samuelson. Indeed, the text points out and analyzes several of my respective differences with Robbins and Samuelson, differences that Hands now rediscovers as a basis for rejecting my choice of labels for the methodological part of my thesis. Hands's critique thus seems to reflect an underlying principle to the effect that no contemporary critic should associate his or her general perspective with an historical precursor unless the new view is a perfect reproduction of the old one. This in turn seems to amount to demanding that everyone other than historians leave history utterly alone. (Toward the end of his paper, Hands comes close to asserting this principle explicitly.) That would be a very radical transformation of prevailing intellectual culture.

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[1]I do *not*, by the way, see my general account's methodological implications as its most important consequence. My book's central topic is the network of ontological relationships between the domains of economics and the surrounding cognitive and behavioral sciences.

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Hands comes closest to putting the core of his generic style of objection in the following passage:

> What [Ross] is trying to do . . . is . . . to build a modern, cognitive science–inspired version of rational choice theory for the 21st century, and my point is simply that such a construction project will not be successful if Lionel Robbins's defense of choice theory is used as one of the foundational pillars. One could of course just pour the modern philosophical concrete and then after the fact put a sign on one of the pillars that says "Robbins"—which often seems, inexplicably, to be what Ross is doing—and it would probably be harmless, but if one really intends the foundation to be constructed in a way that is consistent with the majority of the words that Robbins wrote, then choosing this particular pillar does not seem to be a good idea.

It is of course strictly a matter of judgment, on which there will be wide scope for disagreement, over how much concordance a contemporary theorist must display with an earlier view in order to be entitled to count that view as more 'on' than 'off' side with his or her own. No a priori *general* rule here could have much plausibility. In *ETCS* I am explicit about *which* aspects of my view were given main historical salience by Robbins and Samuelson respectively. It is because these aspects are *central* to my general account that I name that account's methodological implications after Robbins and Samuelson. This, it seems to me, should quite clearly establish the rules of the game for criticism on the point. If someone could show that Robbins and Samuelson did not in fact emphasize the specific features of economic theory I both attribute to them and share with them, then they would have clear grounds for accusing me of offering misleading appeals to the history of thought.

With respect to application of this principle, there is an asymmetry between the two parts of Hands's criticism. Hands doesn't claim that the aspects of Robbins's thought which I take up as central to my account were not emphasized by Robbins. He simply points out that Robbins and I disagree about various other matters. I don't know why this is supposed to constitute any objection to my account at all, especially since, as noted above, I point out these differences in the text myself. Where my relationship to Samuelson is concerned matters are more complicated, partly because Hands's generally sensitive and accurate reading of my general account wobbles out of focus in this part of his paper, and partly because Hands and I don't see completely eye to eye in our critical interpretations of Samuelson as a methodologist. In fact, these problems have a common source in Hands's failure to understand the philosophical perspective from which I evaluate Samuelson's behaviorism, as we will see. I will therefore address Hands's criticism of my discussion of Robbins very briefly, before devoting more attention to questions around the interpretation of Samuelson. So as to try to get more yield out of this exercise than mere defensive parrying, I will conclude by trying to draw some general morals for the relationship between philosophy and history of science, on which Hands also offers remarks in his paper.

Robbins

Hands correctly identifies the strands of 'neoclassical choice theory' (NCT), as emphasized in my general account of microeconomics, which I name after Robbins. These are Robbins's identification of the scope of the discipline with scarcity, and opportunity cost, as opposed to "wealth, money or . . . markets." Hands agrees with me that recognizing Robbins's identification of the economic domain with scarcity and abstract choice, rather than with money and material production, is both accurate and important to a reasonable history of economic thought. The specific significance of this theme in *ETCS* derives from my objective of contrasting classical 'psychologized' economics, which lives on in the work of Amartya Sen and most contemporary behavioral economists, against the alternative ('neoclassical') strategy I favor for integrating the current scientific understanding of the human mind with the conception of economics as an abstract, mathematical theory of relationships between agency in general and scarcity in general. (The theory in question is built around linear and dynamic programming, computable general equilibrium, and game theory.) With respect to that trajectory, I maintain, Robbins was a key transitional figure.[2]

Hands objects to the fact that my philosophy of economics differs strikingly from Robbins's in complementing the latter's emphasis on abstract scarcity with emphasis on abstract agency. As Hands points out, and as *ETCS* likewise makes clear, Robbins identified agency—or, at least, the epistemological basis for attributing agency—with what he took to be specifically human psychology, as opposed to the abstract properties of general relationships between integrated systems and goal-driven feedback to which I ultimately appeal. This is what Hands is getting at when he complains against me that Robbins's economics cannot be 'de-anthropomorphized.' However, I do not see why he thinks that any critical point is scored here. The account in *ETCS* acknowledges, at some length (pp. 91-100), that Robbins's economics cannot be de-anthropomorphized and yet remain literally *Robbins's*. This is for *roughly* the reasons Hands indicates (though I discuss the issues in much more detail): the basis for Robbins's strict ordinalism about preferences was inconsistent with his introspectionism and his anti-behaviorism. In the text, I aim to shed some charitable light on the logical cul-de-sac into which Robbins thereby works himself. I do so by appealing not to Robbins's philosophical commitments but to his economic ones. He saw that J. R. Hicks's indifference-curve analysis of preference was superior in deductive systematicity to classical Jevonsian sensationalism. His introspectionism otherwise should have led him to sensationalism, the historically dominant philosophical strand in British economics. Hands usefully reminds us that Robbins's introspectionism also rendered his selective skepticism about other minds—according to which we can know some qualitative properties of their contents but can't measure them, even indirectly—arbitrary and confusingly motivated. However, Robbins declined to value philosophical consistency ahead of what seemed to be a powerful new analytic technology with which to do economics: Paretian indifference curves interpreted following Hicks and R. G. D. Allen, rather than as reflecting diminishing marginal hedonic utility. Robbins thereby made the characteristic choice of the modern economist (and, indeed, of the scientist in general). This is the key respect in which I identify him as a transitional figure along the trajectory of development of ideas that I aim to make salient in *ETCS*.

[2]This does not imply a claim that he *intended* to be such a figure, since, as I say in the book, he did not know there was going to be any such trajectory.

The only reason Hands presents for disagreeing with any of this is that, according to him, Robbins didn't value *systematic* economic analysis as clearly as I claim he did. This issue in turn matters only because Hands wants to dispute my association of Robbins with the early, neo-Kantian, logical positivists, and that association is partly developed by reference to a common commitment to deductivism. This comes down to Hands assuming that in order for Robbins's view to be associated with neo-Kantian positivism we would have to find that he literally shared the agenda of Rudolf Carnap. This assumption seems entirely tendentious—surely finding agreement with, say, Otto Neurath would be good enough. In general, in this part of his discussion Hands illegitimately reads me as meaning 'formal' everywhere I say 'deductive.' He notes that the neo-Kantian version of positivism emphasizes deductive structuring of thought in order to establish the objectivity of reasoning based on perception (including introspection). He quotes me as identifying this way of defending the objectivity of economics in Robbins, and denies that Robbins had any such view.

This ignores some evidence. As I point out in *ETCS* (pp. 88-89), Robbins's basic charge against his principal theoretical enemies, the German historicists, was that they didn't acknowledge application of abstract categorical principles as a source of generalizations in economics. I acknowledge (p. 89) that Robbins doesn't promote *formal* reconstruction of economic theory, as Samuelson would do soon after him. So Hands is right, though it makes no point against me, to say that "Robbins never emphasized the formal or mathematical structure of economics." He fought Gustav Schmoeller and the historicists in exactly the same way that his neo-Kantian counterpart in psychology, Hermann von Helmholtz, fought those who denied the possibility of psychological and psycho-physical generalizations: by appealing to the power of deductive systematicity under general categories, in Robbins's case those of scarcity and opportunity cost. It is of course true that Carnap developed the ambition to reduce deductive systematicity to strict reliance on formalism, an idea Robbins shows no sign of entertaining. But my claim is only that Robbins's implicit philosophy was much closer to that of the neo-Kantian positivists (as opposed to, e.g., A. J. Ayer's empiricism) than the standard reading of him as a straightforward foil for Terence Hutchison typically allows. Robbins agreed with the neo-Kantian positivists (and the Austrian *a priorists*) in promoting an epistemology based on reflectively or introspectively discovered structuring properties that organize phenomena around deductive argument and generalizations about them. Later, in both philosophy and economics, introspectionism was dropped and deductivism turned into formalism. There were interesting logical similarities, briefly noted in *ETCS*, between these parallel slides from Robbins to Samuelson and from the early Vienna Circle to the Carnap of the so-called 'principle of tolerance.' I find nothing in Hands's criticism to motivate qualification of any of this.

Samuelson

Hands says that "overall, Ross characterizes Samuelson as someone who tried to move choice theory in the behaviorist direction; in consumer choice theory this took the form of substituting the notion of (observable) choice for (unobservable) preference and this view is broadly consistent with the standard reading of revealed preference theory. My main criticisms do not arise here." His objections are instead based on his argument that Samuelson would not have endorsed the microeconomic methodology, the RASP, that I outline in the final chapter of *ETCS*. But at no point in *ETCS* do I say that Samuelson endorsed, or should have endorsed, the methodology of the RASP. Rather, I name the RASP partly after Samuelson because it retains commitment to two core ideas that he made salient in mainstream economics: a behaviorist interpretation of preference, and treatment of aggregate demand as epistemically prior to individual utility. Hands doesn't deny that Samuelson emphasized these ideas—indeed (and confusingly), he stresses them in the very course of thinking he's arguing *against* me. So his objection to my associating my view with Samuelson's seems once again, as in the case of Robbins, to amount to his insisting that successive theories should never be associated with one another by any relation short of identity.

As Hands says, the key difference between the RASP and Samuelson's Revealed Preference Theory (RPT) is that the former mandates identification of particular objective functions of particular agents (indeed, defines particular agents by reference to particular objective functions), while the latter does not. The reason for this is straightforward: the RASP aims to take account of game theory and RPT doesn't. Any contemporary methodology for microeconomics would clearly be inadequate if it didn't make room for game theoretic modeling, since asymmetric information, non-identical strategy sets, and oligopolistic markets are among the real, and recurrently important, economic phenomena for which game theory is necessary to adequate modeling. In light of this, no contemporary microeconomic methodology could be identical to Samuelson's of 1947. It clearly does not follow from this that no such methodology could have important *affinities* with Samuelson's.

Hands's criticism would be reasonably motivated only if he could show that the affinities between the RASP and RPT are entirely or mainly superficial and semantic rather than substantive. This is what he tries to argue. The argument is complicated by his following Wong (1978) in distinguishing very sharply between Samuelson 1.0 (1938), who aimed to eliminate 'utility,' and Samuelson 2.0 (1947), who aimed to recover the generalizations of ordinal utility theory within behaviorist restrictions—that is, to construct individual indifference maps from observable behavior. Unlike Hands, I think that Wong's emphasis on this distinction greatly exaggerates its importance. I acknowledge in *ETCS* (p. 108) that Samuelson initially aimed to eliminate the concept of utility altogether. I also point out (p. 110)—*pace* Wong—that even Samuelson 2.0, had he been philosophically consistent, should have been an eliminativist about intentional states, because if we allow the mathematics of the 1947 *Foundations* to literally speak for itself then we find no *agents* in the formalism. (Ironically, then, I point out a degree of divergence between the RASP and RPT that Hands misses—yet I don't imagine that I'm refuting myself by doing so.) However, the intentional-stance functionalism (ISF) built into the RASP, although joining Samuelson 2.0 in allowing for legitimate talk of preferences, denies that preferences are internal states of people. According to ISF, preferences index agents' behavior to contexts of reference co-constructed and shared among members of interacting communities (especially including, where economics is concerned, contexts of market institutions and exchanges, and games).

It is clear from Wong's book that it never crosses his mind to imagine that preferences could possibly describe anything other than internal psychological states;

and this assumption is crucial to his insistence that Samuelson 1.0 and Samuelson 2.0 stand in flat contradiction to one another, from which no philosophical reconstruction could possibly rescue them.[3] Hands suffers from the same blind spot. Early in his paper he says, correctly, that "the intentional stance does not require the agent to 'really' have beliefs and desires sloshing about in their head," so his chances of avoiding Wong's false dichotomy between eliminativism and internalism about preferences and utility look hopeful. Unfortunately, he replicates it by presupposing a dichotomy of internalist realism and instrumentalism, where instrumentalism is merely the methodological cousin of the ontological thesis of eliminativism. I submit that this is the only reasonable way to read his complaint that "from Ross, you would get the idea that revealed preference theory is a way of getting people to *reveal their preferences*." This reading of me is completely inconsistent with the basic philosophical perspective that is extensively explained in Chapter Two of *ETCS*. Daniel Dennett's ISF as I explicate and defend it is a version of behaviorism. I emphatically do *not* think that behavior reveals inner intentional contents, properties of ghosts in machines, because I do not believe that such things exist in the first place. There is a trivial sense in which I would agree to say that behavior reveals preference: preference just *is* a particular way of describing (consistent) behavior. That is also what Samuelson thought—in 1938 *and* in 1947.

Hands's flawed reading of my view of RPT motivates closer scrutiny of the earlier account of Dennett and ISF that he provides in his paper. Such scrutiny yields clear evidence that Hands doesn't grasp the idea of philosophical externalism about intentional content. He maintains that if people don't have intentional states in their heads then ISF must collapse into instrumentalism, in which case the economic methodology I defend must recapitulate or update Milton Friedman's. However, as I spend much of Chapter 2 of *ETCS* explaining, externalism about content—which has become the *majority* view of intentional content among philosophers of mind—is not a version of instrumentalism. It is the thesis that intentional state ascriptions pick out *real*, generalization-supporting, relationships between patterns in agents' behavior and categorizations of environmental contingencies on which sets of agents converge through interaction. This is the philosophical foundation on which identification of individual utility functions in the context of the RASP is to be interpreted. Such an approach, I maintain, is not a mere philosopher's suggested add-on to microeconomic methodology once that methodology centrally includes game theory. Agents' shared knowledge of the payoff structures of game outcomes reflects convergent categorizations of environmental contingencies. On this basis the theorist attributes utility functions to the agents that rationalize the set of observed patterns of behavior, interpreted strategically. There is no need in doing this to add psychological hypotheses about mutually entertained internal representations taken to be semantic isomorphs to the contents of the utility functions. If there were, it would be mysterious, as I point

[3]In fairness to Wong, as of the time of his book philosophers of mind were just beginning to articulate the idea they called 'externalism about mental content' (Putnam 1975, Burge 1986), though it was subsequently evident that Wittgenstein and Ryle had had the insight earlier. For a less exploratory articulation of externalism, the reader should consult McClamrock (1995). Important related themes pertaining to non-instrumentalist and non-internalist interpretations of intentional explanation are found in Sehon (2005) and Hutto (2008).

out in *ETCS*, that we can usefully apply game theory to strategic interactions among literally brainless animals, such as starfish (pp. 262–264). A main objective point of Dennett's philosophy of behavioral science is to dissolve this sort of mystery. Hands unfortunately gives no sign in his paper of understanding this philosophy, and clear signs that he misunderstands it.

Even if Hands were to grant that there is something important he is failing to grasp, he would no doubt object that nothing like the sophisticated Dennettian picture is found in Samuelson. This is certainly true—but so what? Acknowledging this point mirrors my earlier remark that game theory is central to my account of microeconomics while Samuelson's RPT of 1947 is completely innocent of it. The RASP is an interpretation of microeconomic methodology updated to include game theory that nevertheless retains the behaviorist interpretation of the concepts of preference and utility found in RPT. That is why Samuelson is acknowledged in the label I construct for the doctrine. By what axiom of interpretation is Hands entitled to decree that that isn't a good enough motivation for my making this part of the intellectual lineage explicit?

Hands's least restrained bout of jeering at me for ahistoricism depends entirely on his philosophical blind spot. After he quotes me on attempts in the 1930s and after to measure utility functions in accordance with behaviorist interpretation of them, he hoots that

> [e]ven if one corrects the timeline and moves to the early 1950s this was not what Samuelson's revealed preference theory was about. Determining utility functions? Labs? Coefficients? Measurement? Where and when was any of this happening during the middle of the twentieth century? There was in fact surprisingly little empirical work on revealed preference theory—Koo (1963) and Koo and Hasenkamp (1972) for example—and most of it was negative.

What seems to be going wrong here is, again, Hands taking for granted (as his sentences following those just quoted clearly suggest) that what someone in a lab trying to measure preferences would *necessarily* be up to was inferring the existence and properties of specific internal representational states that govern behavior. Thus he doesn't count the experiment I in fact cite in *ETCS* as the key precursor to the explicit experimental study based on RPT, that of Thurstone (1931). (I in fact *do* note in the book that the result was negative, though this is entirely irrelevant to the point at issue here.) A 'Samuelsonian' experiment indeed shouldn't involve trying to peer inside people's heads; it should begin from observed patterns of demand, and construct utility functions with a view to predicting further observable patterns of demand. Perhaps Hands assumes that I have other sorts of experiments in mind because I avow that microeconomics will (eventually) be constrained by neuroeconomics. This would ignore every subtlety in my chapter-length explanation of why and how 'constrained by' does not at all imply 'reduced to' (pp. 317-375). This chapter—not the outline of the RASP—is intended as the core conclusion of the book, but I don't find any acknowledgment of its significance in Hands's paper, or any trace of its influence on his understanding of my version of behaviorism.

This complaint of mine against Hands generalizes: it seems to me that when he read the parts of *ETCS* other than those he explicitly criticizes, he wasn't paying close attention. At the end of his paper he claims that I fail to appreciate that

> *Foundations* was the most important single document in the neoclassical synthesis of Walrasian micro- and Keynesian macroeconomics. The problem is of course that there is no maximization (from individuals or any other ''agents'') going on anywhere in the Keynesian economics (or other ''business cycle'' models) of the 1940s. If economists were constrained to investigations of the sort allowed by Ross' [RASP] there would have been no Keynesian revolution. In fact the most important shift in attitude between Keynes's Cambridge approach and the earlier approach of Marshall and Pigou was that it is possible to do really important work and make the world a better place without measuring individual utilities (by doing macro). Samuelson's *Foundations* was in fact intended to be a systematic mathematical foundation for all of economic theory—that which involved maximization (micro) and that which involve only aggregate functional relationships that were not grounded in (or ''stanced'' by) optimizing agents (macro).

This is remarkable. The final two chapters of *ETCS* explain, among other things, why the RASP is an account of microeconomics only, and why, according to me, *macroeconomics is more fundamental than microeconomics*. Here are two representative quotes:

> In denying mereological reductionism, I deny that there are general philosophical grounds for expecting macroeconomics to collapse into microeconomics, or for thinking that unless it does so macroeconomics is not serious science . . . (p. 318).

> People, like countries—and for the same reason—are, from the economic perspective, *macroeconomic* objects in the first place. The first-order properties of these objects, as will be explained in the next volume, are things like savings rates, personal accounts and balances of payment, average system-level interest rates, and so on. *Microeconomic* analysis will have useful things to say about people *just insofar* as their behavior sometimes, or in some kinds of situations, approximates that of economic agents, neoclassically conceived (p. 381).

At the very end of my book, I explain that *Microfoundations* is only the first part of *ETCS*. The final volume (following an extended empirical application of the RASP in Ross et al. 2008) will be about 'macrofoundations.' Thanks to Hands's helpful way with words above, I could well describe the project as a whole thus: *ETCS* is in fact intended to be a systematic philosophical foundation for all of economic theory—that which involves maximization (micro) and that which involves only aggregate functional relationships that are not grounded in (or ''stanced'' by) optimizing agents (macro).

I am thus grateful to Hands for clarifying the extent to which my project is even more firmly lodged in the tradition set by Samuelson's than the book makes explicit.

History and Philosophy

The concluding paragraphs of Hands's paper are, especially in contrast to his muddled reading of my interpretation of Samuelson, astute. His characterization of my general philosophical standpoint as ''essentially a realist Dennettian version of the neo-Kantian approach [Ross] ascribes to the early positivists'' is, along with the

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gloss that immediately follows these words,[4] exactly right—indeed, elegantly so. I also applaud the way in which he picks up on my remark in the book about the "delicate tension" that is "recurrent … between history and philosophy":

> It seems that one can only build an interpretation that is anchored "in the actual history of economic theory" and does not exhibit "any radical, across-the-board discontinuity or sudden paradigm shift" if in fact the "actual history" does not exhibit such changes. If the actual history does exhibit discontinuities and the story ends up being one of continuity—say one based on a RASP that remains invariant across almost one hundred years of economic theorizing—then the tension has been dissolved in favor of philosophy.

I would accept the verdict of Hands's final few words here only if I were prepared to agree that there is ever *one correct* history of a body of science. There is not. Histories intended to explain the actual biographical motivations of past thinkers will emphasize different patterns and possibilities from histories that take advantage of hindsight and explicitly search for the logical origins of what we know has happened since.

I address some of the differences between rational reconstruction and 'straight' history of science in the first chapter of *ETCS*. Scholars engaged in the latter must carefully try to forget that they know how the story later played out, since this knowledge will tend to distort accurate perception of the scientists' relatively myopic perspectives. By contrast, philosophical history—rational reconstruction—*is* teleological (or, as I say in Chapter One of *ETCS*, "whiggish"). It searches for tendencies in the development of scientific thought that were influenced by conceptual logic. It is frank about the fact that *we* are in a much better position to discern these tendencies and this logic than were the people who couldn't know where the story was going to go. Such history starts from where we now find ourselves and looks for anticipations in earlier problem settings and conceptual evolution—looks, that is, for the growing seeds of the present in the relatively chaotic past where they were difficult to distinguish from seeds that didn't germinate. Compare, by analogy, a biographical historian writing about Napoleon's conduct of the Battle of Austerlitz with an account by a Staff College lecturer interpreting the Emperor's tactical innovations for the benefit of officers in training fifty years later, with the half-century of refinements of his tactics by other generals crucially informing what is now salient.

Logical tendencies in past science are infinitely dense, and so *definitively* identifying them is impossible. This is what I mean when I say in the book that my history has a "spin": I choose to emphasize some tendencies and let others remain in the background. This does not require denying that they're there. Thus, for example, when I depict Robbins as making some commitments that we can see as beginning the slide towards Samuelson's behaviorism, this does not involve my ignoring the fact that Robbins rejected behaviorism. (Indeed, it's from Robbins's point of view, not mine, that it is a 'slide' rather than 'progress.') Another philosophical historian might

[4]"There are real patterns in the phenomena and what successful science does is to maintain a unity of structure across time and variation in those patterns. The patterns are real but they change with time, technology, evolution, social interests, and a variety of other things. Scientific knowledge involves relatively invariant structures that effectively instantiate these various patterns."

emphasize Robbins as the last in the line of major pre-Keynesians, since in some very important respects nothing in economics could ever be the same again after Keynes. There is no contradiction in Robbins being a transitional figure with respect to some tendencies, a precursor with respect to others, and a terminus with respect to yet others.

Someone trying to write a reasonably complete 'straight' history of the relationship between neoclassical economics and psychology would need to describe the intellectual adventures of a much fuller cast of characters, beginning from W. S. Jevons and Leon Walras, with a complex detour through Alfred Marshall and then onto Vilfredo Pareto, Irving Fisher, and Hicks. Robbins, in this version, might fade into relative shadows, since in general he was a far less important precursor to Samuelson than was Hicks. But the historical aspects of *ETCS* are motivated by the *traces* of that more complete history that are emphasized in economists' *current* self-representation. Microeconomics texts typically give Robbins's definition of the subject and then make Samuelsonian noises about the relationship between preference and utility. I reconstruct Robbins and Samuelson, in part, so that my account of where economic theory seems to be going begins from where it is actually widely perceived to be now. Robbins and Samuelson are part of the culture of present economics, and a philosophical reconstruction of the logic of that culture is constrained to both respect and make sense of this.

So, at the most general level of abstraction, the underlying logic of the historical chapter of *ETCS* is as follows. Along with sociological, political, and cultural forces, logical pressures played a part in driving the evolution of economic theory from Jeremy Bentham to the present. We can attempt to understand the evolved conceptual space by whiggishly reconstructing the history. A full history would be a rich and ambitious work and I do not attempt it. Instead, I concentrate on the two most methodologically self-conscious and methodologically influential (in *hindsight*) economists of the trajectory: Robbins and Samuelson. I call the resulting synthesis the 'Robbins–Samuelson argument pattern' not because I think Robbins or Samuelson would endorse it, but because contemporary economic philosophy (that is, the implicit philosophy entertained by many economists) is fundamentally built around elements of Robbins's and Samuelson's methodological visions for economics, and my project is to show how that philosophy has resources for establishing a form of working relationship with cognitive science that is very different from the prevailing orthodoxy in behavioral economics, which instead harkens back to the classical economists.

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