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context to the development of Hayek’s thought in the post-WWI Vienna (“Hayek’s Spontaneous Order and the Ethics of Free Markets”). One of the best papers in the volume, namely Witt’s paper, belongs to this section. Witt argues that not only may Hayek’s total production be divided into a first period in which Hayek was mostly taken with technical issues in economic theory and a second period marking his cross-disciplinary work on cultural evolution, etc., it is also the case that there is a certain tension between Hayek’s analytical stances in these two periods. While his early work in economics (notably, on trade cycle theory) was equilibrium-oriented, his later work was explicitly evolutionist. Witt then moves on to discuss how Hayek’s later evolutionist approach may fruitfully be applied to the issues that occupied Hayek in his economics work.

The papers in the two other sections, “Farewell to the Welfare State?” (three papers), and “Europe, Widening or Deepening” (two papers), are related to Hayek’s thought in a more distant manner. In particular, Frank Vibert’s “European Constitutionalism” arguably owes more to the thought of James Buchanan than to Hayek. However, the classical liberal stance characterizing these papers is surely consistent with Hayek’s general outlook. A particularly fine paper here is Frank van Dun’s work, “Philosophical Statism and the Illusions of Citizenship: Reflections on the Neutral State,” which contains a deep and encompassing discussion of the philosophy of statism from Plato to contemporary proponents, but which makes very little direct contact with Hayek’s thought.

To sum up, this is in many ways an unbalanced volume. It lacks a clear focus and the chapters are of a very uneven quality. Some of the papers do not deal at all with Hayek’s work, and some chapters do not bring much new on Hayek. The book will, however, be of interest to Hayek scholars as well as to those with a general interest in classical liberalism. An irritating feature of the book is that it is printed in an unusually small font, making it hard to read. The publisher should have considered that the intended audience for the present volume very likely has a substantially higher average age than the average academic.

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Terence Hutchison has been making substantive contributions to the literature on the history of economic thought and economic methodology for more than
sixty years. When The Significance and Basic Postulates of Economic Theory first appeared in 1938, Hitler was still a year away from invading Poland, Keynesian economics was only two years old, and it would be twenty-one more years before Karl Popper’s major philosophical work appeared in English. Despite all of the many changes that have taken place within the social, political, and economic world, in economic theory, and in scientific philosophy, the central theme in Hutchison’s methodological research has remained essentially unchanged throughout his long career. The central thesis of his 1938 volume was that economics should be an empirical Science in the image of the natural sciences, and that in order to be such a Science economists would need to engage in the rigorous empirical testing (attempted empirical falsification) of substantive economic theories, including the discipline’s most basic postulates such as the rationality assumption. The central thesis of this most recent book remains essentially the same. There are, as we shall see, a few new twists and turns, but the basic argument that economics should be a Science, and that Science requires bold conjectures and severe tests, continues to be the primary message.

The book consists of ten essays, most previously published, written between 1975 and 1999. A few of the previously published chapters were excerpted from Hutchison’s earlier books, two originally appeared in The Journal of Economic Methodology, and chapter seven is an expanded version of his Hennipman Lecture from 1990. In addition to the general promotion of falsificationism in economics, the essays are also held together by a more narrow unifying theme: the formalist revolution—the post-World War II formalization of economic theory in general and Walrasian general equilibrium theory in particular. All of the papers, in one way or another, address the issue of formalization in economics. The bottom line in all of the essays—consistent with Hutchison’s general methodological position—is that formalization in economics was, and still is, generally a bad thing. Providing a methodological and historical narrative in defense of his general denigration of formalism is the main task of the various essays in the book.

A brief summary of Hutchison’s historical-methodological story goes basically like this. Adam Smith did economics the right way. In particular, he engaged in careful historical, institutional, and inductive investigation, and he was relatively parsimonious in his use of deduction from abstract/idealized principles such as perfect information and perfect rationality. Economics took a wrong turn with David Ricardo. Ricardo’s “one-sided, abstractionist-deductivist-formalist, methodological programme” (p. 3)—philosophically justified by the a priorist methodology of N. Senior, J. S. Mill, J. E. Cairnes, and others—directed the profession’s attention away from Adam Smith’s realistic and relevant concerns and into the narrow deductive framework of proto-formalism. This unsatisfactory methodological trend was further exacerbated by Austrians such as Ludwig von Mises and fellow-travelers, such as Lionel Robbins who was the main target of Significance. According to Hutchison, this negative turn was at least briefly checked during the first few decades of the twentieth century. The two economists most responsible for these positive methodological developments were Alfred Marshall and John Maynard Keynes. Marshall and, particularly, Keynes liberated economics by resisting deductivism and pushing the field back in the direction
of induction and policy relevance. Marshall and Keynes were “vigorous non-, or anti-formalists” (p. 18) who consistently promoted empirical and predictive economics with a systematic policy-oriented focus. But this breath of methodological fresh air did not last long. After World War II the locus of Western economic research shifted to the United States, where, through a constellation of professional, technical, and sociological factors, the older abstract-deductive method returned, this time fortified with sophisticated mathematics. The profession’s obsession with the Arrow-Debreu mathematical general equilibrium model during the 1960s constituted the apogee of this pernicious formalism; the general equilibrium theorists transformed Adam Smith’s conjecture about the invisible hand “into a piece of ‘rigorous,’ empirically vacuous, hyper-abstract analysis, based on a range of fantastic, science-fictional assumptions” (p. 314), an indictment that also extends to any macroeconomic theory grounded in Walrasian microeconomics or based on rational expectations (p. 167). According to Hutchison there were some brief positive signs of anti-formalist dissent during the 1970s—he repeatedly cites critical remarks by Phelps Brown, Benjamin Ward, and G. D. N. Worwick—but an effective counter-revolution never came to pass. Although Hutchison sees a few recent developments as optimistic (such as David Card receiving the J. B. Clark medal and critical remarks by contemporary economists like Paul Krugman and Lawrence Summers), the deductive formalist methodological regime remains firmly entrenched.

Given the readership of this journal, I suspect the historical sketch in the previous paragraph induced a few winces on the part of readers; different historians of economic thought would of course wince in different places, but it seems unlikely that many readers would be willing to accept Hutchison’s narrative as an unproblematic methodological history of the last two hundred years of economics. More wincing is perhaps in order when the list of villains is examined in closer detail. Not only are all of the Austrian economists and Robbins indicted, as well as the Marxists who surfaced as Hutchison’s target in the 1960s and 1970s, and, of course, all of the mathematical general equilibrium theorists of the post-war era, but the culpability is also extended to a dizzying array of other, often rather surprising, names. It seems that almost everyone writing on the subject of economic methodology, with the exception of Mark Blaug and a few others, has contributed in one way or another to formalism and its problems. Friedman’s methodology of positive economics—an argument, like Hutchison’s, that emphasizes empirical prediction—was partly at fault because it encouraged an attitude of “anything goes” (p. 193). Daniel Hausman, a methodologist who also seems to keep one foot on the positivist base pad, represents for Hutchison just another version of Robbins’s “ultra-deductivism” (p. 256); critical-transcendental realists like Tony Lawson—who are equally hostile to general equilibrium theory—are accused of being “very vague” and “somewhat mystical” (p. 12). Although falsificationism is a good thing, even Karl Popper contributed to the problem in his writings on situational analysis and the rationality principle (pp. 63–65); and another surprise, Joan Robinson, Hutchison’s old Cambridge tutor, is repeated condemned for her “optimistic” methodology and “a ruthless appetite for extreme abstractions (whether neoclassical or Marxist)” (p. 348). Finally, but perhaps less surprisingly, all of those “fashion-conscious sections
of the economic-methodological fringe” associated with the “postmodernist-academicist blight” (p. 40) end up as villains in Hutchison’s methodological parable. In summary then, adding the economic theorists from Hutchison’s historical narrative together with his long roster of methodological miscreants, the problems of economics can be attributed to: labor-theory of value classicals from Ricardo to Marx (and of course all other Marxists); Austrians and other laissez-faire economists including Hayek, who is partially responsible for Hitler’s rise to power (p. 296); all mainstream mathematical economists, general equilibrium theorists, and general equilibrium-inspired macroeconomists (Hutchison does not mention game theory, but it seems that its formalist versions would be indicted as well); and essentially everyone who has ever written about economic methodology from anything other than a falsificationist perspective. Thus it seems that the problem of formalism and bad economic methodology can simultaneously be blamed on the economists who have been most politically concerned (classicals, Austrians, and Marxists), those who have been most historically and philosophically oriented (methodologists), and also those who have exhibited the least political, historical, and philosophical acumen or concern (mathematical economists and general equilibrium theorists). Hutchison’s list of villains certainly contains some very strange bedfellows.

While every essay criticizes formalism and the failure of the economics profession to promote empirical testing and policy relevance, the one chapter that is most explicitly historical—that seems to foreground the history of economic thought over normative methodology—is the last chapter (ch. 10) of the book. The subject of this final chapter is the history of general equilibrium theory. In particular, Hutchison responds to the argument (by Frank Hahn and others) that the existence theorems and all of the other technical results produced within the Arrow-Debreu general equilibrium framework represent simply a “translation” and “clarification” of Adam Smith’s basic economic ideas (particularly the invisible hand). Hutchison’s refutation is based on an economist-by-economist examination of the laissez-faire policy prescriptions as well as the assumptions regarding competition and perfect knowledge that have appeared in the work of economists from Adam Smith to the postwar general equilibrium theorists, including, among others, Mill, Sidgwick, Jevons, Pareto, Pigou, and Hicks. Hutchison finds that the case for laissez-faire and the invisible hand, where it has been made most effectively in the history of economic thought, has been based on empirical-institutional case studies and what most general equilibrium theorists would consider to be “wishy-washy” (p. 319) economic analysis, rather than “rigorous” formal theory. Not only have effective arguments for the invisible hand been made on a case-by-case basis using informal economics, when general equilibrium theorists have in fact attempted to formalize these ideas in the form of the first and second fundamental theorems of welfare economics, the result was “a few tenuous trivialities” (p. 328) rather than a persuasive defense of the efficiency of competitive markets. In other words, we know a lot about the invisible hand and competitive markets, but what we know has nothing to do with the Arrow-Debreu formalism, a formalism that has systematically misallocated our intellectual resources by focusing our attention on a theory that is “not about the market system we actually live in, or about
one that human beings ever have lived in, or could live in” (p. 332). While this final chapter does contain a few rather outlandish claims about the political damage caused by certain economic theorists, Hutchison’s general argument that the Arrow-Debreu formalism distorted, rather than merely translated, the idea of the invisible hand, seems to be both compelling and historically well-defended.

In closing I would like to note that even though my reading of the history of economic methodology is quite different from, and often diametrically opposed to, Hutchison’s reading (see Hands 2001), I do not think that a brief review of Hutchison’s book is the place to attempt either a detailed critique or an alternative reconstruction. What I would like to do in the last paragraph though, is to use Hutchison’s argument about general equilibrium theory from chapter ten to make a critical point about his more general methodological position.

Hutchison, and the few methodological writers he supports, argue for strict methodological rules, in particular, falsificationist rules. We should always rigorously test our theories and exclude any economic analysis that does not pass severe empirical tests; this is an argument for abstract and universal methodological rules. What Hutchison does not seem to see, is that such positivist and/or falsificationist rules are to methodology as mathematical general equilibrium theory is to economics; they are universal, immutable rules, grounded in and deduced from arm-chair formalist philosophy of science. Of course, we know from the vast literature on the history and sociology of science—literature, by the way, not conducted from an armchair, but based on case-by-case inductive studies—that real science is never so pristine. Real science is messy, contingent, context-specific, and if we are able to offer any epistemic policy advice at all, it must be done by means of case-by-case, institutionally and context-sensitive, specific studies: not by deduction from a few abstract epistemic principles. This is of course exactly what Hutchison says about economic (but not epistemic) policy. The real economy is messy, and if we are able to offer any economic policy advice, it must be done on the basis of case-by-case, institutionally and context-sensitive, specific studies, not by deduction from a few abstract principles about perfect competition and perfect information. Hutchison makes some very good arguments about how we should give economic advice; if he were to apply those same arguments to the question of how we give epistemic advice, I suspect that he would end up with a very different methodological position than the one that he has in fact endorsed for the last sixty or so years.

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