



First principles, fallibilism, and economics

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Abstract

In the eyes of its practitioners, economics is both a deductive science and an empirical science. The starting point of its deductions might be thought of as first principles. But what is the status of such principles? The tension between foundationalism, the idea that there are necessary and secure first principles for economic inquiry, and fallibilism, the idea that no belief can be certified as true beyond the possibility of doubt, is explored. Empirical disciplines require some sort of falsifiability. Yet, empirical inquiries also require a starting place—if not a necessarily true one, at least an indubitable one, that is, one that is not actually doubted. Indubitability appears to have necessary consequences, undercutting fallibilism, while fallibilism threatens confidence in the de facto first principles that begin inquiry. This tension is examined in three well-known attempts to define economics and its method: John Stuart Mill’s economics as the science of wealth, Lionel Robbins’s economics as constrained optimization; and George Stigler and Becker’s attempt to reformulate neoclassical economics to square empiricism with Robbins’ deductivism.

Keywords Economics · First principles · Fallibilism · Foundationalism · Empiricism · John Stuart Mill · Lionel Robbins · George Stigler · Gary Becker

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1 The tension between fallibilism and foundationalism in mainstream economics

In the late 18th and early 19th century, political economy, which by the end of the 19th century came to be called *economics*, emerged out of a mixture of academic writings on law, theology, and ethics, and out of writings on statecraft and pamphlets aimed at shaping government policy. Its sources were personal business experience, professional craft knowledge, casual, and, from time to time, more systematic, observation. David Ricardo (1817) and other early 19th-century economists sought to articulate the analytical structure of economics. After Ricardo, some questions frequently recurred: What is the scientific status of analytical economics? What is its relationship to empirical facts? How universal is its theory? Does it apply in all nations and all institutional settings or must it be relativized?

The dominant thread in economics (“mainstream economics”) came to be defined by its commitment to core analytical theory—even as that theory itself changed significantly. There was—and is—a persistent counterpoint to mainstream economics, marching under various banners from *Historicism*, to institutionalism, to today’s “new economic thinking,” including behavioral economics and neuroeconomics, each decrying the excessive abstraction (and, of late, mathematization) of mainstream economics and advocating the elevation of facts over theory and the orientation of economics to the concrete, the contextually specific, and the normative. While each new assault on the mainstream has enjoyed passing enthusiasm, the mainstream has proved remarkably resilient and has managed hold its challengers at bay and to define them as heterodox.¹

Despite a commitment to the priority of theory, mainstream economics sees itself as an empirical discipline. An empirical account must embrace contingent claims that might be either true or false. A thoroughgoing empiricism would embrace *fallibilism*,

¹ An anonymous referee objects to my characterization of “mainstream economics” versus heterodox economics in the early to mid-19th century, holding that the line of thinking that I characterize as “mainstream” was insufficiently dominant to warrant that label. All that is necessary for present purposes, however, is that the views of Mill (and Robbins) are sufficiently important to economics to warrant focusing a *philosophical* analysis on them. Evidence for their status is found in John Neville Keynes’s (1891) classic and self-consciously irenic methodological treatise. Keynes insists, however, on the conceptual distinction between positive economics, normative economics, and the art of economics (i.e., of the application of economics to concrete problems)—a division common to Mill and Robbins. And while receptive to the ambitions and achievements of the German historical school and related approaches in the English-language economics literature, he takes the view that those approaches failed to respect that fundamental distinction. He takes Mill to be a prime exemplar of the definition of economics that takes the pursuit of wealth as a starting point and offers a second definition that takes optimization as a starting point, thus connecting the approaches of Mill and Robbins through a common methodological thread. As Robbins’s definition of economics is standard in modern textbooks, the line that runs from Mill to Robbins to the dominant pedagogy of economics today is fairly clear. A reader who dislikes calling this “the mainstream,” should at least accept the methodological connection among Mill, Robbins, and modern economics and the philosophical interest in understanding their methodologies. And having accepted that, what label is placed on is largely a matter of personal preference.

the doctrine that all contingent scientific propositions are known only provisionally.² A deductive theoretical account requires a starting place—either propositions that are *infallibly* true (that is, beyond any possible doubt), implying *foundationalism*, or ones that are simply *indubitable* (that is, held beyond genuine doubt, at least for the nonce).³ Such propositions have a reasonable claim to be called first principles.

The Logical Positivists' attempt to articulate a foundationalist empiricism crashed on the rock of a naïve account of sense data. Karl Popper's antifoundationalist approach, embraced fallibilism, branding any proposition that is not falsifiable as not scientific. In practice, Popper recognized that, if we were required to provide an empirical test of every proposition, we would face an infinite regress (Popper 1959, p. 30). The propositions that I have labeled "indubitable," are the ones that cut off that regress. The need for such propositions, however, poses a puzzle: in what sense can a science that relies on indubitable propositions as a starting point be empirical and, therefore, scientific? There is a tension between foundationalism and fallibilism: indubitability appears to have necessary consequences, which undercuts fallibilism; while fallibilism threatens confidence in the de facto first principles that begin inquiry. This tension is evident mainstream economics, which implicitly asserts its secure foundations—with strong hints that these rest on a priori first principles—and, at the same time, continues to pursue an empiricist program.

I wish to examine the way in which self-consciously methodological analyses of economics have addressed the tension between foundationalism and fallibilism—the tension between a commitment to first principles and empiricism—through consideration of three case studies from different periods in the history of economics. The first two were influential attempts to systematize the methodology of economics and to articulate its principles, each framed as a mainstream (or orthodox) reaction to the heterodox critics of its day. The first is John Stuart Mill's essay, "On the Definition of Political Economy; and on the Method of Investigation Proper To It" (1874), which was originally published in 1836; the second is Lionel Robbins's *The Nature and Significance of Economic Science* (1935), which served the same function for neoclassical economics that Mill's essay did for classical economics. Finally, I will consider George Stigler and Gary Becker's methodological program as articulated in their article "De Gustibus Non Est Disputandum" (1977). Robbins's definition of economics has become canonical among modern economists; yet modern economics has not in practice embraced Robbins's skepticism about empirical economics; and, still, very little attention has been paid among contemporary economists to reconciling the implicit tension. Unlike the methodologies of Mill and Robbins, Stigler and Becker's methodology has not captured the mainstream—although it does have a significant group of adherents in the so-called Chicago School—but it is worth addressing because it is a rare, recent, self-conscious attempt to address the tension and reconcile infeasible first principles with a workable empiricism.

² A stronger form of fallibilism might hold that necessary truths (e.g., the necessary propositions of mathematics) are also known only provisionally.

³ We will regard the Peircian distinction between *infallibility*, meaning that a belief could not be wrong, and *indubitability*, meaning that a belief is so far beyond current doubt that holding it does not call for immediate justification, to be a technical distinction, whether or not it conforms with common usage.

2 Mill

Mill begins his essay with a distinction between the science and art of political economy that echoes Hume's famous is/ought dichotomy. He then observes that any science is defined by its fundamental principles, which often form the starting points of elementary treatises. Yet,

[i]f we open any book, even of mathematics or natural philosophy, it is impossible not to be struck with the mistiness of what we find represented as preliminary and fundamental notions, and the very insufficient manner in which the propositions which are palmed upon us as first principles seem to be made out, contrasted with the lucidity of the explanations and the conclusiveness of the proofs as soon as the writer enters upon the details of his subject. [Mill 1874, para. 4]

The paradox is resolved by noting “that what are called first principles, are, in truth, *last* principles.” The “chain of proof” on which the science depends is not suspended from the first principles. Rather “they are themselves the remotest link of the chain... the result of the last stage of generalization, or of the last and subtlest process of analysis...” (para. 4).

The importance of the logically first principles is in no way diminished for Mill by the fact that they are temporally and epistemically last principles. The articulation of first principles sets the bounds for a discipline (i.e., how do we define economics) and, vitally, establishes the methods through which it is to function. Since first principles are temporally last, any definition of economics presupposes a large body of material that is already regarded as belonging to economics. A definition, on Mill's view is not required to validate the inclusion of all of these results within the economics discipline, but it could be regarded as successful only if it somehow encompassed what intuitively constituted the heart of the ill-defined, but preexisting, category of the economic.

There is no need to trace Mill's thorough and subtle ruminations, his testing, rejecting, and reshaping of definitions. Rather, consider only his final preferred definition, which defines political economy by its domain or subject matter:

Political Economy... [is the] science which traces the laws of such of the phenomena of society as arise from the combined operations of mankind for the production of wealth, in so far as those phenomena are not modified by the pursuit of any other object. [Mill 1874, para. 39]

By *wealth* Mill understands “all objects useful or agreeable to mankind” that are scarce in the sense of not being, like air, goods that may be appropriated without labor in indefinite quantities” (para. 14). By *society* Mill understands “a union or aggregation of human beings for a common purpose or purposes” (para. 35). The defining first principles of economics, then, play out in society as an antagonism between the pursuit of wealth, on the one hand, and the aversion to labor and the desire for the “the present enjoyment of costly indulgence,” on the other (para. 38). The definition is meant, in part, to capture the heart of preexisting notions of what constitutes political economy; but it is, in part, stipulative, for Mill acknowledges that it sets a boundary that is relatively narrow, so that motives that are deemed noneconomic on this definition

may nonetheless inflect behavior that falls within the his defined scope of political economy.

The epistemic status of Mill's first principles is best understood in the context of his conception of science. Sciences for Mill are properly deductive systems.⁴ Geometry is the paradigm for all genuine sciences. All deductive systems begin with stipulated objects and rules governing their operation. Just as the "definition of the line is prefixed to Euclid's Elements," political economy "presuppose[s] an arbitrary definition of man, as a being who invariably does that by which he many obtain the greatest amount of necessities, conveniences, and luxuries, with the smallest quantity of labor and physical self-denial..." (para. 46). While not proposing a formal axiomatizations, Mill's conception of economics is as a closed axiomatic system, generating economic laws that can be applied to the economic world in the way that the results of geometry are applied to the physical world.

Geometry is an a priori science. Empirical sciences, including economics, are also a priori and accomplish their scientific work deductively. They are what Mill refers to as "a mixed method of induction and ratiocination," the inductive element referring to the status of the initial premises, which are not necessary, but contingent, truths (para. 45). The deductive conclusions of economics are empirical truths, contingent only on the truth of first principles that form their premises. Mill's view is not like Popper's: Mill harbors no doubts about the conclusions and even rejects the idea that testing conclusions belongs to science proper: "To verify the hypothesis itself *à posteriori*, that is to examine whether the facts of any actual case are in accordance with it is no part of the business of science at all, but of the *application* of science" (para. 45).

Mill regards the stipulation of man as a pursuer of wealth as an abstraction, and the conclusions of political economy just as true "in the abstract" as mathematics (para. 57). The stipulative definition of economic man is not the pure abstraction of mathematics, which posits that uninterpreted symbols, such as variables, are the objects of rule-defined operations; rather, it is the abstraction of isolation that selects and characterizes a single motive among many. The domain of economics is only a part of the human experience; the wealth motive is only one among many human motives. The abstraction is a barrier to Popperian testing of economic propositions; for Mill expects us never to find the expressions of the wealth motive isolated in the real world. Perhaps laboratory experiments permit such isolation of the animating forces of physics, but Mill simply assumes that no truly controlled experiments are possible in the human sciences (para. 51).⁵

The pure deductive consequences of economic first principles are fully present, even when they are masked by the countervailing effects of other human motives (paras. 47, 58–61, 75). For Mill, economic laws are exact in themselves, because they are of the same nature as the premises of geometry, whose precision is traceable to their abstractness (para. 46). Hausman (1992) refers to Mill holding economics to be "an inexact and separate science." Mill's view is that the concrete conclusions one draws

⁴ Mill (para. 46) actually refers to "abstract science," which essentially distinguishes in his view the positive core of political economy from the practical and normative elements that constitute the art of political economy.

⁵ Mill's (1843, ch. 8) famous canons of induction suggest that he is more sanguine about experimentation in the natural sciences.

from economic reasoning imprecisely characterize concrete social and economic reality. Yet, Mill (para. 58) asserts that “disturbing causes” (i.e., noneconomic causes) provide the “only... element of uncertainty” in reasoning about concrete economic outcomes. Mill writes, “in any tolerably advanced science [which for him includes economics] there is properly no such thing as an exception.... There are not a *law* and an *exception* to that law... There are two laws, each possibly acting [in every case]” (para. 75). When a law acts in conjunction with other laws, its own action can best be described as providing “a tendency to [a] result” (para. 75).⁶

Mill’s notion of a *tendency* may be explicated with an analogy to the parallelogram of forces in elementary mechanics. The effects of various forces are additive, with only the net effect being directly observed. We can understand Mill’s view as effectively thinking of net outcomes as weighted sums of the deductive consequences of various motivations:

$$\textit{Observed Outcome} = \varepsilon \textit{Economic Outcome} + \sum_j v_j \textit{Noneconomic Outcomes},$$

where weights ε and the v_j represent the relative strengths of the different motives in a particular set of circumstances. The tendency of an economic law is captured in the degree to which the economic outcome contributes to the overall outcome, so that a tendency may be strong or weak, dominant or overwhelmed by other factors.

While the component outcomes might well be certain, Mill expected the weights to be unstable across different circumstances and, at best, imperfectly predictable, relegating assessment of them to the realm of economic art (para. 66). The practical man, who would use economics to support policy or normative projects has to judge the relative weights of the effects of each sort of motive. Experience may force him to reassess the weights; it should never force him to reassess the component outcomes—at least where a mature scientific account of the implications of that motive exists, as Mill believed it did exist for economics. One might imagine that some systematic account might be given of the relationship between different circumstances and the weights that are to be placed on the effects of different motives; yet Mill never seems to entertain that possibility. As a result, given the deductive structure of economics, as well as, more generally, of the other the sciences that explain the different component outcomes, the empirical status of a science must be determined by the empirical status of its first principles. How do we know that our initial premises are true? Mill argues that we know them through direct acquaintance:

The desires of man, and the nature of the conduct to which they prompt him, are within the reach of our observation. We can also observe what are the objects which excite those desires with reasonable consideration of the differences, of which experience discloses to him the existence, between himself and other people. [para. 56]

⁶ Hausman’s characterization of Mill’s view of economics as “inexact” refers to the inevitable presence of other laws working with economic laws and in no way detracts from Mill’s claim that economic laws are exact *in themselves*.

Generally, Mill rejects induction as a scientific method applicable to economics; yet he characterizes science as proceeding fundamentally via “the method *à priori*”—a mixed inductive/deductive method. It is this direct acquaintance that constitutes the inductive element in Mill’s characterization of the method *à priori*, and is the only inductive element that he allows to be fundamental in economics.

Mill’s position bears comparison to Charles S. Peirce’s classification of philosophy—particularly, metaphysics—as an empirical science. Mathematics for Peirce was the study of the implications of hypothetical premises. In contrast, both natural and human sciences and philosophy involve empirical observation. “Philosophy is a *positive science*, in the sense of discovering what is really true; but it limits itself to so much truth as can be inferred from common experience” (Peirce 1931, para. 184). Drawing on a distinction that originated with Jeremy Bentham, Peirce distinguishes between *cœnoscopic* (“directly viewed”) and *idioscopic* (“specially viewed”) sciences (Peirce 1931, para. 184; see also paras. 183, 239–242; Bentham 1816, pp. 177–179; Bentham 1952, p. 85).⁷ For the most part, philosophy (especially metaphysics), which relies on commonly available empirical observation, is *cœnoscopic*; while the “special” natural sciences, which engage the collection of new facts, are *idioscopic*.⁸

Along similar lines, Mill identifies the basis for the method *à posteriori* as “not experience merely, but specific experience”; while he identifies the basis for the method *à priori*, “(what has commonly been meant) [by] reasoning from an assumed hypothesis, which is not a practice confined to mathematics, but is of the essence of all science which admits of general reasoning at all,” as general experience (paras. 45, 50). The method of economics and all the moral sciences is the method *à priori*—“the method *à posteriori*, or that of specific experience [being] altogether inefficacious... as a means of arriving at any considerable body of valuable truth” (para. 50). Economics is, then, an empirical science, but one that in the Bentham/Peirce terminology has a *cœnoscopic* basis. While Mill goes on to notice that what Bentham and Peirce would regard as *idioscopic* knowledge (supplied by his method *à posteriori*) is a useful aid “and even forms an indispensable supplement to” the method *à priori* in economics, he excludes it from *scientific* economics altogether; it is part of the art of economics.

Mill’s account of the method *à priori* is incompatible with fallibilism. How could one construct an empirical test of economic laws? He holds that the laws of economics and their deductive consequences are known with geometrical certainty. We might imagine that, if all the non-economic circumstances could be controlled or accounted for, then a failure of match the empirical data would be evidence against the economic laws themselves. But Mill rejects this position, as the non-economic circumstances are too numerous. This might appear to be simply a daunting, but nonetheless only practical, limitation. But in fact, Mill holds that the complicating circumstances are “infinitely numerous and various”; they cannot be exhausted (para. 54). It is literally

⁷ I know of no evidence that Mill ever used the terminology of *cœnoscopia* and *idioscopia*; but, having been born into Bentham’s circle and reared on his philosophy, he may well have been familiar with it.

⁸ Analytical mechanics for Peirce is a *cœnoscopic*; while chemistry (experimental) and astronomy (requiring special instrumentation and observational expertise) were *idioscopic*. *Cœnoscopic*, it should be noted, is not equivalent to *a priori*, necessary, or indubitable. *Cœnoscopic* conclusions are defeasible on the basis of evidence, although not on evidence grounded in experimental falsification or quantitative measurement. The details of Peirce’s view take us too far afield to be considered further here.

“impossible” on Mill’s view to take account of all the non-economic circumstances (para. 62; cf. para. 58). We may make our deductions better conform to the evidence by taking account of the most important factors, but any failure is to be attributed not to the economic law but to circumstances that, through omission or ignorance, we failed to incorporate into the deduction. As a result, Mill argues that the method *à posteriori* is “not a means of discovery truth, but of verifying it, and reducing it to the lowest point of uncertainty (para. 62). He does not accept any mechanism by which empirical evidence can be turned against economic first principles. He rejects the essence of fallibilism. Any concrete conclusion based on a deduction from economic principles and non-economic circumstances might turn out to be wrong, in the sense of not matching the empirical evidence very closely, but the economic laws in themselves cannot be wrong.

Mill provides a good illustration of the tension between fallibilism and foundationalism. Mill’s first principles are supposed to be empirical and not necessary truths, but for economics to be an empirical subject at all, they have to be beyond genuine doubt, since they provide the only empirical element in an otherwise deductive system. The certainty that Mill claims for the results of scientific economics are purchased with deep uncertainty about the significance of those results—in particular, how important economic outcomes are relative to countervailing noneconomic outcomes. And the modern economist or philosopher surely would regard Mill’s economics as empirical only in a Pickwickian sense, as Mill does not leave open the possibility that anything could count as evidence against its first principles.

Mill also narrows the scope of economics greatly in his focus on the desire for wealth as its defining criterion. Economics excludes all other motives. Yet, even Mill himself feels the tension, arguing that “practical utility” requires the inclusion of Malthus’s principle of population within the scope of economics, even though the first premise of principle of population is sexual passion and not the desire for wealth (para. 38). The principle of population is, of course, one of the linchpins of the Ricardian system of which Mill was a mature expositor.

3 Robbins

Mill’s methodological strategy involved taking political economy as having produced a body of successful analyses and then seeking to codify its scientific content. He used his distinction between the science and the art of political economy to isolate the potentially scientific results. Lionel Robbins in the 1930s pursued a similar strategy, beginning with a body of successful analyses and using the positive/normative distinction to isolate the positive elements. Coming a century behind Mill, Robbins’s target body of successful analyses only partly overlaps with Mill’s. The rise of systematic, positive economic analysis in the meantime had driven the “political” out of political economy and established “economics” as the proper name of the discipline. This reflected, in part, a major upheaval in economics. Mill was the last prominent classical economist. There were, as there always are, precursors; nonetheless, according to the conventional story, around 1870, simultaneously and independently, William Stanley Jevons in England, Leon Walras in France, and Carl Menger in Austria extended the

“marginal” (i.e., constrained optimization analysis) implicit in Ricardo’s theory of profit and rent and explicit in Antoine Augustin Cournot’s (1838 [1927]) theory of profit maximization under production constraints—to found modern demand theory. Smith, Ricardo, and the classical economists had taken the inverse relationship of demand to price as implicit in an analysis that focused on production. Cournot explicitly posited a negatively sloped demand curve, but took it as a primitive, subject to no deeper analysis. The *marginalists* or *neoclassicals* derived demand from the maximum satisfaction of preferences (expressed originally as utility maximization) subject to budget constraints, deriving not only the demand for goods, but also the supply of labor (i.e., demand for leisure) and other factors of production. Walras built a vision of the economy as a set of individual utility and profit maximizers whose decisions were systematically coordinated through market prices.

Whereas Mill saw common textbook definitions of political economy as “misty,” Robbins sees them as confused and as genuinely damaging the discipline, resulting in “gaps in the unity of theory... [and] insufficiencies in its explanatory principles” (Robbins 1935, p. 3). No definition, he maintains, had done more damage than that of Mill, with its focus on wealth and society—a definition that was widely echoed in treatises and textbooks, even in Robbins’s own time. Robbins concedes that Mill’s definition does capture common usage of the term “economic,” but he suggests that common usage should be rejected, as it fails to identify the essential character of successful economic analyses or the common basis of their success. Robbins (1935, pp. 24–25) locates that success in the *method* of economic analysis and argues that an *analytic* definition of economics should replace *classificatory* definitions (such as Mill’s). Again, we need not dwell on the details of Robbins’s argument for his definition, but can simply place it into evidence:

Economics is the science which studies the relationship between ends and scarce means which have alternative uses. [Robbins 1935, p. 16]

The essential point for Robbins is that human ends are various and without certain bounds, while the resources available for fulfilling those ends are limited, so that people must evaluate their ends on subjective scales and choose among those that would be feasible given their resources in order to achieve the best feasible outcome by their own lights. The essence of economics is choice. Were there no variety of ends, then there would be no choosing among them. Could any end be satisfied without a resource limit, then no choice would be necessary.

Robbins’s definition of economics is the dominant definition offered in modern textbooks and, in fact, guides the mainstream economist’s understanding of the nature of economics (Backhouse and Medema 2009a, b). Robbins takes his definition as inextricably linked to a series of first principles or postulates:

The main postulate of the theory of value is the fact that individuals can arrange their preferences in an order, and in fact do so. The main postulate of the theory of production is the fact that there are more than one factor of production. The main postulate of the theory of dynamics is the fact that we are not certain regarding future scarcities. [pp. 78–79]

We need emphasize only the principle that is explicit in Robbins's definition of economics, but only hinted at in the list: most of the things that we value are scarce. He sums it up succinctly:

Life is short. Nature is niggardly. Our fellows have other objectives. Yet we can use our lives for different things, our materials and the services of others for achieving different objectives." [p. 13]

Robbins shares Mill's conception of the nature of science as an axiomatic system, arguing that "the propositions of economics, like all scientific theory, are obviously deductions from a series of postulates" (i.e., the first principles already noted) (Robbins 1935, p. 78). For both, the laws of economics are exact, even if they are not quantitative (p. 66).⁹

Despite sharing a conception of the nature of science, Robbins and Mill differ sharply over their conception of economics. For Robbins, unlike Mill, economics is not characterized by its substantive domain (the study of wealth), rather by its methodology (constrained optimization), which is applicable to a much wider range of phenomena than contemplated by Mill. The first principles of economics, then, on Robbins's account are incapable of producing any substantive results without subsidiary postulates that provide a subject on which to ply the methods of economics—postulates about the content of preferences and the specific nature of the constraints. Robbins's conception of economics has some important consequences.

First, economics is not a closed deductive system or a specific model (to use a term that was not yet in vogue in the 1930s); rather it provides a framework of an approach to a range of substantive problems.

Second, economics is not limited to matters having to do with wealth or to any of the traditional areas that ordinary language considers "economic"; rather economics is everywhere that people make constrained choices. This is the basis for the so-called "economics imperialism," in which the methods and skills of economists—and often the economists themselves—colonize fields that have traditionally been distinct, such a sociology, anthropology, and political science. After Robbins, there has been a considerable widening of the scope of problems addressed as economic. If the modal economist, who is in fact steeped in Robbins's conception of economics, shows little interest in, or hostility to, behavioral economics or neuroeconomics, it is because, on the Robbinsian view, the questions that they raise are not relevant to doing economics. If the behavioral psychologist offers the economist a more realistic utility function, the response will be "fine—let's maximize it."

Third, there has also been a narrowing. Robbins rejects not only the social as a defining characteristic of economics, but also argues for a kind of modularity. Economics presupposes preferences and constraints; yet it does not, on Robbins's view, have any reason to concern itself with what lies behind those preferences—their psychological or sociological basis. Those preferences and constraints may well have a deeper basis, and it is legitimate to study it, but the optimization problems that define economics require only that we know what the preferences and constraints are, not that

⁹ Robbins (1935, p. 66) writes that a law of economics can be exact in the sense that we may "state it in such a way as to make it relate to formal relations which are capable of being *conceived* exactly."

we know their origins or nature. Economics, for Robbins, is an empirical discipline; and, just as it did for Mill, the empirical content enters as premises—subsidiary for Robbins rather than as a first principle—and there is no feedback from the deductive consequences of those premises or their interaction with the world on the defining first principles themselves. Robbins rejects the notion that economics is inductive or that experiments can be informative in economics, just as decisively as did Mill (1874, pp. 74). The modularity (or autonomy) of economics is the isolation from other disciplines that apparently treat elements central to economic explanation and the absence of feedback from facts in the world to the first principles of economic explanation. An economic explanation may misfire if its subsidiary premises are ill chosen, but such misfires can never place its methodological first principles in doubt. It is this modularity that makes it reasonable to identify Robbins's postulates as "first principles." They are *first*, not in the sense, that we are at the rock bottom of inquiry, but *first* in the sense that they delimit the boundary between economics and other fields of inquiry that might be thought to be more fundamental.

Fourth, whereas much of the empirical content of economics for Robbins is introduced by substantive subsidiary postulates, the empirical rationale of his first principles is essentially cœnoscopic, just as it is for Mill. Robbins argues that the reality of his postulates is beyond dispute, once we understand what they assert:

We do not need controlled experiments to establish their validity: they are so much the stuff of our everyday experience that they have only to be stated to be recognized as obvious. [Robbins 1935, p. 79; see also pp. 80–81, 104–105]

Later he claims that our knowledge of these first principles enjoys an epistemic advantage over our knowledge of first principles in natural sciences:

In Economics... the ultimate constituents of our fundamental generalizations are known to us by immediate acquaintance. In natural science they are known only inferentially. There is much less reason to doubt the counterpart in reality of the assumption of individual preferences than that of the assumption of the electron. It is true that we deduce much from definitions. But it is not true that the definitions are arbitrary. [p. 105]

The tension between fallibilism and foundationalism is as acute for Robbins as it was for Mill. The first principles are not simply indubitable; there is no mechanism through which evidence can be brought to bear against them; so they are practically infallible—despite being supposed to be empirical.

In Robbins's hands, however, economics is in some important respects more narrow than in Mill's, even though constrained optimization applies to a wide range of situations unrelated to wealth. The nature of the cœnoscopic empirical support that Robbins claims for his account of preferences leads him to insist on their scientific inscrutability. His first principles refer fundamentally to individuals, as only individuals, he maintains, have preferences or optimize. Those preferences are fundamentally subjective, ruling out interpersonal comparisons of the intensity of human desires: Robbins roundly rejects the psychological hedonism entertained by Francis Y. Edgeworth, among others (pp. 84–85). (Edgeworth even suggested that pleasure might be measured with hedonimeter (Edgeworth 1881, Appendix III; see also Colander 2007).)

For Robbins, not only was *de gustibus non est disputandum*, there was no measuring tastes either. Unlike Mill, Robbins must rule out a range of issues that presuppose measurable preferences or some basis for interpersonal comparisons of utility, such as the positive grounding of the normative Benthamite utilitarianism.

Like Mill, Robbins is deeply skeptical about quantitative economics. But whereas Mill's skepticism arose mainly from the complexity of the social world and the presumed inability of the economist to measure the distinct contributions of economic and noneconomic causes of concrete behavior, Robbins skepticism arises from the fundamental subjectivity of preferences, which, of course, implies a lack of observability as well. Robbins notes the spatial and temporal relativity of demand estimates: "what reason is there to suppose that ["the wretched" Blank, who estimated the elasticity of demand for herring in Britain in 1907–08 at 1.3] was unearthing a constant law?" (p. 108; also p. 109). Of course, if universal, non-temporally, non-spatially relative constant laws were the *sine qua non* of science, then huge swaths of the natural sciences would be similarly dismissed.

In much the same spirit, Robbins points to the impossibility of finding usable generalizations in the quantitative data of economic history. Economic history for Robbins would be limited to "the study of substantial instances in which [the relationships between ends and means] show themselves through time" (p. 38). Robbins means to rule out of the field many of the traditional concerns of economic historians, such as quantitative assessments of income distribution, average incomes, absolute (as opposed to relative) price levels, or interest rates.

Such quantified aggregates often play an important role, not only in economic history, but in some kinds of economic theory. Similarly, institutional information matters to many economic explanations; but, for Robbins, any explanations of them that could not be framed as a problem in constrained optimization would relegate them to the role of noneconomic auxiliary premises. For example, Robbins conceives of monetary economics as taking certain monetary institutions as given. The *economic* explanation of monetary phenomena would refer only to those features that involve preferences or scarcities of individuals (pp. 41–42). The quantity theory of money, which is Robbins's day was regarded as one of the most successful economic theories, relied heavily on such institutional premises and expressed its main results in terms of quantified aggregates. Robbins, who cites Walras principally for his commitment to a conception of economics as an optimizing discipline, fails to note that Walras's account of money is quantity-theoretic and makes no use of the marginal framework that governs the supply and demand of all other commodities in his system. And Walras's account of money employs the concept of the general price level—an aggregate that Robbins explicitly dismisses as lacking a sound economic foundation (pp. 59–63). The problem is two-fold: quantification in monetary theory requires aggregates, for which there is no good optimizing account; and aggregates, even considered abstractly, are superindividual, the behavior of which does not simply sum up the optimizing choices of individuals.

What was true of the quantity theory of money in 1935 is similarly true of macroeconomic theories more generally today. Macroeconomics as a distinct field within economics was just getting started at the time that Robbins composed his *Essay*, though particular theories, such as the quantity theory, which are today regarded as

macroeconomic, were in fact ancient. Reading macroeconomics out of economics, as Robbins implicitly does, sits awkwardly with his announced strategy of trying to codify the principles underlying the accepted generalizations of economics; for the quantity theory was among the most venerable economic theories of his time. Of course, Robbins is free to stipulate a definition of the scope of economics; but he can hardly argue for its acceptance based on the claim that it captures all, and only, those results that economists widely accept as economic. Robbins provides a sharp and consistent definition of economics, but at a cost of excluding from the field areas that are quite generally regarded as part of the discipline.

4 Stigler and Becker

Despite ruling aggregative analysis out of economics, Robbins's definition of economics has become standard among economists. If anything, mainstream economics has tried to beat macroeconomics into a form suited to Robbins's definition—the so-called microfoundational approach to macroeconomics—rather than revising or replacing that definition. To my mind, the effort has not been successful, though this is probably a minority view (see Hoover 1995, 2009, 2015).

In contrast, mainstream economics has refused to follow Robbins (or Mill) in his resistance to quantification and inductive methods. Perhaps the most important reason for the difference is the development of *econometrics*, beginning at just about the same time as Robbins was writing. Robbins argued, taking a position that anticipated John Maynard Keynes's (1939) famous dismissal of econometrics in his review of Tinbergen's volume on business cycles, that the application of the theory of probability to statistics required homogeneous causes that are not to be found in economics (Robbins 1935, pp. 112, 123–125). Statistics were frequently conceived at this time in Fisher's (1935) experimental framework; and, to Robbins, economics was not fertile ground for experimentation. The publication of Haavelmo's "Probability Approach in Econometrics" (1944), however, provided the answer to Robbins and Keynes that quantitative economists sought (see Morgan 1990, ch. 8; Hoover 2014). In Haavelmo's approach, modeling that employed a partition between deterministic causes and residual error allowed the economist to mimic experimental controls and to render the residual errors into a form describable through a well-defined probability distribution. Essentially, Haavelmo saw econometric modeling as a kind of pseudo-experimental design (Hoover and Juselius 2015).

Somewhat later, economists challenged even Robbins's and Mill's presumption that economics was unsuitable ground for controlled experiments. Experimental economics is now an established field. A recent paper even shows that the rate of replicability of economics experiments, although less than what would be presumed on statistical grounds alone, is higher than that in psychology (Camerer et al. 2016).¹⁰

Contemporary economics is *pace* Robbins and Mill favorable to empirical research and to the feedback from empirical results to economic theory. However, the Robbin-

¹⁰ The existence of a flourishing experimental economics, of course, does not establish that experimental economics is adequate to the full scope of economics. But the same is true for many natural sciences for which the scope of experimentation is limited (e.g., geology, ecology, climatology, cosmology).

sian first principles themselves are not the target for any revision within mainstream economics, but are held immutable as something like a Lakatosian hard core. Recalcitrant evidence may result in a revision of the details of the structure of constraints hypothesized in a problem, but is not allowed to weaken the commitment to the framework of constrained optimization.

Although mainstream economists appear to be little troubled by the commitment to Robbins's first principles come what may, they have become a focus of criticism from outside of economics and from heterodox economists within economics. This is particularly true when microeconomists have assumed, contrary to Robbins's radical subjectivism, that utility functions take specific, concrete forms in order to derive testable results. The concreteness of the assumptions about preferences opens them to criticisms from psychology and behavioral economics, questioning their descriptive accuracy. One response, as suggested previously, is to accept empirical evidence as bearing on the specific formulation of preferences but to preserve the methodology of constrained optimization. A second response is to argue, much as Mill did, that the economic model captures an aspect of human behavior and that there are countervailing non-economic factors, which are sometimes relatively small. Such a response does not sit easily with Robbins's conception of economics as embracing human choice generally.

Another response that is illuminating from point of view of first principles is Stigler and Becker's (1977) reformulation of the empirical program of microeconomics. Stigler and Becker's approach, while influential, especially among adherents to the "Chicago school," is by no means canonical. The discussion here is not an endorsement of the approach. Its value in the context of a discussion of the role of first principles arises mainly because, unlike much recent empirical economics that simply ignores the tension in Robbins's approach, Stigler and Becker aim to reconcile Robbins's conception of what economics is with the empiricism that Robbins rejects—that is, they aim to reconcile foundationalism with fallibilism. As observed previously, Robbins adhered to the maxim *de gustibus non est disputandum*, interpreted commonly to mean that tastes are heterogeneous and beyond rational debate. They are *first data*, if not first principles. Stigler and Becker also embrace the maxim, but under a different interpretation:

tastes neither change capriciously nor differ importantly between people.... [o]ne does not argue over tastes for the same reason that one does not argue over the Rocky Mountains—both are there, will be there next year, too, and are the same to all men. [Stigler and Becker 1977, p. 76]

As a radical subjectivist about preferences, Robbins assumed that tastes differed among individuals and changed through time, but were not observable or comparable in any case. The subjectivity of preferences implies that demand theory could say only very general things about the nature and implications of preferences, although subjectivity alone does not imply the nature of those restrictions. Robbins (1935, pp. 91–92) himself argued for transitivity as a general requirement of rationality in choice, while modern demand theory has codified transitivity, as well as some other general restrictions on preferences and shown, for instance, that they imply that demand curves slope downward after adjusting for induced changes in real incomes. Such general

features do not support quantification—especially if tastes are not stable—which is an important basis for Robbins’s skepticism of empirical economics.

Stigler and Becker follow Robbins as far as rejecting the direct observability of preferences. If one continues to accept Robbins’s first principle of constrained optimization, then the premise that tastes are homogeneous among people and over time places the explanatory burden on the constraints and not the preferences: the job of the economist is “to search for differences in prices and incomes to explain any differences or changes in behavior” (Stigler and Becker 1977, p. 76).¹¹ Stigler and Becker’s premise also supports quantitative, empirical conclusions, as these are no longer undermined by the worry that preferences are shifting constantly. They may have as little faith as Robbins in the wretched Blank’s estimate for the elasticity of demand for herring, but their doubts proceed from a lack of faith in the details of the constraints in his empirical model, which *may* be remediable, and not from the assumption that the preference field is constantly shifting in unpredictable ways.

A key move in Stigler and Becker’s approach is to widen the scope of economics in a manner than is compatible with Robbins’s account. Rather than considering a market in which *goods* are supplied by two types of agents, one type driven by a profit motive acting as supplier and another type guided by preferences acting as demander, demand is defined over *commodities*, which are a joint product of market-supplied goods and personal inputs, such as skills, training, and time (Stigler and Becker 1977, p. 78). The point of this formulation is that a wide variety of heterogeneous goods may serve as inputs to commodities. For example, a commodity such as music appreciation is a joint product of the actual musical good (e.g., a compact disk, a music download, or a live performance) and the circumstances in which the good is consumed (including such things as musical education, past experience with music, and available time). Which music is chosen and how it is listened to (i.e., transformed into a the music-appreciation commodity) depends on scarcities (captured by prices and shadow prices), budgets, and other opportunities—that is, by precisely the kinds of things that Robbins contemplates as elements of constrained optimization, though without any implicit assumption that commodities are all mediated through markets.

Stigler and Becker illustrate the potential of their approach by constructing several models that account for observed behaviors and make empirical predictions for phenomena for which others have suggested that changing preferences provided the only workable explanations. Instead, Stigler and Becker offer explanations in terms of prices and incomes. For example, they note that many accounts of addiction—either beneficial addictions (e.g., to music) or harmful addictions (e.g., to heroin)—have often been explained through the idea that tastes adapt to the experience of consuming the addictive good (Stigler and Becker 1977, pp. 77–81). Such an explanation strikes them as thin, since the tastes themselves are unobservable, so that the explanation makes only weak predictions. Not relying on the observability of preferences, but

¹¹ Robbins does not use the terminology “constrained optimization” and a referee reads references to it as implying a commitment to *formal* optimization models. Robbins does refer to choosing among our various ends to get the best result, which may perfectly legitimately be referred to as optimization, given scarcity, perfectly legitimately referred to as a constraint. Thus, while it is convenient to use this language, as it points to a family resemblance and a genuine continuity between Robbins and Stigler and Becker, I agree that Robbins was not an advocate of formal models or their quantification.

only on their constancy, Stigler and Becker's alternative explanation runs in terms of the way incentives and constraints develop with the passage of time and the experience of consuming the addictive good. The details of their model are not pertinent to the present discussion, but suffice it to say that their model makes predictions that are in principle quantifiable, and indeed Becker et al. (1994) have tested a model of rational addiction to cigarettes against the alternative hypothesis of changing tastes and estimated demand parameters that are usable, for example, in formulating a tax-based anti-smoking policy.

Although there is a deep family resemblance between Stigler and Becker's and Robbins's accounts of economics, there are also some critical differences. Mill had argued that the laws of political economy operated constantly, although they might be masked by non-economic factors. Empirically, the laws display themselves only as *tendencies*, even though, as previously noted, they are always fully operative (Mill 1874, para. 75). Similarly, Robbins thought that economic conclusions must all be couched with a *ceteris paribus* caveat. Stigler and Becker explicitly argue that, at least with respect to changing tastes, the *ceteris paribus* clause should be omitted (p. 76). This is what allows them to reconcile a set of first principles that are quite close to those of Robbins with practicable quantification and empirical testing. If actual predictions fail, then Stigler and Becker take their specification of the constraints to have been falsified.

There are two levels of empiricism in Stigler and Becker's approach. The first level is displayed in concrete models that take preferences as constant but also adopt a framing of the problem in terms of the ontology of commodities over which the preferences are defined. Preferences are like mountains: not only are they *there*; they are there, not as a matter of methodological presumption *à la* Robbins, but as a matter of empirical fact.¹² What is more, like mountains, they have some particular character that we have to learn about based on evidence. Conditional on the facts about preferences being given, hypothesis testing of the sort taught in econometrics courses and practiced by empirical economists provides a sound method of empirical evaluation. Econometric evaluation of models can be thought of as an idioscopic investigation within an ontological framework (i.e., supposing the existence of constant preferences and of the categorization of the commodities over which those preferences range). The target of econometric tests, however, is not the maintained ontological framework but the concrete details of the hypothesis, typically conceived of today as the modeled constraints.

The ontological framework does not constitute an infallible set of first principles, but defeasible commitments that are addressed at a distinct, second level of empirical assessment. Stigler and Becker's premise of the constancy of preferences is empirical; it "does not admit of direct proof because it is an assertion about the world, not a proposition in logic" (Stigler and Becker 1977, p. 76). Both Mill and Robbins offered direct acquaintance as the empirical ground for their first principles. Although empirical, such a ground is not empirically criticizable: you either see it or you don't. Stigler and Becker offer what can be regarded as an equally *cœnoscopic* basis for the constancy of preferences, although the standard is pragmatic success rather than direct apprehension: we know, they claim, that preferences are more or less constant owing to the

¹² It is this rather than the fact that Stigler and Becker favor formal optimization models that marks the real substantive difference between Stigler and Becker's and Robbins's approaches to economics.

empirical success of the models that presumes them to be constant: “Ultimately, . . . the only persuasive method of supporting the assumption . . .” is to “offer samples of phenomena . . . usefully explained on the assumption of stable, well-behaved preference functions” (Stigler and Becker 1977, p. 77). They go on to claim the “support of all of the existing corpus of successful economic theory” Their assumption is not subject to a straightforward Popperian refutation in a structured experimental or observational test; yet evidence can support or not support it.

Stigler and Becker’s evidence is cœnoscopic, that is, truth that is inferred from common experience, although, in this case, it is the common experience of the economists rather than of the economic agents. The truth that is supported by the economists’ experience is not simply that there are constant preferences, but that there are preferences that are constant with respect to particular ways of categorizing goods into commodities. So, for example, Stigler and Becker claim success for their economic explanation of addiction, but do not believe that they have an economic explanation for why some people prefer Mozart and others alcohol (p. 89). For the purposes of their model, Mozart and alcohol would be categorized as the same kind of commodity and the pragmatic success of a framework within this ontology is the empirical evidence for the framework. It is not to be maintained a priori or come-what-may; rather to the degree that it proves successful. Relative to econometric hypothesis testing, it is an indubitable first principle. But it is not infallible, and we can have empirical grounds for abandoning or modifying it.

Becker and Stigler’s definition of economics is broader than Mill’s and narrower than Robbins’s. Implicitly, the domain of economics is the set of explanations of human behavior in which Robbins’s first principles plus the principle of constancy of preferences works. There could be different degrees of working (e.g., qualitative or quantitative), and the standard of working could be relativized to particular goals (for example, to those of businesses or policymakers).¹³ Still, the standards are empirical, and the indubitable is not mistaken for the infallible, so that the account is not foundationalist. Empirical support for models in their approach arises out of pragmatically successful applications. Failures of models to succeed pragmatically do not suggest rejection but a narrowing of the domain of applicability. Because that domain may in fact be empty, the failure to find a domain of successful application would count as strong, yet fallible, evidence against the apparent first principles of the model. Thus, the approach is not foundationalist.

5 Where empiricism lives

It has been reasonable since the 1820s to describe economics as dominated by a mainstream view—for half a century by classical political economy and since the 1870s by neoclassical economics. The most influential articulations of the methodologies of these mainstream views, due to Mill and to Robbins, share key features and pose difficult problems for a science that both privileges its deductive structure and claims

¹³ Stigler and Becker’s approach here is similar to the one that Hoover (2006, p. 94) attributes to John Maynard Keynes: macroeconomic theories that guide policy are to be judged empirically successful according to the success of those policies in the eyes of the policymakers.

to be empirical. If a workable empiricism requires a commitment to a thorough-going fallibilism and a rejection of foundationalism, then economics is at best problematically empirical. Any deductive science requires starting points for its deductions—we may call these “first principles”—but somehow these principles have to be potentially empirically defeasible if the empiricism is not to collapse into a pure apriorism and foundationalism. Both Mill and Robbins seek to start their economic deductions with empirical facts that are known cœnoscopically—that is, from direct acquaintance with generally available experience. The difficulty for empiricism is that that they rule out any feedback from idioscopic experience—that is, from the confrontation of the empirical implications of economic science with actual outcomes—or any other means by which experience might force us to reconsider their first principles. That hardly seems an adequate empiricism.

Mill and Robbins sought to make the tension between fallibilism and foundationalism bearable by severely limiting the scope and precision of empirical deductions in economics—resisting quantification and experimentation as practices of scientific (as opposed to practical) economics. Recent mainstream economics refuses to give up empiricism by implicitly adopting constrained optimization of individual preferences as a foundational first principle. It has the status of a Lakatosian hardcore—never to be abandoned and never to be confronted with empirical evidence. For mainstream economics, empiricism lives elsewhere. Much of the criticism of economics—from heterodox economists, as well as from other fields—focuses on the apparent arbitrariness of this hardcore commitment and on the supposed thinness or, even, falseness of its characterization of human behavior.

Stigler and Becker’s “*De Gustibus*” paper, while it is famous within economics, has not been generally adopted as mainstream. Nevertheless, it aims to offer economists the possibility of maintaining a modified form of Robbins’s essential methodology and even the cœnoscopic empirical basis of its first principles, while at the same time, allowing those principles to be empirically defeasible. Whereas for Robbins, the first principles were a priori, logical, and methodological, for Stigler and Becker, they are empirical and factual. They include not only constrained optimization (per Robbins), but also the constancy of preferences and the categorization of goods into the commodities over which preferences are defined. And such principles are empirically defeasible. Empirical evidence, however, appears not in the form of testing (confirmation or refutation), but rather in the form the experience of the pragmatic utility of those principles in crafting empirical economic explanations.

References

- Backhouse, R. E., & Medema, S. G. (2009a). Defining economics: The long road to acceptance of the Robbins definition. *Journal of the History of Economic Thought*, 31(4), 485–499.
- Backhouse, R. E., & Medema, S. G. (2009b). Retrospectives: On the definition of economics. *Journal of Economic Perspectives*, 23(1), 221–233.
- Becker, G. S., Grossman, M., & Murphy, K. M. (1994). An empirical analysis of cigarette addiction. *American Economic Review*, 84(3), 396–418.
- Bentham, J. (1816). *Chrestomatia*. London: Payne and Foss; and R. Hunter.

- Bentham, Jeremy (1952) *Jeremy Bentham's Economic Writings: Critical Edition Based on his Printed Works and Unprinted Manuscripts*, vol. I. W. Stark, editor. London: George Allen & Unwin.
- Camerer, C. F., et al. (2016). Evaluating replicability of laboratory experiments in economics. *Science*, 351(6280), 1433–1436.
- Colander, D. (2007). Edgeworth's hedonimeter and the quest to measure utility: Retrospectives. *Journal of Economic Perspectives*, 21(2), 215–225.
- Cournot, Antoine Augustin. (1838[1927]) *Researches into the Mathematical Principles of the Theory of Wealth*, Nathaniel T. Bacon, translator. New York: Macmillan, 1927.
- Edgeworth, F. Y. (1881). *Mathematical psychics*. London: Kegan Paul.
- Fisher, R. A. (1935). *The design of experiments*. Edinburgh: Oliver and Boyd.
- Haavelmo, T. (1944). The probability approach in econometrics. *Econometrica*, 12(Supplement), iii–115.
- Hausman, D. M. (1992). *The inexact and separate science of economics*. Cambridge: Cambridge University Press.
- Hoover, K. D. (1995) "Is macroeconomics for real?" *The Monist* 78(3), 235–257; reprinted in Uskali Mäki, editor. *The Economic World View*. Cambridge: Cambridge University Press, 2001.
- Hoover, K. D. (2006). Ch. 5. Dr. Keynes: Economic theory in a diagnostic science. In R. Backhouse & B. Bateman (Eds.), *Cambridge companion to Keynes*. Cambridge: Cambridge University Press.
- Hoover, K. D. (2009). Ch. 14, Microfoundations and the ontology of macroeconomics. In H. Kincaid & D. Ross (Eds.), *Oxford handbook of the philosophy of economic science* (pp. 386–409). Oxford: Oxford University Press.
- Hoover, K. D. (2014). On the reception of Haavelmo's econometric thought. *Journal of the History of Economic Thought*, 36(1), 45–65.
- Hoover, K. D. (2015). Reductionism in economics: Intentionality and eschatological justification in the microfoundations of macroeconomics. *Philosophy of Science*, 82(4), 689–711.
- Hoover, K. D., & Juselius, K. (2015). Trygve Haavelmo's experimental methodology and scenario analysis in a cointegrated vector autoregression. *Econometric Theory*, 31(2), 249–274.
- Keynes, J. N. (1891). *The scope and method of political economy*. London: Macmillan.
- Keynes, J. M. (1939). Official papers: The statistical testing of business-cycle theories. *Economic Journal*, 44(195), 558–569.
- Mill, J. S. (1843). *A system of logic, ratiocinative and inductive: Being a connected view of the principles of evidence and the methods of scientific investigation* (Vol. 1). London: John W. Parker.
- Mill, J. S. (1874). On the definition of political economy; and on the Method of investigation proper to it. In *Essays on some unsettled questions of political economy* (2nd ed.). London: Longmans, Green, Reader, and Dyer.
- Morgan, M. S. (1990). *The history of econometric ideas*. Cambridge: Cambridge University Press.
- Peirce, C. S. (1931). *The collected papers of Charles Sanders Peirce* (Vol. 1). Cambridge, MA: Harvard University Press.
- Popper, K. R. (1959). *The logic of scientific discovery*. London: Hutchison.
- Ricardo, D. (1817). *Principles of political economy and taxation*. London: George Bell & Sons.
- Robbins, L. (1935). *The nature and significance of economic science* (2nd ed.). London: Macmillan.
- Stigler, G. J., & Becker, G. S. (1977). De Gustibus Non Est Disputandum. *American Economic Review*, 67(2), 76–90.