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REVIEWS

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The Practice of Principle, Jules L. Coleman. Oxford University Press, 2001, xx + 226 pages.

If one were to create a compilation CD containing the necessary works of jurisprudence, Jules Coleman's recently published *magnum opus*, *The Practice of Principle*, might follow H. L. A. Hart's *The Concept of Law* and Ronald Dworkin's *Law's Empire* on the disc. Coleman's book is relatively short (226 pages) and relatively difficult. But, to understand the arguments and, as important, Coleman's explications of the core debates, is to come to the edge of modern jurisprudence. If Coleman were to set aside philosophy in favor of the full-time pursuit of his musical interests, it could truthfully be said that as his swan song he had produced a comprehensive work that effectively addresses the full range of issues that have occupied contemporary jurisprudence over the course of his career. This is not to say that Coleman is likely to rest content with his current position. One of the hallmarks of his *oeuvre*, and one of the reasons his theory has evolved rapidly over the years, has been Coleman's facility for adaptively revising his account in light of the high quality peer critique his work has engendered. Coleman says good philosophy is like the blues and he is riffing (pp. ix–x).

Coleman expands the argument he developed in the Clarendon Lectures on Law presented at Oxford University in the fall of 1998. Coleman gave three lectures, each of which became a part of the book: part I concerns the nexus of tort law and jurisprudence, part II defends *inclusive legal positivism*, and part III discusses general methodological issues in jurisprudence. I will focus on part I, as it is most directly germane to this journal's purview, given that Coleman's foil in this discussion is the economic analysis of law. The first section will set out Coleman's arguments against the economic approach and their relevance to his own approach. The second section will then critically examine one core aspect of Coleman's theory. Coleman incorporates by reference his previously developed corrective justice account of the institution of tort law

(p. xiii). I will argue that the logic of Coleman's jurisprudential account as developed in *The Practice of Principle* may, by its very success, necessitate revisiting his account of corrective justice. In particular, I will argue that Coleman's conceptualist approach entails paying greater attention to the role of the jury than is currently the case in Coleman's work and that doing so may entail amendment of his account of the principles embodied in the practice of tort (or at any rate, American tort).

I. Conceptualism versus functionalism

As Coleman notes, it would not have been unfair to read his theory of tort as developed in his previous book, *Risks and Wrongs* (1992), as a kind of 'constructive interpretation' in the Dworkinian sense (p. xiv). In *Practice of Principle*, Coleman seeks to clear up this ambiguity regarding the kind of explanation of tort law he is espousing. Coleman triangulates his position between that of the economist and that of Dworkin. Contra the economist, the best explanation of tort law must account for the manner in which it *embodies* non-efficiency-related normative principles such as the principle of corrective justice. Contra Dworkin, despite the respect in which a normative principle is embodied within the core of tort, tort law is best understood in positivist rather than Dworkinian terms. Coleman's careful, subtle and highly original delineation of this broadly Hartian position promises to be a lasting legacy to tort law and to jurisprudence.

In Part I, Coleman contrasts the economist's *functionalist* explanation of tort with his pragmatist explanation. Coleman notes that he does not use the term pragmatism in the currently fashionable sense that eschews theory (p. xiii). Rather, Coleman describes his approach as an outgrowth of philosophical pragmatism of the sort developed by Wilfrid Sellars, W.V.O. Quine, and Donald Davidson. Coleman's pragmatism seeks to develop a set of commitments about the semantic content of theories and about the criteria of theory justification. A pragmatist account should answer the question of what is to count as an explanation of a particular area of the law, such as tort law. Coleman distinguishes among three different kinds of explanation: conceptual explanation, causal-functional explanation, and constructive interpretation. Coleman presents the corrective justice account as an instance of the first kind of explanation, in particular, a pragmatic conceptual one (Goldberg and Zipursky).

Coleman contends that adequate conceptual explanations must seek to tell us what the nature of a thing is or why things are as they are (p. 3). According to Coleman, satisfactory explanations of legal practices typically take the form of analyses of the concepts that figure prominently in those practices (p. 3). Coleman's goal is not to determine how our existing legal practices might be derived from justified principles. Instead, Coleman begins the analysis with middle-level concepts. He asks what principles, if any, are 'embodied' in the legal practices we are presently engaged in (p. 5). Coleman contends that the analytic strategy of starting in the middle with actual legal practices is one of the broadly pragmatic features of the methodology he employs (p. 6). He makes no presuppositions about the moral status of the principles he sets out to uncover. Rather, the goal is to identify the 'normatively significant elements of the practice and to explain them as embodiments of principle' (pp. 5–6) or, in other words, to uncover the *practice of principle*.

On Coleman's pragmatic approach, it is crucial to look at the practical inferences that are drawn from the central concepts of the practice. Of particular importance in the context of tort law is the inference of liability. Coleman's argument proceeds in part by means of an extended critique of the economic account as inadequate due to its faulty account of the inference of liability. Most crucially, the economic account fails to adequately explain the 'bilateral structure' of tort law, that is, the existence and significance of tort suits in which a plaintiff demands redress of a defendant on the basis of having been injured by the defendant. Coleman adds strength to the argument he has developed elsewhere to the effect that the bilateral party structure, while at the core of the practice of tort law, has no intrinsic justification on the economic approach. The identity of the victim, the injurer, and the normative relationship between the two, are of only contingent importance to the economic account. All that ultimately matters for efficiency is that future actors are optimally incentivised to minimize the cost of accidents. It is a contingent matter whether the bilateral party structure serves this larger social purpose.

Coleman's general strategy is to demonstrate that there are important features of the practice of tort law that are not explained by the efficiency account. As Coleman notes, this does not necessarily demean the efficiency account, *qua* functional explanation, but it will be of limited value unless the economist can in addition provide a causal explanation for how the functional account tracks what is actually happening in the real world (pp. 26–7). Without this causal explanation, the economic account is merely a *just-so story*. Coleman notes that while a causal explanation of this sort may be possible, the more significant point is that to date such an explanation has not been forthcoming. Thus, the economist's functional explanation fails to provide a satisfactory conceptual explanation. If we want an explanation that is satisfactory, we will have to look elsewhere. Coleman's pragmatic conceptualist explanation of the principle of corrective justice at the core of the practice of tort law is a likely place to look.

II. The art of noise: Does jury practice introduce principle?

The power of Coleman's argument is the idea that the central features of tort law hold together in a tight manner – they are related and demonstrate

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a more or less coherent enterprise of delivering corrective justice. One question that deserves more attention than Coleman has given it is this: On what basis does a feature of tort law count as sufficiently important such that it should be accounted for within a pragmatic conceptualist account? For example, Coleman does not purport to explain courts' broad use of the fault standard, as opposed to a strict liability standard. One can appreciate the importance of the question to Coleman's enterprise by considering another critical feature of American tort law – the jury. The jury poses two questions for Coleman: (1) What is the intended breadth of his interpretive account, and (2) does it apply to Commonwealth tort law, which generally does not rely on the jury? If the answer to the second question is yes, then whose 'practice' is being interpreted? Coleman here threatens to slide from *interpretation* of an actual practice into pure analytic philosophizing about an ideal form of law. If the answer is no, that is, if the account is meant to be an interpretive account of American tort law, on what basis can Coleman exclude the jury from his account? In the remainder of this review, I will consider whether such exclusion is justified and, assuming it is not, whether the jury might be compatible with his interpretation of the practice of American tort law.

When the jury produces a liability verdict and a damages award, it plays a direct role in the creation of an entitlement. Most tort suits settle prior to going to a jury. Even here the jury plays an indirect role, as there is always the prospect of going to a jury and this possibility will form the backdrop against which settlement negotiations occur.

As already noted, it is an important element of Coleman's pragmatist approach that practice and principle are to be understood in light of one another. Accordingly, the introduction of an additional component of practice will raise the issue as to whether some further principle is thereby implicated. The first question for Coleman will be whether the jury's role is 'normatively significant' and if so, whether this role can be fairly said to 'embody' principle (p. 5). These two questions will be the focus of the following discussion. How they are answered will determine the importance of the role of the jury to Coleman's pragmatist approach to tort.

It may be that that answer to the first question is no, the jury's role is not 'normatively significant', in which case there is no need to consider the second question. On the other hand, the answer to the first question may be yes, while the answer to the second question is no. Yet another possibility is that the jury's role may embody the principle of corrective justice, in which case inclusion of jury practice into the overall account of tort practice may support Coleman's corrective justice account. Finally, jury practice may embody a principle or principles other than, or in addition to, corrective justice. As the following discussion will indicate, I think it is more or less undeniable that the jury's role is normatively significant, once the question is squarely addressed. Regarding the second question, I will argue that the jury's role appears to embody both corrective justice and other principles as well, although the precise specification of this larger set of principles would require greater attention than the present forum allows.

The jury's role in American tort litigation is normatively significant in a number of respects, both substantive and procedural. Most important on the substantive side, juror norms give content to the "reasonable person standard". Courts do not tell jurors how to interpret the concept of reasonableness. Consequently, jurors must draw on their own understanding of reasonable behavior, based on their experience of the world. When the action is an instance of an extant social practice of the community, the jury will be able to consult their personal knowledge of the general functioning of the practice and, equally important, the extent of community acceptance of the practice. For example, if a defendant builds a hayrick with an aperture and it subsequently catches fire and damages a neighbor's property, it will matter to a jury's sense of the reasonableness of defendant's injurious action whether or not building hayricks in this manner is the norm.

The content of juror norms will to some extent depend on the requirements for jury selection. Historically, some courts placed inappropriate requirements on jurors, such as that they be male, or white, or landowners. Empirical research suggests that factors such as race, gender and income may affect outcomes, although apparently not nearly as much as claimed by those norm entrepreneurs selling 'tort reform'.

Research in cognitive psychology has raised concerns as to whether lay jurors, due to their amateur status, may be more prone to cognitive errors and biases than are professional judges. For instance, jurors are prone to take information about the parties and their motives into account in deciding causal responsibility, even information that is legally irrelevant to that question (Alicke 1992). On a more optimistic note, public choice analysis would suggest that juror-produced outcomes may be less subject to distortion based on agency costs, due to the fact that jurors typically are indifferent to judicial outcomes from the perspective of self-interest, while judges, particularly elected judges, may be tempted to skew outcomes so as to thereby promote their own self interest.

The participation of the jury in American tort practice also introduces distinct procedural features of normative interest. The jury plays a role that has been likened to a lightening rod, drawing heat and criticism away from the judge. In addition, litigants may be more inclined to think that outcomes produced by juries are more just than outcomes produced by judges, particularly when the jury as a whole is deemed to be *representative* of the community.

As the above examples indicate, many of the normative issues regarding the jury's role in producing entitlements are empirical in nature and could benefit from further study. For present purposes, a definitive account of the role of jurors is not necessary as the foregoing observations are sufficient to establish that the jury's role is normatively significant. The analysis may accordingly proceed to a consideration of Coleman's second criterion, which is whether the jury's role in negligence law embodies principle. Coleman does not discuss the jury's role in any detail but he does possess a compelling theory of corrective justice. The obvious first question to ask is: what is the result in terms of the embodiment of principle when the corrective justice account is extended to encompass the role played by the jury? Coleman's likely position may be surmised as follows.

The jury's role is simply to follow the judge's instructions as to the appropriate legal standard and determine liability by applying this standard to the facts of the case. Because the fault standard applied in the context of bilateral adjudication embodies (or is at least consistent with) the principle of corrective justice, when the jury finds on the question of negligence under this standard, it is participating in the production of an outcome that embodies corrective justice. Specifically, on Coleman's account, the jury is redressing a 'wrongful loss' that occurred when defendant injured plaintiff (Coleman 2001). Thus, Coleman has a plausible account as to how the jury's role in tort practice may be compatible with his corrective justice account.

This compatibility is not a necessary feature of the relationship between jury activity and corrective justice, however, as the jury may also act in a manner that would not plausibly be deemed correctively just. Suppose, for instance, that the jury found liability in a particular case only because of the 'deep pockets' of the defendant or the ethnicity of the plaintiff, rather than because it faithfully applied governing law to determine the existence of a wrongful injury. Presumably, Coleman would not wish to claim that this outcome embodied corrective justice, despite the bilateral structure of the litigation or the fact that the judge had instructed the jury to apply the reasonable person standard. This example compels the conclusion that a jury's finding on the issue of liability may be, but is not necessarily, an instance of corrective justice.

The important question, then, is what reason there is to suppose that juries are typically acting one way or the other. Coleman does not directly address this question. He does, however, note the existence of 'our ordinary intuitions about corrective justice' (p. 21). Under this assumption, when a situation arises in which people are capable of carrying out corrective justice, such as when they find themselves serving on a jury, they will be able to do so, and they will be inclined to do so, due to their intuitions about corrective justice.

Even if jurors do not have a natural inclination toward duties of repair for wrongful losses of the sort Coleman suggests, perhaps they might deliver corrective justice because that is what they are instructed

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to do by the judge. A look at pattern jury instructions, however, shows that this is not the case. These instructions make no mention of duties of repair for wrongful losses. The jury is instructed on the reasonable person standard. There is nothing in the meaning of the words reasonable, person, or standard or any concatenation thereof that tells a group of jurors who would not otherwise be disposed to redress wrongful losses to go ahead and do so.

In his attack on the economic account for failing to capture the structure of tort, Coleman observes that were the economic model true, 'the judge would instruct the jury to find against the party who is in the best position to reduce harm in the future'. Coleman continues, 'In other words, were the concept of efficiency embedded in the law, we should expect to have an entirely different structure and content to the practice of reasoning' (p. 23). It is not clear that Coleman's corrective justice account can pass this test. Coleman opens himself up to the same criticism he uses against the economist: If his is the best account, then why don't judges instruct juries in terms of 'duty to repair' 'wrongful losses'? What is needed is an account connecting up the language used by judges in their instructions and the calling forth of jury actions that serve to redress wrongful losses. In other words, Coleman needs an explanation for why what the jury does somehow tracks what would be called for by the substantive conception of responsibility for wrongful losses that Coleman argues to be operative. Without this account, his explanation will not be conceptually pragmatic but instead functional.

Thus, it appears that Coleman's corrective justice explanation of tort law depends on an implicit premise to the effect that people are characteristically inclined to act based on a natural inclination to redress wrongful losses when it is in their power to do so. This empirical premise, if it's one on which he relies, is worthy of further exploration. Perhaps this exploration will end up lending support to the corrective justice account. Empirical studies have indicated that people acting in many different contexts may display fairness motivations under certain circumstances. There clearly appears to be more evidence that people are motivated by fairness than by the desire to promote aggregate wealth or efficiency. Thus, the relevant social scientific studies support Coleman's account as compared to the economic account.

Based on his empirical work on tort juries, Neal Feigenson argues that jurors may be motivated by the desire to do 'total justice' (Feigenson 2000, p. 26). This may be too much justice for Coleman, however, as jurors may be correcting for unjust income disparities, a history of chattel slavery, or other injustices that go beyond simply correcting for defendant's imposition of a wrongful loss on plaintiff. In defense of Coleman's account, it could be replied that the judge's instructions will often tell the jury not to take account of factors such as the parties' wealth or whether the defendant is likely to be insured. Coleman could plausibly claim that these instructions serve as structural features that support his claim that tort law embodies corrective justice rather than Feigenson's total justice account, as these are arguably causal factors that push jury deliberations toward correcting wrongful losses resulting from the injury and not some broader set of normative considerations. Other features of courtroom activity may, however, support a total justice interpretation. For example, during the trial, lawyers for either party will try to shape the jury's sympathies toward their own client along any promising normative dimension and not just with respect to a concern for redressing wrongful losses.

Stepping back, then, the larger question for present purposes is: What is the relevance of these various normative influences for Coleman's account? First, Coleman may respond that he has never claimed that corrective justice is the only normative principle embedded in tort. Rather, his claim is that corrective justice is at the core. Coleman notes that a 'comparative justice expresses important moral values' (p. 5). More broadly, Coleman argues that tort must be understood within a broader context of liberal political theory. Coleman does not, however, discuss whether the jury may have a role to play in a liberal conception of tort law.

Arguably, looking at the jury's normatively complex role may support Coleman's claim, as it does appear that even if other normative influences are at play, the impulse toward redressing the wrongful loss to plaintiff due to defendant's injurious act may nevertheless be the dominant impulse. Coleman may also argue that not all normatively significant aspects of jury deliberation embody principle. This clearly seems true in the case of the cognitive biases, which are better seen as noise in the system than as normative features that embed principle. One does not practice a cognitive bias. Rather, a cognitive bias may add distortion to one's practice. If a tort system were a stereo system, the cognitive bias would be the hum or hiss, not one of the instrumental tracks. By contrast, the redress of wrongful losses would be the beat of the practice.

Summing up, then, the jury's role in American tort law may embody both substantive and procedural principles in addition to the principle of corrective justice. Depending on the relative importance of these principles in comparison to the principle of corrective justice, Coleman may still be correct that corrective justice nevertheless forms the core of tort law. This issue bears greater discussion. In particular, it would be of great interest to tort scholars to know Coleman's thoughts on the role of the jury in tort.

Coleman ends the preface to *Risks and Wrongs* by acknowledging his creative muses. These are not scholars but musicians. Neil Young makes the list. Just as Young is the Godfather of Grunge, Coleman may be the Godfather of the New Negligence, a movement that follows his lead in rejecting the traditional economic account of tort in favor of one that embodies jurisprudential sophistication.

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Nouvelle Histoire de la Pensée Economique, ALAIN BÉRAUD and GILBERT FACCARELLO, eds. La Découverte; vol. I, 1992, 620 pages; vol. II, 2000, 614 pages; vol. III, 2000, 525 pages.

1. Introduction

Three volumes, forty chapters, thirty-one authors, and over 1750 pages: The *Nouvelle Histoire de la Pensée Economique* assembled by the editors Alain Béraud and Gilbert Faccarello is an impressive achievement, an essential resource for scholars of the history of economic thought. In the general preface to the three volumes, the editors set out the criteria they used for the overall construction of their work and for the individual contributions. Regarding the former, we are told that it was necessary to transcend the two extreme positions that are still the hallmark of a fair proportion of studies in the history of economic thought: on the one hand, the idea that sees this history as a narration of 'facts' and a mere gallery of portraits (the expository approach); on the other, that of imagining the history of economic thought as a sort of triumphal march towards scientific truth, seen as a survey of discoveries and proven propositions (the analytical approach).

The declared intention of the editors is rather to 'seek out and give back analytic coherence to the numerous authors' (I: 12), to define the background against which a specific theory is formed, and to trace the development over time of the various theories' ability to explain. With regard to the expository strategy, we are told that beyond the differences in the individual contributions, 'the common line of attack' lies in the analytical approach, aiming to get to the heart of, and sometimes to reconstruct, the logical structure behind every theory.

All three volumes contain parts in smaller print, mainly the biographies of economists, but also explanations of the meaning and the history of the economic terms used (for example 'political economy', 'mercantilism'). These would be very useful if the purpose of the work were didactic, though many of the essays, as we shall see, do not seem to aim at this. There are frequent references in the text to concepts known only to specialists (two examples among many are 'the Ricardian vice' and 'the Bernoulli hypothesis'). A similar problem arises for references to authors not yet introduced. Since these are made with the idea of illustrating the thought of the economist under discussion, they shouldn't take for granted that the reader is familiar with parts of the book that have not yet been read (for example, in the chapter on Boisguilbert there are frequent references to Smith; in that on Cantillon and De Gournay, to Quesnay).

Indeed, it would have been better to specify the type of readership the work was intended for. This knowledge would have helped us understand various points of its structure, for example the fact that whereas some chapters (see, in particular, though not exclusively, chapters 23, 35, and 39) contain clear and useful expositions at an intermediate textbook level, other chapters are actual pieces of research, and as such are addressed to a specialist public. As the role of reviewer requires, in what follows we shall try to bring out both the stronger and the weaker points of this monumental piece of work, but we wish to make clear right from the start that the former very much outnumber the latter, making the work a successful one, of undoubted value.

2. From the Scholastics to the classical period: vol. I (620 pages)

Before pausing over what in our opinion is one of the best sections of the volume, that concerning Enlightenment economic thought, we would like to mention the first two parts (I, chs. 1–6), devoted to the economic thought of the 12th–15th centuries and to the mercantilists respectively. The surprising amount of space (77 pages) allotted to writers of these periods is certainly praiseworthy: Works of this kind rarely deal with the Scholastics and the school of Salamanca, let alone the still more ancient sources of inspiration of medieval thought. Also to be singled out for praise is the aim of the authors (André Lapidus and Ramon Tortajada) to provide a clear idea of the real worth of the economic thought preceding 1550, given the uneven fortunes it enjoyed in the centuries that followed. Philippe Steiner's exposition of mercantilist thought clarifies a great deal. He first sketches the actual economic situation of the period, then the economic categories in use at the time, and finally, the economic policy measures put forward. Our only difficulty is that, though often reiterating the lack of homogeneity in the positions of the writers grouped together under the term 'mercantilists', Steiner does not choose to deal with them individually. Instead, he opts for an exposition of a single unified mercantilist thought, to such an extent that he even manages to derive a draft of a 'mercantilist model' from

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the functioning of the economic system.We now come to Enlightenment economic thought, to which the volume devotes a great deal of space (I, chs. 7–11, 159 pages). Faccarello's introduction makes it clear that the choice of concentrating on French economic thought is motivated by the role France played as the leading inspiration of the Enlightenment in the period under consideration. This has led to the exclusion of many other important writers who lived elsewhere at the same time and, given the extreme fertility of French Enlightenment thought, even of some writers from France itself. However much we may agree with the selection, it is open to criticism from several angles, one of which brings us back to the question of the readership to which this work is addressed. The presence of frequent references to the authors excluded (for example Condillac, Condorcet, Beccaria, and Galiani) is certainly useful for the reader who already knows the subject, but raises obvious difficulties for those readers who cannot understand such references. It would perhaps have been possible to add brief paragraphs (maybe in smaller print) on the authors most frequently quoted. We also fail to grasp the reasons for the exclusion of William Petty, the author of *Political Aritmetik* (written in 1676, but published in 1690), a work which at several points anticipates classical theory – from the explanation of 'natural value', via a draft sketch of a theory of differential rent, to the laissez-faire theses on the subject of public finance. Petty also made proselytes of quite some stature (Graunt, Davenant, and Gregory King) and exercised great influence on scholars of the calibre of John Locke and Dudley North.

The wealth of economic thought during the French Enlightenment becomes evident from simply listing the authors considered: Faccarello deals with Boisguilbert as the founder of liberal economic thought; Antoin Murphy traces the stages of development of 'Law's system', illustrates the reactions of Cantillon to the failure of that system, and re-establishes the importance of Vincent de Gournay's group; Daniel Diatkine explains Hume's thought and that of Steuart, while Steiner writes on Quesnay; finally, under the title of *Sensualistes et utilitaristes*, Faccarello writes on Tourgot, and Annie Cot on Bentham. Outlined here are the first phases of the foundation of a liberal economic theory, of the birth of economic categories that were to have a long history, of important theoretical innovations at the macro- and microeconomic levels, of political economy's slow movement towards independence from moral philosophy, and of the project of founding a real economic 'science', transferring the method of the exact sciences to the study of economic problems. Among the many interesting theses put forward in this part of the work, we would like to single out Steiner's (I, ch. 10), according to which in Quesnay's time the agricultural sector had already lost importance in France's economy. Quite a number of scholars explain why the Physiocrats considered the agricultural sector to be the only generator of net income, thus underestimating the potential of the manufacturing sector. In contrast, Steiner brings out the criticisms addressed to Quesnay by his own contemporaries precisely in relation to the presumed primacy of the agricultural sector, leading us to believe that the economic and historical context could not provide a plausible explanation. If in addition, as Steiner would have us believe, physiocratic theory was in some ways outdated, this could also explain the sudden disappearance of Quesnay's school in the 1770s.

Still on this part of the first volume, it must be said that dealing with Bentham before setting out Smith's thought (which deeply influenced Bentham) is not a particularly fortunate decision. As a matter of fact, A. L. Cot, the author of part 11.2, on Bentham, rightly remarks: 'Bentham's theory of political economy cannot be understood without referring to Smith, whom he had read regularly' (295). Not only this, but the reader would certainly have benefited from a discussion, however brief, of the three theses that characterise utilitarian moral philosophy, i.e. the theses of consequentialism, of welfarism, and of sum-ranking. Only by starting with this knowledge can we begin to explain why Bentham's main work, of 1789, 'An Introduction to the Principles of Morals and Legislation' did not receive very much attention from the writers of the classical school, starting with Adam Smith himself, whilst immediately finding a fertile terrain of application in jurisprudence. As is well known, utilitarianism only definitely entered into the central corpus of economic theory with the marginalist revolution.

The text surveyed so far is a little more than half of the first volume (286 pages). While repeating our approval of the ample space devoted to the economic thought preceding the classical school, and also of the wealth of bibliographical information on an age so often neglected in publications of this kind, we believe unjustified the exclusion of what we may call the tradition of thought of civil economy. This is a tradition that goes back to the 'civil humanists' of the 15th century and that was developed, with varying success, through to the economic thinking of the Italian Enlightenment, consisting of the Lombardy school (Cesare Beccaria, Pietro Verri, and G. D. Romagnosi) and above all of the Neapolitan School (Paolo Doria, Ferdinando Galiani, and Antonio Genovesi). Nor should the fact that the first university chair of economic disciplines was established by the University of Naples in 1753 be passed over in silence; it was named the 'Chair in civil economy' and given to Genovesi, the author in 1765 of the important work *Lezioni di economia civile* (*Lectures on Civil Economy*).

The fourth and last part of the first volume (I, chs. 12–15) covers the years from Smith to Mill (273 pages). The introduction and the first two chapters are by Béraud. In chapter 12, after recalling the influence that Locke, Hume, the French philosophers of the 18th century, the physiocrats, Turgot, and above all Hutcheson had on Smith's thought, and after having rightly brought out the synthesis Smith derived from these previous approaches, Béraud analyses the various components of Smith's theoretical system: the division of labour, the theory of prices (and of value), and the theory of distribution. The author aims to demonstrate the inner coherence of Smith's analysis, which in his opinion was contested first by Ricardo and then by Marx, Sraffa and Dobb. He analyses Smith's theory of accumulation and growth, setting out among other things the various interpretations that the distinction between productive and unproductive labour has given rise to. Smith's contribution to the theory of money, generally considered to be negligible, is shown to be anything but secondary in importance. According to Béraud, it opened up the way to the theories that support free banking; for Smith, Béraud adds, '[r]eplacing silver with paper money as currency would increase income per year' (342), a very different outcome from Hume's. Béraud ends by explaining the functions Smith attributes to the state, and clears up an ambiguity here in the usual interpretation of the metaphor of the invisible hand. By contextualising the occasions when Smith makes use of this metaphor, the author discloses its multi-faceted meaning. On the one hand, it is providence that sees to it that the distribution of wealth is fairer than the rich would wish, and on the other there are non-economic reasons that drive men to invest in the various sectors of the economy.

One criticism may be made of the expository style of this chapter. Sometimes the lines of reasoning get jumbled together. For example, in the paragraph on the division of labour (pp. 318–9), it is said that it makes production increase (but less in the agricultural sector); that it develops knowledge, both from the technological point of view (the specialised worker can more easily find what tools make work easier for him) as well as from a social point of view (a specific social class can specialise in intellectual activities, and also subdivide again into groups of experts in various sectors); that it is not a rational choice, but one dictated by the natural instinct to operate exchanges, insofar as its advantages become manifest in the course of a dynamic process, since the difference in talent is not innate but emerges during learning; that it is encouraged by the expansion of markets, and that it reduces the cost of production; that it makes growth cumulative, but that it nevertheless limits the knowledge of the specialised workers just to the one environment in which they operate, making them ignorant of the rest; and that to compensate for this tendency the state should take upon itself the responsibility for popular education. All these consequences of the division of labour would perhaps require broader treatment compared to the barely two pages they are allotted here.

Chapter 13 contains the biographies of Malthus, Ricardo, Torrens, Say, and James Mill, but the ideas of many more authors are set out. Béraud's decision to treat this age of economic thought by subject and not by authors raises some difficulties of exposition, due to the fact that a real 'classical school' does not exist. As the author reminds us, the exponents of the classical school of thought did not for the most part share the same ideas; the only characteristic that the writers Béraud examines have in common is their having started from a reading of the *Wealth of Nations*. In this case, the criticism we voiced about the term 'mercantilist thought' does not apply, because the diversity of the economists who came after Smith is not sacrificed by Béraud on the altar of the construction of a single model. From Lauderdale to Say, from Torrens to Buchanan, from Sismondi to Dupuit, from Malthus to Ricardo, from Barton to James Mill, the author reconstructs all the theoretical controversies on the crucial subjects of economic theory (population, value, distribution, accumulation, and crisis). This chapter belongs without doubt to that part of the work addressed to specialists, both because the author takes for granted the various meanings that certain economic terms have for the various authors whose theses he is describing (one example can stand for all: the concept of 'natural prices'), and because he proposes an original reading of his own of parts of classical economic thought, contrasting it with that of other historians. Finally, we recall in summary form the two final chapters (14 and 15) of the first volume, devoted respectively to the period immediately after the death of Ricardo and to the debates on monetary subjects that took place in Great Britain in the first half of the 19th century. Focusing on three themes (method, theory of value and distribution, dynamics and economic crisis), Richard Arena describes the thought of Senior, Longfield, Bailey, De Quincey, McCulloch, Sismondi, Jones, J. Mill, Torrens, and Chalmers. One criticism that can be made of this part, in other ways very free-flowing and interesting, is that short biographies of the economists under discussion would have been very useful. Anna Maricic outlines with clarity the transitional role J. S. Mill played in the passage from classical theory to the later theoretical developments. Jérôme De Boyer's chapter on the debates on monetary subjects is yet another example of the already mentioned virtue of this work, namely to deal with subjects that works of this kind often neglect.

3. From the first socialist movements to the neo-classicists: vol. II (614 pages)

The second volume is divided into two parts: almost half (Part V, 262 pages) on socialist thought, and the rest (Part VI, 342 pages) on the marginalists and the neo-classicists. Part V covers almost two centuries, from the analysis of the early socialists' thought to the present day return to classical and Marxian themes. In the introduction to the five chapters on socialist thought, Faccarello declares his intention to give a coherent account of the work of several authors whose thought, for various reasons, has got deformed in the course of time. This intention is shared by Ragip Ege who, on dealing with the early English socialists (Owen and the 'Ricardian' socialists), brings out their affiliation to the ideas of Smith rather than to those of Ricardo. Equally, when dealing with French social reformers (Sismondi, Saint-Simon, and Proudhon), he sets out rarely remembered aspects of their economic thought. But it is the very substantial chapter 17 (108 pages) on Marx that it is worth our while to pause over. The declared and brilliantly achieved aim of the author (Faccarello) is 'to give an exact idea of certain essential aspects of Marx's economic thought and of the debates that have arisen from it'. The organisation of the chapter is well considered, and the abundant use of quotations, well chosen and well integrated into the structure of the argument, give the impression of reading Marx's original text with critical 'glosses' by Faccarello.

To start with, the author explains the idea of society that lies behind Marx's belief that it was necessary to study political economy and his reasons for remaining in the tradition of the 'classical' economists (according to his own specific understanding of the term). Faccarello distinguishes three different, yet simultaneously present, strata in Marx's reasoning on economic subjects, and thanks to the choice of this method of analysis, Marx's theses emerge very clearly. Faccarello suggests the three strata, or approaches, correspond to the three requirements on which the Marxian framework is built: the need to place capitalism in a historical perspective and relativise the laws of its functioning; the need to utilise a specific logic for the specific object of Marx's study, i.e. for the capitalistic mode of production; and, finally, the need to demonstrate that the crisis of capitalism will end with the construction of a socialist society followed by a communist one. Within the first approach the author critically examines the Marxian construction of the labour theory of value and leaves the problem of the definition of 'abstract labour' to one side. He sets out the theory of surplus value and exploitation as well as the transformation problem, the problem of passing from values to costs of production, always using the same effective method of outlining Marx's arguments and of commenting on them critically to verify their rigour.

The analysis of the second approach, the uncovering of the distinct nature of capitalism, is utilised by the author to clarify the Marxian categories of 'social labour', the 'social division of labour' and above all the one 'abstract labour', which he had temporarily left to one side. On this subject, Faccarello draws out the contradictions that arise in the definition of the same concepts in the two approaches (for example that of value as embodied labour and of value measured in monetary terms) and also the possibility of resolving certain questions within the second approach that remained open in the first. In his exposition of the third approach, entitled 'the dialectical deduction of concepts', the author traces in Marx's writings the demonstration that 'monetary relations inevitably lead to the . . . capitalist relations of exploitation' (120), a position considered inadequate in the first two approaches. In this part, Marx's debt to Hegel's method is particularly emphasised. As well as setting out the Marxian theses and commenting on them critically, the author places them in a historical perspective, quoting the writers from whom Marx's thought derives and much of the literature on later developments. Even a non-specialist reader may find the exposition of Marx's economic thought admirably clear.

In chapters 18 and 19, on '*Les controverses autour du* Capital' ('*Controversies around* Das Kapital'), Faccarello concentrates on the debates concerning the labour theory of value as an expression of the capitalistic mode of production, and Christian Tutin deals with those concerning the periodic crises of over-production. The authors whose contributions are examined are Sombart, Schmidt, Engels, Hilderfing (his answer to the criticisms of Böhm-Bawerk), and Petry on the relation between the theory of value and relative prices; Bortkiewicz, Sweezy, and Sraffa on the logical coherence of the schemes of transformation of values into prices; Bortkiewicz and Lange on the Marxian thesis of the origin of profit in exploitation and surplus value; and Hilderfing, Tugan-Baranowski, Rosa Luxemburg, Bukharin and Lenin on imperialism, the law of accumulation and the crisis of reproduction. Much appreciated is the ample room provided in these two chapters for the return to these subjects in more recent studies. Chapter 20, the last chapter of this part, by Bernard Chavance, examines the theory of the socialist economy in the countries of Eastern Europe between 1917 and 1989. As the author points out, this is a tradition of thinking that had to come to terms with the ideological legitimisation of the socialist economic system, with the unified theory of the socialist mode of production of the Stalin era, and with the theories that marked the gradual moving away from that dogmatic vision. The contributions examined are those of Kautsky, Lenin, Bukharin, Preobrazhensky, the exponents of the 'Russian mathematical school' (Kantorovitch, Fedorenko, Novojilov, and Nemtchinov) and the authors of the 'national variants' of that system (the Hungarians Kornai and Bauer, the Pole Brus, the Czechoslovakian Sik).

Part VI of volume II, covering chs. 21–27, is about the marginalist and neo-classical theoretical systems. Béraud's introduction quite appropriately traces the differences in method and content between Jevons and Walras on the one hand and Menger on the other, for there is still widespread belief among historians of economic thought that the three writers to whom we owe the famous marginalist revolution share the same theoretical framework. As a matter of fact, while for Jevons and Walras the main and basic aim of economic research 'is to make of economics a mathematical science' (266), for the founder of the Austrian school, mathematics in economics must be used in moderation and, at any rate, not in such a way as to hinder the search for economic laws, which do not have the slightest need of inductive confirmation. Rather, what for Menger defines economics from the point of view of its scientific method is the paradigm of methodological individualism, the position according to which all propositions about the behaviour of collective entities should be traceable to propositions about the behaviour of their individual components.

No less significant are the differences regarding content. If, for Menger, economics has to study primarily the nature of human needs and the temporal dimension of economic decisions, Jevons and Walras aim to construct a theoretical system capable of dealing with the problem of the allocation of given resources among alternative uses. We agree with Béraud's conclusions when he writes: 'It is therefore doubtful that Menger's and his successors' work can be considered simply a variation of marginalism. It is profoundly different both in object and method' (267).

This basic thesis receives interesting and valid elements of support from François Etner in chapter 21 (an effective reconstruction of the origin of neo-classical thought in Gossen's theory of consumer choices and von Thunen's on rent); from Béraud in chapter 23 (containing a substantial and invaluable account of the contribution of the English neo-classicists and of Marshall in particular); from Jerôme Lallement in chapter 24 (who, as well as providing us with an excellent account of Walras' model of general economic equilibrium, manages to explain Walras' dual scientific personality – as defender of the method of pure theory on the one hand, as advocate of social economics on the other); from Antoine Rebeyrol in chapter 26 (who is reluctant to throw light on the most intricate problem of the theory of income distribution and of the theory of capital as faced by Clark, Jevons, Böhm-Bawerk and Walras); and above all from Béraud's long chapter 22, wholly dedicated to the Austrian school. Concerning the latter, certain points need to be underlined.

We fully agree with Béraud when he argues that Menger was the most extraordinary of the three founders of marginalism, as is evident from the fact that at the end of the nineteenth century his influence in the cultural-economic circles of continental Europe was far greater than that of Jevons and Walras. What were the reasons for this? There are two principal ones. First, to Menger we owe the discovery of the problem that the newborn neo-classical theory had to solve: in what circumstances can the principle of marginal utility be considered to be basic to all economic discourse? Any answer would have to leave room for the principle to be extended from the domain of exchange to that of the production and distribution of income. And it is here that the difficulties arise.

In fact, whereas the demand of goods can be traced directly to utility, the supply of goods is regulated by the costs of production. But how can the latter be compared to utility? Menger's imaginative intuition was to reduce costs to some kind of entity homogeneous with utility: His theories of imputation and of opportunity cost serve precisely to solve the costs in utility. From this perspective, demand and supply appear as two aspects of the same problem and can both be explained in terms of utility. But there is more to it. Since what counts as costs for the firm represents income for the owners of the productive factors, the same principle – at the same time as it explains the phenomenon of costs – explains the distribution of incomes. The distribution of income thus ceases to be a chapter apart of economic theory, as it was in classical theory, and becomes an integral part of the theory of prices. Hence it happens that while for the other versions of marginalism more than a couple of decades will be needed to arrive at the demonstration that the marginal utility theory of value can found a theory of distribution, Menger reached this conclusion straightaway.

The second reason for the superiority of the founder of the Austrian school lies in his ability to match methodological individualism with the methodological essentialism of Aristotelian ancestry. This is what allows Menger to start from the famous 'table of needs', a table that, as Georgescu-Roegen will show later, can be held to be the first elucidation of a lexicographic ordering of preferences. According to Béraud, it is perhaps the novelty and the profundity of Menger's thought that explains why von Wieser, his pupil and successor at the university chair, will later reject the theory of imputation (in favour of a somewhat blurred 'theory of natural values'), methodological individualism (in favour of the idea that human needs are social and that the individual is a creature of his nation and his social class), as well as the classical liberalism that had inspired his master. Finally, Béraud concludes with a particularly lucid reconstruction of the constitution of the 'new Austrian school' in America through writers such as Rothbard, Kirzner, Lachman, Rizzo, and O'Driscoll, and of the influence the main ideas of the neo-Austrian research programme had on many other schools of thought.

Chapter 25 concentrates on the economics of Pareto and Walras. The explicit aim of Steiner's contribution is not so much to describe Pareto's entire work, but rather to focus on the substantial differences between the theoretical framework of the great Italian economist and that of Walras. Steiner correctly explains how the celebrated distinction Pareto makes between pure economics, applied economics, and sociology is not the same as Walras' distinction between pure economics, applied economics, and social economics. Moreover, contrary to what is often believed, the theory of action Pareto has in mind is different, as is the role he assigns to pure economics, than what Walras put forward. Also of special interest is the historical reconstruction Steiner makes of Pareto's criterion of optimality, a criterion that brings to completion Walras' efforts to demonstrate the superiority of the competitive market over other market structures.

We feel there are two observations to be made. In the first place, it would have been a good idea to remind the reader that the first instance of the Pareto optimality criterion is to be traced to Edgeworth's *Mathematical Psychics* of 1881, whereas the invention of the famous 'Edgeworth box' is due to Pareto, who makes use of it in his *Manual* of 1905. Our second observation concerns the use Steiner makes of Amartya Sen's famous finding of 'the impossibility of a Paretian Liberal' (1970) to show that the Pareto criterion does not allow us to justify liberal democracy. In fact, Sen's theorem can certainly be employed to show there can be situations in which liberal values enter into conflict with the values underlying Pareto's criterion. But phrased in this way the reader cannot appreciate the true aim of Sen's work, which is to attack Bentham's utilitarianism and its radical incapacity to include a category of rights in its moral calculus. Since questions connected to the theory of social choice are not dealt with anywhere else in the work, it would have been a good idea in this chapter to clarify how the impossibility of a Paretian Liberal captures the profound tension between two principles: that of Pareto, which proposes to base social decisions exclusively on information of utility (ordinal), and the liberal principle that, at least for certain classes of social choices, requires the attribution of a primary role to extra-utilitarian information.

Part VI of the second volume closes with chapter 27, where Jerôme De Boyer gives an accurate and refined account of monetary theory in neo-classical pre-Keynesian thought, in which the figure of Wicksell, the founder of the 'Stockholm school', deservedly stands out.

4. From the institutionalists to the contemporary period: vol. III (525 pages)

The third volume, comprising parts VII, VIII and IX, deals with the history of twentieth century economic thought. In the introduction to part VII, Béraud sets out to defend a thesis that runs like a thread through the three chapters of this part: Historicists and institutionalists cannot be considered heterodox economists, as most manuals of the history of economic thought tend to represent them. Rather, it was the followers and admirers of the classical and neo-classical schools who were the exception in more than a few cultural traditions of the period examined here. In fact, it is not difficult to agree with Béraud's thesis if one takes into account that on both the methodological front (the aversion to the excessive use in economics of the rules of the hypothetical-deductive method) and on the front of the content of scientific research (the rejection of the *homo oeconomicus* paradigm and, more generally, of the fragmentation of knowledge in the social sciences), the criticism that historicists and institutionalists direct at their adversaries was fully shared in the cultural circles of the time in continental Europe and the USA.

Steiner offers us effective confirmation of this in chapter 28, where the entry of American institutionalism and the German historical school into the French economic cultural *milieu* is analysed. The scholar to whom we owe the performance of this role is François Simiand, who takes it upon himself to translate into the terms of economic discourse those categories of thought and those ideas for research that the great sociologist E. Durkheim had already successfully applied to sociology and to the social sciences in general at the end of the nineteenth century.

In chapter 29, Vitantonio Gioia competently deals with the genesis (i.e. the connection with the relevant facts in the economic history of Germany) and the evolution of the German historical school. Three points, in particular, are convincingly defended. First, that the thesis, still widely held among economists, that the historical school was characterised by a strong anti-theoretical attitude is unacceptable. Second, that the new historical school rejected marginalism is false. Schmoller, in particular, makes the neo-classical categories his own, even if these are then used in *sui generis* ways and for entirely specific ends. Finally, that even the idea of an anti-liberal historical school at the level of recommendations for economic policy is unacceptable. On the contrary, scholars like Hildenbrand and Knies even suffer harassment and discrimination of several kinds for their liberal beliefs on the subject of economic policy.

This part ends with chapter 30 by Jean-Jacques Gislain. He presents a brilliant reconstruction of the events, historical and, more precisely, cultural, leading to the birth of institutionalism in America. Veblen is the central figure of this school of thought. Social psychology in its typically American version, instinctivism; the pragmatic philosophy of Pierce and James; evolutionism in the version of the social Darwinism of Spencer; and finally, German historicism (it should not be forgotten that the American Economic Association was set up in Saratoga in 1885 on markedly historicist foundations) are all presented; these are the philosophical roots and the cultural coordinates of American institutionalist thought.

Part VIII of the third volume is entirely dedicated to the 'years of high theory', an era that was, to quote Shackle, 'a great creative space that . . . produced six or seven fundamental theoretical innovations which, taken together, completely modified the direction and character of economic science' (1967, *The Years of High Theory*, p. 8). But perhaps there were more than six or seven, to judge from the fact that most contemporary theories of the cycle, of development, of the firm and of non-competitive markets, of employment and expectations, of distribution and welfare economics, and of international trade and money grow from the seeds sown in the years between the two world wars.

Chapters 31 and 32 deal with the microeconomic subjects of the neo-classical programme of the so-called third generation. Michel Rainelli offers us an accurate historical reconstruction of the events that marked the passage from Marshall's cardinalism to the ordinalism of Slutsky, Hicks and Allen in consumer theory and, more generally, in value theory. The thorny question of the integrability of demand functions is glossed over rather too swiftly. Mention is also made of Georgescu-Roegen's innovative study of 1936 in which it was proved for the first time that the integral curves of the differential equation representing the consumer's equilibrium condition do not necessarily represent the indifference map of the consumer. However, the reader would find it hard to grasp the implications of this study if he or she was not already familiar with the terrain. Then the interesting history of the birth of the theory of imperfect competition is told, running from Sraffa's criticism of the Marshallian theoretical system to the pioneering studies of Hayek and Coase, both of 1937, on market process and functioning costs. The case studies of the rivalry between Chamberlain and J. Robinson over the theory of imperfect competition and the history of the events that accompany the return, after the long period of silence that followed Cournot's contribution, of the theory of oligopoly are effectively presented. André Zylberberg, in chapter 32, focuses his attention on the first theorems concerning the existence of a general economic equilibrium and on the remarkable contribution to the field made by von Neumann on the one hand, as well as on the reception of Walras' theory in England through the work of J. Hicks on the other. Also of great interest is the way in which Zylberberg reconstructs the famous debate on economic calculus in socialism, a debate that sees the contrasting perspectives of Lange and Lerner on one side and von Mises and Hayek on the other.

Chapters 33, 34 and 35 are about the most important macroeconomic and dynamic theories in the years of high theory. Chapter 35 is devoted to an excellent account of the theory of international economic relations. Christian Gehrke and Heinz Kurz offer us a splendid survey of the contribution of continental Europe to the development of macroeconomics, from Swedish theoreticians of monetary economics (Myrdal and Lindahl) to the Austrian scholars of economic cycles (von Mises, Schumpeter, and Hayek) and from the precursors of Keynesianism in German speaking countries (C. A. Hahn, A. Spiethoff, A. Löwe and W. Röpke) to scholars such as Frisch, Tinbergen and Kalecky who, albeit in different ways, return to focus on problems that had been present at the birth of economic science, those of macroeconomic dynamics. We should not be surprised that they took considerable time to free themselves, often without succeeding, from 'techniques of thinking' that served more to conceal reality rather than to reveal it. Nor is it to be wondered at if, in the end, they produced imperfect and incoherent theories. Chapter 33 also sheds light on the quarrels Hayek had with both Keynes and Sraffa. In the long and instructive chapter 34, Rodolphe Dos Santos Ferreira deals with

Keynes and the theory of employment in a monetary economy. He makes three strong points. The first is a convincing defence of the thesis that Keynes produced a real scientific revolution, at least in the meaning Kuhn gave to this expression. The second lies in the precise explanation of why Keynes continues to be considered, even by scholars sympathetic to him, an obscure author and why the *General Theory* can be judged a badly written book. Finally, the summary of how Keynes, like other big-wigs of economics such as Marx and Marshall, was interpreted reductively – not only by his critics, which might seem to be in the nature of things, but also by his defenders and even by his apologists – is much appreciated.

Let us make two criticisms. To us, the concentration on the *General Theory* seems excessive. The episodic and, at any rate, insufficient references to the *Treatise*, the *Tract on Monetary Reform*, and the essay 'The End of *Laissez-Faire*', as well as the absence of a reconstruction of Keynes' thought from a philosophical perspective and of the influence of the celebrated 'Cambridge circle' on the latter do not allow the reader to fully understand how 'Keynes had been able to become Keynesian'. We believe that it will never be possible to grasp those aspects of profound originality of Keynes' scientific work, let alone to criticise them, if they are separated from his philosophical and political matrix. The second criticism is that far too little room is given to the implications for economic policy that arise from the Keynesian theoretical system. Two pages out of fifty-five are really too few when it is remembered that Keynes' revolutionary book ends with a chapter on the 'social philosophy towards which the *General Theory* could lead'. This is all the more the case if one considers that the epistemological stance adopted would lead Keynes to become an opponent of unfettered markets, while remaining a liberal! State intervention should not abolish the 'invisible hand', but help it to function when it ends up with cramps for one reason or another. This is the nucleus of the philosophy of the managed market that exercised so much influence on governments and on the ruling classes of Western countries after the Second World War, at least until the rise of Friedman's monetarism.

Part IX presents and discusses the developments of economic thought of the contemporary period, from 1945 to the present. These make up five of the forty chapters overall, little more than 10% of the entire number of pages. Yet quantitative historiographical research has demonstrated that, whatever index one wishes to use to measure it, scientific production has grown at an exponential rate in these last decades, with the amazing consequence that certainly more than 70% of the scientists who have ever lived have generated new knowledge in the last half century. Certain striking gaps and omissions that characterise this part of the work are owed to this discrepancy in representation.

In chapter 36, Philippe Mongin supplies an excellent critical review, referring to the 1930–1970 period, of the studies of economic methodology: starting with the a priorism of von Mises, Knight and Robbins; passing via Hutchison, to whom we owe the application of the canons of the neo-positivist statute to economic discourse; and ending with an acute critical analysis of Friedman's 'Methodology of Positive Economics' (1953), a real forerunner of Popper's falsificationism in economics. The author's thesis, which we endorse, is that starting from 1970, the important questions of methodology disappear off the horizon of the leading economists, to become the domain of research of philosophers of science and of a small group of specialist economists. Economic methodology sets itself up as an independent discipline, isolating itself from ordinary theoretical production. This would represent an ignoring of Kant's warning that it is the use, i.e. the scientific practice, that gives the method and not vice versa. Of particular interest is Mongin's treatment of two case studies, one about the so-called marginalist controversy in the theory of the firm and one on the theory of revealed preference in its specifically methodological aspects. Quite rightly, the author denies that Samuelson's methodology could be considered operationalist in P. Bridgman's sense; it is rather in the genre of 'refutationist positivism'. A couple of remarks seem to us not out of place. It would have been useful to remember that the theory of revealed preference was not born, at least at the level of intuition, with the famous study of Samuelson of 1938 but with the work of the Pisan G.B. Antonelli of 1888. Secondly, the reader would have been assisted by some reference to the evolution of game theory, starting with the fundamental work by Morgenstern and von Neumann of 1944. A subject so central to contemporary economic theory as game theory is not dealt with in this work (a brief mention is to be found in chapter 37 about the connection between the theory of general equilibrium and game theory). And yet, the problems both at an epistemological level and with respect to history of thought that the employment of the instruments of game theory has presented our discipline with (and continues to do so) cannot be dismissed as irrelevant. One example of the important distinctions introduced by game theory is the placement of strategic rationality and of so-called we-rationality outside the consolidated paradigm of instrumental rationality.

In chapter 37, we find a clear and thorough treatment of the developments of the theory of general equilibrium, starting from the magisterial systematisation that Hicks makes of it in his *Value and Capital* of 1939. After having described, with a wealth of detail, the events surrounding the formulation of the existence theorem by Arrow, Debreu and McKenzie, and after having illustrated the 'political' meaning of welfare economics, the authors – Bernard Guerrien and Claire Pignol – pause to consider the defeat of the neo-Walrasian programme of research with respect to the uniqueness and stability of a general equilibrium. Recent research

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concerning the problem of stability and uniqueness (findings due principally to Sonnenschein 1972 and to Kirman 1989) shows that the behaviour of individuals is not sufficient to give the 'invisible hand' the strength it requires to take the market towards equilibrium. In fact, to obtain a stable equilibrium it is necessary to posit some strong hypotheses on the behaviour of some aggregate variable. The knowledge of the criteria of individual agents' behaviour by itself is not sufficient to justify any of these hypotheses. This is to say that the market of an individualistic competitive economy fails to reach equilibrium, despite the fact that equilibrium exists. This result holds even when the market is regulated by the supreme hand of the auctioneer. In sum, this amounts to nothing less than an erosion of the 'scientific' foundation of the theory of free market economics and of orthodox economic doctrine.

In the absence of a separate chapter, what we are surveying here could have contained at least one paragraph on that fertile line of enquiry set off by Arrow's well-known 1951 study (*Collective Choice and Individual Values*), which is today known as the theory of social choice. Aside from its merely quantitative aspects (quite remarkable, to judge from the number of scholars involved, the volume of work produced, and the scientific journals that have been created), this is a theory that has generated a really peculiar area of research. In fact, though having profound roots in the neo-classical system of thought, positions strongly critical of orthodox approaches have also been able to emerge in the field over the last half century. As a typical example, we think of Sen's intellectual endeavour and his rigorous criticism of utilitarianism, a criticism from which has arisen, among other things, both a profound alteration of the way we conceive of the traditional connection between efficiency and equity, and the abandonment of the celebrated thesis of the neutrality of economic knowledge. It is really a pity that in a work of these proportions no room was made to outline this history, even more so in view of the fact that Arrow's theorem initiated an important school of political research, one that now dominates American political science.

In chapter 38, Antoine D'Autunne presents us with an interpretation of the developments of macroeconomics in the last fifty years, starting from the celebrated 'neo-classical synthesis' that emerges with the fundamental article of F. Modigliani of 1944 and culminates in Patinkin's *Money, Interest and Prices* (1956). As part of the process of 'normalisation', i.e. of re-absorption of the Keynesian heresy into the neo-classical mainstream, the advocates of the synthesis go ahead with a series of studies on specific aspects of Keynesian theory with the aim of correcting some of its specific defects or of refining certain specific points to make them fit in with the findings of empirical research. From these studies (above all those on the consumption function, the money demand function, the theory of inflation, and growth) debates ensue that lead to discarding or emending certain peculiarities of the Keynesian theoretical system in such a way that, in the end, it is rendered quite unrecognisable. Of all this, the chapter in question gives an exact account. Concerning the theory of growth, the link established between Solow's 1956 contribution and Lucas' and Romer's theory of endogenous growth is of particular interest. However, we would have preferred more room (and not just a few lines) for the line of thought of Harrod, J. Robinson, Kaldor, Pasinetti, and Minsky, i.e. for the anti-neo-classical reinterpretations of Keynes in the work of the so-called post-Keynesians. Criticism could certainly have been made of them, but to have cast a veil of charitable silence on their efforts does not seem wise in a summa of history of economic thought.

In the second part of the chapter, D'Autunne seeks a sort of smallest common denominator at the basis of the myriad of studies that, starting from the 1970s, embrace the programmes of the theoreticians of the microfoundations of macroeconomics; of the so-called new Keynesians (theories of non-Walrasian equilibria or rationing); and of the authors of the revolution of rational expectations. We agree with D'Autunne when he writes that it is possible to see, with hindsight, a new synthesis of thought centred on the following nucleus of common points: (a) the explicit consideration given to the inter-temporal dimension of individual behaviour (to give a typical example, think of Barro's finding about Ricardian equivalence); (b) the incorporation of the stochastic dimension into the analysis; and (c) the necessity of giving due consideration to the strategic aspects of economic agents' interaction. It would perhaps have added to the completeness of the treatment if a further point had been included, namely the recognition of the importance of the institutional context within which subjects make their choices. But this would have required some attention to that diverse set of research areas that are today subsumed under the expression of 'new political economy', including the public choice school, behavioural economics, property rights economics, and neo-institutionalism. It is rather paradoxical that after having dedicated a good two chapters to institutionalism, the editors of this work have not felt they should say at least something about modern neo-institutionalism, one of the most flourishing branches of research today.

Part IX closes with chapters 39 and 40, the former on recent developments of the theory of international economic relations, both real and monetary, and the latter on the emergence of a new area of research: the economics of development. These are accurate and decidedly interesting expositions. Bernard Guillochon goes so far as to deal with the more recent contributions, such as 'new economic geography' and the new theories of international trade. However, it is astonishing that no mention is made of the epoch-making phenomenon of globalisation and of the consequences that arise with respect to the study of international monetary questions. Elsan Assidon presents us with a useful panorama of the various schools of thought that have tried to explain the phenomenon of underdevelopment, from the theory of dependence of Singer and Prebish (via Lewis' theory of the excess of labour supply) and Rostow's theories of the stages of development, to Hirschman's unbalanced development approach. But only a few lines are reserved for the programme of research known as the political economy of human rights mostly linked to the work of Sen and others on poverty and inequality. Similarly absent is any kind of discussion of the new world economic order associated with the interventions of the international financial organisations (International Monetary Fund, World Bank, World Trade Organisation).

One final observation: Whatever judgment one might wish to make, it is strange to say the least that the lively (and sometimes ferocious) debate on the theory of capital between the two Cambridges (UK and MIT) should be confined, in a work of over 1750 pages, to fifteen lines of chapter 38, to fleeting references to Sraffa and to Garegnani in chapter 18, and to the bibliography at the end of chapter 26. Nothing is said of the so-called re-switching of techniques, a phenomenon that negates the neo-classical parabola according to which the capitalistic intensity of techniques is found in a decreasing relation to the ratio between the prices of the 'factors' of capital and labour. Nor is anything substantial said about Sraffa's *Production of Commodities by Means of Commodities* (1960) and the reason why this concise book was able to attract, for over a quarter of a century, such an impressive number of scholars of different and sometimes opposing inclinations, only to disappear from the scene in the early nineties. Given the paucity of references Sraffa provided to account for his sources, it is certainly not easy to find a place for this book that unambiguously locates it in the history of economic thought.

But precisely this, we believe, is one of the important tasks a history of economic thought has to perform, if it wants to adopt the history of ideas approach (in the sense of both Arthur Lovejoy and Aby Warburg) – the only approach that can make this discipline pertinent and useful. As is well known, the fundamental question this mode of understanding history tries to answer is: How is a theoretical system formed, where a theoretical system is a general theory that aspires, or at least tends towards, giving a coherent and exhaustive reply to every problem that may arise within a defined field of inquiry? What determines the success and failure of a theoretical system? Why are certain epochs characterised by hegemonic systems, when in others one has the impression of witnessing theoretical anarchy? We think that only a history of thought that does not hesitate to offer at least a tentative answer to questions of this kind deserves autonomy within economic discourse. Indeed, we will never understand the theoretical contribution of any great economist properly unless we understand her time; yet, we do in turn understand that time partly thanks to her scientific endeavour. The impressive work edited by Béraud and Faccarello on the whole satisfies this criterion. For this reason it should be welcomed and, above all, read with care.

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Valuing Freedoms: Sen's Capability Approach and Poverty Reduction, Sabina Alkire. Oxford University Press, 2002, vii + 340 pages.

In recent years, there has been an explosion of interest in Amartya Sen's capability approach to normative evaluations and policy prescriptions. The capability approach advocates that our evaluative judgments focus on what people can do or be, i.e. on their capabilities, and that development should be thought of as the expansion of people's capabilities. The capability approach has thereby become a serious contender for utilitarianism and resources-based evaluations. However, it is still far removed from a mature and well-established framework.

The literature on the capability approach, including both Sen's own contributions and those that Sen's work has triggered, is still characterised by a number of features that make the approach quite complex. For one thing, this literature is highly interdisciplinary and scattered over a wide and diverse range of journals. Sen has tried to address different groups of scholars and has thus written in different disciplinary and methodological styles and discourses. As a result, the capability approach is being discussed in such diverse fields as social choice theory, mainstream welfare economics, heterodox economics, liberal egalitarianism, moral philosophy, development ethics, development economics, social and political theory, education, gender studies, theology, and so forth. In all these fields, scholars have somewhat different understandings of the approach. In addition, Sen has developed the capability approach gradually, which implies that in order to properly understand the capability approach, one needs to read quite a lot of Sen's work, as he has not written a book or article where the capability approach is clarified and explained in its entirety. Due to this cross-disciplinary complexity, too many of Sen's readers seem to have forced the capability approach into a too narrow and single-paradigmatic interpretation. This has led to quite different and not always compatible interpretations of the approach.

A second important feature of Sen's capability approach is its deliberately underspecified nature. Strictly speaking, the capability approach only advocates that our normative assessments of individual well-being and advantage should focus on people's capabilities. It does not specify which capabilities should be taken into account, nor who is going to decide on this selection, nor how trade-offs between different capabilities should be made. It has been Sen's deliberate choice to keep the capability approach assertively underspecified, for two main reasons. First, the selection of capabilities should be done by means of a democratic social choice procedure and public reasoning. Second, the selection of capabilities should depend on the evaluative context. The capability approach can be used both for the design of a poverty reduction program in Zambia and theorizing distributive justice in the USA, but different actors will have to decide on the selection of capabilities that will be taken into account, and this selection is likely to be different. One important consequence of its underspecified character is that it is not immediately obvious how the capability approach can be put to work. Sen does not describe *how* we should select the relevant capabilities, nor does he propose practical methodologies to assess capabilities in an empirical setting. This has led to several criticisms, including Robert Sugden's, that it is not clear at all how Sen's capability approach can be applied or operationalised, and Martha Nussbaum's claim that Sen should endorse one particular universal well-defined list of capabilities. There is a broad consensus that advocates of the capability approach need to show how it can be operationalised, that is, how the general ideas and concepts of the approach can be further developed so as to make the approach a useful and applicable tool for normative evaluations and policy design.

Sabina Alkire's *Valuing Freedoms* is an important contribution to the literature on the capability approach. Her book is written for those who

> wish to consider how to operationalise Sen's approach – to put it into practice in uncomfortable, messy, compromised practical work at the microeconomic level. It is also written for skeptics (philosophical and economic) who claim there to be nothing value-added in the capability approach because it leaves too many values issues unresolved and thus is impractical. (p. v)

Alkire's book has two parts. The first part deals with a number of issues debated in the capability literature in development economics and development ethics. Scholars familiar with these debates will find a thorough and in-depth examination of the various critiques and proposals as to how to solve these issues. However, scholars new to the capability approach or those who know the capability approach from another field might not always see the thread through the chapters of the first part of this book. Nevertheless, these chapters provide the necessary foundational theoretical work for the second part of the book, which discusses how the capability approach can be operationalised in the context of micro-evaluations of participatory non-governmental organization (NGO) projects in poor countries. This case study also provides a test case to establish whether the capability approach really leads to substantially different evaluations compared with standard economic poverty evaluations.

The first operationalisation issue, which is analyzed in chapter 2 of the first part of the book, is the issue of 'the list': How can we select the relevant capabilities? Alkire discusses Nussbaum's list of central human capabilities and Finnis' work on basic human reasons for action. Alkire argues that Nussbaum's list, while being an important contribution to the capability approach, has limited relevance for the evaluation of small-scale NGO projects, due to its strongly prescriptive character, its orientation to international policies, and its lack of an account of *who* is deciding on the list's items and how the involvement and participation of the people most concerned will be guaranteed and implemented. Alkire instead defends the selection of the valuable capabilities based on Finnis' practical reasoning approach. Finnis claims that by iteratively asking 'Why do I do what I do?', one comes to the most basic reasons for acting: life, knowledge, play, aesthetic experience, sociability (friendship), practical reasonableness and religion. Finnis' list can thus be used as a basis from which to further develop the capability approach and Alkire uses his list in her case study in the second part of the book.

A second methodological issue that needs to be discussed in order to advance the capability approach is the kind of rationality and informational pluralism that is needed to operationalise the capability approach (chapter 3). How can we choose between plural ends? What information is required to make complete rational comparisons of different social states? Alkire endorses Sen's claim that standard economic rationality is too narrowly defined and that therefore what she calls 'ethical rationality' should become more prominent. This entails a focus on ends instead of means, a critical scrutiny of the kind of culture or society that people value (including the examination of existing customs and traditions), and a commitment to public discussions and judgments. Ethical rationality is also necessary to accommodate the two types of pluralism in Sen's work, which Alkire calls information pluralism and principle pluralism. The *information pluralism* of the capability approach entails not only that our evaluations should move beyond economic dimensions to include non-economic capabilities, but also that some evaluations will even require going beyond capabilities and include information on rights, responsibilities and unintended consequences. The *principle pluralism* of the capability approach entails that there is no 'royal road' for normative problems. The issue should not be to defend and pick the best principle, but to find a way to coordinate and mediate between different principles that need to be taken into account. For many economists, these aspects of the capability approach are why they lose their belief that this can be a workable framework: It is too open-ended, expensive, and hard to use, and such an approach might not lead to an optimal option. Alkire counters all of these criticisms successfully. Her discussion of the ethical rationality which underlies the capability approach also puts the finger on the real source of why many mainstream economists remain so skeptical about the capability approach. They see its vagueness and its open and underspecified character as a shortcoming, whereas this is precisely what attracts other scholars to the approach. The question remains to what extent the capability approach will motivate and move mainstream economists to rethink their paradigmatic identity and profoundly question the nature of their discipline. Indeed, 'a legitimate question in many minds is whether, after such a transformation, the discipline remaining would be economics in any recognizable form' (p. 114). Chapter 4 looks at yet another theoretical difficulty in the capability approach: the interaction between evaluations and cultural values and identities. How can our evaluations be sensitive to cultures? What are the concerns that outside evaluators need to be aware of? Alkire discusses community participation in poverty reduction projects and analyzes what information outside actors have to provide in order for the decision makers to be able to make informed choices. Drawing on the literature on participatory development, Alkire suggest ways in which the communities can make the decisions, and argues that outside 'experts' must refrain from acting unless the community chooses so. In this area the disciplinary discussions and tensions are not between Sen and mainstream economists, but between Sen and communitarians, traditionalists, and fundamentalists. While Alkire does not *explicitly* take sides in this debate, she seems to lean *slightly* more towards the communitarian side. In her discussion of Alasdair MacIntyre's notion of 'cultural practice', she concludes that 'when practices are destroyed, people's paths to enjoying human development are destroyed' (p. 139). This statement, together with the absence of any discussion of internal (overt and covert) disagreements and unjustified inequalities *within* cultures (such as those based on caste, class, gender, or ethnicity), suggest that her view on culture is more homogenous and less critical than internal critics of a culture might see it. Indeed, the destroying of one cultural practice might be liberating for some groups in that society while taking away the privileges of others.

Chapter 5 investigates the related claims that with respect to absolute poverty, the relevant basic capabilities might be identified from outside the community, and that for poverty reduction the appropriate focus is on achieved functionings rather than expanding capabilities. This discussion is closely related to the basic needs approach which was very popular in the 1970s and 1980s. Indeed, some have argued that the capability approach is a reinvention of the basic needs framework, and is therefore old wine in new bottles. Alkire criticises Sen for misrepresenting some aspects of the basic needs framework, but grants that the capability approach has two advantages over the basic need approach: it does not confine attention to minima only and, in contrast to the basic needs approach, it has philosophical foundations.

Part II of *Valuing Freedoms* consists of a specific application of the capability approach: a capability evaluation of three Oxfam projects in Pakistan. The first question to address is how a capabilities-based cost-benefit analysis could be conducted (chapter 6). How can non-economic capabilities be taken into account in such evaluations? Alkire first discusses two existing participatory methodologies (Rabel Burdge's social impact assessment and the World Bank's social assessment methodologies) to account for those non-economic dimensions but refutes them on two grounds. They both lack a systematic method for identifying the changes that are valued by the involved actors themselves, and they lack a method to give the decision control to the lowest level capable of making this decision. She then presents her own method to account for non-economic capabilities, which she applied during her fieldwork in Pakistan.

Chapter 7 contains the results from this fieldwork. Alkire presents a capabilities cost-benefit analysis of three poverty reduction projects: goat rearing, female literacy classes, and rose garland production. The goat rearing activity is a sound economic investment, although the internal rate of return depends substantially on the choice of women's shadow wages. In addition, there were a number of largely *non-quantifiable* effects, like the acquisition of useful knowledge, cultivation of friendships amongst each other, etc. Whereas for the goat rearing project the evaluation of the economic and intangible social effects go in the same direction, the female literacy project is a prime example of a project that would no longer be funded if it were evaluated *only* based on a traditional cost-benefit analysis. Because markets for female employment are effectively missing in the area of the literacy project, there is hardly any effect on women's earnings. However, 'it had a fundamental and transformative impact on the women students' (p. 256), which a purely economic analysis that only takes the quantifiable dimensions into account would miss. These intangible changes include that women learn that they are equal to men, that they do not need to suffer abuse, that literate women can solve their own problems, that they learn how to read, and their experience of great satisfaction at being able to study. A similar relation between a negative internal rate of return on the one hand and a number of important non-economic benefits on the other holds for the rose cultivation project. In pure economic terms, a comparison of these three projects would clearly conclude that the goat-rearing project dominates the literacy and rose cultivation projects. However, the literacy classes had the strongest impact on knowledge and empowerment. Thus, from a capability perspective no project clearly dominates the other. As a consequence, 'the choice cannot be made on technical grounds but rather is a morally significant choice' (p. 286). The capabilities evaluation becomes *vaguer* and *less precise*, because it includes those dimensions that cannot be quantified but that obviously are important and that can lead to different overall judgments. In other words, Alkire's case study makes the point that a capability evaluation *does* lead to different normative conclusions than those drawn in standard economic evaluations. In my view, *Valuing Freedoms* is a major contribution to the further development and advancement of the capability approach.

This book is very careful and accurate in its explanation of Sen's work. It is very well referenced and Alkire demonstrates a profound knowledge of Sen's work. *Valuing Freedoms* provides a detailed example of how the capability approach can be used for a very messy and practical task, *without* systematically narrowing the approach down to a technical tool or an algorithm that 'measures' non-quantifiable dimensions. As such, it should be able to convince quite a few critics. However, at the same time, Alkire makes clear that it is impossible to use and apply the capability approach without moving beyond economics narrowly conceived. The capability approach is an extremely interdisciplinary framework, and that will make it difficult to convince economists who do not want to cross their disciplinary and methodological boundaries or who believe that their own methods and discipline can handle all questions. In addition, *Valuing Freedoms* also shows that interdisciplinarity is not the easiest path to knowledge. This book is not an easy read and demands a lot from its readers.

The fact that it ranges from very abstract philosophical arguments on human well-being and practical reasoning to standard cost-benefit analysis and that it requires minimal knowledge in the fields of development and welfare economics, economic philosophy, and development ethics will make it challenging for virtually all of its readers. One should also not expect that after reading this book, all questions related to the capability approach will be answered. *Valuing Freedoms* provides *one possible way* to operationalise the capability approach and only for *one area* of application. Different methods need to be developed for other issues – for example, when deciding on the selection of relevant capabilities in a context where there aren't any actors to tell us how their capabilities should be evaluated, as in social policy design at the national level. Nevertheless, this book is essential reading for anyone who wants to take the capability approach forward, and it will certainly become a reference point for much future work in this area.

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The Orders of Discourse: Philosophy, Social Science, and Politics, JOHN GUNNELL. Rowman and Littlefield, 1998, xv + 252 pages.

How Economics Forgot History: The Problem of Historical Specificity in Social Science, GEOFFREY HODGSON. Routledge, 2001, xix + 422 pages.

Wittgenstein spent a life trying to kick the habit of philosophy. He dreamed of chucking it all and living the life of a Tolstoyan peasant. On several occasions he even tried, working as a gardener in a Viennese monastery, teaching school in a remote Austrian village, isolating himself in a hut on the windswept coast of Norway. But wherever he went the furies of logical confusion followed, and these drove him back to Cambridge where he spent the rest of his days tormented by questions he thought you couldn't ask, let alone answer. With aphorisms and strange riddles ('Why can't a dog simulate pain? Is he too honest?') he gestured towards the conclusion that philosophy had come to an end, something he did so well that he eventually succeeded G. E. Moore to the Chair of Philosophy. Tethered to theory while believing it a waste of time, Wittgenstein laid the template for the lives of later post-philosophy philosophers.

Since his day, philosophers have tried a number of strategies for avoiding this two-faced relationship with philosophy, none very successfully. Wittgenstein wasn't very happy with his own. Philosophical therapy of the sort he prescribed – and invented – can take a lifetime to complete. But then what is the point? A serene, zen-like retirement? In the meantime you have wasted your life. If talking your way clear of philosophy takes too long, is there an alternative? Wittgenstein thought there was – he advised his students to drop out. Bertrand Russell had long ago convinced Wittgenstein, when he was a student, to give up engineering and become a philosopher, and now Wittgenstein, in a final act of upending Russell's system, advised his own students to give up philosophy and become engineers. Philosophy, he told them, teaches us nothing; it is just a battle against the bewitchment of the intellect, so if you are not too deeply in its grip, then get out. And who knows, maybe the best of his students did just that. Maybe the century's finest philosophers became engineers, doctors, and carpenters; maybe the philosophers who most clearly understood our predicament are ones we have never heard of.

Whether or not these post-philosophical heroes exist, it does seem that the only way we can avoid Wittgenstein's predicament is by taking up another subject altogether. Post-analytic philosophers like Richard Rorty, however, don't buy this quietism. Even if they agree that foundational philosophy is a relic, they also believe it is dangerous, and refuse to turn their cheeks to the fanatic enemies of common sense and the mono-theorists of epistemology. But then they too spend a life in theory, looking for a way out. It is not just philosophers who face this dilemma. Political scientists who argue for less meta-theory and more political savvy, economists who urge less math and more history – these too can end up entangled in the very philosophy that was to liberate them. In the two books reviewed here we find this devious process at work. In them we can see that even the most concerted attempt to escape high theory can, like walking down an up escalator, bring us right back to the top floor of abstraction, far removed from the hustle and bustle of street life we had longed for. It takes a wily philosopher to avoid this trap.

In *The Orders of Discourse*, an interesting book with an unfortunate title, John Gunnell tries his hand at escaping this theory trap, or what he calls the 'Munchausenian dilemma' of using meta-theoretical terms to criticise meta-theory. To do so he constructs a simple and ingenious hierarchy of theories. In this hierarchy, our activities are stratified into a base layer of first-order practices and, above it, several layers of meta-practices. First-order practices are the basic activities of our lives – politics, business, science, art, sports. They are primary forms of life, givens; they grow their own languages, and they embody the tacit knowledge that is our common sense. Meta-practices have no such independence, but comment on other activities. Second-order practices comment on first-order ones; they include political theory, economics, art criticism, sports casting, and the philosophy of science. Third-order practices, such as the philosophy of the social sciences, comment on second-order practices; fourth-order practices, such as Gunnell's book, comment on third-order one. This review would then be a fifth-order practice. There is in principle no limit to the proliferation of tiers, and Gunnell likens the situation to a hall of mirrors.

Gunnell's schema may be simple, but it brings out some unexpected similarities and dissimilarities between subjects. For instance, we often compare the natural sciences to the social sciences, yet these subjects inhabit different levels of discourse. The natural sciences are first-order activities while the social sciences are second-order ones; the former are creative, the latter commentary. It follows from this that the philosophy of science is a second order subject while the philosophy of the social sciences is a third-order one. Filing these subjects in their proper drawers allows us to see that analogies between the natural and social sciences are misleading. The social sciences, according to Gunnell, have a history and logic that is more like those of the philosophy of science than of science itself. These are intriguing conclusions. But they are incidental to Gunnell's main point, which is that nowadays social scientists are mixing up levels of discourse. Political theorists in particular, he claims, spend too much time thinking about philosophy while believing they are engaged in politics. Here second-order theorists are doing what is essentially a third-order activity while assuming it is a first-order one. When levels get shuffled in this way the result is bound to be nonsense. Political theorists become alienated from politics, and theory degenerates into a series of 'abstract self-referencing issues' and 'a chase for new philosophical authorities' (p. xiii). Far from settling for the modest role of political commentary and criticism that marks out their potential repertoire, political theorists too often clothe themselves in the defensive and self-serving argument that philosophising is by its nature political engagement, that reading is itself a political activity (p. 199). Their argument, says Gunnell, involves a category mistake (p. 215). There is a difference between doing and theorising, between knowing how to do something and knowing about it. This distinction sets up a categorical barrier between levels of discourse, one that allows information to flow up but stops meta-theory from flowing down.

Gunnell's tirades against philosophy have been widely criticised on the grounds that his levels of discourse cannot be separated, that philosophy mixes with political theory just as political theory mixes with politics. Political theorists, his critics say, do affect political culture through their teaching. And when theorists quote Locke or Marx at a political rally or in a letter to the editor they are turning second-order theory into first-order politics, thereby breaching Gunnell's categorical wall. And every now and then, one extraordinary individual may even combine all levels of the hierarchy: Locke, for instance, blended epistemology into political and economic theory and then, through his involvement with Shaftesbury's household, into first-order politics. Finally, good works of history and social science sometimes begin with a methodological preamble, like Quentin Skinner's history of political thought. In all these examples we find meta-theory informing the practice of a lower order activity, and to all appearances contributing to its success.

These examples, however, need not be fatal to Gunnell's argument. For it could be asked in turn if the philosophy in each example is really doing anything. Locke may indeed have combined all levels in one person, but that does not mean that every level was needed. Isn't it possible that the epistemology he quotes in support of life, liberty, and property was just tagging along for the ride, merely ornamental? Wouldn't we get the same result, only in a less roundabout way, by arguing just the first-order issue? Aren't the higher levels of discourse getting what little meaning they do have, if they have any at all, from morsels of first-order wisdom on which the meta-theory is clinging? In one or two places, Gunnell seems to agree. He hints at a distinction between types of meta-theory, between those that are practical, like method, and those that are philosophical, like methodology, and he implies that it is the latter that are wasting our time. Unfortunately, Gunnell does not elaborate on this distinction between philosophical and practical types of theory. As a result, he is backed into the corner of saying that theory cannot inform first-order practices.

But it can. Gunnell's critics are right about that; they are just wrong in thinking that this theory is or can be philosophical. Theorists and commentators can have give-and-take with people in first-order activities. An art critic, for instance, can identify where a painter is going wrong with colour or composition, just as a coach or sportscaster like John McEnroe can pinpoint what is wrong with a tennis player's backhand. But when coaches or commentators offer these bits of advice, what they are engaged in is first-order discussion, not meta-commentary. But in that case, in the sort of dialogue that takes place between coach and athlete (and maybe should take place between theorist and political actor) there is no occasion for anything other than first-order conversation. Theory of this sort does not differ very much from what people say while playing sports or painting a picture or engaging in politics.

If we look at theory in this way, does anything remain of Gunnell's hierarchy? We could continue to use the term 'meta-theory' to refer to any discussion of first-order practices – why not? – but the term no longer does much work, certainly not the work Gunnell wants it to, for according to the coaching analogy, levels of discourse are no longer separated by a categorical wall. If Gunnell had wanted to clinch his anti-philosophy argument he should have developed this point. But had he done so his hierarchy would have collapsed into just two tiers – the meaningful talk of first-order activities on one level and the nonsense of epistemology and methodology on the other. Gunnell does consider this argument, but quickly shies away from it. In fact, losing his nerve, he breaks in the other direction, gathering up insights as he goes from Austin, Wittgenstein, Kuhn, and Winch in order to build a new theory of action, a third-order theory he miraculously believes is an essential first step for the social sciences. So, despite his Wittgensteinian ambition to leave theory behind, he ends up writing about 200 dense pages of it.

The same heroic struggle between history and theory is played out in Geoffrey's Hodgson's book, *How Economics Forgot History*. In it he tells the tale of how economics over the past century, like political theory, has been emptied of historical content and filled with a barren abstraction. The first half of the book consists of richly detailed chapters on the major theorists of the historical and institutional schools of economics – Schmoller, Weber,

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Sombart, Commons, Veblen et al. – and then ones on the theorists he holds responsible for obliterating these schools and contributing to the discipline's current historical amnesia – Robbins, Talcott Parsons, the neo-classical theorists, and, surprisingly, Keynes. In the second half of the book, Hodgson pulls together the lessons he has learned from each of the theorists he has surveyed, as well as lessons from a broad range of contemporary science – chaos, evolution, complexity, systems theory, etc. – to set down some criteria for an economic theory that would avoid on the one side the empty abstraction of neo-classical economics and on the other the theoretical blindness of the historical school. Here he develops the argument that a 'historically sensitive' economics must combine elements from each of five tiers of abstraction, something he calls the *principle of five analytical levels*. At the highest level of his hierarchy there are features and principles, such as evolution and entropy, common to any open, complex system. One step down in the hierarchy are features and principles governing any human society, past or present, primitive or civilised. Below this are ones governing only civilised societies; then ones for specific types of economic system, such as capitalism or feudalism; and finally ones for geographically and historically specific instances of these systems, such as Asian capitalism. Hodgson claims that most social scientists 'centre their work on just one level', something he considers a mistake, for unless an analysis taps each level of abstraction it lacks 'the required combination of uniqueness and generality' (p. 324).

Hodgson has his work cut out for him, or, at any rate, the economics discipline does, for to date no one has produced a single piece of theory that meets Hodgson's criteria. So it is not surprising that in the second half of his book he issues a large number of methodological dictums on how the new science is to be practised: theories must conform to the *provisioning principle*, the *impurity principle*, the *principle of juridical influence*, and the *principle of hierarchical ontological consistency* (italics in original). Hodgson even baptises whole new branches of learning, such as thesmology – the study of institutions – and agoralogy, a sub-branch that studies markets. Unfortunately, in this veritable *principia economica*, we begin to see the historian succumbing to the lure of meta-theory. By the end of the book, Hodgson has amassed so much meta-theory that his argument becomes as top-heavy as the neo-classical theory he set out to correct. None of his recommendations are unreasonable in themselves (if a tad abstract to be of much use), but I suspect the real lesson to be learned from the book is one mentioned by Gunnell (and even he promptly forgot it) – that there can be no return to history based on first principles.

It is odd that Hodgson blames Keynes, almost more than anyone else, for the rise of abstract theory and the obliteration of historical economics. Keynes was himself notoriously dismissive of the slick and abstract forms of theory then coming into vogue, and he preferred to express himself in ordinary language. Any other approach, particularly one derived from methodological first principles, could, he thought, lead us wildly astray. By relying on these principles we can too easily fall 'into the errors of the taylors of Laputa, and by a slight mistake at the outset arrive at conclusions the most distant from the truth' (*Collected Works*, 10:97). There are just too many steps (too many slips between lip and cup, in Keynes' words) between methodological first principles and the eventual argument one wants to make. Rather than being a sure-footed procedure, it is hit-and-miss. The risk-reward of methodology stinks.

There is another problem with methodology and the philosophy of the social sciences – one that is rarely discussed – and that is the quality of writing in these subjects. Discussions in them are pitched by necessity at such a high level of abstraction that the words used in them, severed from everyday use, become uncontrollable. When Gunnell says that 'politics as a first-order practice is a conventional and historically bounded configuration of human activity' (p. 45), or speaks of 'an engagement with the crucial theoretical issue of the generic constitution of social phenomena' (p. xii), do we really know what he is talking about? When Hodgson speaks about an 'activist and reconstructive conception of agency, founded on habits and instincts' (p. 140) or tells us that 'a culture is a complex of shared habits of thought and action within a community' (p. 305), are these statements really doing any work? How could we ever determine if these statements are true or false, useful or not? Our judgments cannot be brought to bear on these propositions because they are just too shapeless. The voice of both authors is muffled under a blanket of amorphous words and phrases – structure, institution, configuration, social formation, pattern of behaviour. These words can mean almost anything, and so end up meaning almost nothing.

Gunnell and Hodgson are relatively clear writers. They are not the problem. The problem inheres in the subject they write about – in methodology and the philosophy of the social sciences. The prose in it, heavy and awkward, clomps along, rarely quickened by an active verb, a concrete noun, or a vivid modifier. The grammatical subjects, as befits third-order theory, are usually abstract noun phrases that don't conjure up anything we come across in our daily lives, and the verbs are weak transitive or linking verbs – appears, becomes, seems, continues, remains, is related to. How this stuff gets past an editor is a real mystery. Gunnell and Hodgson worry about the alienation of political and economic theory from the public, and they think the problem stems from these subjects' misplaced concern with philosophy and math. They are partly right. A more immediate cause, however, is the parlous state of writing in these fields. Who but a small number of other social scientists wants to read this stuff?

It is often said in defence of bad writing in philosophy and the social sciences that these are difficult subjects, ones that cannot be translated easily into readable prose. Yet scientists such as Rachel Carson, E. O. Wilson, and Richard Feynman are masters of the plain style, and if genetics, relativity, and quantum mechanics can be written about clearly, anything can be, especially our everyday world of work and politics. What is at issue here is more than just a point of rhetoric. Bad writing can indicate more than the absence of craft in a writer: it can also indicate that the subject matter being written about is nonsense. In the case of methodology, the awkwardness of its prose inevitably mirrors the emptiness of its subject.

As we ascend the subjects in Gunnell's hierarchy, as the meta-garbage thickens, the writing gets worse and worse. When we leave behind the concrete world of science and literature and other first-order activities, we find grammatical subjects thinning out till we end up, in third and fourth-order theories, speaking abstractly about abstractions and finding fewer and fewer verbs designed to do the job. As Wittgenstein said, the words at these altitudes lack friction. His solution to the problem – back to rough ground ! – is at once a solution to philosophical confusion and a cure for stylistic defects. In an attempt to leave high theory, he wrote in a clear prose untroubled by philosophical jargon. Keynes, when writing the *General Theory*, also adopted a concrete style, for much the same reason. And perhaps this connection between message and style gives us a clue on how to escape philosophy without at the same time amassing a corpus of post-philosophical work. Perhaps the escape, as Orwell recognised, requires nothing trickier than learning the plain style.

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The Myth of Ownership: Taxes and Justice, Liam Murphy and Thomas Nagel. Oxford University Press, 2002, 190 pages.

One develops expectations about new books from one's favorite authors. Such expectations led us to anticipate that this volume would be a clear and compelling book. But one also expects that any book that promises to cover taxes, property, ownership, and justice must be big and heavy, and, if not, flimsy. *The Myth of Ownership* attempts to cover a lot of ground in short chapters on modern notions of economic justice in moral theory, legitimate functions of government, and ways of evaluating tax policies. Big field, little book, great authors. Which of our expectations will be undone: heft or intellectual flimsiness? Luckily, it is heft. This little volume is a fine example of careful application of moral and political philosophy to public policy, accomplished with very high overall quality and welcome brevity.

Murphy and Nagel explicitly declare that they are applying a moral framework to the analysis of tax policy. They argue that to understand justice in taxation, one must understand that property is part of the background system created by government and that taxation is but an aspect, though an important one, of such a system. The book is primarily a critique of philosophical theories that invest property with a moral patina and then reject the morality of taxation as a redistributive tool, such as that found in Robert Nozick's *Anarchy, State and Utopia*. The authors also target those more mundane opinions that view taxation as isolated from the programs they fund and hence a pure burden on citizens. Such a perspective also vests property with a value independent of government even though it is government that creates the underlying property system. They call this myopic worldview 'everyday libertarianism' (p. 15). In order to make their point, Murphy and Nagel adopt the normative position that the nature of property is conventional, as opposed to moral (as in Locke's conception of moral natural rights, later adopted by Nozick among many others). The very definition of property is determined by an overall system of relationships, norms, and transactions – 'employment, bequest, contract, investment, buying and selling' (p. 74) – established and protected through government, of which taxes are an integral part.

Any discussion of tax policy in a context of societal norms should begin from the point of view of an existing society, with foundations and principles designed to sustain those institutions without which the arrangement of our lives would be incoherent and our life plans unrealizable. Murphy and Nagel argue that we should, therefore, decide what norms the design and regulation of that social structure should respect. Such a selection would be an expression of the consideration that is due from each of us to our fellow members as well as the independence from one another that we are entitled to retain (p. 41).

Since the authors explicitly identify taxes as a necessary component of the role of government in its creation of such public goods as law and order, property creation, and property rights protection, they make little distinction between tax and expenditure policies (p. 18). Thus, although they emphasise taxes, their arguments are also relevant to the question of what ought to be the function and size of governmental programs. They identify (and sketch the traditional justifications for) three functions of government: the supply of public goods, the supply of some non-public good programs, and the delivery of just social outcomes. Their stand that property is to be understood as a function of governmental policy leads them to see taxes as one more policy vehicle to correct market shortcomings. This implies that justice in taxation will be tied to the government overcoming the shortcomings of markets in their delivery of material well-being. Taxation is just one of the instruments the government has to do its job. It follows that justice, in the sense of final or post-market distributive justice, becomes the standard that Murphy and Nagel propose to measure tax policies. As Nozick would have characterised their argument, their perspective is based on a 'patterned', rather than a 'process', conception of justice.

The strength of this perspective stems from the accuracy of the general observation. Indubitably, property is a political creation. After all, we Americans have watched the creation of privatised property rights for decades: intellectual property rights over the Internet, over gene sequences, and over delivery of health services to the poor and the elderly. Each such creation of property rights effectively blocked long-term moves for change, e.g. national medical insurance. Similarly, the claim that taxes are but a part of the overall system of property would seem trivial in its simplicity, were it not for the fact that so many people, especially in the public sphere, base their pronouncements on exactly the opposite position. It is important to realise that Murphy and Nagel are making two related yet separate arguments. The more technical argument clarifies the role of taxes as an element of the overall system of property. This argument may be accepted at the same time as the authors' normative prescription that all property be viewed as conventional is rejected (the opposite is, naturally, also possible, though far less plausible).

However, the major weaknesses in their argument arise from this same intellectual move, which gives somewhat short shrift to 'process'. Even if we abjure the extreme positions of Nozick or Hayek, there is something to process that is left out by beginning the discussion on property purely as an a-historic convention. The authors do so by referring from time to time to the notion of a 'benchmark' of no government, which they characterise as Hobbesian. Such a benchmark gives everyone a claim of benefits from government as the difference from an egalitarian sketch of a short and brutal life in a state of nature to the status quo. However, governments, and governmental regimes, do not arise from a state of nature. Rather, they develop as a result of changes in a previous status quo and the moves from one existing condition to a tenuous, better future must be painfully negotiated through political processes. This realization gives its strength to much of the modern game theoretic understanding of political dynamics. It is surprising, given the ultimate aim of Murphy and Nagel's project, that they do not recognise the possible difficulties that ignoring this entail. It is precisely in such negotiations that agreements are reached regarding the status of 'holdings' of all sorts, property and other rights included. These holdings then take on a status a bit above the conventional, for within the system, their violation is seen not as the violation of a convention, but rather of an agreement underlying the system of governance.

Governmental programs and taxes will have to 'honor' those agreements, yet any selection will privilege some patterns and not others. So there is no conception of justice (not even a process one) that does not involve and privilege a pattern of some sort. It is impossible to separate the distribution of goods and wealth from the laws and regulations that make it possible. And such laws and regulations are necessarily based on normative principles that allow certain modes of acquisition, transfer, and distribution and disallow others. Whether these principles are reached through the aggregation of the preferences of the individuals in a society or by other means is an open question, one that Murphy and Nagel do not satisfactorily answer. But to their credit they do correctly recognise that it most likely entails a political competition combining individual self-interest and competing conceptions of social welfare.

Another lacuna in the volume is that the authors' overlook the obvious link between justified individual behavior, expectations, and moral claims. Indeed, Nagel has argued elsewhere (in his *Equality and Partiality*) that moral claims are part of the basis of legitimacy. All but the most radically imposed property regimes emerge from political processes that both preserve and modify property claims. We might roughly argue that legitimate shifts in political outcomes (such as property claims) are the result of legitimate political processes such as negotiation and adjudication. By overlooking them here, property is given too flimsy a foundation. The authors argue property to be a function of convention, but we know better: property is a function of politics. Billions of dollars in property rights are created all the time – politically. This is not simply a case of semantic awkwardness; if property stems from politics, then we must ask what aspects of politics generate moral claims. Murphy and Nagel have an idea of what a just society should look like: Government ought to be concerned not only with the provision of public goods, but also with insuring the overall welfare of all its citizens. Special concern is owed 'the inevitable losers in market competition' (p. 181), but the two authors provide a rather anemic analysis of politics (and only do so in their conclusion). This contributes to a lack of depth in their debate regarding how politics can and ought to be used to restructure the tax and governmental expenditure policy to achieve the goals they advocate (or any alternative goals, for that matter). Since they hold that property is merely a political arrangement, their last chapter (on politics) should have been one of their first.

There are other small, but important, inaccuracies to be noted in the book. Just as Hobbes is an ideologically convenient, but not necessarily justified, starting point in the debate over the nature of property, so is Murphy and Nagel's assumption that political regimes cannot exist without taxation of the citizenry. Their ascribing a logically inviolable status to the claim that taxation of its citizens is necessary for a government to support itself (p. 32 and p. 176) is unnecessarily hyperbolic. Societies have existed with property and without the institution of taxation: Imperial regimes can maintain their coffers through taxing their colonies, and simple tribal societies have supported modest civilizations with political institutions that have no coercive capacity and no taxes. Further, the authors do not fully articulate the patterned conception of justice, which the reader can feel must lie behind the arguments. This ambiguity allows them to avoid contending with some of the more legitimate objections (for instance, that all divisions of wealth, natural talents, ambitions, etc. are morally arbitrary) to the Rawlsian view to which they cozy up, although do not fully adopt. Thus, they avoid considering the possible consequences for policy imbedded in their preferred theories of justice.

Finally, the quite technical discussions sometimes do not allow the uninitiated to understand the debates. For instance, the discussion on vertical equity is opaque and lacking in some of the basic concepts necessary for a full understanding (p. 13ff). In their defense, even the great Musgrave did a bad job on this (see his *The Theory of Public Finance*). Similarly, they outline an argument concerning how a wealth tax is likely to make the government a partner to risks by portfolio shift responses of the taxpayers (pp. 117–20). Such passages are likely to be found by most readers to be non-starters and hence pedagogically unsound.

Yet, our bottom line is clear: the strengths of the book far outweigh the weaknesses. By putting the issue of property rights smack in the middle of the discussion, Murphy and Nagel can correctly give the reader a sense of perspective when approaching such topics as ‘tax equity’. Their position allows them to see the larger questions of justice, liberty, and rights that must underlie any debate of how best to raise resources for governmental action. As a text which successfully applies the detailed arguments of modern normative philosophy to policy, *The Myth of Ownership* is sure to be a book many instructors will find useful in teaching courses of public policy, policy analysis, and the issues related to economic justice. In such contexts, its few misdeeds in an otherwise very competent handling of the concepts of economics and theories of philosophy can easily be rectified. Overall, the outline of the problems will radically simplify the teaching of applied political philosophy to advanced undergraduate and graduate students.

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