

On the Nature and Significance of Economics Redux

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Comments are welcome.

Abstract

Seventy-five years after the original publication of Robbins's methodological masterpiece, many of the points made in that essay are as relevant as ever. In this paper we consider some of these points from a contemporary perspective, assessing both the importance and meaning of Robbins's original formulation and its instrumental role in order to obtain a better understanding of new developments of and challenges to the discipline. Special emphasis will be given to the appropriate definition of the current status of economics as a scientific field of inquiry, namely the characterization of its subject matter and the apparent lack of concern with the solving of economic problems and the facing of daily life economic reality "as a basis for political practice", as Robbins would put it. The current methodological trends in economics will also be discussed in the light of Robbins's own contributions in the field of pure methodology. The paper will address the problem of the nature and use of economic generalizations and the mechanical extension of an allegedly universal method of economics to a diversity of fields of economic agency and behavior. In this context, the paper will also deal with one of the major innovative contributions offered by Robbins's essay which still reveals a strong heuristic capacity, as shown in contemporary debates about the (im)possibility of interpersonal utility comparisons.

It is therefore our purpose to bring Robbins's classic themes to light again, in both a critical and revisited, redux, form, taking into consideration the evolution economics went through since the original publication of the essay in 1932.

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1. Introduction

“The purpose of this essay is twofold. In the first place, it seeks to arrive at precise notions concerning the subject-matter of Economic Science and the nature of generalizations of which Economic Science consists. Secondly it attempts to explain the limitations and the significance of these generalizations, both as a guide to the interpretation of reality and as a basis for political practice” (Robbins, 1935, p. xiv).

Lionel Robbins wrote the above words in the preface to his celebrated masterpiece, *Essay on the Nature and Significance of Economic Science* (ENSES), originally published in 1932. Writing three fourths of a century later, we couldn't agree more that these are essential issues, and we can find no better way to describe our own purpose in this paper.

While some of Robbins's contributions in ENSES were very influential, many important insights which Robbins offers us in ENSES and elsewhere were not successful at becoming part of the current mainstream of economic thinking. Some of these issues are still extremely relevant today, thus we hope that our present paper will contribute to a better understanding of the present situation of our discipline. Only indirectly will we discuss the historical significance of Robbins's *Essay*². Our main concern is to show how a selection of Robbins's topics (hence the “on” in our title) is useful to highlight the status and problems of contemporary economic science.

While respecting alternative interpretations, we consider ENSES consisted of three fundamental contributions to economics:

The first was to provide a novel, clear and robust definition of the subject matter; the second was a series of pure methodological contributions, which included the importation

² We will take briefly into consideration some of the authors and schools of thought Robbins was “fighting against” (institutionalists and historicists), as well as those whose first steps he explicitly acknowledged as a source of inspiration, in particular J. Stuart Mill, Carl Menger, Ludwig von Mises, P. Wicksteed, and F. A. Hayek.

to the English-speaking world and expanding the pro-theory side of the *Methodenstreit*³; the third was opening the debate about the difficulty associated with some kind of economic generalizations, as illustrated by Robbins's claim about the impossibility of interpersonal utility comparisons.

Part of the methodological contribution mentioned in Robbins's second contribution above was to defend the neoclassical program. A starting point of Robbins's essay is that there had been substantial progress in economics:

The efforts of economists during the last hundred and fifty years have resulted in a body of generalizations whose substantial accuracy and importance are open to question only by the ignorant or the perverse (Robbins 1935, p.1)

Except for some restrictions that will become clear, we believe that the above statement can be updated for today.

We also consider that the three issues mentioned above are as relevant as ever today, both by themselves and having their interactions considered. They have more merit and less recognition than the actual contemporary practice of economists seems to suggest. It is for this reason that while having different motivations and intellectual interests, the two of us share and put together a common recognition of the relevance of Robbins's work for a discussion of issues which are universal and permanent sources of interest for practitioners of economics. It goes without saying that we may assess in different ways both the merits and shortcomings of modern mainstream economics, as well as those usually attributed to heterodoxy. But the lesson to be drawn is that economic methodology is a "public good" that should concern the whole profession.

³ O'Brien (1998), Howson (2004) and Hands (2005) argue that Robbins is less Austrian than he seems, and actually closer to Mill's position. We agree, yet Robbins's essay was clearly a defense of theory in reaction to the English historical school (Robbins position was one that in retrospect could be considered as part of the logical positivist tradition). There are important qualifiers however, such as Robbins' rejection of a universal method for science.

Thus this paper is a partial attempt to bring the trio of above mentioned issues to light again, in a both critical and revisited form, and considering the intellectual evolution of the discipline since the publication of Robbins's original essay.

2. The Subject Matter: Robbins's Position

The definition of the economics that Robbins made famous “Economics is the science which studies human behavior as a relationship between ends and scarce means which have alternative uses” (Robbins 1935, p.6) was chiefly methodological: it was based on how scarcity plus unlimited desire necessarily implied choice. The combination of variety of ends with scarcity of means meant that alternative applications are possible, thus, a choice had to be made. Robbins' definition moved away from the traditional definition of economics based on the object of analysis: that of Alfred Marshall, “Political Economy or Economics is a study of mankind in the ordinary business of life; it examines that part of individual and social action which is most closely connected with the attainment and with the use of the material requisites of wellbeing” (Marshall 1920, p.1), or that of Robbins' teacher, mentor and contemporary E. Cannan, “the aim of political economy or economics is the explanation of the general causes on which the material welfare of human beings depend” (Cannan 1914).

Robbins' long term vision was the shift from the science of production, distribution, and (mostly material) welfare to the science of equilibrium, comparative statics and dynamic change (Robbins 1935, p. 68). However, *Robbins' position was much less “anything goes except method” that taking his above often-quoted definition literally would suggest*, and what accordingly is to him usually attributed. In fact, the (largely Marshallian) “price theory” approach, until recently mostly associated with the University of Chicago, which started with Frank Knight, and was followed by Milton Friedman, Gary Becker, and more recently, Kevin Murphy, Steven Levitt and others (including elsewhere), would in fact be closer to the method-exclusive definition and statement of purpose of economics than Robbins'.

As it is amply known, Robbins' Essay was very influenced by John Stuart Mill as well as by a number of Austrian economists, Menger and von Mises in particular, as he often

acknowledged, including in the footnote of the famous definition quoted above (Robbins would frequently say that his definition was not new). In fact, Robbins's definition and approach was on the contrary based upon the notion that individuals act purposely, with designed intentions, a notion that was retained from Robbins' continental influences, in particular Carl Menger's theory of human needs and economic goods. Robbins explains his position exceptionally clear in retrospect, in his 1981 Ely Lecture to the American Economic Association:

“At the beginning of my career, in my salad days, I wrote a slender essay entitled *The Nature and Significance of Economic Science*; and from time to time its contents have been the subject of criticism and discussion... I shall resume my position on the definition of the subject matter of economics...and...I expound my own conception of what I now call *Political Economy*” (Robbins 1981, p.1)

It turns out that like himself explains, Robbins' distinction was already present in the ENSES (though not with that terminology). This distinction is a fundamental one.

“[W]e must recognize that ... the methods and problems of economic science are very substantially different from those of the so-called natural sciences. This springs from the fundamental circumstance that the subject matter is an aspect of human action and therefore must be conceived as including *purpose* (Robbins 1981, p. 2, our emphasis).

Put simply, there are two distinct issues here. The first is that Robbins' famous definition of economics was positive: in the sense that it aimed at simply describing reality as it factually is:

I am not at all indisposed to accept, for purposes of after-dinner conversation, Jacob Viner's wisecrack that "Economics is what economists do." But this only shifts the question one stage further: what is it that they do? What is the *object* of their investigations? (Robbins 1981, our emphasis).

Robbins would conclude in the ENSES that the only all-embracing positive, objective definition of “what economists do” in Viner's sense is in fact the methodological one, the one usually associated with his name:

“[The] definition in terms of behavior conditioned by scarcity ... scarcity being conceived as the relationship between objects, either personal or collective, and the means of satisfying them ... in Menger's *Grundsätze* ... the limitations of goods confronted with conceivable demand is made the necessary condition of the activity of economizing ... I doubt very much that what economists do ... is not covered by this definition” (Robbins 1981, p.2)

Thus the important point is that there was no *prescriptive* content in Robbins's definition. He was not telling us what economics should (normatively) be about: he was just describing what he saw as the defining characteristic of economics in practice.

An incidental point here which will be important to what we have to say in the next section is that, since Robbins's famous definition was descriptive rather than prescriptive, it was in fact immune to many of its contemporary criticism (for example Fraser 1932, Bye 1939), which would argue that *ends are not given*. In fact, as well as the 1930's economic *theory* is concerned, Robbins was right as a descriptive definition. As a description of contemporary economics, however, the definition no longer applies.

A second and distinct issue, not do be confused despite the similar terminology, is that Robbins considered that Economics (or Economic Science) was concerned with positive analysis (in John Neville Keynes' sense), and this was separable from something different, which he labeled as political economy, related to normative propositions. Robbins would lately clarify this distinction, naming Political Economy as the *superset* of Economics, including this as its positive, objective subset, but also embracing its normative dimension.

3. The Subject Matter: Redux

3.1. Economics and the Economy

Robbins's definition of economics as the problem of static allocative choice is no longer a good positive description of contemporary economics for several reasons. One is that it represents a poor description of what economics research is about today:

“What is economics about? Answer: The optimal allocation of resources given ends. This catechism was promulgated in the 1930's ... [but] pause and reflect how poorly this captures the primary concerns of neoclassical economists nowadays: Nash equilibrium, strategic uncertainty, decision theory, path dependence, network externalities, evolutionary games, principal-agent dilemmas, no-trade theorems, asymmetric information, paradoxes of noncomputability” (Mirowski 2002).

Though not accurate as a current concrete definition of economics, Mirowski would not deny that Robbins is as right as ever in his more general point⁴ that the correct (positive) description of economics is *methodological* – economics is defined by its approach at problem solving and data analysis, not its object. As academic stars such as Steven Levitt perfectly illustrate, to economists, economics does not necessarily have anything to do with the economy.

Another is the role that dynamics have acquired at the center of analysis:

Research in economics has undergone a remarkable transformation in recent decades. A generation ago, empirical researchers were typically obliged to add dynamic and stochastic elements as afterthoughts to predictions about behavior, derived from static, deterministic economic models (...) The idea of economic equilibrium has undergone a similar evolution: it no longer carries the connotation of a system at rest (Stokey and Lucas, with Prescott 1989, p.3).

The "transformation" that Stokey and Lucas refer to has received remarkably little attention by historians of economics and economic methodologists, but that makes it no less true as a historical fact.

⁴ On which in fact he spends a much greater part of ENSES than in discussing the above concrete definition.

The problem of dynamics leads directly to the problem of expectations. Robbins' emphasis on ends that are given *a priori*, rejects a whole deal of important research, which considers that non-trivial extension of ends to a dynamic context. For instance, it is by no means clear to contemporary economists how to deal with the fundamental problem of unmeasurable risk. If agents do not know what the probabilities of several different state actions are, the identification of an intertemporal equilibrium (and the prediction of the close by actions by this agent) are by no means clear. This is the fundamental problem of choice under *Knightsian Uncertainty* (the classical references are Knight 1921, and Keynes 1921, and there exists an important strand of contemporary literature).

To sum up, even a visionary methodological definition such as Robbins' has failed to stand up to modern scrutiny. Despite its many successes, the fundamental fact is that Robbins's definition has become obsolete. This is because it was descriptive of what Robbins saw around him in the 1930's (and to a lesser degree in the early 1980's). The point is as that as methodologies change, a methodological definition will have to change too, thus pointing out to its intrinsically temporary nature. On the other hand, a pure methodological definition opens way to an easy attack which Robbins warned us against: This is the "*multiplication of activities having little of no connection with the solutions of problems strictly germane to his subject*". For Robbins there *was* an *object* after all!

Thus a first point we want to make is that the legitimate subject matter of economics, or political economy for that matter, is the economy. We take for granted the usual, standard definition of the *economy* that one may easily find in a good dictionary, with some variation: the system of human activities related to the production, distribution, exchange, and consumption of values like goods and services. If it seems a fairly obvious statement that economics is the science that studies the economy, one should bear in mind that, in fact, the defining characteristic of economics as a scientific field today is not the *object* of analysis, but rather the *method* of the analysis. Robbins taught us that seventy-five years ago, and as we shall show he gets more and more right everyday. This was not left unnoticed, and since Robbins many other brilliant economists have fallen to the appeal of defining economics as a method. Thus J. M. Keynes writes in the introduction to the Cambridge Economic Handbook Series which he edits (1934, p.6):

“The theory of economics does not furnish a body of settled conclusions immediately applicable to policy. It is a method rather than a doctrine, an apparatus of the mind, a technique of thinking, which helps its possessors to draw correct conclusions”.

The definition of economics as a method does not seem limited to any particular methodology to economics. At a very different methodological spectrum from both Robbins and Keynes, the opinion expressed by Gary Becker is more focused on optimization, but nevertheless, perhaps surprisingly similar:

“The combined assumptions of maximizing behavior, market equilibrium, and stable preferences, used relentlessly and unflinchingly, form the heart of the economic approach” Becker (1976, p. 5).

It is crucial to note that we are not saying that good results can be obtained by applying "the economic approach" to a great variety of situations, including but not limited to human behavior. It is undeniable that the methods of economics are useful for shedding light on issues such as "Why do sumo wrestlers cheat" (Duggan and Levitt 2002) or "Is there a positive correlation between the use of the designated hitter in baseball and hit batters?" (Bradbury and Drinen 2007), and this can be fine scientific work. *It just ain't economics.*

Well, you may say, this is not hot news. After all, Gary Becker was accused in the late 50's that what he was doing was not economics, and he prevailed. It is undeniable that Becker's contributions were important in a variety of senses.

But from early a method-based definition was open to attack.

Robbins' friend Hayek wrote in (1945, p.1):

What is the problem we wish to solve when we try to construct a rational economic order? On certain familiar assumptions the answer is simple enough. If we possess all the relevant information, if we can start out from a given system of preferences, and if we command complete knowledge of available means, the problem which remains is purely one of logic. That is, the answer to the question of what is the best use of the available

means is implicit in our assumptions. The conditions which the solution of this optimum problem must satisfy have been fully worked out and can be stated best in mathematical form (...). This, however, is emphatically not the economic problem which society faces.

James Buchanan (1964) would argue along similar lines in an important essay, arguing that economists should be studying exchange relationships, not allocation problems. He even used the expression "applied engineering" to describe this tendency.

However, Robbins' definition was essentially positive. It is useful here to fast-forward to where he wrote:

“It is the object of this essay to arrive at conclusions which are based on inspection of Economic Science as it actually exists. Its aim is not to discover how Economics should be pursued (...) but rather what significance is to be attached to the results which it has already achieved” (Robbins 1935, p.72).

This brings an important clue towards what Robbins meant with his famous definition of economics by the method.

In fact, Robbins does not seem to mean at any moment that economics should not, normatively, be concerned with its traditional object, as defined above. However, he also made a serious complaint concerning the “multiplication of activities having little or no connection with the solutions of problems strictly germane to his subject” (Robbins 1935, p.3), which served to point out that the relevance of the method cannot be appraised without first considering the pertinence of the subject.

Thus, economics is certainly a method, especially in practice, i.e. as its positive description, but it is also and primarily an object. In fact, the object takes precedence over the method: it is the object what has to fundamentally define it. The object of economics is the economy. Nevertheless, Robbins would agree with our compromise on this: economics is certainly about production and consumption, but it is first and foremost about the human mind, because it is the human mind the machinery behind both the supply and demand construction of the very economies.

Though the advance of economic imperialism has been steady, it is a fact that since Robbins other economists have shown some concern about these advances. For instance

James M. Buchanan wrote that: "Economics, *as a well defined subject of scholarship*, seems to be disintegrating" (Buchanan 1964, 222, our emphasis). He was criticizing the standard view at the time that the economic problem was one of allocating scarce resources among competing ends.

As discussed, Robbins's intended definition was much less narrow and method-centered than what the usual quote seems to suggest. A first crucial point is that for Robbins, preferences were exogenous and set a priori. They are the (Millian/Misesian) axiomatic key to the system. They are an accurate starting point, true either by introspection or evidence (the possibility of introspection has long been considered an advantage in the Millian tradition). On this point Robbins's position is almost the opposite of the other great methodology essay of the XXth century, that of Milton Friedman (1953): to Robbins, it is the starting assumptions that matter, and the truthful conclusions will follow necessarily from these. To Friedman, the starting assumptions are unimportant, as long as their interaction permits accurate prediction. Robbins' method is *a priori*; Friedman's method is *a posteriori*.

“[I]f by any chance, my emphasis in this respect casts any doubt on the contention that ability to predict is the sole or necessary criterion of scientific activity, I should not feel unduly depressed. I do not think that the understanding of economic phenomena hitherto achieved, although palpably imperfect, is anything to be ashamed of” (Robbins 1981 pp.3-4).

3.2. Economics and Political Economy

We now turn to the previously discussed (see section 2) distinction between (positive, strictly scientific) economics, and its superset (which in addition has a normative dimension), Political Economy. For a long time economists have been plagued with the accusation that their “science” is just propaganda. People from all political spectrum, from Milton Friedman to Joan Robinson, would complain that their research was discredited by political accusations, from all sides indeed: reporters, the general public, and their fellow economists.

It is of course clear that economics has a normative dimension to which the natural sciences do not have a clear counterpart. Yet, according to Robbins' point of view, there is no intrinsic reason why these cannot be clearly separated. If Economics or Economic Science is a subset of Political Economy, as Robbins believed, then, their union is the set of Economics itself. It is therefore the responsibility of the economists, when writing their research, to clarify where they are leaving the field of Economics and entering that of Political Economy (for example, when imposing a particular utility function which has specific distributional policy consequences). Robbins seems to agree that this perfect demarcation is possible:

“Gunnar Myrdal has devoted a whole book to the argument that, explicitly or implicitly, all propositions of economic theory, all classifications of happenings having an economic aspect, must involve judgments of value. I do not agree with this position” (Robbins 1981, p.4)

It is up to economists to justify the assumptions behind their theorems, and to take preventive defense regarding the attack that so-called “positive results” are an artificial consequence of a normative choice of assumptions. Likewise, it is up to us to admit ambiguity, even when the public asks for a number, like with President Johnson's dubbing “ranges are for cattle. Give me a number”.

In a similar vein Milton Friedman⁵ (1953, p.10) wrote:

“The choice among alternative hypothesis equally consistent with the available evidence must to some extent be arbitrary, though there is general agreement that relevant considerations are suggested by the criteria 'simplicity' and 'fruitfulness', themselves notions that defy completely objective specification”.

As Manski (2007) emphasizes, economists need to cope with ambiguity, thus cannot choose *one* set of maintained hypothesis, as Friedman prescribes, especially if they are

⁵ Who, interestingly, started F53 precisely by referring to John Neville Keynes' distinction between positive and normative economics.

“to some extent...arbitrary”. In fact, we would add, we know so little about the economy that we simply cannot afford to discard sets of hypothesis that are consistent with available evidence. This reminds us of J. M. Keynes's attributed dubbing “I'd rather be vaguely right than precisely wrong”. Well, that's just right (see also, Sutton 2002).

4. Ends and Means

“There can be little doubt that one of the greatest dangers which beset the modern economist is the preoccupation with the irrelevant – the multiplication of activities having little or no connection with the solutions of problems strictly germane to his subject”. (Robbins 1935, p.3)

In “ends and means”, Robbins discussed the implications of his definition of economics. We have suggested before that studying baseball strategy or sumo wrestling is not economics, despite the fruitful potential and practical applications of economic theory - inspired analysis to these issues. Eminent economists worrying about other fields is certainly not unusual: For instance Robbins' friend and colleague Hayek made lasting contributions to psychology and political science, just like many contemporary economists do to epidemiology, medicine, criminology, etc. The point that a good economist is never an economist only, strikes us as valid, but this is different from what kind of science he is making at each point in time.

Referring to the Klammer-Colander survey of graduate students in the late 1980's, McCloskey wrote: “Two thirds, 68 percent, said [knowledge about the economy] was unimportant [to have academic success in economics]. What then, was the number who said that knowledge of the economy was 'very important'? 3.4 percent. Student physicists, not to speak of biologists and chemists and historians, would probably give different answers to a similar question: is it important for a student of chemistry to have a throughout knowledge of chemical phenomena? Is it important for a student of history to have a through knowledge of history?” (McCloskey 1994, p. 173).

McCloskey's criticism is sharp. It is part, of course, and reflects, a broader opinion of parts of the profession (which despite the possibility of statistical correlation have no

perfect overlapping whatsoever with the mainstream/heterodox divide). A broader point that she makes quite explicitly is the accusation that economics is too much about the blackboard: “The oldsters like Samuelson and Solow, and Arrow were traumatized in their youth by the idiotic opposition to all mathematics... They continued to fight in the 1990's the battles won in the 1950's (McCloskey 1994).

The question of where to stop in the development of pure theoretical contributions with no apparent connection to reality or attempt to quantitative testing is a truly tricky one. And it was at the very heart of Robbins’ concerns. Theoretical economists do not get tired of pointing out how non-euclidean geometry ended up being useful for quantum mechanics, and other esoteric examples as such. This is often done by eminent economists: Robert Aumann, for instance, tells the story of how the “useless” (his expression) doctoral research on knot theory ended up being useful for cancer research. Similarly, Paul Krugman has a sensible metaphor where he compares the development of stepping back into rigor if less immediately applicable conclusion in theoretical economics with the development of map-making in the Age of Discoveries: the first maps of Africa and America had imprecise borders, but plenty of information about the rivers and mountains in the interior. As the standards of rigor were imposed in the following century, the border precision was increased, and with them the four-legged monsters in the interior disappeared, but with them useful, if imprecise, information about the interior, such as the above mentioned rivers.

Neither economics, nor philosophy of science, seem to have a clear answer to this “optimal span of non-empirical analytics” problem.

Let us consider an example that clearly illustrates the difficulty in solving this problem. Game theory is today pervasive in economics (not to speak of its influence on the econwannabe fields such as political science or rational-choice sociology), dominating the lion's share of microeconomic analysis and increasingly other fields, including macroeconomics. The founders of the application of the theory of games to economic behavior have prudently warned against successive generations of research furthering away from its empirical source. In particular, Von Neumann and Morgenstern made this warning explicit in their 1944 seminal book. In fact, the main accusation towards contemporary game theory is that it is an endless muddle of theoretical abstractions with

unclear empirical counterparts. Hence the apparent paradox arising from technical improvements in the discipline which are limited by empirical uselessness.

It is too easy to bash this accusation without looking at some evidence, so let us not do so. Ariel Rubinstein, in the last chapter of his 1998 book *Bounded Rationality* (Rubinstein 1998), includes a critique from Herbert Simon himself to his own book⁶.

Simon makes two strong criticisms to Rubinstein's approach. One is that Rubinstein ignores the whole literature on artificial intelligence. The other is, in Simon's own words: "At the moment we don't need more models; we need evidence that will tell us which models are worth building and testing" (p. 190). Rubinstein replies:

Models of economic theory are meant to establish "linkages" between the concepts and statements that appear in our daily thinking on economic situations... [M]icroeconomists are not prophets or consultants; neither are they educators of market agents. Economic models are viewed as being analogous to models in mathematical logic: Those models do not pretend to predict how people apply the values of truth to the statements of a natural language, or to provide instructions for their use; neither do they attempt to establish foundations for teaching "correct thinking."... The crowning point of making microeconomic models is the discovery of simple and striking connections between concepts (and assertions) that initially appear remote. (p.190-192).

This response shows how methodologically naive even the best theoretical economists can be, when facing the impossible dialogue between the beauty of analytical work and the reality of true economies.

In his days Robbins was aware of the same kind of criticism that could be raised about the uses and abuses of mathematics in economics and he makes this implicitly in *ENSES*. He was not alone. As McCloskey (1994) wonderfully describes, twentieth-century economics has a long history of eminent mathematical economists criticizing the uses of mathematics. Thus Marshall, Keynes, Hayek, Friedman, Leontief, and others have made

⁶ For a partially overlapping analysis of the same exchange, see Mirowski (2002, pp. 472-479).

their point. Sharing a similar point of view George Stigler, no maths phobic himself⁷, wrote:

“Mathematics has no symbols for confused ideas ... the increase of mastery over mathematics ... is not free. The budget equation of the mathematical economist applies also to himself: he purchases mathematical literacy with economic illiteracy. An economist, after all, is not an unemployed mathematician” (Stigler, 1946)

It is thus necessary to make clear how two separate problems exist here: the criticism about the *uses* of mathematics in economics (and social science more generally) and the *use* (singular) of mathematics in economics. The former is an important and sensible topic, while the latter is not: since any set of conclusions can be always attained by carefully choosing the initial axioms/assumptions.

We have focused on theory so far: but as Robbins recognized theory is in fact the key. What on his time would be the institutional school anchored to the infamous German-Oxford side of the *Methodenstreit*, is today “the trouble with the back box”: which is that the majority of papers in “applied microeconomics” today (which is a substantial, and media-impact influent, share of the profession's resources) in fact has no microeconomics at all, just statistics.

Thus this brings us full circle back to our case for a “conservative” definition of economics: towards what have been the traditional topics: the study of wealth, distribution, and yes, welfare, including non-material welfare, but certainly not an “anything goes” study of happiness. The down-to-earth question we have to ask ourselves is: Should really economics be about “Do sumo wrestlers cheat?”, “why do crack dealers live with their moms?”, or baseball? Unlike what some will claim, the usually a-theoretical analysis, invariably involving the fashionable I-word, “incentives”, in the conclusion, are always hardly surprising. We need more exciting results than that the expected benefits of some decision exceed those of the expected costs. This type of research may attract media awareness to economists' work, true, but is the purpose of science media awareness? When John Bates Clark medals start coming out for this kind

⁷ See Freedman (2007).

of research, then the issue becomes pointier. If we treat journalism as all that matters, one day it will be all we have.

Finally, we dodge the question why applied microeconomists publish papers without microeconomics. Robbins warned against 'The precarious nature of "sectional investigations', (Robbins 1935, p.42) i.e., extreme specialization in different fields of applied economics. McCloskey (1994) would say, on a much broader sense by the way, it is okay to specialize, but then do not forget to *trade*. Should it be the fault of the theory, or the application, or both, that many applied microeconomists don't actually use microeconomics but just econometrics, we do not know. But the black box has to go.

5. The Nature of Economic Generalizations

The propositions of economic theory, like all scientific theory, are obviously deductions from a series of 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" (Robbins 1935, p.78-79).

Rationality is a central concept in economics. It is even very difficult to conceive economics at all without the concepts of rationality and rational human behavior. However, the fact is that while rationality is a pervasive concept in economics, economists do not have a concrete agreement or understanding of what rationality actually is or means. Different definitions of rationality coexist in different strands of the literature, and from our perspective many discussions would collapse in fruitlessness if only a concrete definition of rationality was agreed upon.

The modern microeconomic concept of rationality is the more straightforward. Rationality is characterized by a pair of clear, but static conditions: an individual is rational if his preferences are complete (which means that he always has a preference over something) and transitive.

Robbins wrote:

Insofar as the idea of rational action involves the idea of ethically appropriate action (...) no such assumption enters the economic analysis (...) But in so far as the term rational is taken to mean merely 'consistent', then it is true that an assumption of this sort does enter into certain analytical constructions (Robbins 1935, p. 91).

And he went on to say:

There is a wider sense, too, in which the conception of rationality as equivalent to consistency can be understood as figuring in discussions of the conditions of equilibrium. It may be irrational to be completely consistent as between commodities, in the sense just described, just because the time and attention which such exact comparisons require are (in the opinion of the economic subjects concerned) better spent in other ways ... [This is the] *marginal utility of not bothering with marginal utility* (Robbins 1935, p.92, our emphasis).

There is much truth and wisdom in Robbins' observation that there is a marginal utility of not bothering with marginal utility (which he attributes to Böhm-Bawerk, but we know Robbins was often too generous in his attributions to precursors). This is quite profound in the sense that some reflection on its meaning sheds a lot of light into some long-standing confusions in economics.

First, take the statement as it is. If we accept the principle of marginalism, i.e. that optimal decisions are taken at the margin, then it is self evident that by its very nature this principle is recursive. Thus there is a marginal utility of not bothering with the marginal utility of the marginal utility of the marginal utility, and so on. This notion is extremely powerful: for example, it renders the currently fashionable concept of “rational inattention” in macroeconomics to one of its trivial corollaries.

At a different level, it seems to annihilate in a stroke the possibility of Buridan's methodological individualist donkey which starves because he cannot decide between two stacks of hay of equal size and quality (Robbins refers to this fable but in a different context).

At the more particular level of economics, much of modern confusion about rationality seems to dissipate if we take that observation into account:

What information consumes is rather obvious: it consumes the attention of its recipients. **Hence a wealth of information creates a poverty of attention**, and a need to allocate that attention efficiently among the overabundance of information sources that might consume it (Simons 1971, pp.40-41).

It is no random coincidence it was in the same time and place, early sixties and Carnegie Mellon University (at the time the Carnegie Institute of Technology), that the two fundamental XXth century schools of rationality were born. In fact, John Muth's conception of rational expectations was developed in reaction to Herbert Simon's bounded rationality program. Muth asserted that expectations “are essentially the same as the predictions of the relevant economic theory.” (Muth 1961). Robert Lucas arrived in 1963. Edward Prescott took his doctoral dissertation there at this time and Thomas Sargent was around for a year. The logical conclusion from the research of individuals like Sargent and Lars P. Hansen at the University of Minnesota in the 70's and throughout the 80's, led to the (sometimes implicit) contemporary observation that rational expectations can be interpreted as a long run equilibrium⁸ of a learning (possibly evolutionary/adaptive) process. Recent experimental evidence seems to confirm that people act rationally in the neoclassical sense in environments which are familiar to them, but may make mistakes and inconsistencies (of the source emphasized by Kahneman and Tversky) in those where they have no experience (List 2004). This seems perfectly consistent with Robbins's observations, while not contradicting the *equilibrium* condition that: “In Economics, as Jevons remarked, bygones are forever bygones” (Robbins 1935, p. 52).

Robbins wrote:

“von Mises ... [argues that] human action ... [is] at all times *rational* in the sense that given belief in the range of technical knowledge available to individuals or collections of individuals, action must be *consistent*. I confess that I have never been able to understand

⁸ Our usage of the term equilibrium is not in rigor the same as Robbins's: See the Stokey-Lucas quote above in section 3.

this contention: I should have thought that one of the main practical functions of economic science was to enable us to detect inconsistencies in plans... “But, putting this conception aside, I would have thought that the contention that explanations of economic relations must involve considerations of purposes, implicit or explicit, to be relatively noncontroversial. Unfortunately this is not so” (Robbins 1981 p.2, original emphasis).

Robbins conception of inconsistency detection (what he would call the *basis for political practice* as in the very first quote of this paper), differs from the above discussed notion presented by Rubinstein, according to whom: “[Economic] models do not pretend to predict how people apply the values of truth to the statements of a natural language, or to provide instructions for their use; neither do they attempt to establish foundations for teaching “correct thinking.” (Rubinstein 1998, p.191).

It is precisely this kind of caveat, this attention paid to the correct use of generalizations, that makes Robbins specially cautious about the need of testing them with the functioning of the real world. In other words, as Robbins himself put it:

“[In Economics], the ultimate constituents of our fundamental generalizations are known to us by immediate acquaintance. In the natural sciences they are only known inferentially. There is much more reason to doubt the counterpart in reality of the assumption of individual preferences than that of the assumption of the electron” (Robbins 1935 p.105).

With this additional warning Robbins reveals the difficulty in dealing with individual preferences, which is the first step to a wider discussion concerning the (im)possibility of interpersonal utility comparisons. This last example taken from Robbins’ work leads us to a final appraisal of the relevance of the current reading of ENSES.

[to be completed]

6. Conclusion

Following the line of reasoning put forward by Robbins in his famous *Essay*, we have argued that the only consistent definition of economics is that it is the science that studies a particular object, the economy, and should therefore not be exclusively concerned with problems of method, as frequently occurs today. We have also argued that the *demarcation* between positive and normative approaches to the subject matter of economics is both possible and recommended, as Robbins strongly believed. Those interested in the development of economics *proper*, in the scientific, purely objective, part of our field, will certainly find in such a statement not only the updating of Robbins's concern, but also the basic condition for strengthening the very subject of the discipline.

We have given a series of examples and considered a set of open issues in economics, some of which are regarded as puzzles inexplicable with the existing economic theory. We hope to have convinced the reader that the jump from these puzzles to the total dismissal of neoclassical economics, as it is often done – either directly by actively pushing other approaches or indirectly, by taking a largely a-theoretical econometric approach as a response to this seemingly impossibility of reform – is unwarranted and corresponds to throwing the baby away with the bathwater. This was also Robbins's claim.

The nature and significance of economics “redux” is the expression that better spells out the need for a regular renewal of the discussion of the issues that make it worth remembering the publication of Robbins's *Essay*, after seventy five, one hundred and fifty, and any other further multiple of years, since these are truly intertemporal issues for better scientific practice.

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