

Unpublished response to Salim Rashid's "Review of *How Economics Became a Mathematical Science*" that appeared in *History of Political Economy*.

By E. Roy Weintraub

Salim Rashid's paper raises some very specific concerns which require a specific response¹. He asks why I don't address the question of "If Volterra followed Poincaré, why did he not follow qualitative, topological dynamics which Poincaré [introduced?]" The answer is that while Volterra and Poincaré in fact were contemporaries, Volterra, though an admirer of Poincaré, is not rightly seen as his follower. Volterra after all was 40 years old in 1900, an age in which one does not become a follower lightly. Perhaps Rashid was thinking of George Birkhoff who indeed was Poincaré's "disciple", and who did follow Poincaré's path into dynamics (as I discussed in my (1991) Stabilizing Dynamics. New York, Cambridge University Press).

He goes on to suggest that I somehow missed how "mathematicians themselves have often turned away from the emphasis on axioms, as a reading of the interviews in Mathematical People (and its sequel) will show." The problem with this observation is that the move away from Bourbakist mathematics is a fairly recent development, one which was prefigured in my penultimate chapter, but one which does not engage my own historical narrative for it took place after my book's time had, as it were, run out.

Rashid then takes strong issue with my characterization and discussion of Gerard Debreu, as he argues that Debreu did not feel the need to engage in any work on dynamics because "he thought it more effective to recruit the efforts of Steven Smale." But Debreu did not "recruit" Smale at all. Smale was working on dynamics for many years before he met Debreu. Moreover Debreu's own account, which I addressed specifically in my interview with him in the book, provided a large number of reasons why he himself was never much interested in dynamics, and why he thought that this work was not going to be useful. Specifically, those comments of Debreu refer to his own activities in the late 1940s through the 1950s when the work on the stability of competitive equilibrium was being developed by Arrow, Nerlove, Enthoven, Hurwitz, Bushaw, Clower, etc. Debreu in particular was no fan of tatonnement dynamics nor of any other kind of dynamic adjustment mechanism that economists in those times (and today) were fond of using.

Second, Rashid claims that Debreu was not really Bourbaki-influenced, a claim he supports by noting that Debreu was classically trained in geometry while Bourbaki's approach was non-geometric or pro-algebraic. But this is simply not true. Bourbaki was immensely involved with the developing geometry. Much of the work that came through the *Seminaire Bourbaki* was a reconstruction of classical geometric results from an algebraic perspective, and required a firm understanding of classical geometric analysis. Indeed this kind of approach to geometry was not new at all, as it had been first set out in Felix Klein's Erlanger Program in 1872! To algebraicize geometry one needs a deep

¹ One minor point: I must reply to the claim that Rashid makes that the book's chapters were originally stand-alone articles that now have been knit together into a volume. This is not true. The project was a book from the beginning (in Venice in 1992), but in order to get feedback, and to get my way paid to various conferences and lovely European capitals to give papers, portions of the large project got carved up in different ways at different times.

understanding of classical geometry. To suggest that a Bourbakist like Serge Lange was not “geometric” in his approach is to misread the contributions of 20th century geometers.

The third and principal objection Rashid makes to my discussion of Debreu though concerns the question of Debreu’s engagement with applied economics. For Rashid: 1) applied economics addresses a comparison of equilibrium positions; 2) Debreu was instrumental in constructing a model of the economy in which equilibrium existed; and thus 3) Debreu can be construed as having provided a basis for analysis of comparative statics. All this may be correct, but that construction is Rashid’s and emphatically not Debreu’s. Debreu’s interest was never applied economics, as he stated on a number of occasions in the interview I conducted with him, and which is reproduced in my book. He was a Bourbakist mathematician (if one rereads the mathematical introductory chapter to Theory of Value, note the Bourbakist references, and particularly the use of the word “adherence” and its defense). We can each make Debreu out as we will, and do. In reconstructing Debreu in the context of Bourbaki, I submit that my narrative has the real advantage of coherence with respect to Debreu’s testimony, and the accounts of others.

Rashid goes on to argue that a theme of my book is “that mathematics went through crises and paradigm shifts”, to which I must reply that one could characterize the history of mathematics over the last hundred and twenty five years in this fashion, but that is not in fact how I so characterized it. The issue of whether or not there are Kuhnian revolutions in mathematics is a very troublesome question for historians of mathematics, and they have engaged that topic in print at length (cf. Dauben, J. (1992). “Are There Revolutions in Mathematics?” in The Space of Mathematics, J. Echeverria, A. Ibarra and T. Mormann. Berlin and New York, Walter de Gruyter.). My reconstructing some history of mathematics, employing as an organizing trope a distinction between the image and the body of mathematical knowledge, allowed me to escape having to argue in terms of Kuhnian revolutions. Ruptures in how the mathematical community understood the notion of proof were not necessarily associated with large changes in the body of mathematical knowledge, and the classification theorem on finite groups did not modify very much the image of mathematics.

Finally in many of the later sections of his discussion Rashid seems discomfited by the fact that I do not address “how mathematical economics itself has moved beyond Bourbaki’s approaches to other methods using computers and inductive methods.” Rashid worries that what he calls my narrative history cannot address with due seriousness the divorce of the “social sciences from reality.” But my narrative was specifically constrained by its title, How Economics Became A Mathematical Science. My argument that this was a story which did not need to be continued much past 1970 was based on the belief that by then, the 1960s, economics was to all intents and purposes a mathematical science. Tracing the evolution of that mathematical science over the ensuing thirty years would be telling the history of that which economics had become, an interesting project but not mine.