

BOOK REVIEWS

Steven G. Medema, *The Hesitant Hand: Taming Self-Interest in the History of Economic Ideas* (Princeton: Princeton University Press, 2009), pp. 272, \$35. ISBN 978-1-4008-3077-0.

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Steven Medema's latest book, delightfully titled *The Hesitant Hand*, is destined to become the go-to reference for all economists and historians interested in the weighing of market failures and government failures.

The book's subtitle is *Taming Self-Interest in the History of Economic Ideas*, and Medema begins his story with Adam Smith, who tamed self-interest by channeling it to the common good, via his system of natural liberty. Medema contrasts that view with those who came before, from the Greeks and Scholastics, with their open suspicion of interest, to Bernard Mandeville's open embrace of vice. Perhaps it might have served Medema's purposes here better if, instead of working with various opinions about a single stable concept—self-interest—over time, he had, with Hirschman (1977), emphasized the Enlightenment's innovation of invoking self-interest itself to tame our wilder passions, as well as Smith's emphasis on the taming role of self-command and fellow feeling, as we try to appeal to one another's interest. But this is a small omission. In setting out various views of self-interest, in its tame and untamed forms, in businesspeople and in politicians, this opening chapter foreshadows future developments quite well.

Those developments begin in earnest in Chapter 2. After briefly introducing the utilitarian philosophy of Bentham and the elder Mill, the chapter quickly moves on to John Stuart Mill (1806–1873) as one of the key transitional figures in the modern theory of externalities. In Medema's reading, Mill introduced critical doubts about the harmony of self-interest and the public interest, including what, with only slight danger of anachronism, we might now call "externalities and behavioral anomalies." But, by the same token, Mill doubted the ability of government to do much better. By his reckoning, the utilitarian calculus thus continued to come out in favor of liberty and markets. A generation later, Henry Sidgwick (1838–1900) adopted the same approach, but tended to see more externalities and was more optimistic about the potential for government to overcome them. Thus, in contrast to the standard history, which points to Pigou as the transitional figure in the theory of market failure, Medema clearly shows that the turning point lies in Mill and especially Sidgwick. This chapter of Medema's story covers a period I have sorely neglected in my own education, and I found it an indispensable introduction.

After a brief introduction of the Marshallian paradigm, Chapter 3 then comes to A.C. Pigou (1877–1959) himself. Medema summarizes Pigou as one who put "Sidgwick's ideas into a Marshallian theoretical framework" (p. 60). Of particular interest to those who know the basics of Pigou, Medema provides a fascinating account of how, after establishing in *The Economics of Welfare* the "prima facie case" for government intervention in the economy on the basis of market failures,

Pigou then went on to consider practical solutions to these theoretical problems in an essay titled "State Action and Laisser-Faire" (1935). Many readers who thought they knew a thing or two about Pigou will be surprised to learn, as I was, that in that essay Pigou was far less sanguine about the role of government than most people assume. He wrote that "[e]very public official is a potential opportunity for some form of self-interest arrayed against the common interest," and that, moreover, new bureaucracies soon develop their own interests likewise arrayed against the commonweal (p. 70). In this respect, Pigou's skepticism not only echoes Smith's, it also anticipates the public-choice schools introduced in the second half of the book. It appears that Pigou might well have exclaimed, "*je ne suis pas Pigovian.*"

Chapter 4 turns, somewhat abruptly, to the Italian public finance tradition, from Pantaleoni's naïve view of Parliament setting out to maximize national welfare to Montemartini's view of it as subject to rent seeking. It then switches to Wicksell views on government failure and his proposal to use super-majorities to limit state actions to true Pareto improvements. Before reading this chapter, I knew next to nothing about this material, and I read it with profit. Yet, to my taste, this chapter is the weakest link in a strong chain. The abrupt change in schools struck me as somewhat jarring and hard to place in the larger narrative. The connection becomes somewhat more clear in Chapter 6 (which might have been better placed immediately after this chapter), when we learn that James Buchanan (b. 1919) was influenced by both literatures. That chapter then develops the Virginia school's more modern rebuttal to the Pigovians. In this public-choice literature, we have a continuation of the theme that self-interest is everywhere, and that the visible hand of the government may do a poorer job than the invisible hand of the market of channeling it to the common good.

Chapter 5 is an excellent summary of Ronald Coase's (b. 1910) *Problem of Social Cost* and his views on "the emptiness of the Pigovian analytical system" (p. 114); Chapter 7 picks that theme back up again along with the development of the Chicago school of law and economics. Medema is on his home turf here, and it shows. In a very clear exposition, he explains how Coase emphasized the importance of transactions costs as the critical reasons for divergence between private net benefits and the theoretical net benefits on the blackboard, which assume those costs away. But transactions costs are as real as any other (analogous to transportation costs, as Dahlman [1979] put it), and so institutions for coordinating production and allocation of goods and services must account for them.

This brings me to one of the few points in the book over which I puzzled. A section of this chapter is labeled "Preaching Pragmatism over Pangloss and Pigou." Who Pangloss is supposed to be in this simile is not clear, nor which world is the best of all possible ones. Presumably the best of all possible worlds is what Coase called the "blackboard economics" of Pigou. But Medema appears to be unaware of a certain irony here, for E.J. Mishan, in an essay titled "Dr. Pangloss on Pollution," actually satirized the Coasian transactions cost approach as developed by Alchian, Demsetz, and others as Panglossian in just that way (Mishan 1971). Essentially, Mishan's critique is that when the transactions cost approach is combined with a strong no-arbitrage condition, then the costs of addressing an externality *must* always be higher than the potential gains, or institutions to address it would have already arisen. It would have been interesting to have continued the discussion into that later literature.

There is something about a great book like this that causes one, no matter how satisfied the impartial spectator would have us be, to get greedy for more. The later transactions cost / property rights literature, just mentioned, is one such place where there might have been more. Another is the local public goods theory of Tiebout (1956). Couched explicitly as a rebuttal of Samuelson, Tiebout's argument that local public goods are bundled with real estate and so are effectively private goods would have been a nice complement to Coase and Buchanan, forming a trilogy of reactions to the Pigovians: such a threefold cord is not quickly broken. But no doubt Medema has more for us on the way. In the meanwhile, we will just have to settle with what is already the most comprehensive historical examination of the subject.

The book is made all the more enjoyable by Medema's writing, which is strong and clear. If there is one fault, it would be that, on occasion, Medema lapses into overly cartoonish summaries of his own history. For example, in the occasional weak moment, Medema summarizes his story as if it were a football match between one side that views self-interest as good (hence, markets as good too) and another that views it as bad (and governments good), with first one moving the ball forward and then momentum switching to the other. But Medema is walking a fine line here, and to nitpick in this way would be churlish; indeed, it would be blind to the true achievement of this book. For, in fact, at the same time, Medema provides a story that is much richer than that, bringing out the conflicts within generations and within even the same individual. Thus, the true achievement of this book is to speak so successfully at so many levels at one time. That is why, at a price of \$35, I am putting it in my syllabus and making a gift of it to more than one accomplished scholar.

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Thomas A. Stapleford, *The Cost of Living in America. A Political History of Economic Statistics 1880–2000* (Cambridge: Cambridge University Press, 2009), pp. 421, \$34.95. ISBN 978-0-521-71924-7.

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"Hey, honey, what happened? How is it that we eat meat so rarely? Have prices risen lately?" These questions, when generalized to every American household, have extremely important political implications. Thomas A. Stapleford's very detailed and

precise book explains the various ways in which the US federal administration, mainly through its Bureau of Labor Statistics (BLS), has answered since the end of the nineteenth century, and analyzes how the answer is tied to political concerns. His book, which pertains to the now-long tradition of socio-historical studies of statistics, describes the subtle and intricate relations between what might appear nowadays as a purely technical and neutral statistical measurement—the Consumer Price Index—and politics.

To tell his story, Stapleford sits precisely where the statisticians deliver their data and are confronted by those who use them: unions, other agencies of the administration, and, finally, the White House. The archives he studies in the BLS are mainly those of the commissioner or his deputy commissioners (very little hierarchically below); the literature he analyzes is mainly written for outsiders of the BLS. The evidently political character of these interlocutors explains why the book justly deserves the subtitle of “political history.” Let us consider three such examples

First, Stapleford explains how, during WWI, the first stable index—called the Cost of Living Index—was established by the BLS to facilitate federal arbitration of hours and wages between unions and employers. He shows that the very existence of such an index had important political consequences in labor relations because it was there to be mobilized by actors during negotiations. This fundamental political nature was, of course, amplified when the actual figure it gave favored one or the other camp.

Second, it is not the single uses, but the technicalities of producing this index that is also shown to bear on politics. For example, in the 1920s, an important theoretical decision had to be taken concerning the index. In effect, a cost of living index measured the variations of the price of a basket of goods. But this basket itself evolved. A worker’s family of 1900 did not buy the same things as an equivalent family in 1930. The problem, then, was that the price variation of the basket that varied had no clear meaning: no one could infer whether the cost of living was cheaper or dearer because the living itself was different. Therefore, it was necessary to stabilize this basket, but how? One could either use a “constant utility” basket (that is, a basket that contains changing products but provides the same utility through time for a family), or one could use a “constant goods” basket, containing always the same goods through a long period, even though their utility changed. The first choice had the advantage of being easy to conceptualize, but proved extremely difficult to put into practice (how to measure the utility of goods?). The second choice had a less clear interpretation, but was much easier to construct. The BLS, under the influence of institutionalists and especially Wesley Mitchell—themselves dedicated to “pragmatic” measurement (but it is unclear whether Stapleford uses this adjective in the philosophical or in the day-to-day meaning)—chose the “constant goods” approach. But the immediate consequence was that the index did not account for the change of quality of the basket, which would become object of fierce debates with unions during WWII.

Finally, the book shows how these statistics also had important consequences for the role played by the federal government in the economy. For example, after the chapter on the New Deal explaining how the personnel changed in the BLS, which became much more academic than it was before, there are two chapters on WWII. The first one is about the use of the index by the government to fight war inflation and the 1943 “little steel” decision, fixing the maximum wage raised to 15% of its

January 1941 level; and the second is about the dissatisfaction of unions towards the BLS and their efforts to replace federal figures and experts with their own surveys during the war.

However, despite the very important qualities of the book, it deserves some criticism. First, some of the influences on the actors building the index do not receive the full treatment they deserve. Second, the very high level of academic capacity of the author leads him to a sort of theoretical conservatism, sometimes self-contradictory.

I have three remarks about the actors' resources. First, at the turn of twentieth century, labor statistics was a highly internationalized topic; European countries and international institutes promoted many exchanges. Carol C. Wright, for example, who created the BLS, spent much time in the sessions of the International Statistical Institute, and had very close ties with British statisticians. Although Stapleford mentions some of them, it is arguable that they were much more influential than they appear here.

Second, a surprise of the book is the very late influence of both unions and the Bureau of Agricultural Economics (BAE) on the BLS index. In effect, within the US, from the mid-nineteenth century on, one of the most influential government statistical agencies, maybe as much as the Bureau of the Census, was the BAE and its antecedent, in particular the Division of Crop and Livestock Estimates (forgotten today because of the fall of the agricultural economical role, but then evident). Since 1863 it produced, among other data, a monthly series called the "Prices of Farm Products," which Stapleford barely mentions, and only with respect to WWII. On their part, unions also had an early influence, and this time directly on the young BLS. As early as the 1890s, New York State, soon followed by Massachusetts BLS, asked the former, through an institutionalized system of correspondence, to estimate the number of their members unemployed. Every trimester, major unions sent such reports to the bureaus, which were the basis for the first unemployment series. Hence, it is surprising that these two sets of institutions (BEA and unions) appear mainly during discussions of WWII in the book (see Topalov 1994). It is true that unions are cited during the discussion of WWI, but not as a resource for producing figures.

Finally, Stapleford argues very convincingly that the main theoretical resource for the Cost of Living Index was institutional economics. But Stapleford's treatment of this group as homogenous poses a problem, especially when we keep in mind that the BAE, whose price index during the New Deal appears to stem from exactly the opposite hypotheses from those of the BLS, was also a nest for institutionalists, especially M. Ezekiel (see Taylor and Taylor 1953). It would be interesting to understand how this unique resource had two contradictory effects.

Now, two points about method. First, Stapleford opens his argument by placing himself under the authority of Max Weber, saying that he will describe a rationalization process: "A history of the ties between cost of living statistics and American Governance is thus a history of such [Weberian] rationalization process" (p. 7). This goal is very classic—sort of a pre-Science and Technology Studies approach. But, in fact, Stapleford shows the contrary throughout his book (especially concerning the "neutrality" of statistics, which, as said above, appear much more political than neutral), and concludes that "the strict separation of the political and the technical that is typically used to justify rationalized governance does not exist." Therefore, holding to his first insight, he proposes in the epilogue to "construct a new vision of

‘rationalized’ governance” (p. 385) all by himself. But the fact that the actors he studied did not help him with this goal might lead one to think that the very Weberian question was not the best possible.

Second, the nine chapters are more or less period driven. The author opens each one of them with a presentation of the historical context and then enters the details of his own statistical story. Of course, this is how history is usually written because it helps the reader understand the argument. But the problem is that it erases one of the crucial and specific tasks with which federal statisticians are confronted: to properly define what they call the “universe”; that is, to define America itself, the territory to which their cost of living applied, and the time period that is of interest for them in a way compatible with their practices, graspable by their techniques. Statisticians are context makers, not takers (Didier 2009). But the author’s writing method leads him to ignore the point. (Exceptions include the very interesting discussion about the kind of families taken into account [p. 85] and the kinds of cities [p. 163], but why and how these families and cities are chosen is not documented.) Stapleford justifies the word “politics” he uses in his title, but not “America,” which is nonetheless a huge problem—and no less political—for federal statisticians.

Stapleford’s historiographic choices may have caused his story to lack inventiveness, but the whole book remains extremely informative, very well written, and of a high professional standard. This, along with the centrality of his subject, makes it clearly a ‘must-have’ resource for students of anything from labor to statistics and/or the federal administration.

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George, Warde Norman, *Taxation and the Promotion of Human Happiness: An Essay by George Warde Norman*, ed. D. P. O’Brien, with assistance from John Creedy (Cheltenham and Northampton: Edward Elgar Publishing Ltd., 2009), pp. xiii, 209, £59.95. ISBN 978-1-84844-485-0.

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In the eighteenth century, Britain established what Brewer (1989, p. xvii) calls “the fiscal-military state.” The British government increased expenditures on military affairs, financed them by making long-term loans, and paid their interest mainly through indirect taxes such as customs and excise duties. However, according to

Daunton (2001, pp. 32–33), the British fiscal-military state reached its limits at the end of the Revolutionary and Napoleonic wars (1793–1815). The outstanding amount of public debt increased to about 280% of GDP at that time, and the national tax burden became more than 25%. The income tax that William Pitt introduced in 1799 was abolished in 1816, because people regarded it as an unfair and unpleasant tax. The legitimacy of taxation and people's trust in the state were being lost.

In this situation, George Warde Norman (1793–1882), who served as a director of the Bank of England from 1821 to 1872, wrote an essay on taxation. Although Norman started writing the essay in 1821, and continued until the 1830s, he could not complete it. D.P. O'Brien, with the help of John Creedy, edited Norman's unpublished essay and brought it to light in the form of this book. Thanks to O'Brien's laborious editing, with an excellent introduction, readers will be able to follow Norman's argument with almost no difficulty.

The essay is composed of three parts. Part I contains two chapters (chapters 1–2), in which Norman reveals his views on the general principles of taxation. As O'Brien correctly points out, Norman's normative background can be called "utilitarian." Although in the essay Norman does not refer to Bentham and Mill, in his autobiography, he writes, "Towards the end of 1821, I commenced an Essay on Taxation, redolent of Bentham and Mill—the object being to shew, how the Revenue of Country might be levied with least pressure on the Taxpayers" (*Essay*, p. xv). Moreover, in the essay, Norman repeatedly uses such terms as "the public happiness," "the general felicity," and "the greatest happiness of the great number" as an ultimate standard to differentiate good and bad taxes. It must be stressed that Norman considers not only the "economic effects" of taxes on the public happiness—effects through changes in the production, distribution, and consumption of wealth—but also their "moral effects" through changes in people's sentiments and actions.

Norman was one of the founders of the Political Economy Club. Hence, it is quite conceivable that in the club he had a chance to discuss the issues of taxation with David Ricardo and Jon Ramsey McCulloch. It is also probable that Norman read not only Ricardo's *Principles of Political Economy and Taxation* (1817) but also his article on the "Funding System" in the *Encyclopaedia Britannica* (1820), and McCulloch's early writings on taxation. In fact, Norman's explanations with respect to the shifting and incidence of taxes employ some theories in Ricardo's system of political economy—for example, the theory of differential rent and that of equalization of profit rate. Strangely, however, Norman does not discuss the influences of taxation on long-term economic growth, to which Ricardo and McCulloch attached a high importance. Norman's analysis of taxation can be called Benthamite, rather than Ricardian, because it focuses on the static, rather than dynamic, effects on public happiness.

Part II of the essay is composed of twelve chapters (chapters 3–14), in which Norman provides eleven maxims of taxation: computability, simplicity, frugality in collection, constancy, divisibility, popularity, noninterference, equality, uncorruptiveness, unvexatiousness, and unevasibility. These maxims can be regarded as an expansion of Adam Smith's four maxims: equality, certainty, convenience, and economy.

In the nineteen chapters of Part III (chapters 15–33), Norman examines various taxes on the basis of the eleven maxims. As a general rule, Norman prefers direct taxes to indirect taxes. This preference culminates in his proposal of a single tax on property, which emerges in the final two chapters. In Chapter 32, Norman shows the scheme of a single tax on property as an ideal. This scheme involves a proportional impost on all kinds of material properties—such as land and material capital—with an exemption of subsistence minimum. Norman’s scheme can be regarded as a proposal to make perpetual a capital levy that Ricardo put forward in his article on the “Funding System” as a temporary measure to redeem the public debt. However, the single tax on property had a problem: it would not capture the incomes of traders and professionals who had no material property. Like Ricardo, Norman sidesteps this problem, stating that in the long run, the free movement of labor between occupations will lower these incomes.

The single tax on property had another serious problem—unpopularity. As Ricardo did not think that his capital levy scheme would be accepted by Parliament, Norman thought his scheme unacceptable for the people. Thus, in the final chapter, as the second-best policy, he puts forward a tax system in which a tax on property at a moderate rate is combined with an improved house tax, a timber duty, and a fixed impost on traders and professionals. Revenue obtained from these taxes will be used in order to abolish oppressive indirect taxes. If people become familiar with the tax on property, its rate can be gradually raised, whereas that of other taxes will be reduced. Thus, states Norman, the system of a single tax on property will finally be established.

Readers of this essay may have the following questions. Why did Norman postpone the publication of his essay? Did he give up on its publication because he considered his scheme too radical to be accepted by the people? Alternatively, did he think that Henry Parnell’s successful book, *On Financial Reform* (1830), which proposed reintroduction of income tax in order to abolish oppressive indirect taxes, diminished the significance of the essay?

Changes in the fiscal conditions in Britain may provide one of the possible answers. After the end of the Revolutionary and Napoleonic wars, until Robert Peel reintroduced an income tax and started tariff reforms, the British government did not carry out a radical fiscal reform. However, continuous economic growth, which started in the 1820s, prevented the British government from falling into a fiscal failure. Although the amount of outstanding public debt was reduced very little, its proportion of GDP fell from 280% in 1815 to 180% in 1842. Moreover, although tax revenue continued to increase, its national burden fell from 25% to 11%. Economic growth, to which Norman paid little attention, was a genuine source of public revenue (Dome, pp. 5–12). Norman himself admits this fact in a pamphlet published in 1850, *An Examination of Some Prevailing Opinions, as to the Pressure of Taxation in This, and Other Countries*. Norman may have thought that his essay failed to use the kind of political economy that emphasized the role of economic growth in fiscal reform.

The true reasons regarding why Norman kept the essay unpublished may be elucidated from his autobiography, which O’Brien is now preparing for publication. Together with the autobiography, as well as the 1850 pamphlet, this essay provides a precious document to all scholars interested in the arguments on the fiscal reform in

nineteenth-century Britain. Thus, this book can be regarded as an important supplement to O'Brien's magnificent editorial work, *The History of Taxation* (1999).

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Ross B. Emmett, *Frank Knight and the Chicago School in American Economics* (London and New York: Routledge, 2009), pp. xxxi, 218, \$130 (hardcover). ISBN 978-0415775007.

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Attempting to come to grips with the Knightian corpus can often—as I can readily attest from personal experience—prove rather daunting for the uninitiated. Fortunately, the uninitiated now have Ross Emmett's splendid volume to help guide them through the thorny Knightian thicket. Emmett has long since firmly cemented his place as the world's leading Frank Knight scholar, and this excellent volume collects a number of his previously published papers (thirteen chapters), adds some new material (two chapters), and makes Emmett's first-rate work—all the papers were written between 1987 and 2007—available in one handy place. Accordingly, I expect the volume to make Emmett's mastery of the Knightian corpus deservedly well-known among a rather wider group of readers than that usually associated with the history of economics community (and various fellow travelers) alone. As the volume contains many previously published papers, there is, unsurprisingly, some near-verbatim reiteration of Knightian points that have been made in earlier chapters. Any such repetition, however, hardly mars the outstanding quality of Emmett's work and his solid and sympathetic appreciation for the specific historical context within which Knight wrote. Anyhow, Knight himself supposedly liked to frequently invoke Herbert Spencer's point that “only by varied reiteration can alien conceptions be impressed on reluctant minds” (or words to said effect). Rather than provide a detailed précis of each chapter, I shall here focus on certain key Knightian themes. Indeed, I urge anyone uninitiated in the mysteries of the Knightian corpus to carefully study my two favorite chapters in the volume: Chapter 6 (“What is truth' in capital theory?") and Chapter 7 (“Maximizers versus good sports: Frank Knight's curious understanding of exchange behavior”).

In Chapter 3 (“The Therapeutic quality of Frank Knight’s *Risk, Uncertainty, and Profit*”), Emmett (drawing on Rorty) very aptly views Knight’s work as “therapeutic” (p. 33). As Emmett explains, Knight—much like the “therapeutic philosophers” invoked by Rorty (e.g., Dewey and Wittgenstein)—sought to remind his professional colleagues of the need “for humility in the face of the dynamic complexity and novelty of human experience” (p. 33). Accordingly, Knight attempted to draw professional attention to the inherent limitations of pure economic theory. As Emmett explains, Knight ought to be viewed as a thinker who seeks to “destroy” grand systems of thought because they are viewed as inherently “inimical to the continued health of that great conversation we call society” (p. 103). As Emmett’s book carefully and abundantly makes clear, Knight deemed pure economic theory—understood to denote the pristine logic of choice per se—to be necessarily limited in what it could contribute to our attempt to adequately grapple with various social problems. Indeed, Knight’s favored ‘solution’—discussion among equals—to social problems primarily focuses on the necessity to engage in a genuine discussion about the kind of preferences we think we ought to have (a discussion about the kind of society we think we ought to have and about the kind of people that we think we ought to be). As Emmett explains, in Knight’s view, efficiency simply cannot provide the primary criterion by which “coordination mechanisms” (collectivist or market) can be judged (p. 101): “They should be judged . . . in terms of the kind of wants and desires they create, and the character of the people they form” (p. 101).

Unsurprisingly, Emmett persuasively argues that Knight would have scant truck with the contemporary Chicago School mantra of *de gustibus* (chapters 11 and 12). Similarly, Knight views economic theory per se—the analytics (inherent tautologies) of a purely timeless and necessarily static equilibrium—as a rather limited exercise in ‘engineering’: the merely mechanical problem of allocating a set of given means—assuming a given technology and given set of ‘social’ institutions—among a set of given and unchanging competing ends (p. 122).

Accordingly, pure economic theory (a set of tautologies concerning the logic of maximizing behavior) is necessarily limited in what it can contribute to any discussion concerning the potential solution (adequate or otherwise) to social problems. Importantly, in Knight’s view, there is “no real difference between deterministic” and stochastic worlds (p. 56). As Emmett wryly explains (his joke can and ought to be readily and shamelessly ripped off for classroom use), Knight views the economist’s ready and hand-waving invocation of the “comfort of insurance against risk rather than the instability of uncertainty” as the analytical equivalent of offering a cold drink to a “prisoner chained in the sun” (p. 56): There is no analytical escape from genuine uncertainty, and the tautologies of pure theory are simply ill-equipped to adequately grapple with the passage of time, the evolution of socially induced tastes and wants, and the evolution of institutions and technology.

Chapter 5, Frank Knight’s dissent from Progressive social science, is a tour de force and very highly recommended. Importantly, Knight’s disagreements with Hayek over the thorny relationship between the “world of theory” and “reality” come to the fore (p. 67). For Knight, uncertainty—the real thing rather than some merely stochastic doppelgänger—provides “powerful reasons” to reject any theoretical claim that we should observe “any necessary movement towards equilibrium in a market or collectivist system” (p. 67). As Emmett explains, the Knight–Hayek

debate, usually characterized as an arcane debate over capital theory (p. 67), “involved competing conceptions of the scientific nature and role of equilibrium theory” (p. 67). This issue comes to the fore in Emmett’s brilliant Chapter 6 (one of my personal favorites in the volume and, I think, providing much insight into Knight’s mindset). As Emmett explains, Hayek and Knight—seemingly having markedly similar ‘free-market’ sympathies and classical liberal worldview—differ markedly on a variety of important margins. Emmett notes their divergence of views in his brilliant and fascinating essay examining the Hayek–Knight exchange over capital theory. As Emmett persuasively explains, Hayek (Hayek and Knight both posit a sharp dichotomy between “the constructed realm of theory and the contingent world of lived experience” [p. 82]) seeks to analytically collapse the future into the present: modeling the allocative moment as a dynamic “process of dated goods existing through time” (p. 82). For Knight, however, pure theory can never capture more than merely “one element”—the one has much importance—of our momentary “lived experience” and has no relevance beyond the purely “allocative aspect of that moment” (p. 82). Accordingly, Hayek thinks that market dynamics “tend toward equilibrium” (p. 83). Knight, refusing to “model equilibrium over time,” argues that change “in the presence of uncertainty” would necessarily “alter the institutional structure that equilibrium theorizing takes as given” (p. 83).

Emmett could have similarly invoked the seemingly prima-facie equivalent—but still by and large incongruent (their disagreements over capital theory surely coming to the fore yet again)—stances that Knight and Hayek took during the rather heated 1930s and 1940s controversy over the possibility of socialist calculation. Indeed, there is much reason why Knight, with characteristically perceptive keenness (conjunct with vituperative literary bent), deemed the calculation controversy a relatively trivial spat over the economics of “sound and fury” (Knight 1938, p. 267). For Hayek, socialist calculation is literally impossible: socialism—I refrain from adequately unpacking what “socialism” (anything akin to full-blown command planning) supposedly connotes—is patently impossible in theory and in the real world of change and uncertainty. For Knight, however, it is unclear why Hayek (Knight clearly thought Hayek was right about the impossibility of adequate socialist calculation in the real-world of pervasive uncertainty) would think socialism is impossible in theory. As Emmett notes, Hayek frequently invokes the supposed tendency with which entrepreneurship adequately coordinates the economy (pp. 67, 83). Knight, however, as Emmett persuasively explains throughout the volume, rather doubts the presence of any such tendency in the real world of change and uncertainty. Accordingly, and much as the auctioneer posited by theory (by Walrasian theory if not by Hayekian theory) adjusts relative prices to attain systemic equilibrium—any false trades and the suchlike are necessarily ruled out of court—the socialist planner-cum-auctioneer can similarly adjust prices by trial and error (thus, in theory, eliminating any hypothetical shortages and surpluses) and supposedly mimic the way in which the Walrasian auctioneer arrives at an overall system-wide equilibrium. The Hayekian might well ask—and with very good reason—how socialist calculation is to adequately grapple with capital investment and the role of expectations in any world where the future is necessarily unknown. How is adequate coordination of production under socialism to occur once we allow those markedly thorny issues into play? Knight would readily nod assent, while simultaneously pointing out that the

Hayekian is pretty much giving the ‘calculation game’ away. Similarly, a Hayekian might wonder how long it will take for the supposed planner-cum-auctioneer’s ‘trial and error’ farce to converge on a systemic equilibrium (particularly when we posit changes in the supposedly underlying conditions of tastes, technology, etc). Again, Knight would agree, and then explain why Hayek’s objections cut with equivalent relevance—and irrelevance—against the Walrasian auctioneer posited and beloved by pure theory (or the contemporary Hayekian argument that entrepreneurship coordinates the economy). As Knight would say, the passage of time—introducing time allows genuine choice, indeterminacy, and all manner of historical changes to rear their patently thorny head to the chagrin of pure theory (e.g., Knight’s objections to Hayekian capital theory)—invoked by our imaginary Hayekian cuts both ways. Any tendency to equilibrium is questionable once we introduce uncertainty. Accordingly, ‘socialism’ per se is feasible in theory, feasible within the confines inherent to an economic model, and thus feasible in a markedly irrelevant conception of ‘feasibility.’ As Knight puts it in his review of Lange and Taylor’s ‘market socialism,’ economic “analysis is essential but purely preliminary and negative in significance” (1939, p. 600).

Is it any wonder that Hayek and Knight would take markedly differing stances when it came to evaluating the possibility of socialist calculation in ‘theory’ and yet stand shoulder to shoulder when it came to objecting to any real-world proposals for anything akin to full-blown command planning? All things considered, Emmett’s splendid volume is a must-read for anyone interested in Knight, the development of 1930s and 1940s economics, the Chicago School (vintage and modern), and the history of ideas.

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Lanny Ebenstein, *Milton Friedman: A Biography* (New York: Palgrave Macmillan, 2007), pp. xi, 286, \$27.95 (hardcover). ISBN 1-4039-7627-9.

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Stephen Moore wrote a commentary (2009) for the *Wall Street Journal* saying that what we most need in the current economic crisis is the advice of Milton Friedman. A lucky accident of my reading the *Biography* mainly in the first half of 2009 is that I encountered several passages, and Friedman quotes, that coincidentally suggest what advice Friedman might give us on what we should be doing during the current

economic crisis. For instance, Ebenstein quotes Friedman as saying that “supply-side fiscal policy . . . consists of cutting high marginal tax rates in order to stimulate innovation and entrepreneurship. . . . Experience suggests that it is very effective in stimulating economic growth. It is a policy for the long run” (p. 178; ellipses in Ebenstein). Friedman emphasized removing obstacles and increasing incentives for the producers and the innovators. Though not an advocate of the Laffer Curve, in a broad sense he was a “supply-sider.”

The *Biography* is divided into three parts, each covering roughly a third of Friedman’s long (but too short) life. Roughly three times as much space is devoted to the middle years as is devoted to either the early or later years. Besides the main body, the *Biography* also includes an extended bibliographic essay and a brief, previously published, 2005 interview with Friedman, conducted by Nathan Gardels. The bibliographic essay is useful, but would need to be much longer to be anything close to comprehensive. The interview is useful in presenting some of Friedman’s final views on some issues, but focuses much attention on foreign affairs—not usually thought of as one of Friedman’s main areas of expertise.

Biography author Lanny Ebenstein (who called himself Alan Ebenstein in his earlier books on Hayek) is currently a Visiting Professor of Economics at the University of California at Santa Barbara. Ebenstein does not reveal his motives for writing the *Biography*. As far as his broad goal, he suggests that he is trying to summarize Friedman’s contributions both to economics and to libertarianism. Although it is not his main goal, he also provides some information on Friedman’s personal life. The *Biography* is friendly to Friedman. While it does not count as an “authorized” biography, it is clear that Ebenstein was given considerable access to Friedman’s papers and to the Friedmans themselves.

So, is this currently the best source of information on Friedman’s contributions to economics and libertarianism? The book is far from a definitive intellectual biography of Friedman, in the way that McCraw’s recent biography of Schumpeter is arguably the definitive intellectual biography of Schumpeter (see Diamond 2009). So far, I believe that the richest and best written source on Milton Friedman’s life and work is his autobiography with his wife Rose, *Two Lucky People: Memoirs* (up to 1997, where *Two Lucky People* ends). *Two Lucky People* is a much longer and denser book than the *Biography*, requiring the reader to make a greater investment in time and effort. For those who want to make a smaller investment, the *Biography* may have some value, especially if they are less interested in Friedman’s contributions to positive economics than they are in his libertarianism. But even these latter readers might be better off reading *Free to Choose* (if they are beginners) or *Capitalism and Freedom* (if they are serious).

The historian of economic thought may find some useful anecdotes and quotations in the *Biography*, but will seldom find sustained and deep analysis. As someone who has read the Friedmans’ *Two Lucky People*, and otherwise followed with interest (and admiration) the career of Milton Friedman, I did not find any startling revelations in Ebenstein’s book, although there were sometimes interesting nuances and details. I’ll mention a couple of these, in case they are also of interest to others.

In *Two Lucky People* (p. 29) Friedman says no more than “whatever the reason” about his switch from majoring in mathematics to economics. But in the *Biography* he is quoted (p. 18) as saying: “Put yourself in 1932 with a quarter of the population

unemployed. What was the important problem? It was obviously economics and so there was no hesitation on my part to study economics.”

In *Two Lucky People* (p. 32), Friedman gives credit to Homer Jones for first introducing him to the “Chicago view” and for putting “major stress on individual freedom.” But in the *Biography* Ebenstein writes (p. 15) that “Friedman received his introduction to libertarian thought at Rutgers through John Stuart Mill’s *On Liberty*, which he read as a freshman or sophomore.” (“Mill” does not even appear in the index of *Two Lucky People*.)

Although those sympathetic to Friedman will find much in the *Biography* to enjoy, there remain interesting, and sometimes important, issues about Milton Friedman’s life and ideas that are not addressed in this *Biography*. It is too late to help Ebenstein, but for the benefit of future biographers, I will provide three examples.

In George Stigler’s *Memoirs of an Unregulated Economist*, he writes of the most intellectually exciting evening of his life being the evening when Ronald Coase convinced the senior economics faculty of the University of Chicago of the truth of the Coase Theorem. According to Stigler, all the senior faculty gathered at Aaron Director’s apartment, and heard Coase out. Stigler continues (p. 76): “We strongly objected to this heresy. Milton Friedman did most of the talking, as usual. He also did much of the thinking, as usual. In the course of two hours of argument the vote went from twenty against and one for Coase to twenty-one for Coase. What an exhilarating event!” Stigler’s account raises several questions. An obvious one is whether there is any evidence confirming or qualifying Stigler’s account. Another arises when we juxtapose Stigler’s account with a comment of Friedman’s that Ebenstein quotes: “Anyone who is converted in an evening isn’t worth converting. The next person of opposite views . . . will unconvert him” (p. 195; ellipsis in Ebenstein). So why, and how, in this case, were Friedman and the rest so quickly converted? What was it about Friedman’s personality, or method of thinking, or method of talking, that made him so persuasive, even with such a high-powered, opinionated, and stubborn group?

Another issue for the future is suggested when Ebenstein quotes Friedman (p. 151) as saying that, besides himself, the two most influential libertarian thinkers were F.A. Hayek and Ayn Rand. Ebenstein includes a whole, albeit brief, chapter on the relationship between Friedman and Hayek, but he tells us nothing about whether there was any mutual learning or admiration between Friedman and Rand.

Consider one final unaddressed issue. Mill gives Harriet Taylor substantial credit for *On Liberty*, and Milton gives Rose substantial credit for *Free to Choose* and the later policy writings. Some have doubted Mill’s claim; what is the evidence on Friedman’s similar claim?

Concerning more mundane issues, the *Biography* clearly would have benefitted from at least one more thorough rewrite by the author, and one more careful check by a copy editor. Typos, awkward constructions, and absent transitions abound. (For an especially egregious example, see the misspelling of “Friedman” on p. 168.) It also would have been useful, or at least interesting, to have included a few photographs, especially (as on p. 7) where Ebenstein takes the time to describe a photograph.

In the brief final chapter, Ebenstein writes that Friedman’s death on November 16, 2006, came right after the *Biography* manuscript had been completed. Apparently Ebenstein’s response was to revise the final chapter, and throughout the rest of the book to change, here and there, a few verbs to the past tense. But many other portions

of the book were left written as though Friedman were still alive. I found the continuous inconsistency to be a bit jarring.

So, on the one hand, it is easy to imagine how Ebenstein's *Milton Friedman: A Biography* could have been improved. But, on the other hand, Ebenstein has given us the first biography that sketches the ideas and events of the whole of a major economist's life. Friedman surely deserves a deeper, more comprehensive, and better written biography. When such a biography is written, its author will find some useful material in Ebenstein's early effort. But while we are waiting for the definitive biography, it is better to have Ebenstein's biography of Friedman than to have no biography of Friedman at all.

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