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## 8 Applied policy, welfare economics, and Mill's half-truths

*David Colander*

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### 8.1 INTRODUCTION

The argument in this chapter is a simple one. It is that sometime around the 1930s the economics profession's use of models in thinking about economic policy changed. The result has been a tendency to draw unwarranted policy implications from models and theory, such as occurred in the recent financial crisis. The chapter argues that to prevent such misuse of models from occurring, the economics profession needs to return to the earlier methodological approach, which recognized the complexity of the economy and the relative simplicity of our formal models.

Up until the 1930s what might be called the Classical method predominated in applying models to policy.<sup>1</sup> This method assumed that the economy was too complicated for formal modeling, and that any formal model would have to be seen as providing at best what John Stuart Mill called half-truths (Mill, 1838 [1950]). These half-truths from models would have to be integrated into a much broader implicit theory before they could be applied to real-world policy. Because this broader implicit theory was so complicated, it was accepted that economists would focus only on the economic portion of that broader theory, leaving it to other social scientists, or to economists who were operating outside the science of economics, to add the other elements necessary to draw policy results from economic models. This meant that for Classical economists, welfare economics was not, and could not be, a stand-alone field. Only when these other elements were added could one arrive at policy conclusions.

Classical economists who specialized in methodology recognized that economists would have a tendency to justify their policy prescriptions by claiming the imprimatur of economic science. To help insure against that, Classical economic methodology maintained a strict separation between the science of economics and economic policy analysis. Science was concerned with understanding for the sake of understanding, and was not concerned with policy. This meant that if there was any welfare economics which gave prescriptions for policy, it was not part of the science of economics.

The Classical method reflected a skepticism of models and theory, and of what economics could contribute to policy. As an advocate of this Classical method, Lionel Robbins stated: ‘What precision economists can claim at this stage is largely a sham precision. In the present state of knowledge, the man who can claim for economic science much exactitude is a quack’ (Robbins, 1927, p. 176).

Put in modern context, Classical economists saw the economy as a highly complex and interrelated system that was impossible to model formally. This did not mean that they did not use models; it simply meant that they saw a model’s results being blended together with philosophical views, feelings, sensibilities and institutional knowledge to arrive at a policy conclusion. For Classical economists applied policy was an art, not a science.

We can see this separation of policy from models early on in Classical methodological writing. For example, Nassau Senior, the earliest Classical economist who took a strong interest in methodology, writes:

[An economist’s] conclusions, whatever be their generality and their truth, do not authorize him in adding a single syllable of advice. That privilege belongs to the writer or statesman who has considered all the causes which may promote or impede the general welfare of those whom he addresses, not to the theorist who has considered only one, though among the most important of those causes. The business of a Political Economist is neither to recommend nor to dissuade, but to state general principles, which it is fatal to neglect, but neither advisable, nor perhaps practicable, to use as the sole, or even the principle [*sic*], guides in the actual conduct of affairs. (Senior, 1836 [1951], pp. 2–3)

For Senior, and for most early Classical economists concerned with methodology, the economic science of the time was a branch of logic. In the pure science of economics at the time one did theory, which meant that one developed theorems from almost self-evident principles. But, as Senior makes clear, economic theory was not meant to guide policy directly. To move from the theorems developed in the science of economics to the precepts of policy-relevant economics, Classical economists believed that one had to rely on commonsense judgment and institutional knowledge, and that discussing policy involved different skills than did doing economic theory.

This theory–policy divide can also be found in J.N. Keynes’s famous summary of economists’ methodology at the turn of the nineteenth century (J.N. Keynes, 1891). Like Senior, J.N. Keynes saw the pure science of economics, which he called positive economics, as a relatively narrow branch of economics, which needed to be strictly separated from the applied policy branch – which he called the art of economics. He argued that the

two branches needed to be separated because they had quite different methodologies. He writes: 'a definitive art of political economy, which attempts to lay down absolute rules for the regulation of human conduct, will have vaguely defined limits, and be largely non-economic in character' (J.N. Keynes, 1891, p. 83).

## 8.2 'NEOCLASSICALS' FOLLOWING CLASSICAL METHODOLOGY

This separation of applied policy from the science of economics did not end with Classical economists. It also characterized the approach of numerous economists who are often classified as neoclassical. These include Alfred Marshall, Lionel Robbins, John Maynard Keynes and even A.C. Pigou. In my view, in terms of method (by which I mean methodological views about how economic theory and models relate to policy), all four of these writers belong much more in a Classical tradition than in what has become known as a neoclassical tradition. By that I mean that they maintained the same strict separation between policy and theory that Classical economists did, and saw models as aids to judgment, not as definitive guides to policy.

Consider Marshall and Pigou. While it is true that Marshall and Pigou both developed more formal models than did most earlier Classical economists, and used those models in discussions of policy, it is also true that they were very careful to add a large number of qualifiers that could change the results of the model. Like their Classical ancestors, they both saw economic policy as an art that involves issues outside the domain of economics, and not as a set of prescriptions that followed directly from models. They were careful in their writings to emphasize the limitations of their models. For example, in the core text of Marshall's *Principles* he carefully specifies the limitations of the models rather than developing the analytics of the models. Often, he placed his formal analytics in appendices, not in the core chapters. His *Principles* was designed to teach students how economists thought about policy issues, and to introduce them to some models that could help integrate economic reasoning into their thinking. His textbook was not designed to teach students about how to model the issues formally. Put another way, he was teaching students to be 'consumers' of theory, not 'producers' of theory.

Pigou, the economist most associated with the term 'welfare economics', also carefully limited the applicability of his models. He tells his readers that his analytic work provides only 'vague judgments' and 'instructed guesswork'.<sup>2</sup> He specifically does not draw definitive policy conclusions from his models. For example, in Pigou (1935) he argues that his formal

model showing that certain policy actions will improve welfare ‘only takes us a little way’ in arriving at a policy view. He points out that there are many other issues that the model does not take into account, any of which could reverse the policy argument following from a model. He further states:

The issue about which popular writers argue – the principle of *laissez-faire* versus the principle of State action – is not an issue at all. There is no principle involved on either side . . . Each particular case must be considered on its merits in all the detail of its concrete circumstance. (Pigou, 1935, 127–128)

Contrary to popular opinion, Lionel Robbins also falls into this Classical methodological tradition of not drawing policy conclusions from formal models.<sup>3</sup> In his Ely Lecture (Robbins, 1981), Robbins reflects back on how his famous 1932 essay (Robbins, 1932) was incorrectly interpreted by the profession. He states explicitly that the economics profession needs a separate branch, which he calls political economy, to deal with policy. He writes that this policy branch of economics ‘depends upon the technical apparatus of analytical Economics; but it applies this apparatus to the examination of schemes for the realization of aims whose formulation lies outside Economics’ (Robbins, 1981, p. 8).

It was not only in microeconomic policy that the Classical methodology of strict separation of models and policy continued beyond what is generally thought of as the Classical period. It was also in macroeconomic policy. By that I mean that J.M. Keynes also followed this Classical method, and carefully did not derive policy conclusions from his models.<sup>4</sup> Instead, he used many different models and arrived at a policy conclusion through reasoned judgment. He writes:

Economics is a science of thinking in terms of models joined to the art of choosing models which are relevant to the contemporary world . . . Good economists are scarce because the gift for using ‘vigilant observation’ to choose good models, although it does not require a highly specialized intellectual technique, appears to be a very rare one. (Keynes, 1938)

In summary, the economist’s method through the 1930s was a method that separated applied policy work from formal models and theories. Applied economics was seen as an art that used economic models, but that also involved much more than those models. To arrive at any policy conclusion, one had to go beyond economic models. In the Classical method, any ‘theory’ of welfare economics was not a theory to be applied directly to policy. Instead, it was a guide to reasoned thought about applied policy issues. The results of theory were meant to be used with caution, judgment

and knowledge of the institutional details. No policy conclusion followed directly from economic theory or from economic models. Consistent with this applied policy approach, discussions of applied policy were to carry warning labels about the limitations of the models. This approach did not mean that economists, in their role as private individuals or statesmen, could not or should not arrive at policy conclusions. What it meant was that if they did so, they should make it clear that they were not claiming economic science as underpinning their arguments.

### 8.3 THE ABANDONMENT OF THE CLASSICAL METHOD AND THE RISE OF THE NEOCLASSICAL METHOD

Beginning in the 1930s, that Classical 'strict separation' methodology became less and less strict, and by the 1970s it was replaced in the textbooks with a more direct approach of connecting models and policy. Instead of maintaining a strict separation between models and policy, and emphasizing the importance of broader issues in arriving at policy conclusions, models and policy prescriptions became blended into one. To contrast this direct blending of models and policy with the above described Classical methodological approach, I call it the neoclassical methodological approach.

The neoclassical method does not seem ever to have been formally defended in methodological writings, as the Classical method was. It simply evolved over time, as the strict separation qualifications that the Classical methodologists emphasized faded from memory and practice. A full explanation of why this occurred is beyond the scope of this chapter, but my initial thoughts are that the change was associated with a change in the institutional structure within which economists worked, and with the development of empirical methods, which allowed economists to hope that the models could be chosen on the basis of statistical tests, and hence could have empirical foundations that would not necessitate the subjective judgment that Classical economists believed that it did.

The change in institutional structure involved the development of economics as a separate discipline with its own separate training. Up until the 1930s, a majority of those who wrote on economics were not in economics departments. They either were not primarily employed as academics, or were in broader political philosophy departments. Increasingly after the 1930s that changed; economics became a separate academic discipline, and training in economics became narrower as it focused more on pure economic issues, and less on the broad social science and philosophical

issues that characterized earlier training. As that narrowing happened, methodology no longer became a topic that economists studied. Instead, economists' work became more focused on the technical issues of modeling. As that happened the extensive discussions of scope and method of economics, which contained the caveats on the use of models in Classical writing, disappeared, either because the writers assumed that economists knew these caveats, and hence they did not require further discussion, or because such methodological issues were not for economists to discuss.

This institutional reason for the change was supplemented by a technological change in how economic analysis was done. Beginning in the 1930s, empirical methods of testing models expanded. This allowed economists to hope that the precision of the models could be increased beyond a 'sham precision'. With developments in econometric theory, there was hope that economics could become a positive science, in which theories and models could be tested, and shown to be true or false. That hope was largely unfulfilled, but the hopes for empirical work likely played a role in changing the economic method guiding applied policy work. If models could be selected on scientifically acceptable empirical grounds, then they could be considered scientific truths, and implications for policy could be drawn from those truths.

It was during this shift from Classical to neoclassical methodology that the formal subfield of welfare economics, which drew relatively firm policy precepts from economic models, developed. Welfare economics moved beyond Marshall's partial equilibrium approach in which models were used as a tool for reasoning about particular policy issues. In the Marshallian approach to applied policy the reasoning chains were kept short, and one would continually emphasize the limitations of the models. In the new welfare economics approach to policy this Marshallian partial equilibrium framework was replaced with a Walrasian general equilibrium framework. This Walrasian framework that pictured the economy as a system of 'solvable' simultaneous equations was much more mathematical than the Walrasian approach, and it drew out policy conclusions from models based on long chains of reasoning, with little to no discussion of the limitations of the models as they related to real-world policy.

These developments led to an enormous burst of creative technical work that extended the partial equilibrium models of Marshall to general equilibrium models. The limitations against using long lines of reasoning to arrive at policy conclusions faded away. In the 1930s and 1940s work in this area advanced economic theory enormously, and a wide range of theoretical issues were cleared up in the writings of economists such as John Hicks (1939), Paul Samuelson (1947) and Abba Lerner (1944). It was a change from a Marshallian economic vision of the economy as a

complex system too complicated to model fully to a Walrasian economic vision that was captured by a formal model.

It was during this time period that most of the qualifications of models that Classical economists had emphasized were moved to the back of economists' minds. As I argue in 'The Sins of the Sons of Samuelson' (Colander and Rothschild, 2010), with each successive generation the qualifications about the use of models faded further and further back, and by the 1960s Classical methodology had been replaced by neoclassical methodology in young economists' thinking and in the textbooks.

Of the three economists mentioned above, Abba Lerner was the most likely to draw policy conclusions directly from models and, in many ways, the policy discussion in his *Economics of Control* (1944) served as the template for the teaching of both micro and macro policy starting in the 1950s and continuing until today. Lerner drew specific policy conclusions from his theoretical models in microeconomics and provided few discussions of nuances or limitations.<sup>5</sup> He framed microeconomic policy as a technical issue of meeting the appropriate marginal conditions that were to become the fundamental theorems of welfare economics. Instead of students being taught that models provided half-truths, they were taught that by following the rules of welfare economics that equated marginal social costs with marginal social benefits, policy makers could lead society to a Pareto optimum. These rules, which were known as the Lange–Lerner rules, became the guiding rules of welfare economics, and have become the central frame of undergraduate micro theory in the textbooks.

Similarly, Lerner framed macroeconomic policy as a technical issue of meeting what he called the rules of functional finance (Lerner, 1944). These rules structured macroeconomic policy as following directly from an IS/LM type model, which led to specific policy actions: if income is below what is desired, use expansionary fiscal policy; if income is above what is desired, use contractionary fiscal policy. Use monetary policy to set interest rates so as to yield the optimal amount of investment.

Lerner's rules of both microeconomic policy and macroeconomic policy, because of their simplicity and clearness, became the template for the textbook presentation of both micro and macro policy discussions.<sup>6</sup> These policy rules that Lerner developed were not presented in the texts as general guidelines to be used in combination with non-economic considerations, as were the policy precepts found in Marshall and Pigou. Instead, Lerner's policy rules were presented as firm rules following directly from economic theory. Models were presented as forming the basis of policy – the blueprints that governments should follow – if government wanted to work in the social interest. For example, in the introduction to his *Economics of Control* Lerner writes:

[we] shall concentrate on what would be the best thing that the government can do in the social interest – what institutions would most effectively induce the individual members of society, while seeking to accomplish their own ends, to act in the way which is most beneficial for society as a whole . . . Here we shall merely attempt to show what is socially desirable. (Lerner, 1944, p. 6)

Unlike Marshall, and Pigou (1920), who carefully discussed the limitations of economic models when arriving at policy conclusions, following Lerner, the new pedagogical presentation of applied policy aggressively related theory and models to policy conclusions. The new pedagogical presentation did not make the Classical distinction between precepts (derived from the art of economics embodying value judgments in the theory) and theorems (derived from pure theory, and quite irrelevant for direct policy application).<sup>7</sup> Lerner's work was the core of much graduate teaching in the late 1940s. Then, as others expanded the models and developed more complicated models that showed the limitations of the arguments, Lerner's work simply became a stepping stone to a much wider range of increasingly complex models taught in graduate school. It remained, however, the central framework of undergraduate presentations of economic policy in both micro and macro, and thereby provided the frame that most economists who do not specialize in welfare economics bring to policy analysis.<sup>8</sup>

I am not arguing that the limitations of that framework were not known or understood. Although in Lerner's presentation, and in the textbook presentations of welfare economics that followed from it, there was little to no discussion of the nuances of application, in more technical advanced work there was a clear exposition of the limitations. Specialists in welfare economics fully understood that the formal models had little value for actual direct policy guidance. For example, in his *A Critique of Welfare Economics*, I.M.D. Little (1950) showed the limitations of the welfare economics as a guide for policy. Similarly, J. de V. Graaff concluded his famous consideration of welfare economics, *Theoretical Welfare Economics*, with the statement: 'the possibility of building a useful and interesting theory of welfare economics – i.e. one which consists of something more than the barren formalisms typified by the marginal equivalences of conventional theory – is exceedingly small' (Graaff, 1957, p. 169). Unfortunately that advanced work was not imprinted on the minds of most economists in the way that the limitations of the models for policy analysis were imprinted on Classical economists.

The decreasing emphasis given to the limitations of economic models for policy can also be seen in the evolution of the presentation of advanced social welfare theory, which abandoned the Pareto optimality approach to welfare economics found in Lerner's approach and replaced



it with a social welfare function approach (Bergson, 1938). Analytically, the social welfare function approach was a major improvement; it solved the Hume's dictum problem that one cannot derive a 'should' from an 'is'. It recognized that to derive policy recommendations that involve value judgments, one must explicitly state what underlying value judgments one starts from. But it did so primarily in theoretical expositions, not in real-world applications. By that I mean that while this social welfare function approach helped to clarify the formal structure of the micro policy model, and more correctly specified the analytics of what policy implications could be drawn from the analytics of the model, it nonetheless lost many of the nuances about limitations of applying models to reality that the earlier strict-separation Classical method had maintained. Consider Bergson's initial discussion of the social welfare function (Bergson, 1938). In it, he distinguishes an economic welfare function, in which only economic variables are considered, from a social welfare function, in which all the variables which affect welfare are taken into account.

He emphasizes that his 1938 discussion is of an economic welfare function, not of a social welfare function, which means that he accepts that the economic welfare function approach is only a partial analysis which needed to be combined with insights of other social sciences and philosophy to arrive at a policy conclusion. He justifies his focus on economic variables in his article with the following argument: 'For relatively small changes in these variables, other elements in welfare, I believe, will not be significantly affected. To the extent that this is so a partial analysis is feasible' (Bergson, 1938, p. 314)

But Bergson went even further than that, and later questioned whether a useful separation could be made. In a later article (Bergson, 1954), he expanded on this distinction where he discusses new developments in welfare theory. In this article he repeats his earlier argument that welfare analysis 'must rest on "value judgments" no matter how broad or narrow the scope' (Bergson, 1954, 249) He also writes that his 'own ethical thinking has evolved in the course of time'. He states: 'If value criticism of a deep sort can be meaningful, I still feel that it is also largely philosophic, at least in the present primitive state of psychology.' He concludes: 'I cannot imagine any sensible alternative to ethical counseling.' By this, he meant that the conclusions of any of economic models could not be translated into policy without their being placed in a broader philosophical and ethical context. Welfare economics as a separable branch of the science of economics could not exist, and policy advocacy had to integrate ethical and value considerations. The economics profession did not follow Bergson; instead, it moved further and further away from any discussion

of broader issues and concentrated on drawing direct policy conclusions from formal economic models.

What is relevant about Bergson's qualifications and differentiation between the economic welfare function and the social welfare function were soon lost and forgotten in most economists' discussions of applied policy and in the textbooks. To my knowledge, no textbook differentiated an economic welfare function from a social welfare function and made the point that Bergson made that economic welfare was only a small part of social welfare, and policy had to be decided on social welfare grounds, not on economic welfare grounds. Instead microeconomists have translated their economic models' results into direct policy recommendations with few of the broader qualifications that were emphasized in the Classical and Bergson's method.

More advanced critiques of the economic welfare function frame, such as Graaff's and Bergson's, or later Amartya Sen's (1970), seldom made it to the textbooks even in watered-down form, and thus between the 1940s and 1960s there was a major change in how economics was taught and how most economists thought about applied policy. Robert Solow (1997) makes this difference in pedagogy clear in his comparison of 1940s textbooks and textbooks beginning in the 1960s. He writes that books through the 1940s were discursive in nature. He states: 'Most provide more institutional descriptions, very sensible discussions of economic policy, and serious looks at recent history as it would be seen by an economist . . . The authors ruminate more than they analyze' (p. 88) Solow continues:

the student is not encouraged to make literal use of the apparatus of supply and demand curves. Both books spend time discussing monopolistic elements in real-world markets, but most of the discussion is institutional. Their reflections on the workings of economy are worth reading. They inspire bursts of nostalgia; words like 'civilized' came to mind. (Solow, 1997, p. 89)

Starting in the 1950s, following Samuelson's famous text (Samuelson, 1948), the textbook approach changed. The new style texts placed economics in a scientific framework with the microeconomic presentation organized around supply and demand graphs and a general Walrasian conception of the economy. While the principles-level microeconomic presentations did not present the full optimality presentations, the policy frame that they provided students was one that focused on marginal conditions, and micro policy was discussed in terms of models without significant discussion of the limitations of models. Similarly, its macroeconomics was organized around a Keynesian aggregate expenditures, aggregate production model, in which fiscal policy was needed to keep the economy at full employment, and monetary policy was used to set an

optimal interest rate. Samuelson fully recognized the limitations of the models, and some discussion of those limitations show up in addenda in the text and the footnotes. But there is none of the discursive presentation emphasizing how other issues enter into the analysis. Neither is there any broad discussions of limitations of the models such as found in Marshall's *Principles* (1890) or in economic principles textbooks through the 1940s.

Other books followed Samuelson's lead, and that modeling presentation of policy became embedded in economists' thinking. Just how embedded can be seen in Solow's (1997) description of how economists approach problems today. He writes:

Judicious discussion is no longer the way serious economics is carried out . . . In the 1940s, whole semesters could go by without anyone talking about building or testing a model. Today, if you ask a mainstream economist a question about almost any aspect of economic life, the response will be: suppose we model that situation and see what happens (Solow, 1997, p. 89–90)

## 8.4 CONCLUSION

The above history demonstrates the changes to the economists' approach to applied economic policy, and to teaching applied economic policy, which occurred in the transition from Classical methodology to neoclassical methodology. In the Classical period and up until the 1940s in the neoclassical period, textbook presentations carefully developed economic policy as only a part of a broader philosophical or social policy; the books were focused on training students to be consumers of economic theory, not producers of economic theory, and the textbook authors saw their role as guiding students in being good consumers of economic reasoning. This meant pointing out the need for context and the limitation of models simultaneously as they taught the models. The narrower neoclassical methodological approach moved directly from economic models to economic policy recommendations. It concentrated on teaching students modeling and understanding the analytics of the model.

These differences are primarily pedagogical differences, and do not necessarily reflect deep changes in economic methodology specialists' beliefs in how economics relates to policy. But, over time, pedagogical decisions have effects, and in economics, they had an enormous effect. They led more and more economists to lose sight of the limitations of models and the need to be humble about what the models are telling us, and what implications can be drawn.

Even if one accepts that economic policy is part of moral philosophy, and that models have to be put in context, an argument can still be made to

continue teaching as we do. The issue guiding what economics teaches its students involves practical trade-offs. Consider John Siegfried's consideration (2009) of Stephen Marglin's (2008) call to broaden economic teaching to include much more than the algorithmic knowledge taught in the neo-classical texts. Siegfried agreed with Marglin that teaching students about how models relate to economic policy required much more than what is currently taught, but argued that: 'a persuasive case for a concentrated dose of algorithmic knowledge in economics classrooms can spring from its scarcity elsewhere . . . In the absence of assurance that logical deduction will be emphasized elsewhere in the curriculum, maybe the best use of economics courses is to fill that gap aggressively' (Siegfried, 2009, p. 219).

The difficulty with this argument is that it assumes that students and economists are being trained on the limitations of models elsewhere. But that is not the case. Graduate economic programs provide little discussion of context, and instead concentrate heavily on teaching students modeling techniques. Economic training is geared to creating producers of models, not consumers of models who have the contextual and institutional knowledge, and the incentive to worry, about whether the model is the appropriate model for the purpose. Some economists of course, intuitively or through outside training, incorporate the nuances of applying models to policy problems. But that ability is neither selected for in the admission process, nor is it taught in terms of core content of graduate programs. Those programs emphasize the teaching of modeling techniques, not modeling interpretation. For applied policy, this presents a problem. As Keynes noted in his quotation above, an applied policy economist needs to know both how to model, and how to choose the right model.

The problem with our current approach to teaching models is that it leads to economists who are not trained in the subtleties of applying models to apply models, and to claim the imprimatur of economic science in doing so. Thus, for example, we can see two top macroeconomists, Chari and Kehoe (2006), writing in the AEA's *Journal of Economic Perspectives* that 'recent theoretical advances in macroeconomic theory have found their way into policy' and claiming that:

The message of examples like these is that discretionary policy making has only costs and no benefits, so that if government policymakers can be made to commit to a policy rule, society should make them do so. (pp. 7–8)

and:

Macroeconomists can now tell policymakers that to achieve optimal results, they should design institutions that minimize the time inconsistency problem by promoting a commitment to policy rules. (p. 9)

Such hubris about the strong policy implications of highly abstract models whose assumptions do not come close to fitting reality helped lead to the recent financial economic crisis. Such claims of policy certainty flowing directly from models do not sit well with economists trained in the Classical methodology that questions how well the model being used fits the situation being described. For example, Robert Solow, who was trained in the Classical methodology even though he strongly advocates a concentration on modeling, responded to their claims by arguing that their conclusions are totally spurious, and do not deserve to be taken seriously because the dynamic stochastic general equilibrium (DSGE) model that their claims are derived from is so far from the institutional setting of the real-world economy that lessons from the model cannot be directly applied to policy issues.

The primary recommendation following from the arguments in this chapter is that economics policy training could be improved by instituting specific training for applied policy and welfare economics that emphasizes the skills needed to interpret models. It would involve economic history, history of economic thought, real-world institutions, methodology and moral philosophy. This training could exist as a separate track for applied policy economists within economics departments, in public policy programs, in transdisciplinary programs, or in a separate program in political economy as distinct from economic science.

Such training would be much closer to the training that the Classical economists received. The training would involve discussions of technical models, but the goal of the training would be to provide students with a consumer's knowledge of theory and models, rather than with a producer's knowledge of theory and models. The graduates of these applied economics programs, or applied policy tracts, would be seen as the specialists in choosing among models produced by others, and these programs would have their own measures of output quite separate from the measures of output used by current graduate economics programs. Creating a cadre of economic policy specialists could go a long way toward restoring the humility about what claims can be made from our limited models in the face of the enormous complexity of the real-world economy that was expressed in Mill's recognition that analytic models provide at best half-truths.

## NOTES

1. Although I call it the Classical method, as I discuss below, its use extended well into what is normally called the neoclassical period.

2. See Stephen Medema (2010) for a nice discussion of how Pigou limited the applicability of his models.
3. For a further discussion of Robbins's approach, see David Colander (2009).
4. For an expansion of this issue, see Colander (2011).
5. Specifically, government should adjust resources until a set of marginal conditions are met (Lerner, 1944, p. 96). His rules on income redistribution did not become part of the textbook template. Lerner agreed that we had no basis for making interpersonal welfare comparisons, but argued that because of the uncertainty principle, redistribution was more likely to improve social welfare than hurt it, and thus he supported redistribution, and defined his welfare rules to include redistribution. Later developments switched to a welfare economics focus only on Pareto efficiency.
6. Lerner's early writing played an important role in the socialist calculation debate that was ongoing at the time, and very much concerned the arguments behind the role of the state in the economy. In that debate Lerner advocated market socialism, and argued that socialist planners could give directives to managers to set price at marginal costs, and thereby achieve maximum social welfare.
7. Lerner even extended the analysis to get around interpersonal comparisons of welfare by arguing that while interpersonal comparisons of welfare were impossible, 'probable comparisons' were not, and that redistribution policy should be based on 'probable total satisfaction' (Lerner, 1944, p. 29). Consistent with this view he drew out specific rules for how government could achieve the optimal distribution of income.
8. Ronald Coase's work provides an alternative frame, but few texts are structured around his more Marshallian approach.

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