Journal of the History of Economic Thought,
Volume 31, Number 1, March 2009

REJOINDER TO ROSS: MORE ON THE ROBBINS–SAMUELSON ARGUMENT PATTERN

BY
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Since Don Ross introduces his reply (Ross 2009) by thanking me for my "carefully considered response to just one chapter" (p. 93)[1] of his book, perhaps I should return the favor by thanking him for his carefully considered response to a few selected parts of my paper. Ross organizes his comment into four sections: introductory remarks, a short section on Lionel Robbins, a longer discussion of Paul Samuelson, and a final section on history and philosophy. I will preserve his framework, in part because of its convenience and in part because I think the division is essentially the right one. I agree that our differences are less substantive on Robbins than Samuelson and I also think it is proper to conclude on historiographical issues (as I did in my original paper).

It is difficult to know how to respond to the various broad accusations contained in Ross' introduction. It is suggested that I believe 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" (p. 93). I have no idea what a "perfect reproduction" in this sense could possibly mean—perhaps the "bit-map transcription" of Dennett's real patterns (Dennett 1991) or Ross' "rainforest realism" (Ross 2000; Ladyman and Ross 2007)—but however one might define it, I obviously suggested (or think) no such thing. Of course, there is no "a priori *general* rule" about "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' with his or her own" (p. 94); it is, as Ross says, always "a matter of judgment" (p. 94). I completely agree, and would add that it is ultimately the judgment of the intellectual community that decides whether the historical argument is persuasive and the reconstruction is reasonable. My point was that even though it is a matter of judgment (and I would add, context), we do have some information about how scholarly communities have made these decisions in the past and it seems that a minimum condition for acceptability has been that the particular thread that the author wants to highlight in the broad tapestry of an economist's professional work (or a single text) should be either a thread that has been identified by other observers, or one that others can identify once the author helps them focus attention on the right spot in the argument/text/work. I simply

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[1]Page references without further citation information refer to Ross (2009) throughout.

ISSN 1053-8372 print; ISSN 1469-9656 online/09/01000105-114 © 2009 The History of Economics Society
doi:10.1017/S1053837209090087

https://doi.org/10.1017/S1053837209090087 Published online by Cambridge University Press

argued that Ross's reconstruction didn't (and couldn't) satisfy even this minimalist standard.

Robbins

Ross says that "If someone could show that Robbins and Samuelson didn't 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" (p. 94). As I will argue in the next section, I remain convinced that this is exactly what I did in my discussion of Samuelson, but I am willing to admit—in agreement with what seems to be Ross' own assessment—that our differences over Robbins are more about the relative importance of particular aspects of Robbins's position than about whether particular arguments/threads were, or were not, in Robbins work at all. Ross and I both recognize three main themes in Robbins (1935): his scarcity-based definition of economics, his commitment to introspection as the main source of information about the qualitative characteristics of individual preferences, and his criticism of interpersonal utility comparisons. The source of our disagreement seems to be that Ross wants to emphasize Robbins's definition (what Ross terms "abstract scarcity") while I am more concerned with his use of introspection and critique of interpersonal utility comparisons. Although I believe I have very good reasons for emphasizing the things I do, it is also clear that Robbins's scarcity-based definition was an important aspect of his contribution and thus the thread that Ross wants to highlight can in fact be identified within the broad tapestry of Robbins's work.

Given this, I really have only one critical point to make on Ross' discussion of Robbins in his reply. The point is that Ross continues to package Robbins's theory of individual choice as having much more in common with the ordinal utility theory that stabilized at the end of World War II than is consistent with anything Robbins wrote or how his work was interpreted by the profession. Ross is willing to admit (as in Ross 2005, *ETCS*) that Robbins relied on introspection and that it produced certain philosophical tensions within his work, but continues to argue that it was ultimately Robbins's deductivism and ordinalism that ultimately won out (that Robbins's ultimate commitment was thus RASP and some version of behaviorism). Ross says that

> . . . 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 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 Hick's indifference-curve analysis of preference was superior in deductive systematicity to classical Jevonsian sensationalism . . . . 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 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) (p. 95).

https://doi.org/10.1017/S1053837209090087 Published online by Cambridge University Press

Although I agree that the tension Ross identifies existed within Robbins's work, and that he ultimately came down on the side of economics rather than philosophical consistency, I explain the relevant causal forces in a very different (and less Whiggish) way. The story I offered in Hands (2007) was that Robbins wanted to defend marginalist economics against a number of serious threats at the time, and given this context, Robbins's approach—the scarcity definition, introspection, and the critique of interpersonal utility comparisons—worked effectively to accomplish that task. Ross' story (not surprisingly) is one of a common conceptual thread running through almost all of twentieth century choice theory—RASP—while mine (perhaps also not surprisingly) is about the various historically contingent forces that worked on Robbins and marginalist economics more generally during the 1930s and 1940s and eventually led to the acceptance of his position by the majority of economists in spite of its philosophical tensions. We both argue that Robbins's economic commitments ultimately trumped consistency; we disagree about the character of those economic commitments.

Now, while I will not argue for my own interpretation here—those who are interested can read Hands (2007)—I would like provide a bit more evidence against Ross' reading. His argument is based on a number of empirical claims that are summarized nicely in the above quote. He reads Robbins as a systematic/deductive anti-hedonist strict-ordinalist with strong commitments to the indifference curve characterization of preferences and drawing heavily on Vilfredo Pareto and Hicks and Allen (1934). The disagreements that Ross and I have over the systematic/deductive are far too complex to deal with here, and we both agree about Robbins's anti-hedonism, so I will focus on the other issues. First of all the key paper by Hicks and Allen was not published until 1934, so it was not cited in the first edition of Robbins's *Nature and Significance*. Robbins did use the word *ordinal* in the first edition (Robbins, 1932, p. 56, n. 1), but he attributes it to Carl Menger; he adds the reference to Pareto and Hicks and Allen in the second edition (Robbins, 1935, p. 56, n. 2). More importantly, the argument that Robbins was a "strict ordinalist" that completely abandoned diminishing marginal utility in favor of convex indifference curves is not supported by the evidence. Even in the second edition after the publication of Hicks and Allen, Robbins never totally abandoned the (cardinal) assumption of diminishing marginal utility, and when he did mention the concept of increasing marginal rate of substitution his evaluation was "Personally, I prefer the established terminology" (ibid., p. 138, p. 1). His criticism of interpersonal utility comparisons did not challenge the assumption of diminishing marginal utility—he accepted it and equated it with "the convexity downwards of the indifference curve" (ibid., p. 141)—just whether the marginal utility of one person could be compared to the marginal utility of another. Robbins ambiguity about ordinalism is also retained in his later work. For example in his remarks on Dennis Robertson, published in 1953, he argues that Hicks and Allen did not represent a substantive change:

> Description of the conditions of consumer equilibrium in terms of equality between objective exchange rates and marginal rates of substitution happens to seem more elegant and more correctly stated than the older proportionality of exchange rates and marginal utilities. *But it is surely wrong to pretend that it is an entirely different type of solution*. To use the terminology chosen by their authors, the Hicks-Allen

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> constructions are a *reformulation; but they are not a revolution* (Robbins, 1953, p. 103, emphasis added).

I thus remain convinced that Ross' view of Robbins's choice theory is extremely difficult to reconcile with the documentary evidence. There was a tension among the various positions Robbins took in *Nature and Significance* and it was ultimately resolved in favor of his economic commitments, but those commitments were far more complex (and more interesting) than simply being pulled along by the white light of RASP as Ross would have it.

Samuelson

As I said in the introduction, and Ross seems to agree, our differences are more significant with respect to Samuelson than Robbins, and in a sense this is to be expected. If twentieth century neoclassicism has a scientific champion it was Paul Samuelson; it was Samuelson "who by both word and deed was responsible for the twentieth century self-image of the neoclassical economist as 'scientist'" (Mirowski, 1989, p. 182). From Ross' perspective of trying to unify twentieth-century scientific economics on the basis of mathematical structure, consistency with some version of positivism, and compatibility with a version of intentional-stance–based behaviorism, Samuelson is absolutely essential. If the story doesn't get off the ground with Samuelson, it can't get off the ground period. But I too have a vested interest in Samuelson. For many years my historical work has focused on the mid-twentieth-century stabilization of neoclassical theory—particularly demand theory—and I have tried to weave a fairly complex story of historical contingency involving a wide range of contextual, epistemological, and agent-specific factors (Hands 1994, 2006; Mirowski and Hands 1998 for example).

Even though I have many complaints about things Ross says in the Samuelson section of his reply—such as his suggestion that Thurstone (1931) supports his interpretation of Samuelson when Thurstone's indifference curves could just as easily be interpreted as the *reference-dependent* indifference curves of contemporary behavioral economics as the reference-independent indifference curves of neoclassicism (Moscati 2007), or his attempt to dismiss the fact that half of *Foundations* was dedicated to non-optimization–based models on the basis of his various comments on "macrofoundations" (existent and forthcoming) when I made it quite clear that the real issue was not macro, but the lack of optimization. But rather than turning this into a litany of minor complaints, I will just focus on two main criticisms and then end by explaining the one sense in which I think Ross *is* on the right track about revealed preference.

One of Ross' major criticisms of my discussion of Samuelson involves the "sophisticated Dennettian picture" of intentional-stance functionalism (ISF) that he believes can clearly be identified in Samuelson 1938 and 1947. My "failing to grasp" (p. 98) these ideas leads is my "flawed reading" (p. 98): "Hands unfortunately gives no sign in his paper of understanding this philosophy, and clear signs that he misunderstands it" (p. 99). I of course did examine these issues in detail in the first section of my paper and explained how they fit into Ross' general program as well as how they are related to both instrumentalism and eliminative materialism. I even cited Dennett's "Real Patterns" (Dennett 1991) and Ross' own modified version of it called "Rainforest Realism" (Ross 2000). This evidently was not enough; it seems

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the only way to demonstrate that one ''grasps'' Dennett's intentional-stance functionalism according to Ross is to *agree* with him about it (both in general and as he finds it in Samuelson's work).

The most disconcerting things about Ross' discussion of these issues is his tendency to ''spin'' the subject matter—the ''sophisticated Dennettian picture''—as something unambiguous and uncontroversial. As he has it, the problem is entirely with me; I just don't understand. But if the argument is so unambiguous, and it is so obvious how it is *not* an instrumentalist position, then these facts should be reflected in what Ross himself has said about the Dennettian position over the years. So let's just see what he said on the matter.

Ross explains that '''Real Patterns' is best read as the culmination of a series of earlier, unpersuasive attempts by Dennett to show that his 'stance' theory with respect to intentional states does not imply instrumentalism about them'' (Ross, 2000, p. 149). The initial argument—primarily in Dennett (1989)—''fails'' (Ross, 2000, p. 149) and ''Real Patterns'' is the next major attempt to solve the problem. But ''Real Patterns'' is also problematic. Dennett ''waffles inconsistently'' (Ladyman and Ross, 2007, p. 200) and fails to ''escape from the tangle of threatening inconsistencies'' (Ross, 2000, p. 160) leading Ross to conclude: ''I fail to see how Dennett's denial of instrumentalism is anything but a *mere* rejection of the label, rather than the substance of that position'' (ibid., p. 160). This is worth repeating: Ross himself sees Dennett's anti-instrumentalism as ''*mere* rejection of the label, rather than the substance of that position.'' But perhaps the anti-instrumentalist ''sophisticated Dennettian picture'' that I fail to grasp is not the picture Dennett himself has provided, but rather Ross' own modified version of it. Okay, but if so then Ross' own version must be unambiguous and clear why it is not instrumentalist. Unfortunately that is not what we find. While Ross provided his own definition of ''real pattern'' (Ross, 2000, p. 161) and defended it against those who would call it instrumentalist (and other critics) the various questions were not put to rest. So now, only a few years later, we are told that the ''first, crude, version was given by Ross (2000)'' (Ladyman and Ross, 2007, p. 226), but that it too had problems so yet another more sophisticated version was needed. The newest version appears in *Every Thing Must Go* (2007, p. 233) after a long discussion of the problems of both the Dennett (1991) and the Ross (2000) version, and a detailed explanation of how the newest iteration (ostensibly now finally) solves them. The point is this: Why is the burden entirely on me when Ross himself cannot even provide a definition of real pattern that is unproblematic or reasonably stable? Of course I am not suggesting there is something wrong with modifying one's ideas in light of criticism by self and others—that is how we, as scholars and scientists, make progress—it is simply that given the evolving character of the various writings on the subject matter, Ross' entire line of criticism seems inappropriate.

My second criticism is that Ross continues to repeat things that I have argued are not defensible as if my arguments did not exist. I do not expect him to agree with what I wrote, but I do expect him to acknowledge it and address it, rather than engage in what often seems to be a version of argument by repeated assertion.

Ross argues (correctly) that I followed Stanley Wong (2006) in distinguishing between Samuelson's project in the 1938 paper that eventually came to be called ''revealed preference theory'' and the later mid-twentieth century literature on revealed

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preference theory by Samuelson and others. Ross says we ''distinguish 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'' (p. 97). Although I certainly agree that Samuelson 1.0 ''aimed to eliminate 'utility','' the rest of this quote is not supported by the argument in my paper. First of all, I made the case that *Foundations* (Samuelson 1947) was not really about revealed preference theory at all. The whole book has only a few passing references to revealed preference; it is a systematic attempt to provide a single mathematical framework for obtaining comparative statics results for a wide class of economic models defined by systems of (generally differentiable) equations. It is neither about recovering ''the generalizations of ordinal utility theory with behaviorist restrictions,'' nor about constructing ''individual indifference maps from observable behavior.'' Samuelson (1948) was about constructing indifference curves, but as I explained in my paper that is not how revealed preference theory (WARP) came to be interpreted within the demand theory of the second half of the twentieth century. What WARP came to mean *for the profession*—Ross' ''institutionally *bona fide*'' scientists (Ladyman and Ross, 2007, p. 36)—was a set of conditions *on individual demand functions* that would imply other conditions *on individual demand functions*. Of course, and not coincidentally, the implications of a WARP demand system were the same as the implications of ordinal utility theory (actually Slutsky symmetry is missing, although SARP picks it up), but ''preference,'' ''utility,'' or ''indifference'' do not appear anywhere in the standard formalism (as shown in the second system of expressions on p. 18 of my paper).

As another example, Ross says: ''preference just *is* a particular way of describing (consistent) behavior. That is also what Samuelson thought—in 1938 *and* in 1947'' (p. 98). Here too Ross makes a statement that is in direct conflict with one of the main points of my paper without even attempting to offer any evidence for what Samuelson ''thought'' in 1938. Ross argues that by employing ISF and all that, *he* can explain how an economist who takes the position that ''preference just *is* a particular way of describing (consistent) behavior'' can be seen as consistent with recent developments in cognitive science (and to do so without being an instrumentalist). That is fine—in fact, quite interesting—but it provides no argument or evidence that Samuelson ''thought'' anything of the sort. My argument was that in 1938 Samuelson was a radical behaviorist—an eliminativist about mental state entities such as utility and preference—and Ross seems to agree (p. 97). This means of course that Samuelson did not think that ''preference just *is* another way of describing (consistent) behavior'' any more than he thought that ''Zeus's anger just *is* another way of describing thunder.'' Preference *just was* something to be *eliminated* not redescribed. This is the position I took on Samuelson 1938 in my paper and I have argued for it in a number of other places over the last decade. Now the fact is *that I could very well be wrong*. Perhaps some enterprising scholar with access to Samuelson's archives will in fact discover—based on correspondence, unpublished notebooks, or some other documents—that the best available historical evidence does not support the claim that Samuelson endorsed radical behaviorism at that time. I hope not—I obviously like my argument—but I understand, and might even come to accept, *that kind of an argument*. Ross offers nothing of the sort.

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Although I have said many critical things in this section, I would like to end on a bit more positive note. Although I do not think Ross' ISF-real patterns reading of rational choice theory fits (any version) of Samuelson's revealed preference theory, I do think that it has much in common with what some *recent* economic theorists say about rational choice and revealed preference. Without loss of generality let me pick just one recent exposition of choice theory (Bernhein and Rangel 2008)—what the authors call ''standard economics''—because I think it is particularly clear how certain contemporary economists view ''choice'' and ''revealed preference.'' They say

> Though we often speak as if choices are derived from preferences, the opposite is actually the case. Standard economics makes no assumptions about how choices are actually made; preferences are merely constructs that summarize choices. Accordingly, meaningful assumptions pertain to choices, not to preferences . . . . Under the weak congruence axioms, the relation R is an ordering, commonly interpreted as ''revealed preference.'' Though this terminology suggests a model of decision making in which preferences drive choices, it is important to remember that the standard framework does not embrace that suggestion; instead, R is simply a summary of what the individual chooses in a wide range of situations. Further technical assumptions allow us to represent R with a continuous utility function (Bernheim and Rangel 2008, p. 158).

Now *this* seems to be a version of revealed preference theory that fits (or could be made to fit) quite well with Dennett's ISF and with what Ross says about RPT. Ross could construct a persuasive story about *this* version of ''standard theory'' and achieve his goal of bringing standard choice theory ''into closer coherence with the surrounding behavioral and cognitive sciences'' (p. 93): a very valuable philosophical/methodological project. Notice that I am not suggesting this is *the* way that ''choice'' and ''revealed preference'' are used in recent economic theory,[2] but it certainly is *one* way, and one that seems to be a natural for Ross' philosophy of economics. The problem of course is that this version of standard theory, like Ross' RASP, is not the theory of Samuelson in 1938 or in 1947 (or the WARP interpretation of mid-twentieth century demand theory, or Arrow–Debreu general equilibrium theory), or even an identifiable thread within any of these research programs. However, I do think that once this version of ''choice'' theory is clear, it is easy to put a finger on the source of the difficulties with the historical part of Ross' project; he implicitly takes such definitions of ''choice,'' ''utility,'' and ''revealed preference'' from such recent literature and projects them back onto historical contexts where they do not fit. But of course that is what Whig history does, which moves us to the subject of the final section.

History and Philosophy

I was pleased, but also a bit surprised, that Ross wanted to reply to my paper. He clearly warned the reader that he was doing philosophy and not ''documentary''

[2]See Hausman (2008) for a careful philosophical discussion of the various uses of ''revealed preference'' in the contemporary economics literature.

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history (Ross, 2005, p. 75). As I said in the conclusion of my original paper, it seems to me the project would be much more persuasive if he just dropped the Robbins–Samuelson story altogether and did philosophy of economics. He articulated a very sophisticated argument about how economic theory could be brought into line with recent developments in cognitive science, be consistent with Dennett's ISF, and do so without being reduced to instrumentalism; and as the example at the end of the previous section demonstrates, some economists already conceptualize choice theory in a way that is quite close to Ross' framework. Why not just provide a philosophical characterization of scientifically adequate and cognitively sophisticated economic theorizing, point to some current economic research that is doing it, and stop? Regardless of which side one takes on the debate over Robbins and Samuelson, it is clear that the whole project would be much less controversial if the historical aspect were simply dropped. In fact it would not need to be entirely eliminated. I certainly would not have initiated my challenge if Ross had simply noted that ''abstract scarcity'' and a version of behaviorism about preferences are essential components of his program and that Robbins and Samuelson (respectively) were the two most influential economists of the twentieth century to introduce those ideas. Who could disagree with that? Certainly not me. But he needs/wants so much more. He needs/wants RASP—a consistent and systematic *argument pattern* that is identifiable across time (at least from our perspective, if not theirs). It is an argument pattern that must be present in inchoate form in the work of Robbins and Samuelson, matured—driven by its own inevitable inner logic—during the mid-twentieth-century heyday of neoclassicism, and manifests itself in (and is the source of) the progressive economic science we have today. Ross accuses me of thinking there is ''*one correct* history of a body of science'' (p. 101), but his story is teleological historicism at its worst. Such history does not deny that factors such as technology, the reigning ideological and epistemological values, psychological and/or personal factors, force and fraud, and even luck, can have an impact on the specific details of history; but all of these things are just inessential jetsam and flotsam relative to the real underlying historical process unfolding deep within the science of economics. My response is ''no.'' No, this is not the way to understand the history of economics (or any other history) and no, Robbins and Samuelson did not write words that can be persuasively recruited to serve this end.

Of course as I explained in my paper, Ross' version of *naturalism* is one of the things that paints him into the corner of needing history: ''it is not among the rights of philosophers to pronounce some institutionally *bona fide* sciences methodologically or metaphysically defective'' (Ladyman and Ross, 2007, p. 282).[3] He must, in some sense, be ''true to'' the discipline by finding the right stuff present in the actual practice of economic science. But even this naturalism would not alone require him to reconstruct large portions of the history of the discipline; alone it would allow some version of ''start the scientific clock now'' naturalism (i.e. show some version of recent economic theory fits the program, but not necessarily Robbins, Samuelson, and such). The other force contributing to Ross' philosophical–historical problem situation

[3]Of course *bona fide* is the real issue. Notice Ross didn't include Thorstein Veblen, or a non-neoclassical mathematical economist like Griffith Evans, in his reconstruction.

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is his commitment to a concept of scientific *progress* that requires subsumption under single unified argument pattern.

> We make progress ... every time we show that two or more phenomena are explained by a common *argument pattern*. An argument pattern is a kind of template for generating explanations of new phenomena on the basis of structural similarity between the causal networks that produced them and causal networks that produced other, already explained phenomena (Ladyman and Ross, 2007, p. 31).

When this conception of scientific progress is combined with his naturalism and his desire to demonstrate the scientific adequacy/progress of neoclassical microeconomics, it results in the need to demonstrate that something like RASP is present, if hidden, in the history of twentieth-century neoclassical economics. And thus in the end Ross *needs* textual support from Robbins and Samuelson—institutionally *bona fide* neoclassical economists—and he needs to show that in some significant and identifiable sense the seeds of RASP were in fact present in their work. Of course it is fine for Ross' story that Robbins and Samuelson believed all kinds of other things, even some that create tensions with RASP—as he says: "I choose to emphasize some tendencies and let others remain in the background" (p. 101)—but to some degree the tendencies actually need to be identifiable there. If they are not, then the whole philosophical edifice—a very impressive philosophical edifice—comes tumbling down. Given his goals Ross cannot "just do philosophy," nor can he take the line that "economic science starts now"; he needs certain arguments to actually be *in* the work of Robbins and Samuelson. I simply do not believe that Ross has demonstrated what he would need to demonstrate to make his case. I cannot, nor can anyone else, say how much of "it"—the things Ross says are in Robbins and Samuelson that make his case—needs to be in the relevant texts, or what form "it" must take, to make the argument persuasive. As I noted above, that decision will be made by the community of scholars who examine the argument and the historical evidence, but nothing Ross says in his reply makes me any less certain that future scholars will find the historical part of his story to be just as deeply problematic as it is necessary for his broader project.

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