Economic Theory: Economics, Methods and Methodology

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Economic theory comprises three types of inquiry. One examines economic phenomena, one develops analytical tools, and one studies the scientific endeavor in economics in general and in economic theory in particular. We refer to the first as economics, the second as the development of economic methods, and the third as the methodology of economics. The same mathematical result can often be interpreted as contributing to more than one of these categories. We discuss and clarify the distinctions between these categories, and argue that drawing the distinctions more sharply can be useful for economic research.

THÉORIE ÉCONOMIQUE: ÉCONOMIE, MÉTHODES ET MÉTHODOLOGIE

La théorie économique comprend trois types d'approche. L'une examine les phénomènes économiques, l'autre développe des outils d'analyse, et la derniére étudie l'effort scientifique en économie en général et en théorie économique en particulier. Nous appelons la première approche « l'économie », la seconde, « le développement de méthodes économiques » et la troisième, « la méthodologie de l'économie ». Le méme résultat mathématique peut souvent étre interprété comme contribuant à plusieurs de ces trois catégories. Nous discutons et clarifions les distinctions entre ces différentes catégories, et soutenons qu'établir des distinctions plus nettes peut s'avérer utile pour la recherche économique.

Keywords: economic theory, economic methods, economic methodology

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INTRODUCTION

When considering the body of work that is generally referred to as "economic theory," some distinctions may help economists interpret the literature, place papers in context, evaluate papers, identify trends, and assess future directions for research. For example, a given mathematical result often can be interpreted either as a positive or as a normative statement.[1] Being explicit about this distinction can add clarity to academic discourse, forestalling criticism of a positive paper because "this is very unjust," or of a normative paper because "people don't behave that way," and can make clear whether a paper should be motivated in terms of examples, observations and data or by appeals to principles, aspirations and introspection.

We suggest another distinction that may prove useful in discussing economic theory. Economists often draw informal or implicit distinctions between three modes of theory, which we refer to as *economics*, *economic methods*, and the

1. We use the terms "positive" and "normative" in the general meaning of "is" versus "ought," without further distinctions between description and explanation, recommendation and prescription, etc. Importantly, we also do not examine here questions of how one separates the value judgments inherent in normative work from ideally-objective economic analysis, the extent to which one can claim that "economics" supports a normative conclusion, how one assesses or tests a normative argument, and so on.

methodology of economics. In many cases there is little risk of confounding these different types of academic study. However, some results in economic theory can be hard to place or can fall into multiple categories. We provide and discuss definitions that will more clearly distinguish these categories.

We take "economics" to include the study of various social phenomena in economics and related fields, such as political science, finance, decision theory, game theory, and so forth. By "economic methods," often shortened to "methods," we refer to the development and study of techniques that economists may employ in their research. Such techniques are sometimes borrowed from other fields, including mathematics, statistics, computer science and machine learning, and are sometimes developed by economists for specific applications. The "methodology of economics," often shortened to "methodology," takes the scientific endeavor of economists as the object of enquiry.[2] Thus, both economics and methodology belong in the social sciences, where the former deals with economic behavior, and the latter deals with the behavior of economists. Methods, by contrast, are tools that are designed to be used by scientists, but do not model a reality.

We focus on theoretical rather than empirical work. The distinction between these two causes less confusion than the other distinctions discussed here, but is nonetheless worth a definition. We refer to an analysis as "theoretical" if it relies primarily on logical or mathematical arguments, and as "empirical" if it works primarily with data, including administrative data, experiment results, historical documents, survey responses, and so on.[3] Some cases are difficult to classify. For example, computer simulations may be viewed as theoretical, consisting of the mathematical analysis of numerous examples, while they may also be considered empirical, with the computations giving the analysis many of the properties of a random sample. Clearly, a paper may contain both theoretical and empirical analysis, and many do. When discussing particular results, this distinction tends to be sufficiently straightforward that we will take it for granted.

There are many examples of works we would obviously classify as "economics." The majority of the papers that get published in mainstream journals fall into this category. There are also many examples of papers we view as "methods." Most econometric theory papers would be so classified, as would much of the work that gets published in economic theory, game theory, and mathematical economics journals. Descriptions of how the proof of the existence of a competitive equilibrium was developed or how expected utility theory came to be accepted in economics are methodological, as are admonitions that economists should put more

2. "Methodology" is typically defined to mean either "a body of methods, rules, and postulates employed by a discipline" or "the analysis of the principles or procedures of inquiry in a particular field." We adopt here the latter meaning. This research is often referred to as the philosophy of economics when it takes a normative approach, asking how economists *should* conduct their research, and is often referred to as the sociology of economics when taking a positive approach, asking how the discipline of economics *actually* operates.

3. We do not attempt to tell apart different types of empirical work, and, indeed, the line between interpretive anecdotes, cases studies, and statistical analysis can be blurred.

emphasis on the empirical implications of their models or the accuracy of their predictions.

We first argue that methodology should be interpreted more broadly. The second section begins with examples of work that we view as usefully being characterized as modeling and analyzing the scientific work of economists, namely as belonging to "methodology," despite not obviously appearing to do so. We also note cases are often open to multiple interpretations. The third section offers examples of methods. The fourth section discusses the boundaries between economics, methods, and methodology and offers examples of each. The fifth section illustrates the usefulness of these distinctions in understanding economic theory.

Two caveats may be called for. First, the categories discussed here are suggested for specific results or contributions, rather than papers. Indeed, a paper would often make contributions in more than one category. Further, determining the main import of a given paper is a matter of subjective judgment. The main contribution of a particular paper in the eyes of economists is, in the final analysis, an empirical question. Our task here is mostly to suggest the categories, and to this end we use examples that we believe would elicit a large degree of agreement. Second, the categories are not offered with any value judgment. For example, referring to a result as "methods" as opposed to "science" is meant to be neither derogatory nor laudatory. It merely suggests what type of questions should be asked in understanding and evaluating the contribution.

EXAMPLES OF METHODOLOGY

In the following we will refer to an economist (E), who studies economic phenomena, and a methodologist (M) who studies E's work. Importantly, these refer to logical entities and not to specific researchers or even papers.

There are many results which we tend to think of as "methodological." While we do not attempt a complete taxonomy here, a few classes seem to emerge. First, there are results about assumptions, showing what a set of assumptions implies, or does not imply. We identify here three subclasses: results that show that a set of assumptions is 1) too strong—these include impossibility or *reductio ad absurdum* results; 2) too weak—in particular, results whose message seems to be "anything is possible"; or 3) equivalent to another set of assumptions, without making a clear statement about their strengths. Second, there are results that seem to be mostly about definitions. Some are positive, showing that a definition makes sense, and some are negative, convincing the reader that a formal definition misses its intended purpose.

Our conception of methodology does not require M to study *how* E does her work—M need not discuss whether E first constructs models and then seeks

interpretations (or vice versa) or first seeks general results or counterexamples, and so on. We view M's work as methodological if its primary effect is on how E does her work.

Assumptions Are Too Strong: Impossibility Theorems

It is hard to overstate the impact of Arrow's Impossibility Theorem (Arrow [1950]). It is studied in economics, political science, law, and philosophy, and it appears to have changed the way many scholars think about their disciplines. It is a standard fixture in first-year graduate courses in economic theory.

Technically, Arrow's theorem states that there exists no function aggregating profiles of preference orders while satisfying certain (arguably intuitive) properties. Does it describe an economic or political reality, i.e., is it a contribution to economics? In some sense, the answer is in the affirmative. Indeed, any polity we choose would serve as an example in which such a function is not implemented. But this hardly seems to be the point of the theorem. Focusing on aggregation of preferences as in referenda, most polities that can be thought of as democracies use some form of plurality or majority voting, and it had already been proved, almost two centuries before Arrow's result (Condorcet [1785]), that the basic decision rule they employ does not always aggregate transitive orders into a transitive order. It thus cannot come as a surprise that countries adopting plurality rule use aggregation functions that fail Arrow's conditions. And yet, personal experience and casual observation suggest that the theorem does surprise, and even shocks readers.[4] What message does it send, then, and why is it important?

We argue that the theorem is best described as a contribution to economic methodology. Imagine that the researcher, E, perhaps having been exposed to Condorcet's paradox, sets herself the goal of coming up with social decision rules that would perform better than majority rule.[5] The methodologist M (Arrow in this case) comes along and points out that the quest for certain properties is futile and should better be discarded. This impossibility result is then the beginning of a new philosophical debate. One may question Arrow's IIA assumption, or introduce domain restrictions, or alter some other conditions so as to proceed with the scientific project with some hope for designing better social systems. Arrow's theorem identifies the constraints and provides the framework for such inquiry, and thus directs subsequent research. The formal model helps sharpen the conditions that one might wish a function to satisfy, and the result aids research by ruling out many futile directions.

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4. The authors have heard at least two prominent economic theorists who cited Arrow's impossibility result as a reason for which they had chosen economic theory as a vocation.

5. Arrow's path to the impossibility theorem started in just this way, with a search for a voting method that would avoid the pitfalls of the Condorcet paradox, turning to a quest for impossibility only after repeated failures.

Along similar lines, the impossibility results of Hurwicz [1972], Gibbard [1973] and Satterthwaite [1975] also seem to say little about reality. Again, it seems silly to ask for examples of social choice functions that are potentially manipulable—these are everywhere. But these results can push experts away from futile quests for better mechanisms or democratic systems that cannot exist, and can direct research in more promising directions. Moreover, they may have important implications for philosophical discussions, for example, in legitimizing strategic behavior.

Yet another example of an impossibility result that is a thought-provoking puzzle is provided by Mongin [1995]. Mongin showed that Harsanyi's [1955] utilitarian aggregation result cannot be extended from the risk to the uncertainty setting (where individuals may vary not only in their tastes but also in their beliefs). This was interpreted by many as a critique of the Pareto criterion, which might reflect "spurious unanimity" (Mongin [1997]). Thus, Mongin's result convinced many economists that the Pareto principle was not as compelling as it had seemed at first sight. This realization influenced economists' work by making them reconsider the definitions they had been working with.[6]

The classification of a result as "economics" vs. "methodology" depends not only on the question, but also on the answer. For example, had Arrow proved that preference aggregation functions that satisfy his conditions do exist, the result could have been more readily interpreted as "economics." For the sake of argument, imagine that majority vote between any two alternatives were the only such aggregation function (see May [1952] and Goodin and List [2006]). In that case we would have viewed the result as normative economics, supporting a very specific decision rule. The impossibility result, by contrast, says little about how economies should be run, and more about what economists should be doing in their research. In between these extremes, we could imagine possibility results that are not constructive: as discussed below, a result that says that a certain theoretical construct exists, but doesn't identify it or provides ways to compute it, can be interpreted partly as "economics," and partly as "methodology." It contributes to the discussion of what the economy may look like, but also to the question of what economists should study.

When discussing these impossibility results, we imagine the social scientist E attempting to find better voting schemes. Thus, her work is theoretical and normative: her goal is to find an algorithm for social choice and to convince a polity to adopt it. The methodologist's work is also theoretical and normative. In the following examples we discuss cases in which E engages also, or mostly, in a positive endeavor.

Assumptions Are Too Strong: *Reductio ad Absurdum*

6. Arrow's Independence axiom had a similar fate: given the impossibility result many economists concluded that it was not as compelling as it had seemed to them at first sight.

There are theoretical results that make economists question their basic (often implicit) assumptions, or the appropriateness of certain theoretical definitions. These arguments often proceed by pushing models to their logical extreme, and then noting that the result is counterintuitive or even absurd.

A class of results designed to question assumptions includes the no-betting and no-trade results (starting with Aumann [1976], Geanakoplos and Polemarchakis [1982], and Milgrom and Stokey [1982]). These are "puzzles," showing that a list of seemingly innocuous and rather prevalent assumptions lead to counter-intuitive results. By and large, these results are not making a statement about economic reality. It would be inappropriate to argue, for instance, that Milgrom and Stokey are unsuccessful economists because their theorem proved that there can be no trade, while in reality trade exists. Rather, their no-trade result is (and should be) interpreted as saying, "Fellow economists, if we wish to explain trade, we can't hold on to all of these assumptions." That is, the no-trade theorem is an impossibility result. However, while results such as Arrow's [1950] refer to the impossibility of an economist E who engages in normative economics, here the subject of analysis is an economist E who deals with positive economics: E tries to explain the fact that trade exists, and the methodologist M (Milgrom and Stokey) look over E's shoulder, saying "These assumptions, taken in conjunction, will lead nowhere." Observe that, as in the case of the impossibility results in social choice, the classification of a result as "economics" or "methodology" might depend on whether its answer on the question of existence is in the affirmative or the negative. If Milgrom and Stokey were to show that, under their assumptions, trade *were* possible, we could think of their result as "economics": it would be discussing an economic phenomenon and explaining it. But, as the result states that no such explanation is possible, it is more readily interpreted as "methodology," telling fellow economists where an explanation is not going to be found.

The main difference between the results discussed here and the impossibility results discussed in the previous subsection is the nature of work of the economist E: in the above, E was mostly interested in normative economics, trying to design better voting schemes, and M told her that she expects too much of the system she hopes to design. In the present discussion we focus on an economist E who's interested in a positive exercise, trying to explain a phenomenon (such as trade), and M tells her that some assumptions, taken together, will not deliver the desired outcome. In both cases, however, M's role is mostly normative: by showing that E might be assuming too much, M suggests a more fruitful course for E's work, whether the latter is normative or positive.

Again, the distinction between economics and methodology might differ between possibility and impossibility. A possibility result, such as Harsanyi's [1955], is readily interpreted as part of "economics": it makes a normative claim in favor of a specific way of making social choices. An impossibility result, by

contrast, does not say anything concrete about these choices (neither positive nor normative). Rather, it is more readily interpreted as a result in "methodology," discussing the work of economists.

Assumptions Are Too Weak: Possibility Theorems

Seemingly at the opposite extreme from impossibility results, possibility results are also often methodological. The Sonnenschein-Mantel-Debreu theorem (Debreu [1974]; Mantel [1974], [1976]; Sonnenschein [1973]) establishes that any putative excess demand function satisfying homogeneity of degree zero and Walras' law is indeed the excess demand function for some competitive economy. The theorem thus says that the theory of competitive equilibrium says very little: it does not make any predictions beyond two obvious facts that can be regarded as accounting identities (i.e., that only relative prices matter and that an asset for one person is a liability for another). This result makes no predictions and tells us nothing about the economy. Nor does it provide new tools for economists to use in their research. However, it provides a caution to economists that they should not seek circumstances under which the hypothesis of competitive equilibrium alone allows one to draw inferences about economic outcomes. In this sense, it fills a role similar to impossibility theorems in identifying lines of research that cannot lead to useful results.

In a similar vein, consider folk theorems in repeated games (Fudenberg and Maskin [1986]). This again is an "anything can happen" result—any payoff profile that is feasible and individually rational can be supported as the equilibrium outcome of the repeated game, if the players are sufficiently patient. As in the case above, this result provides no help in characterizing the outcome of a repeated interaction. However, it warns economists about the predictive content of a theory, in this case, pointing out that the hypothesis of subgame-perfect equilibrium in a repeated interaction does not allow one to draw any nonobvious inferences, and directs research toward the study of coordination problems and equilibrium selection.

Boldrin and Deneckere [1990] is yet another example of this type. The paper shows that a simple dynamic model of an economy can readily give rise to chaotic dynamics. Again, "anything can happen." And, as in the previous examples, because anything can happen, nothing can be predicted. This paper differs from the Sonnenschein-Mantel-Debreu theorem and from folk theorems, in that the latter study the implications of a set of general assumptions, while the former focuses on the inferences that can be drawn from imperfectly measured initial conditions. But in all three cases the possibility of almost all scenarios implies the impossibility of prediction.

If these results are viewed as results in economics, they are not very meaningful. According to the standard Popperian view, theories should say what

cannot happen, thereby providing predictions and risking refutation. Stating that "everything goes" or "who knows?" is hardly the goal of a scientific theory. But when viewed as results in methodology, they become very powerful. Precisely because everything can happen in terms of economic behavior, something *cannot* happen in terms of scientific behavior: the economist will not be able to rule out any scenarios. By pointing out the impossibility of prediction, these papers are useful in diverting effort away from unproductive inquiries and focusing attention on the important aspects of an interaction. We thus view them primarily as normative contributions to economic methodology.

Equivalence of Assumptions

Revealed Preference

Afriat [1967] provides a path-breaking result about the possibility of explaining consumption data by utility maximization. The paper characterizes the databases that are compatible with utility maximization, and finds the intriguing result that concavity of the utility function can be assumed without loss of generality. The result is widely known, and has sparked interest in similar questions under different assumptions and with various decision models in mind. (See Chambers and Echenique [2016] for a survey of the literature.)

Revealed preference results illustrate the study of the methodology of economics. Imagine an economist E who observes a database of household consumption within various budget sets. Assume that the economist wishes to estimate the household's utility function for the purposes of prediction. Thus, she tries to fit a utility function from a given class (say, continuous and monotone) to the data. Her first task is therefore to describe the data. She can be likened to a painter who paints a picture of reality on canvas.[7] The painter's painting is the counterpart of the economist's formal model of utility maximization; the reality that the painter sees is analogous to the database of economic choices that the economist observes.

Let us now introduce the methodologist M (Afriat in this case). M joins the scene and tries to formally describe E's act of modeling. M is therefore analogous to a ("second-order") painter who paints a ("first-order") painter in the act of painting or a lecturer who explains how E's painting captures the observed reality. On M's canvas one would expect to see both the reality that E observes and the canvas that she paints on. Similarly, in the formal theorem that M proposes, there will be formal representations both of the data that E observes and of the models that E develops. Indeed, to state Afriat's theorem one needs to formally describe a

7. This analogy is due to Wittgenstein's [1922] *Tractatus*, where proposition 2.12 reads, "The picture is a model of reality."

database, such as the bundles $\left(x\left(p_i, m_i\right)\right)_i$ that were observed to be chosen out of their respective budget sets, and utility functions u that can rationalize these choices. That is, M's formal model describes both E's data and her formal model.

In this depiction we describe E's work as empirical. She fits a formal model to actual data. Note that we are silent on the goal of her model; she may be using the estimated utility function for positive or normative purposes. M's work in this case is theoretical, proving a theorem that says which databases E would be able to explain when choosing among certain classes of formal models. This theoretical result tends to be interpreted as a contribution to economic methodology, making a statement about the models used by economists. It may be interpreted as a positive contribution, identifying the circumstances under which economists *can* usefully invoke models based on utility maximization, but it is more often interpreted as a normative statement, arguing that there are circumstances under which economists *should* appeal to utility maximization. The fact that two classes of utility functions are shown to be observationally equivalent (i.e., the class of concave utility functions is observationally equivalent to the class of all utility functions) can also be taken to make a normative contribution to economic methodology. In particular, it could be read as saying, "Let us not waste our time on the theoretical debate, whether the utility function is concave, because this debate is void of empirical content." Revealed preference results can also have other interpretations. Some would argue, typically appealing to some mix of intuition, introspection and casual observation, that the generalized axiom of revealed preference is either obviously innocuous or clearly a desirable consistency condition. This allows one to interpret revealed preference results such as Afriat's theorem either as an indication that most economic behavior *is* consistent with utility maximization, or that people *should* maximize utility. Revealed preference results are then interpreted either as positive or normative contributions to economics proper rather than to economic methodology. It is precisely because some results can have more than one interpretation that we find the distinction useful.

Preference Orders

Axiomatic derivations of decision rules, such as the celebrated axiomatizations of expected utility maximization by von Neumann and Morgenstern [1947] and by Savage [1954], can be interpreted both as economics and as contributions to economic methodology. When interpreted normatively, these results can be viewed as addressing a decision-maker and attempting to convince her to use a particular model for her decision-making. This is indeed their most prominent application in the field of decision analysis (as opposed to applications of decision models in economics). One can imagine explaining that "these axioms make good sense—no one would deliberately and knowingly make choices that violate these axioms. You should endeavor to satisfy these axioms, which is to say that you should endeavor

to behave as an expected utility maximizer." While the normative statement is about the model of choice, it is not directed at fellow scientists but at the decision maker herself. As such, we tend to categorize it as "economics": it is what normative social science is expected to do in its primary application.[8]

Consider next a positive interpretation of these axiomatic results. As argued in a previous paper (Gilboa et al. [2019]), the role of such results in describing or explaining data is not entirely clear. After all, a positive theory should be judged by its empirical success, and the latter would not change as a result of introducing a novel representation of the same theory. Because a characterization theorem cannot change the degree to which the theory fits data, one might tend to dismiss axiomatizations when decision theory is used positively (whether the economic theory to which it is applied is positive or normative). However, Gilboa et al. [2019] list several reasons for which such theorems might affect the way economists conduct research, all having to do with the choice of a conceptual framework within which theories are to be developed. Importantly, all these reasons are methodological in nature: they describe the role of axiomatizations as rhetorical devices used in the discourse among economists. (See Moscati's [2016] description of the way von Neumann and Morgenstern's axioms were used by Savage to convince prominent economists that expected utility theory was the right tool for economic analysis under risk.[9])

Axiomatizations of utility maximization under *certainty*, as in Debreu [1959], can also have both normative and positive interpretations, and similar interpretations as economics or philosophy. The normative interpretation seems to be weaker in this case, due to the absence of structure: a decision-maker can be convinced by the axioms that she would like to be a utility maximizer, but the axioms provide no clues as to the nature of the utility function involved in this maximization. This contrasts with the case of choice under uncertainty, where the axioms imply that the decision criterion should be linear in probabilities, i.e., should be an expected utility function. And yet, it is a very important result because of its methodological application in positive economics: an economist who wishes to describe economic choices might be convinced that she should use utility maximization as a model of household choices.

Taking the methodological interpretation of axiomatic decision theory results, we can again think of an economist E who attempts to fit choice data by utility or expected utility maximization. The methodologist M describes E's work, and he needs to formally describe both the data that E observes and the model that E constructs. While the formal model appears in E's work explicitly, the data she discusses would typically not. It is M's job to propose a formal model for the data

8. Admittedly, one could argue that, by dint of being a normative claim about the choice of a model, this claim is methodological in nature. According to this view, any decision-maker who is expected to comprehend abstract axioms and their implications is considered to be part of the (normative) scientific endeavor to some extent.

9. As opposed to being "just a special case of convex preferences."

as well. As in the examples above, he may argue that E's data are given by a binary relation over a set of alternatives. Alternatively, another philosopher might argue that the data are choices from sets (which may contain two alternatives or more). Thus, a philosopher typically makes some assumptions in her model of E's work, and these assumptions may be questioned for empirical validity. This question should, however, be kept separate from the question of the empirical validity of the axioms themselves, or of E's theory.

Stochastic Choice

Luce [1959] pioneered stochastic choice theory, which, like revealed preference theory, has seen a revival in recent years. (See the manuscript in preparation by Strzalecki [2021], based on his 2017 Hotelling lecture.) As in the case of revealed preference theory and axiomatic decision theory, this body of work lends itself to methodological interpretation: the stochastic choice theorist is a methodologist M who models the work of the economist E. E studies actual choices, and attempts to fit a model to them, viewing choice as inherently stochastic. M has two types of formal entities in his model: one is the E's formal model, that is, the stochastic choice model, and the other is a formal model of E's data: E's database is summarized by probability distributions, capturing empirical frequencies of choice in the database.[10]

A Comparison of Characterization Results

There are therefore at least three methodological approaches that model the phenomenon of economists fitting utility functions to data: revealed preference, preference orders, and stochastic choice theories. One aspect in which they differ is the formal representation of the database that the economist (E) has: Debreu [1959] assumes a complete binary relation on an infinite set of bundles; Afriat [1967] only considers a finite collection of observations of choices, and these are made out of budget sets; while Luce [1959] considers choices between pairs of alternatives, but allows these choices to be described by probabilities. We can think of three methodologists (M1, M2, M3) proposing these three models of E's work, and imagine them having a disagreement about the positive question, what do databases look like? M1 (say, Debreu) suggests that E can observe a complete and transitive binary relation between any pair of bundles. M2 (Luce) criticizes the empirical validity of M1's model, saying "Real data are never that neat; they are better captured by probabilities of choice rather than by a binary relation." Then M3 (Afriat) comes along and says, "Well, this isn't very accurate either. In fact E only

10. An interpretation of stochastic choice models as economics is less obvious, as it is less intuitive that one could argue that others should aspire to the stochastic choice axioms.

gets to observe finitely many choices from well-structured sets, and both your models are too idealized to be realistic." Viewed thus, the three methodologists offer three different models for presumably the same phenomenon, and they have a disagreement that is empirical in nature. It should not come as a surprise that the choice of "the most accurate model" might depend on the application. For example, if E is a theorist who wants to justify the assumption of utility maximization in her model, she may engage in the mind experiment of infinitely many observations of pairwise comparisons. Debreu's axiomatization might then be a good model of her work, and the axioms might convince her that utility maximization is a reasonable assumption for her purposes. By contrast, if E does empirical work and attempts to estimate utility functions from household expenditure data, Afriat's model might be more realistic, and his theorem would therefore be more relevant for identifying the conditions under which E would be able to rationalize a database by utility maximization. Finally, if E does empirical work that is closer to marketing, she might have many observations of choices from finite subsets, but no access to the household's budget, and then Luce's model might be the most realistic.

A Definition Has Merit: Existence Results

Existence results, such as Nash [1951], may also be interpreted in more ways than one. Taken literally, these results can be viewed as positive economics, arguing that under certain assumptions a given notion of an equilibrium would exist. Moreover, in some cases they are also backed by results about the convergence of dynamic processes to an equilibrium. For example, potential games (Monderer and Shapley [1996]) possess equilibria, and, furthermore, there are reasonable dynamic processes (of better-response-dynamics) that can be shown to converge to such equilibria. Results of this nature can thus be viewed as economics, in the sense of being "first-order" and stating something directly about the phenomena of interest.

However, this is not the case for the general existence results. No intuitive dynamic process is guaranteed to converge to a Nash equilibrium in all games, and, similarly, no such process has been proved to converge to an equilibrium in a competitive economy. One might ask, what is the significance of the existence results, then? Why should we care if all games (or all economies) possess an equilibrium if we have no guarantee of convergence to that equilibrium? And how much comfort can we derive from the general existence result if in some games, as in Shapley [1964], there is a unique equilibrium which is unstable under any seemingly reasonable dynamics?

Along similar lines, the existence result for general equilibrium of Arrow and Debreu [1954] is not proved in a constructive way. There are processes that converge to equilibria generically, and some of these might be thought of as actual descriptions of the economy, offering a predictive theory of convergence to

equilibrium. Yet, it appears that economic theory cherishes existence results in a way that by far exceeds their predictive power. We suggest that the philosophical import of such results can explain this discrepancy: when suggesting a solution concept to the community of theorists, one might be expected to ask, "And what shall we do should the solution not exist?" It is therefore reassuring to know that, whatever game (or competitive economy) a theorist analyzes, she will not be at a loss for predictions. In other words, existence results are powerful rhetorical devices in the methodological discourse among economists; they sometimes say less about economics than about the work of economists.

A Definition Misses the Point

A result that sheds doubts on definition is the "calibration" result of Foster and Vohra [1998]. Their result showed that the definition of a calibrated predictor (suggested by Dawid [1982]) was too weak, and that it offered a test that one could pass without having any substantial knowledge about the phenomenon predicted. The result was not important because it was used by predictors to pass such tests; rather, it changed the way economists were conducting theoretical work by pointing out that a seemingly plausible definition was actually missing the intuitive concept it was trying to capture.

EXAMPLES OF METHODS

Contributions to economic methods often come from outside economics. Paradigmatic examples include theoretical results in statistics or econometric theory, which guide empirical research in economics, or fixed point theorems that are developed within mathematics and can be used in theoretical research in economics. Methods are also developed within economics, often with an example of an application. Thus, a paper in economics would often have contributions both to methods and to economics (or to methodology).

Mathematics and statistics tend to offer methods that are not specific to economics. Estimation techniques and fixed point theorems can be useful to psychologists and biologists just as they are to economists. There are, however, results that are quite clearly about economics, and that we might still think of as methods. Consider, for example, the revelation principle in mechanism design (Gibbard [1973]; Rosenthal [1978]; Myerson [1979]). It is undoubtedly a key result in this literature. Without the revelation principle, there is no obvious way to describe the "set of possible mechanisms" and no way to talk about an optimal mechanism or to draw the line between feasible and infeasible outcomes. The revelation principle cuts through this difficulty by showing that there is no loss of

generality, when identifying possible equilibrium outcomes, in restricting attention to the well defined set of incentive compatible direct revelation games.

This allowed questions concerning the optimality of mechanisms to be defined and solved, and provided methods that are now standard. We tend to think of such a result as belonging to "methods" because, in and of itself, it is devoid of any statement (either positive or normative) about an economic phenomenon. It is a useful tool for a theorist to prove results. Yet, it is rather specific to economics, dealing with mechanisms, messages, and so forth. Indeed, the papers that suggested the principle proceeded to use it to say something about a reality. Gibbard pursued a methodological direction, using the revelation principle to establish an impossibility result for social choice functions. Myerson used the revelation principle to design a bargaining solution that is most obviously motivated as a contribution to economics. Rosenthal points in both directions, establishing both impossibility and possibility results for arbitration schemes.

There are other fields of economic theory in which results seem to be in the category of methods rather than science (economics or methodology). For example, the early results in repeated games proceeded by presenting candidate equilibrium strategies and then verifying that they indeed constitute an equilibrium. The ideas of decomposability, self-generation, and enforceability (Abreu, Pearce and Stacchetti [1986], [1990]; Fudenberg and Levine [1994]) provided the tools to characterize the set of equilibria, opening the analysis to new questions and more powerful results. Perhaps the most common use of these techniques is to establish more folk theorems (e.g., Fudenberg, Levine and Maskin [1994]), which we have placed in the methodology category. This may reflect the fact that the idea of enforceability becomes especially tractable in the limit as players become arbitrarily patient. At the same time, these techniques can be used to construct equilibria in games with fixed discount factors that are of interest for their economic content, giving us a contribution to economics (e.g., Abdulkadiroğlu and Bagwell [2013]).

SHARPENING THE DISTINCTIONS

With the help of these examples, we can suggest some guidelines to distinguish the three categories. We then return briefly to the distinctions between normative and positive work and between theoretical and empirical work, and offer examples of the type of work falling into each category.

Economics, Economic Methods, and the Methodology of Economics

Some work falls clearly into a particular category. A paper documenting recent changes in the US income distribution is an exercise in economics. A paper proving asymptotic properties of an estimator is clearly in the methods category.[11] A paper examining the dangers of *p*-hacking in economic journals is a study in the methodology of economics. Explanations of how economic theories can serve as analogies or critiques (Gilboa et al. [2014], [2018]) fall into the methodology of economics, as does the current paper.

Other work is more difficult to classify or straddles categories. We suspect that such papers are sometimes misunderstood, and may be inappropriately evaluated due to confusion regarding their intended contribution. Avoiding such confusion is the main reason to suggest criteria to tell the categories apart. We emphasize again that many papers make contributions in more than one area. Moreover, papers that turned out to be influential in terms of substance often received substantial following also in terms of methods. Thus, if we insist on classifying papers into categories, this classification will not only be fuzzy and subjective, but also context-dependent, and change with time. Yet, we believe that, at least in order to judge specific contributions, these categories can add clarity.

We think of work in economics as being motivated primarily by the problems to which it is applied. Theoretical work in economics may involve sophisticated modeling and may require the reader to navigate a sea of technique, but is motivated by the connection of the model and the technique to an economic problem. One might pose the questions "Is it about the economy?" or more generally, "Is it about an external reality?" If yes, then we can think of this as a contribution to economics.

Papers that address economic questions often provide answers or make predictions that can be investigated empirically, and so one characteristic often associated with contributions to economics is that they can be empirically tested. In many other sciences, the vulnerability to testability is regarded as the sine qua non for being a contribution to the discipline. In contrast, testability is neither necessary nor sufficient for work to fall into our category of economics. Work in methodology may make positive statements about how economists conduct their work that are testable. Perhaps more importantly we have argued elsewhere (Gilboa et al. [2014], [2018]) that there are many ways to make contributions to economics that involve no testable statements.

Where would one draw the line between economic methods and methodology? We think of "economic methods" as dealing with how economists might do their work, while "the methodology of economics" deals with what economists actually do (or should do). That is, a piece of research that suggests a tool economists might use (in empirical, experimental, or theoretical work) is considered to be part of

11. Much of the work that we refer to as "methods" is theoretical work addressed at empirical economists. But some would be addressed at theoreticians. As we have noted, the revelation principle is a contribution to methods.

"methods," while a statement about what economists do, or should do with these tools, by contrast, is categorized as "methodology." Of course, one would typically expect an economist working on a model to believe and argue that other economists should be interested in the model, causing the work to shade into making a normative methodological contribution, and to believe and argue that the model is useful to directly address economic problems, causing the work to shade into economics.

Where would one draw the line between economics and methodology? One possible criterion is the following: consider a PhD seminar in which students are asked to present papers. The students are technically sophisticated but do not know the culture or history of the profession—anything that has to do with the sociology of economics and history of economic thought is the instructor's job to add. We suggest that a paper falls into the "methodology" category if a student would not be regarded as having understood the paper and appreciated its significance without discussing the work of economists in their presentation.

For example, we tend to classify Milgrom and Stokey's no-trade paper as "methodology" because we would be disappointed by a student who presented it and concluded by stating "and thus we know that there is no trade." We also would not be happy with a student criticizing Milgrom and Stokey for being poor economists as they predicted no trade while trade exists. Rather, to say that the student understood the paper we would like to hear the message, "and thus we face the difficulty that a combination of assumptions routinely used in economic models leads to such a counterfactual result." Similarly, we would not appreciate a presentation of Arrow's theorem concluding with the statement that "and so we should not be surprised to see many dictatorships." "Understanding" the theorem would require, to most of us (in the role of the teacher), a conclusion along the lines of, "and so economists must abandon at least one of these seemingly intuitive assumptions in their quest for a social choice function." By contrast, consider Samuelson's [1937] model of discounted utility model. It surely changed the way economists conduct research. Yet, a student could present the paper, concluding that "this model provides a tractable way to examine intertemporal consumption decisions" without leaving us with the feeling of having missed the point. That is, the student can discuss only the paper's contribution to economics. The instructor may complement the discussion with an explanation of how the paper also had an effect on the use of mathematics in economics and other thoughts on its historical importance, but the heart of the paper is a (potentially testable) model of economic behavior.

Normative vs. Positive

In general, work in economics tends to be positive, while becoming more normative as economists edge into consulting and policy advice. Techniques in

mechanism design may be used in a positive analysis, explaining why existing institutions elicit certain behaviors, or in a normative analysis, suggesting how these institutions might be redesigned. We tend to view work in economic methods as neutral: economic methods provide tools of all sorts, but make no statements about economic reality, apart from the implicit normative claim that the tools analyzed should be used. By contrast, the methodology of economics is a scientific activity—it provides insight into the working of economists rather than the working of the market, but still squarely within the social sciences. It can naturally be either normative or positive, while tending to be normative. In the case of axiomatizations of decision rules, or existence results for solution concepts, we have emphasized the normative aspect, where a theorist M addresses an economist E and says which model she should use. But such results can also have positive interpretation. For example, Afriat's theorem can be read as stating that analysis that has been conducted using general utility functions could also have been done using concave functions. Such a claim might be useful in generating predictions without necessarily taking a stance regarding the "right" model one should use.

Within the context of normative analysis, papers falling into different categories may be directed at different audiences. Normative work in economics may be directed at people at large or at policy makers, while normative work in the methodology of economics is typically directed at economists.[12] We could distinguish these with yet more terms, but will rely on context to distinguish them.

Theoretical vs. Empirical

We have focussed on economic theory. More generally, work in economics can obviously be either theoretical or empirical, with the mix varying over time and across fields. Work in economic methods can similarly be either theoretical or empirical—we need both theoretical models and the collection and processing of data. Work in the methodology of economics tends to be theoretical, and our discussion has centered on such work, but it can also be empirical. Economic theorists often make casual observations about the popularity of methods and it is not uncommon to hear positive as well as normative statements about the norms in the profession. Much of the work in the history of economic thought can be viewed as an empirical investigation of economic methodology, as can be work on the current sociology of the profession.

Connecting the Dots

12. Again, we note that there are gray areas. Savage's [1954] representation results could be viewed as a prescription for how people might want to make decisions, or how economists might want to model people making decisions.

We can offer some representative examples of the contributions that fall into various categories:[13]

	Theoretical	Empirical
Economics, positive	Theoretical IO Heckscher-Ohlin model Endogenous growth models Real business cycle models	Measuring tax incidence Estimating demand Predicting deficits Financial stress tests
Economics, normative	First welfare theorem Second-best theory Optimal taxation	Discussions of capital and inequality minimum wage
Methods	Revelation principle Econometric theory Fixed point theorems Self-generation	Collecting, cleaning and documenting data Validating survey instruments
Methodology, positive	"Life among the Econ" "Economic models as analogies"	History of economic thought Documenting publication biases
Methodology, normative	Voting impossibility theorems Axiomatic decision theory No-trade theorems Calibration results Sonnenschein/Mantel/Debreu Chaotic dynamics	Discussions of *p*-hacking experimental registries fishing for hypotheses replication

There will always be ambiguous cases. Spence's signaling paper (Spence [1973]) contributed to economics, offering an explanation of why college graduates might enjoy a wage premium even if their education provided no productivity enhancement, but also contributed to economic methods, providing a tool that could be used in many other settings (e.g., Milgrom and Roberts [1986]). The introduction of the Heckit procedure (Heckman [1979]) was a contribution to both the methodology of economics (in arguing that selected samples constitute a specification error for which one should correct) and to methods (by presenting an estimator to do so). In these cases it can help to be clear about the nature of the exercise each time the original work is invoked.

The category into which a paper falls can be ambiguous, and can depend on the current state of the discipline and the perspective of the reader. Barro [1974] argued that in an economy with perfect capital markets and consumers who maximize discounted expected utility, and who are either infinitely-lived or are members of infinitely-lived dynasties, the mix of taxation and government debt in financing government expenditures has no effect on the economy. Instead, any effect the government has on consumers' current balance sheets will be offset by consumer activity in the capital market. Bernheim and Bagwell [1988] push this

13. The two entries in positive, theoretical methodological contributions refer to Leijonhufvud [1973] and Gilboa et al. [2014].

idea to its extreme, showing that in an economy with sufficient links between consumers, virtually every government policy has no effect.

Where do we put these arguments? One could make a case that these are contributions to economics, identifying reasons why government policy may be ineffective. This may in turn induce work on identifying and estimating the effects of forces that dampen the effects of government policy. In this sense, these contributions are analogous to real business cycle models, which made the case that an efficient outcome can still exhibit employment fluctuations. Alternatively, one might argue that it is obvious that government policy does have a significant effect, and that the contribution of these papers is then to show that the combination of seeming innocuous assumptions (discounted expected utility maximizing dynastic consumers, perfect capital markets, and so on) leads to clearly absurd results. In this sense, the arguments are analogous to the no-trade theorem. The choice between these categories depends upon whether economists in general, or the reader in particular, take it for granted that government policy has an effect on the economy. Empirical and historical studies may resolve the first of these questions, while the second is a matter of introspection. It should then be no surprise that views may differ on the nature of these contributions, though it is still important to be clear as to how they are viewed when discussing or using them.

Similarly, the revenue equivalence theorem (Vickrey [1961]) shows that under certain conditions, a variety of auction mechanisms all generate the same expected revenue. On the one hand, one may view this as a contribution to economics, explaining that under certain conditions, people conducting auctions should be indifferent as to what form of auction they choose. On the other hand, it can be viewed as a contribution to methods. A researcher may be modeling a resource allocation procedure that involves an auction at some point. If the salient feature of the auction is the expected revenue it raises, then the researcher may invoke the revenue equivalence theorem to model the auction as (say) a second price auction, regardless of any considerations as to what types of auctions are actually used in such settings. This may bring tractability to the problem without sacrificing economic content. The result has thus become a tool, used to identify models that are equivalent apart from any consideration of the economics to which they are applied.

Economics or Philosophy or Sociology?

Methodology, by definition, deals with scientific activity as its subject matter. It is therefore related to the philosophy and the sociology of science, depending on whether it makes normative or positive statements about the scientific method. One could even imagine philosophical discussions as described in the second section as part of research conducted in a department of philosophy of science. Clearly, we do not aim to suggest a reorganization of academic institutions. There are several

reasons for which the methodological research discussed here should be conducted in departments of economics. Such research is likely to require detailed, context-specific knowledge of the relevant subject matter, reflected in a movement on the part of philosophers of science away from a quest for general methodological principles that cut across all disciplines. The multiplicity of interpretations that motivates this note provides another reason for maintaining contact with the subject matter when studying methodology, and hence studying the latter within the discipline. This also renders it all the more important to bear in mind that some research in "economics" is best thought of as the philosophy or sociology of economics.

DISCUSSION

Is the Distinction Useful?

The distinction between economics and methodology can aid in describing and predicting trends in economic theory. As an example, we suggest to consider a pattern that, we conjecture, can be observed in methodology, and whose counterpart in economics would be less common: 1) a particular tool of analysis rises to the coveted status of a "gold standard"; 2) it is considered the one and only way to conduct analysis for a certain period of time, which may last decades; 3) at some point in time it becomes widely accepted that the tool is not necessarily a panacea for all problems of analysis, where this recognition may be coupled with the suggestion of a specific alternative tool; 4) this results in a flurry of activity in search of alternative tools; 5) the flurry of activity produces some ideas that are robust and incorporated into standard methods, while others are discarded, at which point the topic fades from interest.

For example, Nash equilibrium was suggested as a general tool of analysis of interaction in Nash [1951]. It took a few decades, as well as Harsanyi's [1967], [1968a], [1968b] contribution, to allow the concept to become the gold standard for economic analysis. However, Selten [1965], [1975] pointed out that the concept of equilibrium may not be perfect (no pun intended), and his examples were accompanied by alternative tools. Starting in the late 1970s, many solution concepts were suggested, most of which refined Nash equilibrium, whereas others generalized it. At some point (based on the authors' casual observations) there was a sense that the discipline is less interested in the question of finding the "right" notion of equilibrium.

A similar pattern could be detected in the context of decision under risk. Expected utility theory (EUT) came to the foreground in the 1940s, with von Neumann and Morgenstern's book (in particular, the second edition of 1947). It soon became the method of choice for modeling decision-making under risk, and,

Allais [1953] notwithstanding, retained the status of an unchallenged gold standard for roughly three decades. Only with Kahneman and Tversky's [1979] findings, and the suggested alternative of Prospect Theory, did EUT come under sufficient pressure to allow for other tools of analysis. This development ushered a spurt of activity in developing models of decision under risk. Yet, interest in the question has soon waned. The examples are not perfectly analogous, as Prospect Theory was designed to deal with violations of EUT, and not to refine it. But there seems to be some similarity between the two cases in terms of the speed with which new methods are developed, once there is more than one, and in the relative short duration of the interest in such new methods. Similar patterns can probably be discerned in the case of social choice, decision under ambiguity, and other subfields dealing with methods.

Clearly, fads exist in all branches of economics and in other academic disciplines as well. New topics of research pop up, whether by raising new questions or by doubting existing answers, and they often spur intellectual activity. However, we suspect that methodology differs from economics in the speed with which interest in the question wanes. This conjecture is empirical in nature, both because it should be tested by data on papers published, dissertations written, etc., and because the very distinction between economics and methodology is an empirical matter, depending on the common views of economists. We have no claim to have conducted such an empirical study or of having proved our conjecture. However, if we assume, for the sake of the argument, that the pattern discussed is indeed more typical of methodology than of economics, we can try to explain this phenomenon, along the following lines.

There are two reasons for which a field might, or even should prefer fewer methods to many, and ideally adopt a single method. The first is convenience. Without necessarily justifying this practice on normative grounds, it stands to reason that scientists who are interested in a given phenomenon do not wish to spend resources studying many methods on the way to their question of interest. The second one is the signaling aspect, which we refer to as "Grice's principle of scientific method": a scientist who wishes to make a claim about a given phenomenon would normally attempt to establish it using standard tools. Should she, for the sake of her analysis, also introduce new methods, her audience would be justified in wondering how robust are the results obtained. Whether the scientist analyzes data using non-standard statistical methods, or solves a theoretical model using a little-known game-theoretic solution concept, her listeners might draw the plausible inference that the reported results do not hold when standard methods are used.[14]

14. This is similar to Grice's principle (Grice [1975]) for natural language, stating that people opt for the simplest utterance that conveys their message. Thus, a more complex utterance implicitly suggests that the simpler ones do not hold.

The Gricean scientific inference appears rather natural and we can think of it as a positive theory. Moreover, it can be partly justified on normative grounds as well. If a discipline insists on a single method, it may well be criticized for narrow-mindedness. But if, at the other extreme, it allows each study to adopt a newly-developed method, it will find it difficult to tell which results are robust.

Neither the convenience nor the signaling arguments apply to studies in economics (as opposed to methodology). Interest in a new economic question will often be sparked by a new phenomenon (provided by reality) or by a particularly innovative paper. But we do not expect this interest to reach satiation as quickly as it could when methods are concerned, nor do we expect interest to wane as quickly as it often does for methods. Work in economics is ultimately guided by the need to understand economic phenomena, make relevant decisions, and implement economic policy. If new economic quandaries arise and persist, we can expect work in economics to do likewise. Our analysis in this note is positive, for the most part. We attempt to better understand some of the scientific discourse that we observe, and clearly define some terms that are used in it. We stop short of making any normative statements, and do not purport to know what balance the profession should strike between known and novel methods, avoiding the dangers of methodological stagnation on the one hand and futile intellectual exercises on the other.

Some Questions

We noted in the fourth section (subsection "Connecting the Dots") that the classification of some papers can depend upon one's view of the intent of the authors and the prevailing practice in the discipline, which may not be obvious. We close by pointing to some papers that pose particular challenges in this respect (to the extent that the authors of this paper sometimes come to different views).

The idea of rational expectations (Muth [1961]; Lucas [1972]) and the Lucas critique that came out of it (Lucas [1976]) revolutionized macroeconomics. Is this a contribution to economics, or to methodology? One could perhaps argue either way. The current authors have heard claims that Muth's intention was to portray the idea of rational expectations as preposterous. A reading of his paper is inconclusive, though one notes that he discusses the cobweb theorem and appears to focus on the idea that expectations are not invariably rational. Under this reading, this is a contribution to methodology. Alternatively, the importance of Lucas' work appears to stem not from criticism of the idea of rational expectations, but from changing the way economists think about the economy and government policy. The Lucas critique is commonly invoked as an important insight, no matter what one thinks of rational expectations models—it is not uncommon to argue that in evaluating the revenue to be raised by a change in tax policy, one should incorporate the

behavioral response.[15] Under this reading, this literature is a contribution to economics.

The Spence [1973] signaling model similarly opened a new literature. One might argue that the Spence signaling model was a contribution to economics, offering an explanation for the college wage premium that did not require that education enhance productivity. However, the model itself is too stylized to be taken seriously as a model of the role of education in the labor market, and much of the influence of the model has been in applications outside of education—there are now signaling explanations for a vast variety of activities. An alternative argument is that the model is a contribution to methods, providing the technique that made this variety of applications possible. But what would this contribution be? Spence did not formulate his argument precisely as a game of incomplete information (that task fell to Cho and Kreps [1987]), and the basic techniques for working with games of incomplete information had already been put in place by Harsanyi [1967], [1968a], [1968b]. The answer depends partly on how demanding one is in recognizing a contribution to methods. One could reasonably argue that Spence used a game of incomplete information to present a new way of thinking about information transmission in markets of incomplete information.

We have argued that the categories of economics, methods, and economic methodology can be useful in thinking about how economists think about work in economic theory. In many cases, the categorization of a result or paper is obvious. However, as mentioned above, a given result may contribute to more than one category, and the degree to which various economists would emphasize the various contributions of specific results remains an empirical question.

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15. It is this behavioral response that lies behind the Laffer curve. Economists may debate the position of the economy on the Laffer curve, and whether the Laffer curve provides a relevant guide to tax policy, but none doubt its basic features, namely that tax rates of 0% and 100% raise no revenue, while intermediate tax rates can raise revenue.

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