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Review of Edward Nelson, *Milton Friedman and Economic Debate in the United States, 1932-1972 (volumes 1 and 2)*, Chicago and London, The University of Chicago Press, 2020, 737 + 587 pages, ISBN 978-022668377-5 and 978-022668489-5

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Review of Edward Nelson, *Milton Friedman and Economic Debate in the United States, 1932-1972 (volumes 1 and 2)*, Chicago and London, The University of Chicago Press, 2020, 737 + 587 pages, ISBN 978-022668377-5 and 978-022668489-5

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How a non-philosopher and non-historian monetary economist does methodology and history. Edward Nelson's recent book on Milton Friedman

Edward Nelson's name is one of those widely known brands in monetary economics that need no special introduction. He has been a fixed star of the profession for decades. Right after earning his PhD degree at Carnegie Mellon University under the supervision of Bennett T. McCallum and Allan H. Meltzer, household names in the field, he started his professional career as a central bank economist at the Bank of England, then proceeded to the Federal Reserve Bank of St. Louis. His current position is at the Division of Monetary Affairs of the Board of Governors of the Federal Reserve System, Washington DC, where now as a senior advisor (and former assistant director, and former senior economist of the monetary studies section) he is responsible for supporting the decision makers at the Board and the Federal Open Market Committee.

An outstanding publication record comes with this high impact in monetary policy affairs. Nelson owns dozens of papers in leading journals and has acted as the associate editor of the *Journal of Monetary Economics* and the *Journal of Money, Credit and Banking*, and as reviewer for a wide range of periodicals including the *American Economic Review*, the *European Economic Review,* or

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the *Journal of Political Economy*. This line of research and policy advising, however, is only one thread in Nelson's portfolio. His profound expertise in monetary policy matters is combined with an unflinching interest in Milton Friedman's life and work. These two lines sometimes converge and unite in papers like *Reaffirming the Influence of Milton Friedman on U.K. Economic Policy* (Nelson 2017) or *Friedman's Monetary Economics in Practice* (Nelson 2013). It is thus no wonder that Nelson put forward his recent, two-volume monograph on Milton Friedman, the subject of the present review, not as a historian of economic thought or a methodologist but as a monetary economist specialized in the same field of research as Friedman was. It leads to a unique perspective having the promise of a more in-depth understanding of Friedman's theoretical achievements and his involvement in the professional and public debates on economics and economic policy between 1932 and 1972, in the heyday of his activity in monetary economics research, than what common historical accounts could offer.

Nelson's main rationale for this claim is the interest that historians show in unpublished materials like letters, notes, and drafts. As he argues, historians rely on such items only to hide their ignorance about theories. The exact theoretical message, Nelson continues, can always be reconstructed accurately on the basis of published works, so unpublished materials cannot take us closer to this core content (vol. 1, ix-x). What is needed is thus not a broadening of the textual basis but the ordering of published works into a coherent picture. Coherence by no means presupposes consistency, though. Following the general trend in the recent Milton Friedman literature (Forder, 2014; 2019), Nelson also admits that Friedman was not free from inconsistencies – although from this fact, unlike others in the field, Nelson draws no inferences regarding Friedman's sanity or honesty (vol. 1, 22-26). The fact that some of Friedman's views and statements cannot be taken at face value is just a fact to highlight. By setting his own standards and priorities for historical work, and by claiming to look at Friedman in his own way, Nelson's purpose is to keep as huge a distance from the existing Milton Friedman literature as possible.

This attitude, serving as the backbone of the whole book, turns into an open negligence of the rival interpretations. Nelson neither takes issue with the alternative readings, nor casts doubt on their plausibility. This is rendered obvious by the reference sections of the volumes in which the works of the widely known historians and methodologists of Friedman scarcely come up. A good example is where Nelson sets out to provide an interpretation of Friedman's (1953) positivist methodology – a paper that played a special role in the emergence of economic methodology as a subdiscipline of economics. Initiated by Uskali Mäki (1989), since the 1980s a growing body of literature has aimed to approach the positivist methodology in philosophical terms with the purpose of elaborating a genuinely methodological terminology, while depriving the paper of its historical context to a large extent. Even if the Mäkian realist reading has never become generally accepted, it stirred up a controversy that presupposed a specific terminology and knowledge in the background – so economic methodology distinguished from both the history of economic thought and history of economics was born in this debate. As the positivist methodology was taken in itself and not as a constituent of Friedman's oeuvre (only years after the start of the methodological controversy did Hoover (2009) pose the question whether Friedman had actually followed the methodological principles he laid down in the paper), methodologists had no problem in labelling the study as F53, which unintentionally implies that the positivist methodology is an iconic, typical and representative item of 1953 for Friedman.

By contrast, Nelson approaching from a historical point of view does not intend to separate the paper from Friedman's lifework, so he disregards the whole methodological literature on Friedman for allegedly missing the mark by attempting to evaluate a paper in isolation. As the methodological paper inherently raises some methodological questions, and these questions are normally treated in the realism-instrumentalism framework of the economic methodology literature, however, Nelson's originality in interpretation and his prejudice result in a compatibility problem – the negligence of the realism-instrumentalism duality is not the same as replacing it with a more effective and illuminating interpretive framework. Accordingly, Nelson boils down the problem of Friedman's assumptions, the focus of the methodological debate, to the tolerability of the inaccuracy of predictions (vol. 1, 352-353). This point is admittedly in accordance with Friedman's ideas – for him, any model was acceptable as long as it provided sufficiently good predictions. However, there are questions that can hardly be addressed outside the realism-instrumentalism context. Such a question regards the nature of the assumptions that Friedman found preferable. Is it true that Friedman judged models by their predictive performance only, hence was he a pure instrumentalist subscribing to the option of *anything goes*? Or, being a realist, did he add truth requirements to the basic need for satisfactory predictions? Such methodological questions cannot be answered by referring to the commonplace that macroeconomics in the 1950s was empirical.

By the same token, Nelson skips a related methodological problem where he dwells on the professional and personal relationship between Friedman, Lucas, and Sargent (vol. 2, 297-318). Nelson attributes to Lucas and Sargent the introduction of the rational expectations hypothesis into macroeconomics that amounts to the idea that expectations about future developments affect current macroeconomic outcomes. Assigning an explicit role to expectations was not alien to Friedman either: they served as the basis on which he in his presidential address (Friedman 1968) and Nobel lecture (Friedman 1977) overwrote the previously presumed stable trade-off between inflation and unemployment. There was a dramatic difference, though. Friedman assumed adaptive expectations under which only past forecasting mistakes counted; he thus deprived his agents of the basic ability of looking forward. Quite surprisingly, when talking about Friedman, Lucas, and Sargent, Nelson aptly documents that Friedman was aware of the theoretical importance of forward-looking behaviour and rational expectations. He goes so far to argue that Friedman himself also did a lot about the subsequent rational expectations revolution. In Nelson's narrative, Friedman was thus one of the minds who inspired the switch to the assumption of rational expectations. However, there is a fly in Nelson's ointment. It is one thing to claim that Friedman was a forerunner of the rational expectations revolution, but it is another thing to explain why he refrained from using this assumption in papers where expectations crucially affect the theoretically derived outcomes. On this latter front, however, Nelson does nothing at all. Paying no attention to the realism-instrumentalism dyad, he cannot realize that Friedman's strategy can adequately be sorted under the instrumentalist banner – even if the assumption of adaptive expectations implausibly curtails the information basis of decisions, Friedman insisted on it as in its absence he must have abandoned the idea of the short-run non-neutrality of money in his Phillips curves (Galbács 2020, pp. 270-275). Used properly, methodology can help a great deal in interpreting theoretical achievements. When Nelson sets aside methodology as an interpretive framework just because he sees no point in labelling F53 as F53, he cannot but throws the baby out with the bathwater.

The fact that Nelson walks his own way in understanding Friedman stems from his basic position of a practicing monetary economist. His methodological toolkit is that of a non-philosopher economist. Likewise, Nelson by no means turns into a historian of economic thought. He considers Friedman's theory from the viewpoint of a modern successor of his, and this stance inevitably implies a historical perspective. However, Nelson remains the man of today throughout the book, who looks back at the past of our discipline only to draw conclusions for current practice – his tools and methods are all rooted in today's knowledge (a good example is the section where Nelson reformulates Friedman's aggregate demand framework under the rational expectations assumption; vol. 1, 188-189). In this respect, Nelson's attitude is typical of the cutting edge. There is one thing that the history of economic thought and economic methodology have in common: once both aspired to be the forums of disciplinary self-critique or self-reflection. The history of economic thought, which is the oldest of the two, succeeded (Emmett 2009, p. 131), even if by now it has been marginalized. Historians may easily happen not to be reckoned as economists at all (Marcuzzo & Rosselli 2002), and by browsing the recent issues of the most popular journals in the field of the history of economic thought it is not difficult for one to think that being an economist is no longer a prerequisite for joining the discussions. Likewise, methodology, as a consequence of its failure to draw the attention of practicing economists, is exiled from the cutting edge and sorted under the history or the philosophy of science (Mäki 2021; Vromen 2021).

Now, however, we have a two-volume Friedman monograph by a prominent practicing economist. Is it not a contradiction with the comments above? I do not think so. Just as methodology is inseparable from doing economics (Weintraub 1989), economics has never lost interest in looking back on its past. A prominent example for histories done by non-historians is how Lucas (1977; 1980), partly teamed up with Tom Sargent (Lucas and Sargent 1978), assessed Keynesian macroeconometrics in the light of their own achievements. As further instances, we can find papers squeezed into economic journals like *Cambridge Journal of Economics*, *Oxford Economic Papers, Journal of Economic Literature* or even the *Journal of Political Economy*. Instead of relying on outsider accounts, economists thus write their own histories. This is exactly what Nelson does.

Consequently, he regards Friedman's system not as a relic from the past but as a relevant theoretical response to the events and problems of the socio-economic environment. This is one of the merits of the book – Nelson keeps Friedman's professional milieu and the embedding socio-economic setting in the focus of his discussion. To this end, he shows great proficiency in applying the interview technique (vol. 1, xiii-xviii). Dozens of interviews complement the material Nelson assembled by thoroughly browsing Friedman's works. Unfortunately, the reader receives no information about the contents or the structures of such conversations – they were likely to be informal chats conducted along some pre-established lines to remain in touch with the thread of the discussion. Especially with these interviews can Nelson provide an insider look into the personal and scholarly connections around Friedman – and in this regard Nelson, thanks to his exhaustless working capacities, goes farther than any piece in the existing literature. What is more, Nelson's brilliance aided him in treating these interviews as pieces of a puzzle to combine with the facts found under Friedman's pen and to put them together into a comprehensive big picture in the end.

Perhaps as the main rationale for his interest in Friedman's life and work, Nelson does not go into the reasons why Friedman's theory has become obsolete by today. Instead, he wants to show how and why Friedman once was able to provide effective answers to substantial questions raised by his own society. Friedman's theory is no longer a fossil, which it unavoidably would become in the hands of an 'armchair' historian, but a framework having changed as both society and knowledge changed. Nelson can clearly see the theoretical connections leading *to* and *from* Friedman's theory (e.g., vol. 2, 297-318), so for him studying Friedman is a stimulating way of learning about today's practice. Doing so, Nelson is not in need of the existing historical literature. He is an active monetary economist who falls closer to the cutting edge of our science than to the cluster of passive chroniclers. His negligence is only a symptom of the gap between practitioners of economics and historians (Galbács 2020, pp. xi-xii). A cutting-edge economist is interested in the past only insofar as it helps him to find adequate methodological and theoretical answers regarding present and future. This attitude renders Friedman's theory alive, which is out of use now just because society has changed by today. This is admittedly true of all social theories – we are always shooting at a moving target. It, however, does not stop Nelson from treating Friedman and his lifework as an outstanding lesson of how to form meaningful theories.

Once sharply distinguished (Emmett 1997), *history of economic thought* and *history of economics* are merging into each other (exemplified by the fact that the journal of the 'History of Economics Society' is called the 'Journal of the History of Economic Thought'), and today it is not uncommon for one to come across 'history' papers that have nothing to do with theory or the social background. The merging of the history of economic thought and the history of economics is also present in Nelson's book – but in a stimulating way. Instead of camouflaging an history of economics as an history of economic thought, he does both in full-fledged forms. Realizing this twofold aspiration helps us to find an adequate scope for Nelson's endeavour. His Friedman monograph can be an effective next step towards the reintegration of historical studies into cutting-edge economics.

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