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Introduction

On December 5, 1871, John Stuart Mill wrote to his friend and disciple John Elliot Cairnes expressing dismay at the work of William Stanley Jevons, one of the pioneers of the new abstract mathematical style in economics. Jevons had "a mania for encumbering questions with useless complications," Mill wrote, "with a notation implying the existence of greater precision in the data than the questions admit of" (Mill 1972).

At the time of writing, Mill had not yet read Jevons' recently published *Theory of Political Economy*, but if he had, he would have found no reason to change his view. Jevons, for his part, was equally critical of Mill's work – and used remarkably similar language to make his complaint. According to Jevons, it was *Mill's* economic doctrines – and those of the then-dominant British Classical School more generally – that were unnecessarily complicated, because they were based on "mazy and preposterous assumptions" about the basic concepts of political economy (Jevons 1965: xliv).

What Mill and other classical political economists failed to see, Jevons argued, was that despite the apparent complexity of human social activity there was a fundamental simplicity and unity at its core. Standard economic notions such as utility, wealth, value, commodity, labor, land, and capital all reflected a single underlying theme: the basic human tendency to "satisfy our wants to the utmost with the least effort – to procure the greatest amount of what is desirable at the expense of the least that is undesirable – in other words, to maximise pleasure" (Jevons 1965: 37).[1] This tendency manifested itself in human behavior in a manner that was uniform across people, quantitatively (Jevons thought cardinally) measurable, and separable from influences that were more context-dependent, such as morality or culture. Recognizing this, Jevons argued, would allow many of the issues that had troubled classical political economists to be bracketed, enabling the

[1] Jevons borrowed this formulation (with acknowledgement) from J.-G. Courcelle-Seneuil (Jevons 1965: 41; Courcelle-Seneuil 1858: 36).

articulation of a precise "*mechanics of utility and self-interest*" on the model of physical mechanics (Jevons 1965: 21, emphasis original).

According to Jevons, the analogy with physical mechanics ran deep. The "laws and relations" governing utility mechanics had to be "mathematical in nature," because they "*dealt with quantities*," i.e. "things . . . capable of being *greater or less*" (Jevons 1965: 3, emphasis original). These laws could also be isolated from potentially disturbing factors, not only conceptually but also empirically. Although the economist could not conduct controlled experiments to effect this isolation directly, Jevons believed that the effects of disturbing factors could be dealt with systematically, even when economists were largely in the dark about their nature and operation.[2]

Consequently, it seemed to Jevons that scepticism about the possibilities of a precise science of political economy, like that expressed by Mill in his letter to Cairnes, was merely conservatism standing in the way of progress. This sentiment was expressed clearly in the concluding comments to the *Theory of Political Economy*, in a section titled "The Noxious Influence of Authority" Jevons wrote:

> I think there is some fear of the too great influence of authoritative writers in Political Economy. I protest against deference for any man, whether John Stuart Mill, or Adam Smith, or Aristotle, being allowed to check inquiry. Our science has become far too much a stagnant one, in which opinions rather than experience and reason are appealed to . . . Under these circumstances it is a positive service to break the monotonous repetition of current questionable doctrines, even at the risk of new error. (Jevons 1965: 276–7)

Looking back on the disagreement between Mill and Jevons from the perspective of 2015, it would seem that Jevons has been vindicated. Contemporary academic economics is a thoroughly mathematical enterprise, reflecting many features of Jevons' approach. And one finds few doubts within the professional mainstream as to the aptness of the mathematical analysis of economic behavior.[3] To most contemporary economists, Mill's views on the methodology of political economy are at best an interesting piece of intellectual history. They are irrelevant to the actual practice of economics.

Yet Mill's skepticism toward Jevons' approach to political economy may be more than a mere historical curiosity. Mill's position, especially when

[2] See Jevons (1958: 376, 554–5) and Peart (1995: Section III).
[3] One may, however, find misgivings about the development of highly abstract mathematical models without clear empirical application – see, for example, Colander (2005b) for reflections on the discipline's changing views on the importance of empirical content.

understood in the context of his broader philosophy of science, poses a fundamental and formidable challenge to those who, like Jevons, would wish to use the power and precision of mathematics to investigate social phenomena. In fact, the issues Mill discerned continue to vex mathematical economics to this day. To see that, however, we need to understand the basis of his misgivings.

As a committed empiricist, Mill held fast to the value of experience. The general principles of science were, in Mill's eyes, contrivances in its service and subject to its discipline. Although abstractions were necessary to formulate general principles, Mill insisted that one must not make the mistake of taking the *abstractions* to be the object of scientific inquiry, rather than the phenomena they were supposed to represent. If a scientist lost focus on the actual phenomena of interest in that manner, the concepts advanced in their service might well become detached from them. It would then become unclear what, if any, epistemic value the principles formulated using those concepts would have. As Mill explained,

> If any one, having possessed himself of the laws of phenomena as recorded in words, whether delivered to him originally by others, or even found out by himself, is content from thenceforth to live among these formulae, to think exclusively of them, and of applying them to cases as they arise, without keeping up his acquaintance with the realities from which these laws were collected – not only will he continually fail in his practical efforts, because he will apply his formulae without duly considering whether, in this case and in that, other laws of nature do not modify or supersede them; but the formulae themselves will progressively lose their meaning to him, and he will cease at last even to be capable of recognising with certainty whether a case falls within the contemplation of his formula or not. (Mill 1974: Bk. IV, ch. vi, sec. 6, 711)

Since experience can always reveal new possibilities and complexities, ensuring that abstractions remained firmly rooted in it required constant vigilance. "We must not only be constantly thinking of the phenomena themselves," Mill wrote, "but we must be constantly studying them; making ourselves acquainted with the peculiarities of every case to which we attempt to apply our general principles" (Mill 1974: Bk. IV, ch. vi, sec. 6, 710). To the extent that experience revealed that one's principles had become untethered from the subject matter they were supposed to represent, those principles would have to be revised accordingly.

Significantly, this requirement implied that the scientist must take care to articulate scientific principles in language that was *capable* of expressing whatever kinds of complexities might arise in relation to the phenomena under investigation. The more complex the subject matter, and/or the less known about what kind of complexities lay behind one's observations, the more important it was to maintain flexibility. This was why Mill was particularly concerned about Jevons' use of mathematical notation. Mill saw mathematical language as capable only of expressing relationships between purely quantitative concepts. Mathematical symbols, he wrote (by which he meant symbols denoting entities that take on values, such as variables and parameters, not operational symbols such as "+" and "–") are "mere counters, without even the semblance of a meaning apart from the convention which is renewed each time they are employed" (Mill 1974: Bk. IV, ch. vi, sec. 6, 708). As a language of empirical science, mathematics was for Mill sufficiently sensitive only in cases of purely "mechanical" subject matter, which he defined as "those of which the investigations have already been reduced to the ascertainment of a relation between numbers" (Mill 1974: Bk. IV, ch. vi, sec. 6, 710).

In other words, mathematical language was capable of representing adequately only subject matter constituted by strictly quantitative objects and relations. Moreover, this was in Mill's view a *practical* requirement. Even if one somehow *knew* that, for example, wealth-generating activity was (as Jevons supposed) intrinsically mechanical and therefore in principle open to mathematical analysis, mathematical language would still not be appropriate unless scientists themselves could discern that mechanical nature in their observations. The observer herself needed to be able to perceive quanta in order to gather the data necessary to put hypothetical mathematical principles to use and/or to test them (Mill 1974: Bk. IV, ch. vi, sec. 2, 877–8).

The prime example of mechanical subject matter, according to Mill, was the physical universe. In his view, it was appropriate to express (for example) Newton's principle of universal gravitation in mathematical language because human beings are capable of discerning specific quantities corresponding to "mass," "force," and "radius" (or, more generally "distance") with sufficient precision that there could be no relevant qualitative differences among observations within each category. From the standpoint of Newtonian mechanics, it would not matter if one set of forces, masses, and distances occurred in France and another in England (or on the Moon or anywhere else in the universe), or if one set of observations were associated with a morally reprehensible purpose and another not. The only relevant difference between observations of the same type was their magnitude.

When confident that one was dealing with mechanical subject matter, it was not only appropriate but ideal to articulate general principles in mathematical language. Doing so enabled scientists to take full advantage of its purely quantitative nature. In particular, they could use their observations to derive and test precise empirical laws from those general principles. This, for example, is what Henry Cavendish did when estimating the value of the gravitational constant, G, in Newton's principle of universal gravitation, $F = Gm_1\, m_2/r^2$ (which expresses the force exerted by a body of mass m_1 on a body of mass m_2, and vice versa, at a distance of r) (Cavendish 1798). That calculation would have been impossible – or rather, its result would have been meaningless – if Cavendish had not been warranted in taking each successive observation of mass (or the distance between the two objects, or the degree of displacement of the objects due to gravity) as qualitatively identical to his preceding observations.

Mathematical language is thus extremely useful in investigating mechanical subject matter. But, Mill argued, it would be perilous to use it to investigate subject matter that was not mechanical. There were two possible causes of concern. First, in such cases mathematical principles might simply project an underlying mechanical structure onto the subject matter whether or not the latter was mechanical in nature. That is, mathematical language might generate a purely quantitative conceptual map of the subject matter it purported to outline, with no way of telling whether the outlines on the map corresponded to the subject's own contours. As a result, scientists would not be able to feel confident that data gathered according to the conceptual map accurately reflected the underlying subject matter. And because of that, it would be inappropriate to interpret any apparently precise empirical laws derived from that data as empirical laws applying to *the actual subject matter*.

Second, and still more worryingly, Mill argued that the commitment to mathematical language could actually prevent scientists from detecting when their conceptual map had become untethered from the subject matter under study. As will be recalled, Mill's prescribed defense against this kind of detachment was ongoing close contact between the scientist and the object of study. But if exploration of the subject matter itself developed only through the lens of mathematical language – which necessarily obscured any qualitative distinctions among the observations being made within each category – then the scientist would become blind to signs of that mismatch arising. As a result, the mismatch might persist indefinitely. Because of this danger, Mill warned that when the scientist was not certain of the mechanical character of the subject matter, the language of any general principles "should be so constructed that there shall be the greatest possible obstacles to a merely mechanical use of it" (Mill 1974: Bk. IV, ch. vi, sec. 6, 707).

The risk that mathematical principles might ascribe mechanical features to non-mechanical subject matter, and thus become untethered from the subject matter they were meant to represent, was precisely what concerned Mill about Jevons' approach to political economy, and indeed about mathematical social science generally. Human social activity was, for Mill, a paradigmatic example of a non-mechanical subject. It was a realm of almost unfathomable complexity, in two important ways. First, social phenomena were subject to innumerably more causes than physical phenomena. And second, crucially, the operations of those causes were inextricably intertwined.

> Whatever affects, in an appreciable degree, any one element of the social state, affects through it all the other elements. The mode of production of all social phenomena is one great case of Intermixture of Laws. We can never either understand in theory or command in practice the condition of a society in any one respect, without taking into consideration its condition in all other respects. (Mill 1974: Bk. VI, ch. ix, sec. 2, 899)

Thus, although Mill believed it was possible to form reliable general principles (perhaps even mathematical ones) about certain aspects of human nature in isolation, the fact that human beings always and only observe behavior in the welter of society meant it was impossible to discern whether and to what extent those general principles operated empirically. If indeed one knew, as Jevons presumed one would, that the influence of economic factors on human behavior was cleanly separable from the influence of all other factors, *and* one possessed a reliable method for screening off those influences, then a precise empirical science of political economy might be possible. But for Mill, whether the social world was parsable in this way was an empirical question – and, moreover, a question that could only be addressed through continual immersion in the social world itself – not a simple statement of fact or a self-evidently valid postulate, as Jevons assumed.[4] To take Jevons' route was to invite a split between model and target that would be undetectable using mathematical methods alone. One could go blithely on with mathematical explorations – gathering data, estimating the precise functional forms and parameters of the principles, and testing them against new data – unaware that in point of fact one had ceased to be exploring the phenomenon of interest in any meaningful way.

Mill's challenge to Jevons may seem distant from the modern discipline of economics. Yet it finds strong echoes in the debate over the implications for economic methodology of the recent financial crisis. A central question in

[4] See Jevons (1958: 2, 8).

that debate has been whether the highly abstract mathematical modeling methods that dominated macroeconomics in the years leading up to the crisis – in particular, Dynamic Stochastic General Equilibrium (DSGE) modeling – actively prevented economists from seeing the gathering storm. Critics of DSGE have charged that these models became untethered from the phenomena they were meant to represent in precisely the manner Mill feared. In a 2010 review of DSGE modeling in the *Journal of Economic Perspectives*, for example, Ricardo Caballero wrote that the practice of DSGE modeling "has become so mesmerized with its own internal logic that it has begun to confuse the precision it has achieved about its own world with the precision that it has about the real one" (Caballero 2010: 85). The primary culprits in that confusion, critics charged, were the extreme simplifying assumptions necessary to ensure the tractability of DSGE models – in particular, (i) the representation of aggregate economic activity as being generated by a small number of representative agents; (ii) the expression of the macroeconomy as a linear (generally log-linear) system; and (iii) the assumption of efficient financial markets. These assumptions rendered the model incapable of taking into account many kinds of complexity that turned out to be crucial factors in the crisis – for example, the perverse incentive structures at play in the financial sector in the late 1990s and 2000s. In effect, the models became mere mathematical exercises – toy models that were not models of the late 1990s–2000s economy in any meaningful sense.

Critics have also been concerned with the manner in which the mismatch between DSGE models and the actual economy gave rise to certain analytical blind spots. In a 2009 *New York Times Magazine* piece cataloguing the failures of economic methodology in the lead-up to the crisis, Paul Krugman argued that DSGE models caused a kind of tunnel vision in which the central causes of the crisis lay outside the realm of consideration. Conceiving of the economy through the lens of the model essentially required the economist to

> [turn] a blind eye to the limitations of human rationality that often lead to bubbles and busts; to the problems of institutions that run amok; to the imperfections of markets – especially financial markets – that can cause the economy's operating system to undergo sudden, unpredictable crashes; and to the dangers created when regulators don't believe in regulation. (Krugman 2009)

As Willem Buiter pointed out: these assumptions not only prevented questions about insolvency and illiquidity from being answered, "[t]hey did not allow such questions to be *asked*" (Buiter 2009, emphasis original).

The concern with excessive abstraction in mathematical economic modeling is not new or unique to the post-crisis era. Indeed, it has been a persistent concern since the apotheosis of abstract modeling in the 1950s and has occasionally risen to the surface of intra-disciplinary discussion. In 1969, Frank Hahn used his presidential address to the Econometric Society as an opportunity to bring it to the fore. Commenting on the achievements of economic theory in the previous two decades, he argued that while they were "impressive and in many ways beautiful," there was nonetheless "something scandalous in the spectacle of so many people refining the analyses of economic states which they give no reason to suppose will ever, or have ever, come about." He added: "It is probably also dangerous" (Hahn 1970: 1). Wassily Leontief made a similar point in his Presidential Address to the American Economic Association the following year (Leontief 1971), as did Milton Friedman twenty years later in an article reviewing the trends in economics during the previous hundred years (Friedman 1991). Asked to reflect on the views expressed in that article in a 1999 interview, Friedman summed up his position as follows: "What I would say is that economics has become increasingly an arcane branch of mathematics rather than dealing with real economic problems" (Snowdon and Vane 1999: 137).

Yet the particular circumstances that gave rise to the current debate over DSGE modeling have brought out the perils of excessive abstraction with special clarity. Unlike previous discussions, this debate was precipitated by perceptions of a specific failure of economic methodology, and one with severe social consequences. It has even led to public calls for accountability.

Perhaps the most dramatic calling-to-account occurred on July 20, 2010, in a special hearing of the Science and Technology Committee of the US House of Representatives convened to investigate the failures of DSGE models. In its introductory statement, the committee commented on the inability of DSGE models to perceive the signs of the coming crisis, noting that "[t]he implosion of the subprime mortgage market came as almost a total surprise to most mainstream economists." And it noted that this blindness had affected even those explicitly charged with remaining aware of such issues: "The chief steward of the US economy from 1987 to 2006 [Alan Greenspan] said he was in a state of 'shocked disbelief' because he had 'found a flaw in the model that [he] perceived [to be] the critical functioning structure that defines how the world works'" (US House Committee on Science and Technology 2010: 3). But the committee also asked a broader question: essentially, given that DSGE models were so widely lauded within the discipline and that they seemed to have failed so spectacularly on such an important issue, why should anyone have confidence in economists' ability to assess their own models? The committee's words are worth quoting at length:

> [T]he insights of economics, a field that aspires to be a science and for which the National Science Foundation (NSF) is the major funding resource in the Federal Government, shape far more than what takes place on Wall Street. Economic analysis is used to inform virtually every aspect of domestic policy. If the generally accepted economic models inclined the Nation's policy makers to dismiss the notion that a crisis was possible, and then led them toward measures that may have been less than optimal in addressing it, it seems appropriate to ask why the economics profession cannot provide better policy guidance. (US House Committee on Science and Technology 2010: 3)

Within the discipline, discussions of what went wrong and what (if anything) to do about it have mainly been couched in terms of the "realism" of the accepted models[5] – and, in particular, the need to incorporate into macroeconomic models certain features of the economy that were excluded from DSGE models but are now recognized to have been centrally important. The basic message in those prescriptions has been that while abstraction is a necessary, and indeed desirable, feature of any model, it is important to ensure that the information lost in that process is not essential. As Ricardo Caballero put it: "It is fine to be as 'goofy' as needed to make things simpler along inessential dimensions, but it is important not to sound 'funny' on the specific issue that is to be addressed" (Caballero 2010: 90).

Considered in the light of Mill's more general concerns, however, such prescriptions may seem inadequate. The goal of preserving essential information about the subject matter certainly fits well with Mill's understanding of the requirements of valid induction. But the approach advocated by Caballero and others leaves open two crucial questions: first how to determine what is essential, and second how to ensure that the representation of those features remains faithful to the underlying phenomena.

This book begins the work of answering those two questions. It starts, in Part I, with a detailed analysis of standard economic modeling practice, and finds an important cause for concern. The internal logic of mathematical economic modeling, I argue, entails a commitment to the view that the

[5] In the context of economics, the "realism" of a model generally refers to the extent to which the model accurately captures features of its target. This is in contrast to the way in which the term "(scientific) realism" is used in philosophical discourse – roughly, to denote the position that "the entities, states and processes described by correct theories really do exist" (Hacking 1983: 21). To capture this distinction, Uskali Mäki has referred to the first sense of the term as "realisticness" rather than "realism" (see, e.g., Mäki 1992; 1994).

phenomena under investigation are mechanical in the manner that Mill suggested. Yet there is no *ex ante* reason to suppose that that is the case – and, crucially, any mathematical model will itself be *inherently* incapable of proving the situation either way. If we have independent reasons to believe that the phenomena under investigation are mechanical in Mill's sense, well and good: mathematical modeling will prove an apt mode of representation (though this does not imply, of course, that any *given* model will be a *good* representation of the subject matter). But if we have independent reasons to believe that there is more going on in the phenomena under investigation than a mathematical model can suggest – that is, that the phenomena in question are *not* in fact mechanical in the required sense – then mathematical modeling will prove misleading. The result will be precisely the kind of mismatch between the principles discerned by the scientist and the phenomena under investigation that Mill and others warned about. Moreover, as will be discussed, the empirical assessment of such models using econometric methods will not be sufficient to reveal that mismatch.

Part II discusses some trends in recent economic research – including the reliance on DSGE models in the run-up to the financial crisis – in light of the analysis presented in Part I, and Part III argues that new research in the interpretative aspects of economics may be necessary to address the problems identified in Parts I and II. These problems cannot themselves be addressed through reforms to mathematical methods. That would simply be to produce a more refined version of the wrong tool for the job, like sharpening one's knife when what is needed is a spoon. Rather than striving to improve the quality of mathematical models *given* the assumption that the subject matter under investigation is mechanical in Mill's sense and therefore susceptible of mathematical analysis, we need to ask a prior question, which is whether there is sufficient reason to feel confident that the subject matter under investigation is mechanical in the first place. That means scrutinizing the subject matter in the first instance in non-mathematical ways.

In brief, this book argues that we as scientists must remain sensitive to information about the phenomena in which we are interested that lies outside our models' conceptual maps. In the case of economics, what this requires is a new field dedicated to qualitative empirical methods that would play a similar role to that played by econometrics in the matter of quantitative empirical methods. In closing, I provide concrete examples of current research in this field, and suggest avenues for future work.