WHAT WAS "IT" THAT ROBBINS WAS DEFINING?

BY
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There has been a renewed interest in Lionel Robbins's famous *Essay on the Nature and Significance of Economic Science* (Robbins 1932). An important reason for this interest is that currently economics is in a state of flux. The rational agent maximizing model is no longer the glue that holds the profession together, as the old economics trinity of greed, rationality, and equilibrium is giving way to a new trinity of enlightened self-interest, bounded rationality, and sustainability (Kreps 1997). Behavioral economics is flourishing, and new branches of economics, such as neuroeconomics, experimental economics, econophysics, evolutionary game theory, and complexity economics are developing and changing the face of economics theory (Colander, Holt, and Rosser 2004).

Today, economists distinguish themselves from other social scientists more by their ability to bring sophisticated statistical and mathematical methods to the analysis of social issues than by the model they use or the subject matter they study (Colander 2007a). If the study of "the allocation of scarce resources among alternative ends" ever was the defining nature of what economic science was, it no longer is (Colander 2005). Because economics is changing, it needs a new definition. In thinking about this new definition, it is only natural that economists reflect on those articles that have been central in shaping economists' image of what it is that they do. Robbins's essay is clearly one such article.

As discussed by Denis O'Brien (1988), and more recently by Roger Backhouse and Steven Medema (2007), Robbins's 1932 essay provoked much discussion when it was published and afterwards. Much of this discussion has focused on Robbins's definition of economics, even though, in responding to critics, Robbins argued that his definition was not all that novel or important to the point he wanted to make. He states this explicitly in his paper "Live and Dead Issues in the Methodology of Economics" (Robbins 1938). He writes

> Economics, we have suggested, is essentially the study of the disposal of scarce goods and services. This suggestion has not met with universal acceptance. But the difference between this and other definitions now current is not a very serious matter (p. 344).

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Backhouse and Medema see this downplaying of the definition as misplaced, and given the emphasis the definition has played in economics that view is certainly understandable.[1] My reading of Robbins, however, suggests that for Robbins the definition of economics was not his central concern. He downplayed the differences between his definition and others because that definition was not key to the central message of his essay. That central method was that economists needed to distinguish economics science, which he believed should avoid value judgments, from the broader field of economics (which he called political economy), which he believed should not only include, but should necessarily embrace, value judgments.

In my interpretation of Robbins's essay, the definition he provided was not meant to be prescriptive in the sense that anything other than work that fit that definition should not be considered economic science. His definition was *descriptive*. It reflected what he believed that economic theorists were then doing at the time when they did economic science. This interpretation of Robbins essay provides an explanation for why he downplayed the definitional issues; it wasn't the central point of the essay.

In his later writing about the *Essay*, Robbins is clear about what he thought the central prescriptive message was. In that same article he writes

> The only question with regard to the scope of economics which can be said to be in any sense alive, is the question whether economics, as such, can be said to include judgments of what is good and bad in the world of relative scarcities (Robbins 1938, p. 345).

Robbins's prescriptive answer to whether value judgments belong in the "it" that he was defining is also clear; in Robbins's mind value judgments had no place in the "it" that he was defining, no matter how "it" is defined. If you think you are doing "it" and doing "it" involves value judgments, then you are not doing "it."

Let me now turn to the question of what "it" is. The argument in this paper is that the "it" being defined was not economics inclusive of all that economists did in their role as economists. It was a more narrowly defined "it" that included only the "economic science" portion of what economists did. That's why he entitled the book "An Essay on the Nature and Significance of *Economic Science*" (my emphasis). *For Robbins, the science of economics and the entire field of economics were quite different*. So the "it" being defined was "the pure science of economics," which Robbins, following Keynes (1891), interpreted very narrowly (Colander 1999). Both Robbins and Keynes saw this pure science of economics as only a small sub branch of economics—a branch, which in his view, almost by definition, had nothing to do with policy. He specifically saw another branch of what economists do—political economy—as the branch primarily concerned with applied policy, not with science. Here, he wanted value judgments to have free rein, and to play an important part in the analysis.

So the prescriptive message he hoped to convey was that *value judgments and policy analysis belonged in the political economy branch of economics, not in the economic science branch of economics.* This prescriptive message was misinterpreted by many in

[1]They write "Though Robbins portrayed himself as saying nothing original, the idea that his definition did nothing more than sum up the way economists thought about their discipline is a myth." (Backhouse and Medema, p. 3)

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the profession as implying that we need to eliminate value judgments from the field of economics. That was definitely not Robbins's intent. Robbins was quite explicit about the need for this separate branch of economics to deal with applied policy, and he stated it repeatedly. For example, in his Ely Lecture (Robbins 1981), he states that he favors the revival of political economy as a branch of economics. He writes

> My suggestion here, as in the Introduction to my *Political Economy: Past and Present,* is that its (political economy) use should be revived as now covering that part of our sphere of interest which essentially involves judgments of value. Political Economy, thus conceived, is quite unashamedly concerned with the assumptions of policy and the results flowing from them. I may say that this is not (*repeat not*) a recent habit of mine. In the Preface to my *Economic Planning and International Order*, published in 1937, I describe it as ''essentially an essay in what may be called Political economy as distinct from Economics in the stricter sense of the word. It 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; and it does not abstain from appeal to the probabilities of political practice when such an appeal has seemed relevant.
>
> It should be clear then that Political economy in this sense involves all the models of analysis and explicit or implicit judgments of value that are usually involved when economists discuss assessments of benefits and the reverse or recommendations for policy (Robbins 1981, p. 8).

As he states in that quote, his position that there should be two branches of economics and that policy belonged in political economy sub branch was not a position that he came to late in life. In his 1938 discussion of methodology he writes

> those who adopt this latter attitude [the view that his definition will preclude economists from being interested in policy] are concerned not so much with problems of definition as with problems of conduct or deportment. They think that the economist will sacrifice opportunities for usefulness if he abstains from excursions into social philosophy and (apparently) they think that if such excursions are not dignified by the title, economics science, he may feel precluded from making them. The warning may be necessary. But the psychology may be questioned. To me at least, it seems difficult to believe that recognition of the distinction between the two kinds of propositions will prevent any man of spirit from being interested in both (Robbins 1938, p. 345).

I. THE CONTEXT FOR ROBBINS'S VIEW

Robbins's view of economics as consisting of both an economic science and an applied policy branch has a long history in economics. As Denis O'Brien makes clear in his retrospective essay, Robbins's position relates back to David Hume, Max Weber, the Austrians, and Phillip Wicksteed (O'Brien 1988). The division was part of classical economist's way of thinking about what they did. For example, in J. Neville Keynes's famous *Scope and Method of Political Economy* (Keynes 1891), which was the standard work summarizing classical methodology in the late nineteenth century, Keynes argued that it was necessary to separate out the art of economics (his name

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for political economy) from positive economics (his name for the science of economics).[2] Like Robbins, Keynes argued that maintaining a separate positive science, which avoided value judgments, quite distinct from applied policy, which included value judgments, was necessary to avoid confusions about the relation between theory and policy.

Up until the late nineteenth century, the pure science of economics was quite small, and economists called what they did political economy, not economics. The term ''economics'' was reserved for economic theory, which consisted primarily of logical deductive models.[3] Economic science for Classical economists, and for Robbins, focused on abstract theory and deductive logic, not on empirical work or applied policy. It was a very small branch.[4]

With the dawn of the neoclassical era, the economic science branch of the study of economics started growing, as the marginalists and Austrian economists started framing the economic question in calculus and exploring the nature of constrained optimization mathematically for the whole economy through general equilibrium models. But even in the 1920s and 1930s, this formalization into a clear model was still at a very early stage. In terms of its insight for policy, it was speculation, with few conclusions. Robbins is quite clear about this and in his review of Hawtrey (Robbins 1927), a review that included many of the ideas that would later become embodied in his 1932 essay, Robbins wrote

> 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. The problems of human motive we have to analyse with the ''vast amorphous phantoms'' of psychology at their back, are nebulous enough in all conscience. It is not because we believe that our science is exact that we wish to exclude ethics from our analysis, but because we wish to confine our investigations to a subject about which positive statement of any kind is conceivable (Robbins 1927, p. 176).

One reason why Robbins believed that economic science included primarily logical deductive work and not empirical work was at the time the tools and statistical methods available to do empirical work were too crude to allow scientific conclusions. But he hoped and believed that with the advancement of statistical methods, the situation would change. He writes: ''Fortunately there is reason to suppose that in the future the alliance between the economy theorist and the statistician will be even closer than it has been in the past'' (Robbins 1930, p. 21).

For Robbins, science could consist of both theoretical exploration and empirical testing of those theoretical explorations. Robbins's prescriptive point was that propositions that were not even, in theory, empirically testable, but were instead

[2]Keynes also distinguished a normative branch of economics where the goals of policy were to be discussed. See Colander (1999).

[3]Nassau Senior (1836) was probably clearest about this when he limited the scope of economic science to purely deductive reasoning, and explicitly excluded empirical work.

[4]Robbins was a scholar of Classical economics (Robbins 1953); he knew the history of economics and the highly normative nature of many aspects of classical political economy. He believed that their prescriptive policy analysis, which included moral judgments involving liberalism, was an important tradition that needed to be continued.

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based on normative judgments, did not belong in the science of economics. All applied policy questions fit this category since they interrelated with so many non-economic issues that developing formal applied policy models that captured them was far beyond the scope of economics. So he opposed including any of that work under the mantle of economic science.

In the 1930s when Robbins wrote, constrained optimization was what many pure economic theorists were working on, and in his definition of the science of economics Robbins focused on constrained optimization.[5] But if I am correct in saying that he meant it as a descriptive not a prescriptive definition, and if one had a time machine, and moved current economic research that has little to do with the allocation of scarce resources, such as modern work in neuroeconomics, econophysics, or behavioral economics back to the 1930s, Robbins would have modified his definition of economic science to better include this modern work, because it meets his key prescriptive requirement of his definition—it is attempting to understand economic issues in a way that is in principle subject to empirical verification. (He was writing before Popper moved the debate from verification to falsification.) He stated his strong support for empirical work, writing "Clearly, quantitative exactitude is the object of all scientific inquiry, and it is only by continually testing our theories by reference to the facts of the situation that we can discover how far they proceed from assumptions that are appropriate" (Robbins 1930, p. 21).

II. ECONOMICS, POLITICAL ECONOMY, AND THE TEACHING OF ECONOMICS

Debates about method and definitions have little direct effect on economists; they do what they do; practicing economists don't worry about methodologist's prescriptions. Where debates about method and definitions have an effect is in the teaching of economics, or at least the way economics is presented to students. Through the texts, definitions and discussion of method indirectly influence future economists. Thus, much of the initial interest in Robbins's *Essay* and definition reflected a debate about what economists should be teaching students.

Through the 1800s what was taught was political economy—a set of ideas as they related to policy. Theory was embedded in policy discussions. With the rise of marginalist theories, and a mathematical structure for economics, there was a pedagogical debate about what part of economics to present as students. The classical position was to present a combination of theory and policy analysis under the heading of "political economy." Some marginal theorists were pushing for a more formal presentation of economic theory.

One such economist was Maffeo Pantaleoni. He wrote a text, *Pure Economics* (published in 1889 and translated into English in 1898), that was consciously about *economic science* rather than *political economy.* He states this explicitly in the preface, writing: "This manual is intended as a succinct statement of the fundamental definitions, theorems and classifications that constitute economic science, properly so

[5]This helps explain how he could choose the constrained optimization definition for economics even as the economy was in a depression. In Robbins's view the work in macroeconomics that was being done did not measure up to the standards of science.

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called, or Pure Economics. Thus all questions pertaining to economic art, or Political Economy, are beyond its scope'' (ivi, p. VII). He continues: ''This is a departure from the lines on which textbooks of economic science are usually prepared, their authors' objects being to equip the reader forthwith for the discussion of the most important economic problems as presented by everyday life.''

Pantaleoni's usage of the term ''economics'' as being about theory was consistent with the Classical usage of the terms. However, that usage was changing, and the use of the term ''political economy'' as a separate branch of economics was declining. Part of the reason for this was Alfred Marshall's text. In 1890 Alfred Marshall faced the same problem as did Pantaleoni, with his principles textbook. But unlike Pantaleoni, he did not believe that what should be taught was only the deductive logic of the marginalist model. Marshall straddled the fence between the marginalists and the historical/institutional approach to teaching economics. He believed that economists could teach both political economy, which involved lessons for policy, and the marginalist model.

This desire to teach both left him with a problem of whether to call his book Principles of Economics, or Principles of Political Economy.[6] While he chose the term ''economics'' to designate what he was teaching, he was very clear that what he called ''economics'' was not pure theory separate from policy, but rather a set of tools, and a ''method of analysis that helps its user arrive at reasonable conclusions.'' Under the older use of the term ''economics,'' Marshall's ''economics'' would have belonged under the heading political economy.

Marshall's approach to teaching principles blended in theoretical constructs with discussions of real world issues, showing how those theoretical constructs could shed light on economic issues. He gave little discussion of general equilibrium, with it showing up only in Note 21; he concentrated on developing partial equilibrium tools that could be used to analyze real-world policy problems. His tools embodied value judgments, as they had to, if they were to be applicable to policy, but he attempted to be clear about what these value judgments were. For him, and for many other economists of the time, the blend was science—not a pure science, but an applied, or moral science as J. M. Keynes called it (Wright 1989, p. 473). Marshall was clear that he was using economic tools as rough and ready tools, not for providing definitive results, but for guiding thinking about policy issues.[7] Consider the concept of consumer surplus, which Marshall developed as a theoretical tool to shed light on policy questions. It integrated all individuals' welfare into an area under a curve, and

[6]In making this choice he faced a problem; he was pushing for a separate trypos in economics at Cambridge. If he chose the term political economy, it would have been harder to justify the separate trypos. I do not know whether this influenced his choice or not, but it seems possible that it did.

[7]Even with his limited interpretation of theory, Marshall was extremely hesitant to draw policy conclusions from his analysis. Policy was too complicated, and involved too many non-economic variables. For example, when he discussed the art of economics in the fourth edition he wrote: ''Of course an economist retains the liberty, common to all the world, of expressing his opinion that a certain course of action is the right one under given circumstances; and if the difficulties of the problem are chiefly economic, he may speak with a certain authority. But on the whole, though the matter is one on which opinions differ, it seems best that he should do so rather in his private capacity, than as claiming to speak with the authority of economic science'' (Marshall 1898, Vol. II, p. 154).

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thereby included the implicit value judgment that individuals' welfares were comparable and interchangeable. Thus, Marshall's ''economics'' had one foot in the science of economics and one foot in political economy.

A. C. Pigou, Marshall's follower at Cambridge, was more explicit than Marshall about the methodology he was using, and the approach he and Marshall used *did not belong in the pure science of economics.* He states explicitly that he was doing *realistic theory.* He writes, ''Hence it must be the realistic, not the pure, type of science that constitutes the object of our search'' (Pigou 1920, p. 6). To make this point even clearer, Pigou distinguishes between *fruit-bearing theory* and *light-bearing theory* (Pigou 1920, p. 3). Fruit-bearing theory (realistic theory) is essentially political economy; it is theoretical apparatus that is designed to solve particular policy problems. It allows value judgments to be built into the analysis, and it makes no attempt to be pure; it was a type of engineering science, not a pure science. It can still be objective, in the sense of being open about the value judgments, and having the value judgments reflect economists' estimate of society's value judgments rather than their own, but it accepts value judgments as a necessary part of the analysis—necessary to make the analysis relevant to policy. Light-bearing theory is pure theory, or theory belonging in Robbins's economic science branch of economics, and it was not a branch of economics that Marshall or Pigou had anything to say about.

Marshall's broad use of the term ''economics'' to include work that was previously included only under the term ''political economy'' is likely, in part, responsible for the misinterpretation of Robbins's methodological point, and about what ''it'' he was defining. Robbins, using the terminology of the classical tradition, was referring only to the pure science of economics when he used the term ''economics.'' Many of the critics, thinking of the term in the Marshallian/Pigovian tradition, were referring to the more engineering branch of economics that was designed to answer policy questions. Because Robbins thought of ''economics'' as a pure science, he opposed this usage. The problem he saw with that Marshallian/Pigovian tradition was that it made it seem that the applied policy work had the imprimatur of science on policy conclusions.[8] He felt doing so was inappropriate.

The economics profession did not follow Robbins's prescription. Its applied policy work followed Marshall and Pigou's approach and was classified under the name welfare economics. Over time, applied policy work became more and more theoretical and was integrated into a general Walrasian, not partial equilibrium Marshallian, framework. Applied policy work gave up the ''engine of analysis'' approach that was the Marshallian hallmark. It worked toward eliminating value judgments inherent in the Marshallian tools, as it tried to meet Robbins's concerns about keeping value judgments out of the analysis.

In many ways, the approach taken by the profession in its applied policy work was the worst of both worlds. It combined Marshall and Robbins, but it did so in a way that undermined both of their positions, and left one with an approach that satisfied

[8]As Richard Wright (1989, p. 472) points out, the impetus to Robbins's concerns was the Committee of Economists Report, which he believed had inappropriately intertwined political and ethical concerns under the guise of science.

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no-one. Theoretical and applied welfare work became completely separated. The result was bad for both sides. On the theoretical side, as J. V. Graaff concluded in his famous consideration of welfare economics, *Theoretical Welfare Economics*, without making some interpersonal utility comparisons "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" (J. V. Graaff 1959, p. 169). On the applied side, economics went on making interpersonal welfare comparisons in applied policy by hiding the interpersonal comparisons that embody hidden value judgments in the assumptions of the model (Slesnick 1998).

In his Ely Lecture (Robbins 1981) Robbins states his dissatisfaction with the direction that applied policy took. In it he argues that the name "welfare economics" is inappropriate. He writes "the raison d'être of welfare economics is to be "able to pronounce *as a matter of scientific demonstration* that such and such a policy was good or bad" (his emphasis) (p. 4). He states: "In the great work of Marshall and, still more, Pigou, we are assuming comparisons . . . (that are) . . . not warranted by anything which is legitimately assumed by scientific economics" (pp. 4–5). Given his pure science conception of economics, this argument is understandable, but so too is Pigou's argument, as long as one interpreted welfare economics as belonging in Robbins's "political economy," which is where Pigou believed it belonged, as is demonstrated by his carefully specifying that he was not doing pure science, but realistic science.

Marshall's and Pigou's "applied policy" work was too formal for Robbins's tastes to be an appropriate methodology for political economy; it did not take adequate account of the many non-quantifiable issues that impinged upon policy. Robbins saw the analysis of applied policy more in the J. N. Keynes's framework, where it involves "all modes of analysis and explicit or implicit judgments of value."[9] For Robbins, as for Keynes, the science of economics was to be used as a backdrop for thinking about policy problems, useful to help organize one's thoughts, but was not to be directly applied to real-world problems.

Given Robbins's advocacy and support of Classical political economy, and admiration for the policy advocacy work of Adam Smith and Robert Torrens (Robbins 1953), it seems clear that the last thing that Robbins wanted was for applied policy work to further restrict the value judgments used in them. Thus, welfare economics' retreat away from the interpersonal utility comparisons that characterized the evolution of welfare economics as it retreated to a narrow focus on Pareto optimality *was specifically not what Robbins had advocated.* Those changes were retrograde changes, and did nothing to achieve his prescriptive message. They just replaced one set of value judgments with another. In Robbins's view welfare economics could not get around Hume's dictum that "you cannot derive a '*should'* from an '*is*"' no matter how welfare economics was formulated. He writes "the old or the new Welfare economics are unlikely to be helpful and may well miss the main point entirely." The reason they are unlikely to be helpful is that the name "Welfare

[9]The approach to applied policy work suggested by J. N. Keynes and Robbins would be more like that suggested by Swann (2006) than the type of applied work that economists typically do.

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economics conveys an impression of value-free theory which it should be just our intention to avoid'' (Robbins 1981, p. 7).[10]

III. IMPLICATIONS OF ROBBINS'S ESSAY FOR MODERN ECONOMICS

As I stated at the beginning of this article, Robbins's essay is of interest today because, as was the case in the 1930s when Robbins wrote, economics is currently in a state of flux. This is true in both the applied policy (political economy) branch of economics and the pure theory branch of economics. What economists do now is fundamentally different from what is presented in the texts as the definition of economics. Also, as I stated above, this is of no concern to most economists; they do what they do. Definitions and discussions of methods are for students, not for economists. So, in conclusion, let me consider the implications of Robbins's essay for the textbook presentation of economics.

The first implication is that, since, as a description of what economists do—even referring to the pure science of economics—Robbins's definition is no longer applicable, it needs to change. The most important change in the way the economic science of economics is done is that modern economic science is no longer solely deductive and far less tied to the constrained maximization model than it was in Robbins's time.[11] It is also empirical. Theorists today collect and organize data, try to pull information from data using the latest econometric techniques to see what the data are telling them. They use natural, laboratory, and field experiments to provide insights into how the economy works and to test theories. They also use simulations and game theory constructs to attempt to gain insight into economic problems. The science of economics today has made enormous strides from the science of economics in Robbins's time, and is essentially about finding robust patterns in data and finding explanations for those patterns. How that work relates to the allocation of scarce resources is of less importance than that is it what economic scientists are doing.

Even that part of modern economic theory that is primarily deductive studies much more than constrained optimization models, and might not fit well into Robbins's definition of the allocation of scarce resources among alternative ends. There is a burgeoning branch of behavioral economic theory, while other theorists study nonlinear dynamic models, evolutionary game theory models, biological generation models, statistical mechanics models, and a whole lot more. Any descriptive definition of modern economic science needs to be sufficiently encompassing to include all this work.

[10]The problem of not dealing with the value judgments in applied empirical work has continued to exist in applied economics, just as Robbins thought it would. As Daniel Slesnick notes, the standard applied economist's approach of assuming a representative consumer in applied policy work is ''unappealing both because distributional issues are ignored and because much evidence shows that aggregate demands are inconsistent with the behavior of a single representative agent'' (Slesnick 1998, p. 219).

[11]It remained primarily deductive through the 1980s (Rosenberg 1991) but more recently it has changed and become more empirical.

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My suspicion is that Robbins would be the first to push for a redefinition.[12] My reasoning here, as I stated above, is that Robbins's definition is best considered descriptive, not prescriptive. As a descriptive definition, Robbins would have wanted the definition of economics to include all the recent scientific developments in what economists do. So the lesson I take from Robbins's essay concerning the definition of economic science is that it be sufficiently wide to include what it is that economic scientists do, as long as what they are doing falls within the confines of science, and those confines exclude any role for value judgments in the analysis.

But there is a more important prescriptive lesson that I believe comes from a reconsideration of Robbins's essay. While Robbins would not have been concerned about the change in the definition of economics, he would have been very concerned that the textbooks make clear the separation of the pure science of economics from the political economy branch of economics. He would stand by his prescriptive advice about the need for *two clearly distinguished separate tracks* in economics—one a science track designed for questions of understanding the economy, and one an applied policy track designed for guiding policy.[13] Thus, to meet Robbins's prescriptive message, the definition in the texts of the pure science of economics will need to be accompanied by a supplemental definition in the texts of another applied policy track.

Whether you call both of these tracks science, but distinguish a realistic science—the applied policy branch—from the pure science branch, as Pigou and Marshall would do, or call one track the "science of economics" and the other track "political economy," as Robbins would, or call one track positive economics and the other track the art of economics as J. N. Keynes would do, is less important than the fact that these two tracks be explicitly distinguished. The current texts do not do this, and thus miss the prescriptive lesson Robbins wanted to convey in his essay.

I believe that this applied policy track is also most usefully defined descriptively, not prescriptively. Considering what economists do when they do applied policy, we see an enormous change in what economists do compared to what economists did in Robbins's day. Whereas in Robbins's day what he called political economy was largely heuristic, today it has become much more applied mathematical and statistical in nature.[14] Economists see themselves as bringing technical expertise and modeling expertise to applied policy. Applied economists use experiments, they use game theory, and they use statistical methods extensively.

The evolution of method means that today the two branches of economics have come closer in approach, and less distinct than they were in Robbins's time. But that *does not* mean that the two can be combined into one. In fact, I believe that Robbins

[12]Robbins emphasized the changing nature of economics in his inaugural lecture at the London School of Economics (Robbins 1930), where he noted Ricardo's remark that economics was still in its infancy and Marshall's remark that economists had enough to occupy them for the next three thousand years. He explicitly states that "No one pretends that what is being done to-day is anything but provisional" (Robbins 1930, p. 15).

[13]These two tracks are, of course, related, with the theorems developed in economic science guiding the policy prescripts, and they can be done by the same economist. But that economist would see him or herself as engaging in two separate tasks, and would carefully separate the two.

[14]Robbins predicted that it would change in this way, but he also noted that as it did change, it would become more useful. He wrote "The theory becomes more complex, but its application becomes more practicable" (Robbins 1930, p. 21).

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would continue to insist on the need to separate these two branches, and that his line of demarcation would be whether the work was designed to establish a fact or come up with a *theorem,* or whether it is designed to arrive at a *precept*, which were the terms J. N. Keynes used to separate the output from the science of economics and political economy.

An *empirical fact* is an empirically agreed upon observation. A *theorem* is a conclusion that follows from economic theory; theorems concerns the way the economy works. It does not directly concern policy questions, although it may have implications for policy. A *theorem is not debatable* by serious economic scientists. A *precept* is a rule of thumb that concerns policy that follows from political economy. Precepts are derived from economic theory, introspection, induction, educated common sense, the standard ethics of the day, and judgments on normative issues. A *precept* reflects the conventional wisdom of the profession, and while based on theory, *is debatable* by serious economic scientists. In Robbins's mind, if the goal of research is discovering a fact or a theorem, then it is part of the science of economics (or whatever one is calling that branch). If the goal of the research is a precept, then it is part of political economy (or whatever one is calling that branch). Making that separation explicit in the principles of economics would go a long way toward meeting Robbins's prescriptive goal of his essay.

Whether it was possible or not to teach principles students the science of economics in Panteleoni's and Robbins's day, I do not believe it is today. The modern science of economics is far too sophisticated for principles students to understand without extensive study. We can teach students about the scientific economic process, and some of the findings; we can try to give them an appreciation of the science, but that is quite different from teaching them the science of economics. In reality, most principles texts don't try to give students an appreciation of the science of economics (as Robbins would define it); they try to give students an appreciation of political economy, as Robbins would define it. But because they don't separate two branches of economics, and don't distinguish precepts from theorems, they fail to meet Robbins's prescriptive message, which was in many ways simply a restatement of Hume's Dictum.[15] Reminding economists that the "it" that they are studying encompasses two branches of economics, and that the science of economics branch

[15]To differentiate a precept from a theorem, students could take what might be called the Hume/Robbins test, where the teacher asks a question about the policy implications of a theory. For example, he or she could ask: Does economic theory prove that international trade makes countries better off? If the student answers the question yes, then he or she fails the Hume/Robbins test, and the teacher would explain to them that to come to a conclusion about "better off" one needs to make value judgments, and that while it is a precept of political economy that international trade makes countries better off, it is not a theorem. To move from the theorem of economic science about free trade, to the precept of political economy about free trade, one must specify what one means by "better off," which requires value judgments for which we do not know how to develop a scientific foundation.

Then, to see if the students understood the argument, students might be given the Hume/Robbins/Sen test, which would consist of asking them the following question: If by better off we mean achieving a Pareto Optimal improvement where everyone is made better off and no one is made worse off, can we then say that economic theory states that international trade makes countries better off? Again, if they answer yes, they fail, because as Sen (1970) has shown, Pareto optimality does not get one around Hume's Dictum.

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is subject to Hume's Dictum, was Robbins's goal, and hopefully, this time around, his message will be heard.

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