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"Economic Models as Analogies, Second Version"

by

Itzhak Gilboa, Andrew Postlewaite, Larry Samuelson, and
David Schmeidler

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Economic Models as Analogies [*]

Itzhak Gilboa[†], Andrew Postlewaite[‡],
Larry Samuelson,[§] and David Schmeidler[¶]

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Abstract

People often wonder why economists analyze models whose assumptions are known to be false, while economists feel that they learn a great deal from such exercises. We suggest that part of the knowledge generated by academic economists is case-based rather than rule-based. That is, instead of offering general rules or theories that should be contrasted with data, economists often analyze models that are "theoretical cases", which help understand economic problems by drawing analogies between the model and the problem. According to this view, economic models, empirical data, experimental results and other sources of knowledge are all on equal footing, that is, they all provide cases to which a given problem can be compared. We offer complexity arguments that explain why case-based reasoning may sometimes be the method of choice; why economists prefer simple examples; and why a paradigm may be useful even if it does not produce theories.

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[†] HEC, Paris, and Tel-Aviv University, tzachigilboa@gmail.com.

[‡] University of Pennsylvania. apostlew@econ.sas.upenn.edu.

[§] Department of Economics, Yale University, Larry.Samuelson@yale.edu.

[¶] The InterDisciplinary Center in Herzliya, and TAU, davidschmeidler@gmail.com.

1 Introduction

Many economists think of their discipline as a successful social science. Economics relies on rigorous and non-trivial mathematical and statistical analyses. The paradigm of microeconomics is viewed as a unified approach that can deal with all problems of social interaction, and it is indeed adopted by other disciplines. Economics seems to be a popular topic among students, and economics professors appear to be in high demand within the academic world and outside it.[1]

At the same time, economics is also heavily criticized on several grounds. First, people are often disappointed by the quality of economic predictions. Second, the presumed objectivity of the "science" of economics has been brought under attack in post-modern circles, arguing that economists support particular theories not because of their objective veracity but because they help promote the economists' own interests. Lastly, the basic assumptions of economic theories have been harshly criticized by psychologists, presumably showing in laboratory experiments that these assumptions frequently do not hold.

The first two types of critiques do not suggest a clear alternative to the way economic research is conducted. Economists would have liked to generate better predictions, but these are not always possible. Indeed, there is no reason that it would be easier to predict stock market crashes than it would be to predict earthquakes. In fact, the opposite is true: because people react to theories, predicting human behavior has theoretical bounds beyond those that are shared by the natural sciences. In any event, economists would welcome new methods of prediction that would prove more successful than the current ones.

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[1] Throughout this paper we make various claims about the sociology of economics without providing any concrete evidence. We draw on our personal impressions of the field over several decades, based on many discussions with colleagues, editorial work, and the like. However, our casual observations are not backed by any scientific data.

The critique of objectivity is an important reminder to any scientific activity, and to the social sciences in particular. Yet, rather than embracing the impossibility of perfect objectivity as a normative standard, economists are inclined to say that they try to be as objective as they can. As long as there is no obvious alternative method of economic analysis that is guaranteed to be more objective, economists seem to be justified in conducting the research they conduct.

The third critique seems the most puzzling. If the assumptions of economics are all wrong, why do economists keep using them? Why do they develop sophisticated mathematical models based on such flimsy foundations? This question is our starting point. We elaborate on it and discuss two additional puzzles as our motivation in Section 2. We then offer a possible resolution to these puzzles in Section 3, namely that some of the reasoning in economics is case-based rather than rule-based, and that economists view themselves as generating the "cases" to which real problems might be analogous. True to the method of our discipline, we construct a model (in Section 5) that can formally explain why economists sometimes choose this mode of research. This model is, as can be expected, highly idealized and unrealistic. Readers who reject the method of microeconomic modeling are unlikely to be convinced by the formal model any more than they will be convinced by the verbal explanation, and they may wish to skip this section. Yet, some readers may find that the model adds clarity to the arguments. In Section 6 we discuss the virtues of a general paradigm, or, to be precise, of the standard language that such a paradigm employs, in light of the preceding discussion. Section 7 closes with a discussion of the lessons we think we have learned from this exercise.

2 Puzzles in the Sociology of Economics

In this section we describe three puzzles that, we will later argue, may be explained by understanding the way economists think about models. It is worthwhile emphasizing that the questions we raise and answers we purport to provide are descriptive rather than normative.[2] We start from puzzles about the state of affairs in economic research, as we perceive it, and we offer explanations of how and why it has evolved. However, we do not make here any claims about the optimality of the method of research that economics has adopted.

2.1 Assumptions are False

That the assumptions of economics are false is one of the most poorly kept secrets in science. Already in the 1950s, Milton Friedman felt that the issue was important enough to deserve a serious treatment. In Friedman (1953) he made the claim that economists should not worry if their assumptions (on individual behavior) are wrong, as long as their conclusions (regarding market phenomena) are relatively accurate. Friedman's defense came under various attacks, which will not be reviewed here. We only mention in passing that, since Friedman proposed his defense, microeconomics has changed its focus, and nowadays more instances of individual behavior are considered part of the domain of economics than in the past, rendering the defense more problematic.

Starting in the late 1960s, Daniel Kahneman and Amos Tversky launched a decades-long project that is sometimes summarized as "proving that people are irrational". Amos Tversky used to say, "Give me an axiom [on individual behavior] and I'll design the experiment that refutes it". Indeed, the psychological literature today is replete with examples of such experiments. After

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[2] We use these terms as is common in economics, where "descriptive" or "positive" refers to a description of reality as it is, and "normative" refers to a recommendation, or a statement about what reality should be.

several decades in which economics has practically ignored the Kahneman-Tversky project, in the mid-1990s certain changes were noticeable. Behavioral economics has been developed, as a way of making economic models more realistic by incorporating psychological findings. The field has recognized the importance of behavioral economics as evidenced by awarding the Nobel Prize to Daniel Kahneman in 2002. Many economists remain skeptical about the field, but, importantly, not because they believe that the classical assumptions are literally true.

The question then arises, why does economic theory engage in relatively heavy technical analysis, when its basic premises are so inaccurate? Given the various violations of fundamental economic assumptions in psychological experiments, what is the point in deriving elaborate and carefully proved deductions from these assumptions? Or, why do economists believe that they learn something useful from analyzing models that are based on wrong assumptions?

2.2 Mathematization

A scientific field can sometimes be reduced, in principle, to another. Chemistry is, in principle, reducible to physics, biology to chemistry, and psychology to biology. By the same token, the social sciences, namely, economics, sociology, and political science, are in principle reducible to psychology. Of course, these reductions are highly theoretical and no one would seriously suggest that the behavior of states should be analyzed by studying the motion of elementary particles. Yet, it is often useful to think in terms of the reliance of one scientific domain on another.

One typically finds a heavier reliance on mathematical analysis as one moves down the reduction chain. Physics is inarguably the most mathematized field, chemistry is less mathematical, and so forth. However, economics seems to be an exception to this rule. Economics is reducible to psychology and it does indeed use psychological findings. But it engages in mathematical analysis that appears to be, for the most part, more sophisticated than that employed by psychology or even biology.

There is no a priori necessity that more basic fields will be more mathematized than the fields that rely on them. Hence, the phenomenon we mention here is mostly a curiosity. But it may serve as a hint that economists think of their mathematical models differently than do other scientists.

2.3 The Scope of Models

Daniel Kahneman once noted[3] that psychologists and economists treat models very differently: psychologists are careful to define the scope of applicability of their models very precisely. Trying to avoid refutations of their theories, or failure to reproduce their findings, they seek a narrow definition of the applicability of the model or the theory in question. By contrast, he argued, economists tend to find their models useful in a wide variety of examples, viewing the latter as special cases of their model.

Clearly, these observed tendencies are not meant to be sweeping generalizations, and whatever differences in scientific culture there may be, they can result from sheer fads. However, one is tempted to ask whether cultural differences between the fields might reflect differences in the way these disciplines view the nature of knowledge that they are supposed to produce.

2.4 Related Literature

A widely accepted observation is that the use of models in modern economic theory is sometimes quite different from its use in other sciences, as well as in some sub-fields of economics itself. Indeed, our casual sampling of colleagues and coauthors suggests that most economic theorists found it necessary to discuss the methodology of economic modeling in classes and in interaction with scholars from other disciplines.

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[3] At a talk at the Cowles Foundation, Yale University, September 2001.

Many economists and philosophers have also written on this topic. While we do not provide here an exhaustive survey of the philosophical and methodological literature on the topic, we mention several contributions.

Gibbard and Varian (1978) likened economic models to paintings, drawing, and caricatures. They argue that there are economic models that are supposed to mimic reality, as do paintings; others are supposed to simplify reality as do drawings; and yet others are meant to be exaggerated and distorted depictions of reality, as are caricatures. Hausman (1992) pointed out that economic theory models differ from econometric models, and that the former can be viewed as explorations. Maki (1994) highlighted the role of modelling as isolation, and more recently (Maki, 2005) argued that models can be viewed as (thought) experiments (as well as that experiments can be viewed as models). Sugden (2000, 2009, 2011) discussed models in economic theory (as well as in mathematical biology) as "credible worlds" that are used to reason about reality. He describes these models as often lacking a direct motivation in terms of an unexplained phenomenon, as well as concrete guidance about the applicability of these models. He also emphasizes the role of subjective judgments of similarity in using these models. Rubinstein (2006) likens economic models to fables or fairy tales. As such they are in his view only remotely related to reality, reaching absurd conclusions, and being only remotely related to reality, and not directly testable. Grune-Yanoff and Schweinzer (2008) highlight the role of stories in applying game theory. Cartwright (in press) compares models to parables and to fables, arguing that the latter have a moral, corresponding to a model's conclusion. Walliser (2011) provides an extensive taxonomy of the use of models in economics, ranging from the more standard scientific practices to the more unique to economics.

Sharing many of the views above, this paper aims to highlight the similarity of the reasoning in parts of economic theory to case-based reasoning as studied in psychology and as practiced in statistics. These analogies can help us understand why social scientists might be prone to develop models that are merely theoretical cases, as well as why and when such a practice may be useful. According to this view, models are the same type of entities as are empirical data, experimental results, and other sources of observations.

We first describe case-based reasoning as manifested in fields other than the philosophy of science: as a description of how lay people tend to think in everyday life, and as method for analyzing data by statisticians. We then apply this view to the philosophy of economics, and argue that it can explain some of the puzzles in the way refutations of theories are treated in economics. Finally, the case-based model will allow us to explain the preference for simplicity and the adherence to a common paradigm, as explained in the following sections.

3 Case-Based Scientific Reasoning

3.1 Case-Based Reasoning

Case-based reasoning is also known as analogical thinking. As opposed to rule-based reasoning, in which the reasoner engages in the formulation of general rules, or theories, in case-based reasoning one only finds similarities between different cases, and uses these similarities to draw analogies. Case-based reasoning and rule-based reasoning appear in human reasoning both for the purposes of making predictions, classifications, and diagnostics and for making ethical and legal judgments.

The term “case-based reasoning” was coined by Schank (1986) (see also Riesbeck and Schank (1989)). However, the discussion of this type of reasoning dates back to Hume (1748) at the latest. Rule-based reasoning was formally studied already by the ancient Greeks, in the development of logic.

Rule-based reasoning has several advantages over case-based reasoning. First, a rule is a concise description of a regularity, compared with a large and ever-growing database of cases that conform to this regularity. Second, and perhaps relatedly, formulating a small set of general rules that conform to the database of cases gives people a feeling of understanding and explaining a phenomenon in a way that the database of cases does not. Thus, even if the two methods perform equally well in terms of prediction, there is a preference for rule-based approaches, and one is often willing to sacrifice some accuracy of prediction in return for the compactness of rules and the associated feeling of "cutting nature at the joints". However, when simple rules do not seem to be satisfactorily accurate, people might resort to case-based reasoning, making predictions in each problem by re-drawing analogies to past cases in the database.

3.2 Psychology, Statistics, and the Sociology of Science

Psychology has long recognized that people may reason using general rules, or theories, as well as using analogies to specific cases. It is interesting to note, however, that the two modes of reasoning exist also in statistics, which is not attempting to model human reasoning but to provide tools to improve it.

The basic problem of statistical inference is finding the underlying distribution that governs a data generating process based on observations. However, for the sake of prediction, modern statistics often uses the entire database of observations, without attempting to summarize it in a simple rule. Kernel estimation methods in non-parametric inference[4] and k-nearest neighbor approaches[5] are, in this sense, case-based methods: they do not summarize the available data in a simple rule; rather, they retain the database and every new problem is compared to the entire database in order to generate a prediction.

Thus, both research in psychology and the practice of statistics recognize

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[4] Starting with Akaike (1954); see Silverman (1986) for a survey.

[5] See Fix and Hodges (1951, 1952).

case-based reasoning alongside rule-based reasoning, either as a descriptive account of how people make predictions or as a normative recommendation regarding how they should be making predictions. Both are relevant to the sociology of science: psychological research enriches our understanding of the way people, including scientists, make predictions. Statistics, on the other hand, attempts to systematize effective methods for making predictions based on past observations, which is arguably the basic task of a scientist. Thus, there is reason to believe that scientists may sometimes be thinking in a case-based rather than rule-based way. We proceed to argue that this is what happens in economics, and viewing parts of research in economics as case-based helps understand the puzzling phenomena with which we started.

Before proceeding we emphasize again that our focus is on the descriptive question, of how economists tend to think about their models, rather than on the normative question, of how they should be thinking about them. The fact that statisticians sometimes use case-based methods is an indication that such methods are not foreign to scientific activity. One may further argue that this fact also suggests that case-based methods are sometimes optimal, and that, when rule-based methods perform poorly, relying on analogies may be the right thing to do. We find this claim plausible, but we do not attempt to substantiate it here.

3.3 How Does it Work?

We suggest that economic reasoning is partly case-based, where one role of theory is to enrich the set of cases. That is, the analysis of a theoretical model can be viewed as an "observation" of a new case. Such a case is not real; rather, it is theoretical, and observing it is akin to a gedankenexperiment, that is, an observation that is arrived at by pure logic. An observation of this type is new only to the extent that one has not thought about it before. But if the question has not been previously raised, or if the proof is not trivial, one learns something new by reading the result. (See Maki (2005) for a related view of models as experiments.)

Consider the following example. Akerlof's (1970) celebrated "lemons market" paper presents an example of buyers and sellers of used cars. The example makes certain general assumptions about the agents' behavior and information, as well as more specific assumptions and even particular numerical values. Under some such assumptions, it can be shown that the market will collapse completely. This example does not inform us of a new observation from the field or about a laboratory experiment. Nor is it a new finding from a long-forgotten archive or the result of a lengthy computer simulation. It is a mathematical proof, which happens to be rather obvious post-hoc. And yet, it is highly insightful, and economists tend to think that it has changed the way they think about markets.[6]

Despite the fact that this example can be stated as a mathematical result, it may be more useful to think about it as a case rather than as a general rule. As stated, the example can be viewed as the claim "I have observed a case in which idealized agents, maximizing expected utility, with the following utility functions and the following information structure, behaved in such and such a way". The relevance of this observation for prediction will depend on the perceived similarity between the idealized agents and the real agents one is concerned with, the similarity between the situation of the former and that of the latter, and so forth. An economist who is interested in real agents would therefore have to judge to what extent the situation she studies resembles the idealized situation in the "case" reported by Akerlof.

It is natural to think of experimental and empirical data as inputs for case-based reasoning as well. Indeed, the notion of *external validity* of an experiment involves the degree of similarity between the experiment and the real problem one is interested in. An economist who is asked to make a prediction in a given problem will then use case-based reasoning to learn

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[6] Indeed, Akerlof was among the recipients of the Nobel Prize in 2001 for this contribution.

from empirical data, experiments, theoretical models, and perhaps also historical examples, casual observations, and computer simulations. All cases, real, experimental, and theoretical, are aggregated, weighing their similarity and relevance, to generate predictions for the case at hand. In this sense case-based reasoning does not endow any type of information—empirical, experimental, or theoretical—with any privileged status.

When one engages in rule-based reasoning, one is expected to state rules that are accurate, for the most part. To this end, the domain of applicability of the rules should be clearly defined. Observing counter-examples to the rule suggests that the rule has to be revised, or that its domain should be restricted. By contrast, when one employs case-based reasoning, there is no domain of applicability, and no universal statements are involved. A formal model that is offered as a theoretical case does not come with the specification of the domain of problems to which it may be likened. This similarity judgment is often hinted at by the economist analyzing the model, but it is not part of the formal model. Moreover, the readers of a model may not agree with its author about the problems that resemble it. Rule-based knowledge is not complete without the "user's manual" that specifies the domain of applicability. By contrast, case-based knowledge allows for greater flexibility, separating the "hard" knowledge of cases from the "soft" judgment of similarity.

Rules can be refuted by cases.[7] By contrast, cases are not contradicted by other cases. Typically, for a given prediction problem different cases will suggest different predictions. The reasoner should then consider the totality of cases that make a certain prediction, judge their similarity, and compare it to that of each other possible prediction. The same practice is followed when some of the cases are theoretical. For example, assume that a theoretical analysis of the "ultimatum game" (Guth, Schmittberger, and

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[7] Often, a rule is stated or interpreted probabilistically, and it can only be refuted statistically, that is, by a database of cases.

Schwarze (1982)), in which utilities are only defined by monetary payoffs, suggests that player I will offer a minimal amount to player II, and that player II will accept the offer. Next assume that an experiment reveals a different outcome. If one conceives of the model as a general rule, one would have to conclude that the rule was violated, and perhaps re-define its scope of applicability. By contrast, if the theoretical analysis is construed as a case, as is the experimental result, the two coexist peacefully. Given a new prediction problem, an economist who is asked to make a prediction would have to ask herself, "is this real problem more similar to the theoretical analysis, assuming common knowledge of rationality with purely monetary payoffs, or is it more similar to the result of the experiment?" In making this judgment the economist may draw on her knowledge of the players, the amounts of money involved, the time they have to make a decision, and so forth. Neither the theorist nor the experimentalist are expected to state *a priori* which real life problems belong to the same category as their case. Their job is only to contribute these cases as additions to the literature, and to leave similarity judgments to the practitioners who might use these cases in real life problems.

3.3.1 Is This Science?

Can case-based reasoning be a basis for science? The answer depends, of course, on the definition of "science". It is worthwhile to note, however, that case-based reasoning can generate refutable claims, if it is coupled with (i) an algorithm for the computation of similarity judgments; (ii) an algorithm for the generation of predictions based on judgments, such as kernel classification, a nearest-neighbor method, and the like. Should one commit to a similarity function and to the way in which it should be used, one would make predictions that can be tested and possibly refuted.[8]

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[8] For example, if one uses kernel estimation, one may test hypotheses about the kernel (or similarity) function, as developed in Gilboa, Lieberman, and Schmeidler (2006). Kernel estimation is hardly a candidate for the learning from theoretical cases, because repeatedly

As mentioned above, the common practice in economic theory is to use models without a clear specification of the similarity function that should be used to apply them to concrete problems. An economic theorist who offers a model prepares the ground for a practitioner who should employ her judgment in using this model; but the theorist's contribution falls short of a testable prediction. As such, models that are theoretical cases might be viewed as pre-scientific: they can be complemented to become standard scientific claims.

We mention in passing that rule-based knowledge can also be suggested without the "user's manual" that needs to accompany it to become scientific. Indeed, proverbs may be viewed as universal statements that are made without a specification of the ranges of the variables over which one quantifies. Hence, in principle both rule-based and case-based knowledge can be presented in a pre-scientific way, without a specification of the way they should be applied, or in a scientific way, with explicit algorithms for their application. However, our empirical observation is that in the sciences rule-based knowledge tends to appear in a specified guise, whereas case-based knowledge in economics often does not. Moreover, the absence of the "user's guide" might be a source of confusion, where a theoretical case is wrongly interpreted as a rule.

3.4 Explaining the Puzzles

Going back to the sociological puzzles discussed above, we argue that viewing economists as generating knowledge that is partly case-based may explain these anomalies. First, as explained above, one need not wonder why economists feel that they gain insights, and understand economics better using models whose assumptions are wrong. In the case-based approach, models

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"observing" the same theoretical case does not add to our belief in its prediction. However, this example illustrates the general point, namely, that once one commits to a particular way in which the similarity function is to be applied, hypotheses about the similarity function become scientifically meaningful.

cannot be wrong. As long as the mathematical analysis is correct, a theoretical case is valid, the same way that an empirical or experimental case are valid as long as they are reported honestly and accurately. Cases do not make any claim to generality, and therefore they cannot be wrong.

Consider the example of the ultimatum game again. In the standard, rule-based model of science, the ultimatum experiment is a refutation of a rule, which presumably should make one reject the rule or at least refine it. But in the case-based model of science, the ultimatum experiment is but a case, as is the formal model, and economists should weigh both, along other cases, in making their predictions. Whether a case originates from empirical data, experiments, or theoretical analysis, it has the same epistemological stature in the economist's mind.

This approach can also explain the high degree of mathematical sophistication in economics. One role of mathematical analysis is to obtain more observations, namely, theoretical cases. Similarly, analysis can extend the scope of existing cases. For example, if there is a proof that a certain result holds for two agents, and one proves that it holds for any number of agents, the new theoretical case may have a higher weight in further reasoning because it is more similar to some real cases of interest. In this sense, generalizing a mathematical result plays the same role as repeating an experiment with participants drawn from a different population.

Mathematical analysis often requires some assumptions that are patently unrealistic. According to the case-based view of science, such a practice might make sense: the unrealistic assumptions are only applied to the theoretical case. When applying the knowledge, practitioners would have to judge the importance of the unrealistic assumptions in determining the similarity between the theoretical case and a concrete problem under consideration. We suggest that this may be one of the reasons that economics tends to use mathematics more than do psychology or biology.

Finally, using the case-based view, one can also understand why economists and psychologists view their models differently. True to the standard, rule-based model of science, psychologists try to avoid refutations by being very explicit about the domain of applicability of their models. Economists, on the other hand, often offer models that are merely theoretical cases. These models cannot be refuted, hence there is nothing to be lost by trying to draw analogies between them and new, remotely connected problems. On the contrary, every problem that may end up being similar to the model increases the model's popularity. As a result, economists have an incentive to view more real life cases as examples of their model, without risking their theory's reputation in so doing.

4 Related Phenomena

In this subsection we argue that the conceptualization of economic models as theoretical cases can also explain additional phenomena in the sociology of economics better than the standard view of science. The phenomena discussed here differ from the "puzzles" we started out with in that they are less conspicuous to academics outside of economics.

4.1 Intuitiveness

Economists are often expected to provide intuition for their results, and it can be problematic for a theory to be judged counterintuitive. As in the case of mathematics or theoretical physics, economic theory definitely values results that are difficult to prove. Indeed, in all of these disciplines results that are considered too obvious will typically not be published. However, in mathematics and in physics, once a non-trivial result has been established, one can hardly dismiss it based on its proof being counterintuitive. In contrast, in economics it appears to be legitimate for a referee to say "The proof is difficult, but, because I do not understand its intuition, I cannot support publication". Why does economic theory value intuitive proofs? Why isn't it sufficient for a result to rely on intuitive assumptions and to be mathematically correct?

It might be necessary to first define what it means to say that a theory is intuitive. We suggest that theories, explanations or results are judged intuitive if they are familiar, that is, if they bear similarity to existing cases. For example, Newtonian physics is relatively intuitive because we are acquainted with billiard balls, and thinking of particles as analogous to such balls makes the scientific explanation familiar. By contrast, the quantum mechanics view of particles is less intuitive because it does not bring to mind any familiar concepts from our everyday experiences. Along similar lines, thinking of the relationship between a nucleus of an atom and the electrons as the relation between the sun and the planets is intuitive because it reminds us of phenomena we already know, albeit from a different domain.[9] Thus, a theory is more intuitive, other things being equal, the more cases it reminds us of, and the stronger is the association (or, the greater the similarity) between the theory and these cases.

With this view of intuitiveness, let us consider an economic model as a theoretical case. Having a prediction problem at hand, the reasoner needs to compare such a case to that problem, and judge their similarity, which will determine the relevance of the case to the prediction problem. However, the case-based view of economics does not restrict the similarity judgment to the assumptions of the model; in fact, the judgment is often performed for an entire proof, as if it were a story. Furthermore, each step in the proof may bring to mind other analogies, between the prediction problem and real past cases.

For example, consider the relevance of Akerlof's model to a given prediction problem. Judging the similarity of the model to the problem, one should ask, how similar are the agents in the model to the agents in reality? Are

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[9] This analogy is nowadays considered misleading. Thus, modern physics can be said to view the similarity between the two systems as superficial.

the people in the real problem expected utility maximizers like the players in the model? Do the former entertain subjective probabilities as do the latter? And so on. But one can also look at the first step in the proof, and ask whether the result of that step is familiar from other cases. For instance, if the proof suggests that buyers will realize that they face a product of uncertain quality, and therefore might not be willing to pay to high a price for it, the reader might well be reminded of real cases in which quality was an unobserved variable, resulting in a lower price of the good. The fact that this step in the proof brings to mind real past cases, and that these, in turn, make certain predictions more vivid, help to convince the reader that the theoretical case is relevant to the problem at hand. Moreover, if certain steps in the proof are familiar (and thus intuitive), the reader need not accept the original assumptions in order to agree with the conclusion.

The importance of intuitiveness of economic explanations was noted more than a hundred years ago by one of the founders of modern economics. Alfred Marshall wrote (quoted in Brue [4, p. 294]):

> “I went more and more on the rules - (1) Use mathematics as a shorthand language, rather than an engine of inquiry. (2) Keep to them till you have done. (3) Translate into English. (4) Then illustrate by examples that are important in real life. (5) Burn the mathematics. (6) If you can’t succeed in (4), burn (3). This last I did often.”[10]

Interestingly, Marshall makes here an explicit reference to cases – “examples that are important in real life” – as a way of judging the value of economic models.

We do not claim that the preference for intuitiveness is a clear-cut proof that economic models are perceived as cases rather than as rules. Indeed, one

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[10] Clearly, economics was not nearly as mathematized when Marshall wrote these lines as it is today.

may attempt to make an argument for intuitiveness also in a rule-based view of science, arguing that our degree of belief in general assumptions is bolstered by similarity to known instances. Further, the ability to directly assume intermediary steps in the proofs as general rules increases the confidence in the conclusion also in the rule-based view of science. Yet, if one subscribes to the classical view of science, according to which one relies on empirically valid assumptions and derives conclusions from them, one should not be allowed to rule out theoretical results based on the absence of an intuitive explanation of their proofs. Thus, we find the high value placed on intuitiveness as supporting the case-based view of economic models more than the rule-based one.

4.2 Axiomatizations

Economic theory seems to value axiomatic derivations of models of individual decision making, even when the models and their implications are well-known. For example, Rozen [25] provides an axiomatic derivation of intrinsic habit formation models that have appeared in the literature. Maccheroni, Marinacci, and Rustichini [21] axiomatized the general class of "variational preferences" and Strzalecki [29] axiomatized the class of "multiplier preferences" used by Hansen and Sargent [17]. Again, these axiomatizations were done long after the decision rules had been incorporated into economic theories. One may therefore ask, why does the profession value the exploration of foundations when a theory is already developed? Shouldn't the theory be directly tested based on its predictions, their fit to reality, and so forth?[11]

While there are many reasons to be interested in axiomatic derivations of behavioral models, we hold that the case-based view of economic theory explains the interest in axiomatizations better than does the rule-based view. Consider a simple, textbook example. Economists typically assume

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[11] It might be worth mentioning that we are not dealing with a marginal phenomenon. All three axiomatizations quoted here were published in the best theory journal.

that each agent maximizes a utility function. This assumption is supported by an axiomatic derivation, saying that a preference relation that satisfies basic requirements of completeness and transitivity can (in a finite set-up) be represented by maximization of a certain function.

Such an axiomatic derivation is a characterization theorem. As such, it cannot make a theory more or less accurate. If we were to test how many economic agents do indeed maximize a utility function, or how many have a preference relation that is complete and transitive, we would necessarily obtain the same results, and conclude that the theory has the same degree of accuracy in its two equivalent representations. Moreover, when statistical errors are taken into account, one may argue that it is advised to test the theory directly, rather than to separately test several conditions that are jointly equivalent to the theory. Hence, if economists were taking their theories as general rules that should fit the data, axiomatizations would be of little value for the selection of theories.

However, let us now consider the more modest, case-based view of economic theory. According to this view, no general claim is made about economic agents. Rather, the economic theorist suggests certain theoretical cases in which agents who maximize a utility function behave in certain ways. These theoretical cases are to be judged according to their similarity to real prediction problems. When we ask ourselves, "Are people in this problem similar to the agents in the model?", we may indeed find out that different representations of the same mathematical structure result in different similarity judgments. For example, one might find it unlikely that a randomly chosen consumer would maximize a utility function, but, at the same time, quite plausible that such a consumer would make decisions in a complete and transitive way. %% The previous sentence does not make sense. Do you mean: For example, one might find it unlikely that a randomly chosen consumer would consciously maximize a utility function, but, at the same time, quite plausible that such a consumer would be aware that his decisions are consistent with a complete and transitive ranking.&&Thus, axiomatizations point out to us similarities that are not obvious *a priori*.[12]

In other words, we argue that the field values axiomatic derivations because axiomatizations and, more generally, equivalence theorems, can be powerful rhetorical tools. The standard view of science leaves, in principle, little room for rhetoric: theories are confronted with the data, and should be tested for accuracy. By contrast, the case-based view of science lets rhetoric occupy center stage: scientists only offer cases, and these should be brought to bear upon prediction problems, where similarity and relevance should be debated as in the court of law. With this openly-rhetorical view of science, the importance of axiomatizations is hardly a mystery.

5 A Formal Model

In this section we provide a formal model of rules and analogies, and quote some simple complexity results that capture some of the considerations behind the choice of method of scientific reasoning. The model we present is in the same spirit of the economic models it discusses, and should therefore be judged as a potential analogy rather than as a general theory. It follows that this model cannot support our claims without engaging in circular reasoning. At the same time, we find it encouraging that our way of viewing economic models is consistent with our own thinking about economic modeling. Indeed, the phenomenon that is the focus of our interest is the activity of economists, which is broadly in the realm of the social sciences. Hence it should not come as a surprise that we analyze this phenomenon as (we claim) one often analyzes other social science phenomena. In any event, we remind the reader that this section can be skipped.[13]

As mentioned above, there are several sources of information that may

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[12] Dekel and Lipman (2010) provide a similar motivation for axiomatic representations.

[13] This paper is written of economists and by economists. This section is also for economists.

be relevant for economic analysis: empirical data, historical cases, casual observations, as well as experiments, theoretical results, and computer simulations. The model that follows is designed to capture all these types of information. For simplicity of exposition, we will give examples only of the three most relevant types for economic analysis: empirical data, experiments, and theoretical models.

Assume that there is a set of entities $\mathcal{E}$, describing all objects of analysis. This set includes all the agents that act in real economic situations, as well as in economic models. It also includes goods that are being traded, whether in a real example, in an experiment, or in a model. Thus, John Smith may be a member of $\mathcal{E}$, and another member might be "player 1 in the prisoner's dilemma model". If John Smith has a car, the car is another element of $\mathcal{E}$, as is "a used car" in Akerlof's model. It is convenient to lump together all entities in a single set for two reasons. First, this definition obviates the need to classify all types of entities that are relevant to economic models, ranging from people and households to goods and money. The nature of these entities will be reflected in the predicates that are true for them. Second, using predicates one can capture the fact that the same entity may play different roles in different contexts. For example, a firm owns goods, but it is itself owned by people.

When applying the general model, it will be useful to have a disjoint set of entities for each theoretical model, as well as for each type of experiment. Thus, "player 1 in the prisoner's dilemma" and "player 1 in the battle of the sexes" will be different entities, each belonging to a theoretical model. Similarly, "player 2 in the ultimatum game played between two women" and "player 2 in the ultimatum game played between two men" will be different entities, belonging to experiments. By contrast, when empirical data are concerned, one discusses concrete, real-life entities and it makes sense to map them to the same element in our model.[14]

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[14] Alternatively, one may replicate real entities, and reflect their identicality in the pred-

Facts about the entities are described by a language, consisting of predicates. Predicates are typically defined as relations, or subsets of k-lists of entities. It might be more convenient to define them as functions into $\{0, 1\}$. Thus, a *k-place predicate* is a function $f : \mathcal{E}^k \to \{0, 1\}$ for some $k \geq 1$. For example, a one-place predicate $H : \mathcal{E} \to \{0, 1\}$ might denote which of the entities are households, so that $H(i) = 1$ if and only if i is a household. One-place predicates can also capture behavioral assumptions such as "the agent never chooses dominated strategies". A two-place predicate $Own : \mathcal{E}^2 \to \{0, 1\}$ might denote ownership, so that $Own(i, a) = 1$ if and only if i owns a, presumably applied to entities (i, a) such that the former is a legal entity and the latter is a tradeable good. Similarly, a three-place predicate $Pref : \mathcal{E}^3 \to \{0, 1\}$ might denote preferences, such that $Pref\,(i, a, b) = 1$ iff entity i (presumably a person) prefers object a to object b. Other predicates might describe what an agent can do, what an agent knows at a point of time, and so forth.

Observe that we do not impose any a priori restrictions on the domain of the predicates. Since we only have one set of entities, a predicate such as Own should tell us not only which person owns which good, but also whether the goods own the people, or one person owns another, and so forth. Such propositions might be viewed as meaningless, rather than false. However, for simplicity of exposition we do not make this distinction, so that $Own(x, y) = 0$ might hold if x is a person who does not happen to own good y, but also if "x owns y" is a nonsensical statement.

Of all the predicates one can imagine, only a few correspond to meaningful concepts or to words in everyday language. Thus we define the *language* to be a subset of predicates

$$\mathcal{F} = \cup_{k \geq 1} \mathcal{F}^k$$

where

$$\mathcal{F}^k \subset \{0, 1\}^{\mathcal{E}^k}$$

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icates.

is the collection of k-place predicates that are in the language.

Importantly, we assume that the set $\mathcal{E}$ is rich enough relative to $\mathcal{F}$ such that, for every integer l and every desired profile of values of the predicates $\mathcal{F}$, one can find a set of l entities, E, that have the pre-specified values of the predicates in $\mathcal{F}$, and so that E is disjoint from the entities that are used for other profiles of predicates. This guarantees that an economist can always express new models in the language: if the economists wishes to say, "assume that there are agents with the following preferences and endowments...", she can find yet-unused entities to serve as names for the agents and the goods, in such a way that the predicates in $\mathcal{F}$ take the values corresponding to the economist's model.

A *prediction problem* p is a pair (E, F) where $E \subset \mathcal{E}$ is a finite and non-empty set of entities and $F \subset \mathcal{F}$ is a finite and non-empty set of predicates, whose values over E are known. The analyst must associate an outcome r with the prediction problem. For simplicity we assume that outcomes are binary, namely, that an outcome is $r \in \{0, 1\}$ and thus a case is a pair $c = (p, r)$ with $p = (E, F)$ and $r \in \{0, 1\}$. For example, the outcome might be whether trade occurs in the case. This assumption is a simplification in two ways. First, an outcome can often be a real variable, or a vector of real variables, such as the level of inflation, level of employment and so forth. Second, it is implicitly assumed that the entire analysis focuses on a single question, so that the meanings of "0" and "1" are implicitly understood. In reality scientists collect data, run experiments, and analyze models that can be used for many different research questions, some of which may not even be specified at the time cases are collected. A more general model might describe outcomes as abstract entities, and capture their relevant aspects by functions that are defined on them (similar to the way predicates describe the prediction problem).

A *case* c is a prediction problem p coupled with its *outcome* r.[15] If a case

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[15] The formal structure of a case can also be interpreted as a rule. The mathematical

designates a data point that was empirically observed, the values of F and of r are observed simultaneously. The economist can choose which entities and predicates to observe, but she cannot control the values of the predicates. For example, the economist might choose to observe whether trade takes place between individuals, and she can choose to focus on their endowments and preferences, but she has no control over the values of these variables. By contrast, if a case is an experimental observation, the experimenter is free to set the values of the predicates F, and the only unknown is the outcome r. For example, an economist can decide to run an experiment in which she controls the participants' endowments and opportunities to trade, and observe whether they end up trading. Similarly, if the case is a theoretical study, the economist is free to assume any values of the predicates, and the outcome r is determined by mathematical analysis.

The set of all conceivable cases will be denoted C. A *memory* is a finite collection of cases, $M \subset C$. The scientific challenge is to consider a memory M, and make a prediction about the outcome of a new prediction problem p.

5.1 Analogies

An *analogy* between prediction problem $p = (E, F)$ and prediction problem $p' = (E', F')$ is a 1-1 function $\varphi : E \to E'$. The analogy is viewed as relating the two prediction problems, p and p', though the only freedom is in the mapping between the entities. Prediction problem p will be referred to as the *origin* of the analogy, and prediction problem p' as its *target*.

The strength of the analogy depends on the sets of predicates mentioned in the two prediction problems, and on the values taken by the predicates that appear in both. The analogy φ between $p = (E, F)$ and $p' = (E', F')$ will be considered a *perfect analogy* if (i) $F = F'$ and (ii) for every $f \in F$,

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entity $\{p, r\}$ can thus be interpreted as either a case or a rule, depending on how it is used. We focus here on their interpretation as cases.

letting k satisfy $f \in \mathcal{F}^k$,

$$f(e_1, ..., e_k) = f(\varphi(e_1), ..., \varphi(e_k))$$

for every $e_1, ..., e_k \in \mathcal{E}^k$. Thus, an analogy is perfect if all that is known about the prediction problems is identical. It can be less than perfect (i) if some facts are known about one prediction problem but are not known about the other; or (ii) if some facts that are known to be true in one prediction problem are known not to hold in the other.

There are several reasons for which one may find analogies between prediction problems where $F \neq F'$. First one might often be able to observe more variables about the prediction problem at hand than about a past case. Thus, the target (present prediction problem) may have some predicates that are not observable in the origin (past prediction problem). Next assume that a theoretical model is mapped into a present prediction problem. The theoretical model might include an assumption such as "the agents maximize a utility function". This will be reflected in a predicate of the theoretical case, but this predicate might not be directly observable in the prediction problem at hand. Thus, the origin (theoretical model) may have some predicates that are not observable in the target (present prediction problem).

It stands to reason that the similarity one finds between two prediction problems will depend on the values of the common predicates, as well as on the existence of predicates that exist in one but not in the other. Along the lines of Tversky's (1977) theory of similarity, one may assume that the similarity between $p = (E, F)$ and $p' = (E', F')$ decreases as $F \backslash F'$ or $F' \backslash F$ becomes larger (in the sense of set inclusion).[16]

As was noted in Aragones, Gilboa, Postlewaite, and Schmeidler (2001), finding analogies is not a simple computational task. Even if one restricts attention to two prediction problems, the number of possible analogies between them grows exponentially in the number of predicates. To be precise,

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[16] One need not assume, however, that the numbers of predicates in $F \cap F'$, $F \backslash F'$, and $F' \backslash F$ are the only determinants of similarity.

if $|E| = k$ and $|E'| = n \geq k$, the number of 1-1 mappings $\varphi : E \rightarrow E'$ is

$$n\,(n-1)\ldots(n-k+1) = \frac{n!}{(n-k)!} = \binom{n}{k} k!$$

The very fact that this number might be exponentially large (for example, it equals $n!$ when $k = n$) does not necessarily imply that one cannot find whether a perfect analogy exists in an efficient manner. However, the following simple result establishes that the problem of finding analogies is as difficult as all problems in NP.

Proposition 1 *The following problem is NP-Complete: given two prediction problems $p = (E, F)$ and $p' = (E', F')$, is there a perfect analogy $\varphi : E \rightarrow E'$ between them?*

Proof It is straightforward that the problem is in NP. To see that it is NP-Complete, observe that it is NP-Complete even if we restrict attention to pairs of prediction problems in which $F = F' = \{f\}$, where f is a 2-place predicate (i.e., $f \in \mathcal{F}^2$). The analyst's task is then to identify whether, given two directed graphs, one a sub-graph of the other. This problem is NP-Complete (for instance, the Clique problem can be reduced to it.) ■

This simple result supports the intuition that it is easier to find analogies between prediction problems that do not have too many entities. In particular, consider the task of finding which theoretical models apply in a given prediction problem (and how). As mentioned above, the set of all possible mappings from E to E' is of size

$$\frac{n!}{(n-k)!} \leq n^k$$

for $k = |E|$ and $n = |E'|$. If k is bounded, this is a polynomial in n. More importantly, if k is low, the computational task of finding analogies may be manageable, even if solved by brute force. Hence, our model explains why economists prefer theoretical models with few “moving parts”: a lower number of entities in the model makes it more likely that the model will be useful as a source of analogies for a prediction problem at hand.

5.2 Rules

A *rule* is again formally defined as a prediction problem and an outcome, or $((E,F),r)$, just as is a case. The distinction between rules and cases lies not in how they are defined by in how they are used. A rule is interpreted as saying “*whenever* a set of entities E satisfies the relations defined by the predicates F, the result r will occur”. For example, consider the rule $((E,F),1)$ with $E=\{i,j,a,b\}$, $F=\{Own,Pref\}$, and $Own(i,a)=Own(j,b)=1$, $Pref(i,b,a)=Pref(j,a,b)=1$. The rule is interpreted as “whenever there are two individuals who own one good each, and each prefers the good that the other has to her own, they will trade”. (Here and elsewhere, when we specify the arguments for which a predicate takes the value 1, it is implicitly understood to take the value 0 elsewhere.)

We emphasize that the mathematical object $((E,F),r)$ can be used either as a case or as a general rule. In the preceding example, when $((E,F),1)$ is interpreted as a case, we may think of it as saying, “once there were two individuals, i and j, who owned one good each, a and b respectively; each preferred the good owned by the other to her own, and they traded”. Such a case could be an empirical observation, or a result of an experiment in which two individuals playing the roles of i and j, are induced to have certain preferences over a and b, and end up trading. The case can also result from a theoretical analysis, if one adds to it an appropriate assumption such as “Agents i and j always reach Pareto efficient allocations”.[17] However, none of these cases—empirical, experimental, or theoretical—is assumed to be a general theory, and thus none can be refuted by another case. By

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[17] This assumption would have to be stated as a predicate, as would other behavioral assumptions about each agent separately or about several agents as a group.

contrast, when the case $((E, F), 1)$ is interpreted as a rule, it makes a general statement that can be refuted. In particular, if we observe an experiment with entities $E' = \{I, II, c, d\}$ where I and II are players, c, d are goods, $Own(I, c) = Own(II, d) = 1$, $Pref(I, d, c) = Pref(II, c, d) = 1$, and trade does not occur, we will say that the rule was refuted.

More generally, a rule $((E, F), r)$ is *refuted by* a case $((E', F'), r')$ if (i) $F \subset F'$ and (ii) there is an analogy $\varphi : E \to E'$ between $p = (E, F)$ and $p' = (E', F')$ such that for every $f \in F$, letting k satisfy $f \in \mathcal{F}^k$,

$$f(e_1, ..., e_k) = f(\varphi(e_1), ..., \varphi(e_k))$$

for every $e_1, ..., e_k \in \mathcal{E}^k$, but $r \neq r'$.

That is, to determine that the case $((E', F'), r')$ refutes the rule $((E, F), r)$ we first need to establish that the prediction problem (E', F') is indeed lies in the domain of applicability of the rule, given by the general template (E, F). To this end, the prediction problem has to specify all the predicates that are postulated in the antecedent of the rule $(F \subset F')$, and we need to verify that each one of them holds in the prediction problem. Only when it is established that the prediction problem is indeed an example of the general rule, will a different outcome $r' \neq r$ consist a refutation of the latter.

Note that the definition of a refutation (of a rule by a case) differs from the definition of a perfect analogy (between two cases), in condition (i): a rule will typically have fewer predicates than a case. Nevertheless, the act of generalization consists in making the prediction (r) whenever the predicates in F are satisfied, without further qualifications.

It is straightforward that

Proposition 2 *The following problem is NP-Complete: given a rule* $((E, F), r)$ *and a case* $((E', F'), r')$*, does the case refute the rule?*

Proof Restricting attention to $F = F'$, the problem is as difficult as (and almost identical to) the perfect analogy problem. Clearly, it is still in NP. ■

Thus, it might not be a simple task to find out whether a given rule is refuted by a single case. Evidently, it is much more complicated to find out whether a set of rules is refuted by a database of cases.

As with analogies, complexity considerations suggest that one should prefer simple rules, that is, generalizations $((E, F), r)$ where $|E|$ is low. However, such generalizations in economics tend to be easily refuted. Thus, rule-based reasoning is often of limited success: simple rules are easy to test, but tend to fail the empirical tests. More refined rules become less useful due to complexity. As a result, case-based reasoning seems like a viable alternative. With simple models, one can find analogies. These will not be correct generalizations in general, but when viewed as tools for case-based reasoning, one need not worry about their refutations.

6 Standard Languages and Second-Order Analogies

6.1 Definition and Examples

Psychologists distinguish between different order of analogies. First-order analogies are between entities for which the same predicates presumably hold. Second-order analogies are not only between entities, but also between the predicates. For example, comparing Mary's relationship with her advisor to John's relationship with his advisor is a first-order analogy. By contrast, comparing Mary's relationship with her advisor to John's relationship with his father is a second-order analogy, where the binary relation "is an advisor of" is likened to the binary relation "is a parent of".

Some of the more powerful and surprising analogies in economics are of second order. Consider, for example, Hotelling's (1929) famous model of two ice-cream sellers on a beach. The model predicts that the two will locate very close to each other, at the middle of the beach (measured by the density of consumers along it). This is the equilibrium of the game played by the two sellers, assuming that the buyers choose to walk over to the seller who is closer to them. Indeed, any other location on the beach by one seller allows the other seller to gain more than 50% of the market. The same model was later re-interpreted as a model of political competition, suggesting that two political candidates will express views that are centrist, for the same reasoning: assume that views are ordered on a line, and that every voter votes for the candidate whose expressed views are closest to the voter's. Under these assumptions, a candidate who expressed views that are not at the median allows her opponent to locate himself so that he gets more than 50% of the votes.

This analogy is insightful because it is "cross-contextual": it relates different domains of knowledge. A priori the two stories are very different: one is about trade, the other about elections. In one story the key agents are trying to sell products and get a larger market share, whereas in the other they are politicians who attempt to draw votes. Indeed, the analogy is not perfect for these reasons: the ice cream sellers also determine prices, which do not have a clear equivalent in the political competition. Moreover, political candidates might have ideologies, or perceived ideologies, that restrict their freedom of location on the political opinion axis. Yet, the analogy certainly allows us to think about political competition in a new light, and make some qualitative predictions that appear to be rather successful. Clearly, such an analogy is second-order: it not only maps voters to buyers, it also maps the predicate "votes for" to the predicate "buys from".

Consider another example. A principal-agent model might deal with a manager (the principal) who is trying to motivate workers (the agents) to exert effort even though their effort level is not directly observable. Such models have been analyzed extensively and have been useful for the understanding of moral hazard in the insurance market. For instance, assume that John insures his car. Should the car be damaged, the financial cost will be borne mostly by the insurance company, rather than by John himself. John might exert different levels of effort in trying to minimize the probability of such a damage, but his level of effort is not observable by the insurance company. Thus, the situation is akin to the principal-agent problem: one player (the worker, or John) can affect the expected payoff of another player (the principal, or the insurance company), where the latter cannot observe the action taken by the former.

This analogy is not transparent. When John buys insurance, he is not employed by the insurance company. If anything, one would think of John as the customer who buys the insurance company services. Yet, when the possible acts and their outcomes are analyzed, it turns out that John is similar to the worker in affecting the other player's utility. This analogy is sometimes difficult to see, because the predicate "sells insurance to" in the insurance case is mapped to the predicate "hires" in the principal-agent case. Further, John, as the owner of the car, might be viewed as the more powerful principal, rather than as the agent whose services are hired. The analogy reverses the roles of buyer-seller, and yet it unveils a similar structure between two economic stories.

6.2 Standard Languages

Second-order analogies are difficult to find, because they allow for a much richer set of possible mappings. When the analogical mapping only maps entities into entities, it is easier to search a database for possible analogies. Moreover, the words describing the predicates, such as "votes", can serve as indices that allow one to search one's memory for cases that are similar to the prediction problem one is faced with. By contrast, when the analogical mapping allows "votes" to be mapped to "buys from", there are many more possible analogies, and, worse still, the lexical indices provided by words do not suffice to bring to mind all the relevant cases.

One way to facilitate the task of finding second-order analogies is to use a standard language. One may view a "paradigm" or a "conceptual framework"[18] as consisting of a language that is supposed to be able to describe a large set of cases, coupled with certain principles for prediction. For example, the game-theoretical paradigm in economics starts with the language of players, strategies, information sets, outcomes, beliefs, and utilities. This language is somewhat abstract, and it allows one to describe a vast variety of situations of interaction, ranging from economics to political science, from biology to computer science.

The game theoretic paradigm, or "conceptual framework" consists of more than a language. For instance, it also has solution concepts, such as Nash equilibrium, which can make predictions in various domains of applications. However, our focus here is not on the entire paradigm but only on its language: the standard language allows economists to see cross-contextual analogies more easily. Once one abstracts from terms such as "voters" and "buyers", "candidates" and "ice cream sellers", %%Maybe add: and refers to them as "players",&&one sees the analogy between the two stories that fits Hotelling's model. Similarly, when ownership and employer-employee relations are stripped from the stories, it is easier to understand why buying insurance is akin to working for a principal. In other words, a standard language allows one to see more similarities without resorting to second-order analogies.

There are fields of science that use standard languages, and that can also formulate general rules in these languages. This is arguably true of physics, whose standard language involves no more than five forces, and which succeeds in formulating theories that are both general and accurate. Unfortunately, the social sciences don't seem to be able to achieve this type of success. There are, in principle, two main directions in which a field might proceed: it can sacrifice generality for accuracy or vice versa.

When sacrificing generality, one would attempt to formulate general rules that are supposed to hold only in very specific and well-defined situations.

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[18] See Gilboa and Schmeidler (2001).

This is largely the direction taken by experimental psychology. It is also the way that much of economics is conducted. For instance, consider the general rule saying that demand goes down as the price goes up. To make sure that this rule is rather accurate, one may specify the domain of application so as to rule out speculative assets, goods of uncertain quality, or conspicuous consumption goods. With these restrictions, the rule appears to be a good approximation of the data.[19]

The other possible direction is to give up accuracy and aspire for generality in return. In an extreme version of this approach, one gives up the claim to formulate a general theory, so that accuracy is not an issue, but aims to have a language that describes a wide range of phenomena, and allows for higher order analogies. Thus, rule-based reasoning is discarded in favor of case-based reasoning, and, in return, the latter becomes very powerful. The claim we are trying to make is that this is the direction taken by much of microeconomic theory is the past few decades, using game theory as the standard model, and generating insightful analogies rather than accurate rules.

7 Implications

Our main goal in this paper is to offer a descriptive account of the way economists reason. In this section we tentatively suggest possible normative implications. We mention several on-going scientific debates and indicate how our view might help clarify some of the issues discussed.

7.1 Behavioral Economics

Behavioral economics has generated both great activity and great debate. It is not uncommon that statements like the following are made for and against behavioral economics.

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[19] Giffen goods are a counter-example used in class, but they are certainly rare.

> **Pro:** Conventional economic models incorporate all sorts of unrealistic assumptions, such as that people maximize. We reject these models, and replace them by more realistic behavioral models. Economics should be attuned to violations of its assumptions and improve its predictions as do serious sciences.
>
> **Con:** Each behavioral model is constructed to explain the author's favorite behavioral puzzle, with a new and separate model for each behavioral stylized fact. It is not clear that we learn anything from this exercise other than, given enough freedom, a model can be constructed consistent with any behavior.

The standard view that economic assumptions are false and that economic theories are therefore useless follows the classical, rule-based view of science. Indeed, if economic models were only meant as general rules, their violations would require serious revisions of the theory. Yet, many economic theorists are not so perturbed by such violations. As explained above, this is partly because they tend to think of their models as theoretical cases. Theoretical and experimental cases can sometimes suggest different predictions, but they do not refute each other. Thus, while behavioral economics appears to be the only way to proceed for a rule-based discipline, it is but one way in which a case-based discipline can improve.

One might also ask, what type of knowledge does behavioral economics seek to generate? One possibility is to render economic theory rule-based, and, following the example of psychology, offer a collection of specific theories, each of which attempts to follow the classical model of science. Another possibility is to view behavioral economics as an extension of economic theory, providing more theoretical cases, but cases that are based on different assumptions.

We believe that both views are valid and potentially fruitful. According to the first, behavioral economics should be very careful to define its scope of applicability, testing the validity of behavioral assumptions in various populations and in various contexts. Such a scientific endeavor would be highly valued for specific applications, such the choice of incentive schemes to motivate employees or similarly concrete questions.

At the same time, behavioral economics can also be viewed as offering theoretical cases that delimit the scope of similarity of standard ones. For example, economists might analyze simple models of decision making over time in order to understand saving behavior. Assuming that agents are dynamically consistent would lead to certain conclusions. Alongside these models, one might wish to analyze equally idealized models in which dynamic consistency is violated in a particular way. These alternative models might be useful in analyzing real problems, where one is made to ask whether real agents are more similar to the theoretical agents in one model or in the other.

Thus, behavioral economics might be useful according to both views of economics. However, the evaluation of each model in this field might benefit from clearer explanation of its goal: is it offered as a general rule with a limited domain of applicability, or as a theoretical case whose domain of relevance is yet to be determined?

7.2 Experimental Economics

Experimental economics has similarly generated both energy and controversy. One senses the tension here in the saying that "Daniel Kahneman got the Nobel prize for showing the economics doesn't work, and Vernon Smith got the prize for showing that it does". With a similar apology, we can caricaturize the debate around this field as follows:

> **Pro:** Conventional economic models predict poorly. Experimental economics documents this by running controlled experiments that allow us to isolate the forces described in a particular model, only to find that behavior does not match the model.
>
> **Con:** No one ever claimed that economic models are universal. The predictions of any model (that makes predictions) can

> be falsified in a sufficiently tailored and contrived environment. Many of the experimental findings are not a representative sample of economic decisions, and, worse still, they do not seem to be very robust.

Our view here is, again, that experimental economics has room according to both views of science, but that a clarification of the way one views experiments might reconcile some debates.

Specifically, experimental economics fits very naturally into the classical, rule-based view of economics. According to this view, the field makes predictions, and these should be tested. While macroeconomic predictions cannot be tested in laboratories, some microeconomic and game theoretic predictions can be, and there is no justification for ignoring such experiments. However, according to this view economics should follow the example of social and cognitive psychology, running carefully designed experiments while also carefully delineating their scope of applicability. One should not assume a priori that the result of an experiment should be independent of culture, education, context, stakes, and so forth. In particular, experiments that are run in a laboratory might never be applicable to certain macroeconomic questions, as the latter require experimentation with entire economies.

According to the other, case-based view of economics, experiments are run not in order to test or refute theories, but to remind us of additional considerations that might be relevant for a problem at hand. For example, the ultimatum game experiment mentioned above does not prove, for example, that economic agents care about pride more than about monetary payoffs. Rather, such as experiment is a case, useful in making predictions by reminding us what another case might be missing.

As in the case of behavioral economics, we find that some debates may be more easily settled if experimental papers were more explicit about their intended use: as a test of a general theory with a specific domain, or an example of behavior that might bear resemblance to some, typically unspecified, problems.

7.3 History versus Theory

While mathematical modeling has become dominant within mainstream economics, its role in other social sciences is a topic of debate. In political science, for example, there exists a viable community of researchers who develop formal models, while many of their colleagues tend to view their work as close to useless. Since the phenomena of interest tend to be very complex, people often doubt the value of over-simplified models. It is sometimes argued that the phenomena "cannot be reduced to a couple of equations", and the mathematical language is sometimes criticized for serving as a barrier to entry. Similarly, in law and in sociology formal mathematical models are viewed with suspicion, and it is argued that they cannot replace historical and institutional detailed study of real cases.

The standard view of science seems to suggest that the formal-mathematical camp and the historical-institutional one are on an inevitable collision course. The former seeks general truths, whereas the latter seeks accuracy of detail. One blames the other for lack of theoretical depth, while being blamed, in turn, for useless unrealistic theorizing. In short, the two do not seem to be easily reconcilable.

However, in the case-based view of science, the difference between formal modeling and historical analysis becomes a difference of degree, not of kind. A historical case is rich in detail, while a theoretical one is leaner. The former allows a high degree of similarity to fewer problems, whereas the latter allows weaker similarity to a larger set of cases. However, these are extreme points on the same scale, rather than competing world views. In particular, one may expect that a balance between these types of reasoning may be more useful than adhering to either one of them on its own.

7.4 User's Guides?

Rule-based knowledge is not complete without the "user's manual" identifying when it is meant to be applicable and when silent. The flexibility inherent in case-based reasoning is often carried to the opposite extreme, with new cases, whether in the form of new theoretical models or new experiments, offered with no guide as to when they are relevant.

We cannot expect cases, whether theoretical, experimental, or empirical, to be accompanied by a precise statement delineating their domain of applicability or a specification of the similarity function they should be used with. However, we believe that the meta-categorization, suggesting that whether a model should be classified as rule-based or case-based, is a helpful first step in judging its contribution.[20]

7.5 Case-Based vs. Rule-Based Reasoning

The relative weight of case-based vs. rule-based reasoning can also be analyzed in the context of a formal model. For example, building on the general framework of Gilboa, Samuelson, and Schmeidler (2010), Gayer and Gilboa (2012) consider this problem and show that when reality is simple (in an appropriately defined sense), a reasoner would find a theory that explains it, and will converge to reason mostly by this theory. By contrast, when reality is complex, and theories are being consecutively rejected, the reasoner will, under certain assumptions, tend to put more weight on reasoning by analogies. Whereas these models attempt to capture the reasoning of economic agents, they might help us understand scientific reasoning as well. Specifically, they can explain why economists, who would prefer to have general, accurate theories as in other sciences, might be resorting to case-based reasoning when reality proves too complex to be satisfactorily explained by theories.

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[20] It is certainly possible that a theoretical model will prove useful both as a rule and as a case. Still, we hold that it is useful to make explicit the claim to both types of contributions.

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