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Are Economic Kinds Natural?

I

This essay is primarily about some foundational problems in economic theory. I shall approach these problems by focusing on the natural kinds postulated in economic theory and physical theory. I hope to develop some understanding of how we can think of natural kinds in scientific theories, but my main goal is to try to answer some more practical questions about the foundations of economics. In what follows, 'natural kinds' means natural kinds in the world – the *kinds* of *things* that there are. Furthermore, I do not mean the kinds of things that fall into the extensions of the so-called natural-kind *terms* of the vernacular. Instead, this essay is about the kinds of things that are picked out by our best scientific theories. Perhaps I am not justified in appropriating the term 'natural kind' in this way, but for the present it can be regarded as a stipulative definition.[1]

How do scientific theories pick out natural kinds? Let's assume that a scientific theory is a sort of linguistic object containing some items, natural-kind terms, that are semantically related to the natural kinds in the world. This assumption needs plenty of defending, but none will be provided here. Which items in the theory are the natural-kind terms? If we could identify within theories scientific *laws* of the form, "All Ps are lawfully associated with Qs," then it would be sensible to think of 'P' and 'Q' as natural-kind terms. There are too many problems with this approach for us to count it a general analysis, but in this essay I shall concentrate on some real examples from physics and especially from economics for which the analysis has some plausibility.

The particular question about economics dealt with in this essay is this: Why

I received many helpful comments when presenting some of the ideas in this paper to audiences at the following institutions: University of Minnesota; Caltech; Northwestern University; University of California, Irvine; University of Arizona; Indiana University; and the University of California, Berkeley. Special thanks to Philip Kitcher, Wade Savage, and the participants at the NEH seminar at Minnesota. I have also specially profited from discussions and correspondence with Arthur Fine, Scott Gordon, Daniel Hausman, Alexander Rosenberg, and Paul Teller.

does economics do so much worse than the physical sciences in applications when its formal structure often rivals the others with respect to mathematical sophistication, rigor, and aesthetic appeal?[2] Many answers have been proposed. Pessimists say it is because social science is inherently impossible, or because contemporary economics surreptitiously incorporates bad ideology, or because it is simply a failure like phlogiston theory. Optimists say it is because it is a young science, or because the phenomena it attempts to deal with are so complex, or because we cannot run carefully controlled, repeatable experiments.

I want to defend another answer that has both pessimistic and optimistic elements. I shall provide new arguments for the old pessimistic position that says there are very good reasons for supposing that economics does not apply to the world successfully because its central concepts do not represent natural kinds. The optimistic part comes from a characterization of the sorts of empirical results we would need to get in the future to show that the natural-kind terms of economics do refer to natural kinds after all. I shall also have something to say about procedures that might be successful for obtaining these results. To begin the argument we require an account of what the natural-kind terms in economics are.

II

The economic theory I shall concentrate on is General Equilibrium Theory (GET). GET is about economies of individual, independent agents, both consumers and producers of commodities. It analyzes economies with attention to the interdependence of the economic decisions they make. It is assumed for the sake of simplicity that decisions invariably eventuate in the intended economic actions (although these actions may not have all the intended results). The basic goal of the theory is to determine what configurations of relative prices of goods will yield outcomes consistent with rational behavior on the parts of all agents. The properties of these so-called equilibrium outcomes are then studied. It is called *general* equilibrium when the prices of all commodities are more or less constant. GET is especially appropriate for the purposes of this essay because of its great generality. Since almost all branches of contemporary capitalistically inspired economics are either forms of GET or special cases of it,[3] understanding it is close to understanding virtually all of modern academic economics. Moreover, GET is characterized by a fruitful and well-developed formalism, so there is little doubt that the empirical problems of economics are the result of a logically or mathematically defective formalism. It must be stressed, however, that the empirical problems of economics do not result simply from clumsy attempts to apply the general and highly abstract mathematical formalism of GET to particular actual cases. Good empirical work is supposed to be, in principle, conformable to the GET framework, but almost everyone realizes that empirically useful results require careful extensions and applications of the basic GET framework.

The theory's structure can be briefly summarized as follows.[4] It contains a small number of foundational elements. Each of these elements, or theoretical concepts, can be thought of as representing a natural kind. To understand them, it is necessary to study them together, because the concepts are closely intertwined.

Consumers. These represent people—the principal agents in the economy. It is their actions, their *exchanges* of commodities, that determine the nature of most economic phenomena, including production. From the standpoint of GET and its special cases, individual consumers' identities consist solely of their *preferences* for and *endowments* of commodities, so one can think of them simply as repositories of preferences and endowments—they are not quite actual people. Thus their economically interesting actions consist of only exchanges of commodities that maximize utility (or perhaps tend to maximize utility, or perhaps exchanges that the consumer thinks maximize utility, and so on). It is also true of every consumer that he has the capacity to annihilate and create commodities by consuming and laboring, but the mechanisms that are involved in these activities are not part of the subject matter of economics.

Preferences. Each consumer has preferences that put all of the bundles of commodities available to him in an *order*.[5,6] They are represented in GET by an ordering of vectors having an element for each kind of commodity. Preferences are often conveniently represented by a *utility function* that assigns a scalar measure of utility to the consumer for every commodity vector. In virtually all cases of interest, use of utility functions is equivalent to use of preferences, and I shall follow economic practice in exploiting this fact. In particular, I shall freely shift back and forth between utility functions and preferences, depending on which make the point in question more perspicuously.[7]

Endowments. Each consumer has an endowment, a bundle of commodities that she *owns*.[8] The endowment is represented by a vector of the same commodities as the preference vector. (Since few have everything that they like, some elements will be equal to zero.) How much utility a consumer "gets," how well her preferences are satisfied, depends on how much and on what commodities are in the endowment. These commodities include not only goods in the sense of "dry goods," but also claims to "services" of all kinds (TV repair, foot massages, musical performances, etc.). Perhaps the most important services owned by the consumer are the ones that she can provide herself, in short, her labor. The endowment, or parts of it, can be either exchanged on the market for other commodities, or consumed.

Firms. Firms are organizations of people (i.e., consumers) and commodities (i.e., capital) that transform bundles of commodities into other, different bundles of commodities. The idea is that the firm employs "factors of production," labor and relatively raw materials, and *produces* "finished" commodities. In the same

way that consumers are fully characterized by their preferences, endowments, and propensity to maximize utility, firms are fully characterized by their technological possibilities for converting commodity inputs to commodity outputs—their "production possibilities set." The stream of commodities produced by the firm is owned by a group of consumers, the stockholders—it becomes part of their endowments. Since consumers are utility maximizers, the firm's owners will direct it to maximize output in a way that benefits the owners in terms of utility.

Netput Vectors. The firm's production possibilities set can be represented by a set of vectors of quantities of commodities. The elements represent inputs and outputs of each commodity, hence the name. Each netput vector represents a production process that is technologically possible for the firm. If an element of one of these vectors is positive, that commodity is an output of the process (a product); if an element is negative, that commodity is an input (a factor of production). It is often convenient to represent the set of netput vectors by a function that takes inputs of factors of production to outputs of product, a "production function."

Prices, the Market, and Equilibrium. It is assumed that commodities exchange at prices that are standardized for the entire economy. Some elaborations of GET attempt to model the institution of fiat money (intrinsically worthless money like bank notes, as opposed to intrinsically valuable bullion), but in general a price is simply a relative rate of exchange—five avocados for three bananas. In the absence of fiat money we can, for the sake of simplicity, calculate all prices in terms of a "numeraire," for example "cost in number of avocados." A market is constituted by the existence of a price system. It is usually, but not always, assumed that the price of finding exchange partners and then transacting exchanges is zero. Equilibrium is then said to obtain when the prices of commodities are such that quantities demanded equal quantities produced for every commodity because all consumers (including stockholders) achieve utility maxima by exchanging parts of their endowments at those prices.

III

Two concepts clearly emerge as the most fundamental from this way of laying out the structure of GET. One is the *commodity*. I did not give it its own paragraph because it is difficult to fully explicate in terms of the others (though one might say that commodities simply are the things that give consumers utility), but it was prominently featured in the explication of each of the other concepts. Consumers are exchangers of commodities. (And producers and annihilators of them, though these processes are not objects of economic scrutiny.) Preferences are for commodities. Utility functions are from quantities of commodities to an index of utility. Endowments are of commodities. Firms transform some commodities into others. Netput vectors are of quantities of commodities, and production functions

are from quantities of commodities to quantities of commodities. Equilibrium obtains when all agents' plans for doing things with commodities are mutually consistent. And prices are relative exchange rates of commodities.

The other fundamental concept is *preferences* (or utility functions). Even commodities can almost be characterized in terms of them. Karl Marx, for example, said, "A commodity [*Ware* in German] is, in the first place, an object outside us, a thing that by its properties satisfies human wants of some sort or other" (1967, 35). Marx did not have a GET, but we might try to adapt his idea. If we take preferences as the most fundamental concept and then say that commodities are those things over which consumers have preferences, we can continue with the above explication of all the other key concepts in terms of *commodity*.

It would be very interesting to establish a complete hierarchy of fundamentality for economic concepts, but the main argument of this essay does not require it. I do, however, want to concentrate attention on *commodity* and *preference* and leave aside the others, which are all arguably less fundamental than these two. I think it is plausible to take *commodity* as fundamental in the *description* of economic phenomena. What we observe and wish to explain are exchanges of commodities and the properties of these exchanges. *Preferences*, then, are fundamental in the *explanation* of the phenomena. We understand the commodity exchanges as having the properties they do primarily because of facts about preferences.

First, however, a potential objection needs to be considered. It is sometimes claimed that economics is primarily about relative price levels and the fact that equilibrium exists, instead of about facts about consumers. But relative price levels are nothing more than the rates at which commodities exchange; prices are properties of what is really basic, commodities. The fact of equilibrium (if it is indeed a fact) is also, at root, a fact about preferences and commodities. Equilibrium obtains when all relative prices are constant and all agents are maximizing their utility given that price level and their endowments.

This encapsulation of the GET framework is, I hope, an aid to understanding the structure of the theory, how the parts of the model of actual economic phenomena fit together. I now want to proceed to the matter of interpreting the model. What does GET tell us about actually observed economic events? It is convenient to begin with an analysis of the concept of *utility*.

IV

Theoretically coherent economics that is clearly ancestral to contemporary GET can be regarded as beginning around the 1870s, with what is called the "Marginalist Revolution." Utility was at this time incorporated into economic theory as a psychological or psychophysical quantity that was measurable in principle. It came to be called "cardinal utility" because it was believed that a person's

utility level could be assigned a numerical index that was objective in the sense that it served for making interpersonal comparisons of utility levels.[9] Economists became disaffected with the commitment to cardinal utility in the face of growing evidence that most kinds of human behavior are not primarily motivated by cardinal utility maximization and that there probably is no such psychophysical quantity in the first place.[10] It was, however, not possible for this disaffection to take root because there was no competing theory with as much potential for explanation and for guiding economic political policy as the utility theory.

This changed around the beginning of the 1930s with the speedy development of a well-developed and mathematically consistent theory based on "ordinal" instead of "cardinal" utility. J. Hicks and R. Allen, in their seminal paper (1934), showed how demand functions for individuals, theoretically and empirically important functions from prices of commodities to the quantities of that commodity exchanged for, could be derived from something less than psychophysically based rankings of available commodities. All that is mathematically required is utility functions that preserve only the consumer's ordering of options and not the "cardinality" or magnitudes of the utility differences between options.[11] In other words, ordinal utility functions come in classes that are equivalent up to linear transformation. Therefore, they demonstrated that cardinal utility functions contain more information than is needed to derive demand curves from first principles about individual economic agents. (The derivation of demand curves was, and still is, considered very important because some of our best economic data is for market demand curves.)

It was natural to hope that the excess content in cardinal utility functions was exactly the part of the theory that was in conflict with the psychological facts that had been emerging. In 1938, P. Samuelson showed how this hope could be realized and, in a way, exceeded. He mathematically derived the most characteristic and important results of consumer theory from a formalism that did not require the postulation of even ordinal utility functions. Instead of deriving the properties of individual demand curves from ordinal utility functions and budget constraints, points on the demand curves were taken as given. Samuelson was then able to prove that these two hypotheses are equivalent:

> i) The commodity bundles chosen by the consumer (i.e., the points on the demand curve) conform to the results of maximizing a well-behaved ordinal utility function.
> ii) Points on a demand curve that satisfy the Strong Axiom of Revealed Preference (SARP) fully characterize a well-behaved preference map.[12]

The interpretive significance of Samuelson's formalism is considerable. Since SARP is a constraint on the *choices* that consumers make and not on their utility functions, the new formalism suggested[13] that the old interpretation, the one in which the theoretical concept of utility (or preference) plays a role in explaining

how economic behavior is partly caused by psychological facts about consumers, be abandoned. In short, it suggested that economists instead use overtly observable choices and SARP to *construct* utility functions when they might prove *convenient* as instruments for prediction. The machinery of consumer theory was thereby almost completely severed from the psychological thinking that had begotten it. It was easy to see this as emancipating economics from constraints imposed by another science whose relevance to purely economic concerns was no longer entirely clear.[14] As Samuelson put it,

> The discrediting of *utility* as a psychological concept robbed it of its only possible virtue as an *explanation* of human behavior in other than a circular sense, revealing its emptiness as even a construction. . . . The introduction and meaning of [any fact about indifference curves][15] independent of any psychological, introspective implications would be, to say the least, ambiguous, and would seem an artificial convention in the explanation of price behavior. . . . I propose, therefore that we start anew in direct attack upon the problem, dropping off the last vestiges of the utility analysis. This does not preclude the introduction of utility by any who may care to do so . . . (1938a, 61–62)

This provides a partial characterization of an interpretation of consumer theory based on the Revealed Preference formalism. I shall call it the "Utility-as-Revealed interpretation."

Utility-as-Revealed Interpretation

> Consumers' psychological states are not part of the economic explanation of their behavior. Any reference to concepts such as utility and preference is for the sake of convenience and is fully eliminable from the theory.

Something like this is often called the "Revealed Preference Interpretation." It is good to avoid this name because Revealed Preference is not an interpretation or even a theory—it is an axiomatic formalization of a part of consumer theory.

According to this interpretation, we are not to think of observed choices as revealing what the *preferences* are at all. If a consumer makes choices that are consistent with SARP, then he acts *as if* he were maximizing utility or getting onto the highest indifference curve, but there is no ontological commitment to these mentalistic things. And since the theory is not ontologically committed to them, then any facts that other scientists (e.g., psychologists) discover about them can safely be ignored.[16]

So the revealed preference interpretation provides a rationale for disregarding the severe problems that empirical psychologists were discovering in cardinal utility theory. The ordinal utility theory, however, does not *require* the revealed preference interpretation.

There is a second important interpretation in which preferences (represented

by ordinal utility functions) are understood to occupy a much more important position in the theory. They are regarded as providing part of a causal *explanation* of why economic agents behave as they do, instead of simply describing their behavior; I shall call it the "Utility-as-Explanatory Interpretation."

Utility-as-Explanatory Interpretation

Consumer theory explains economic behavior as well as describing and predicting it. The explanation comes from a causal story: a) An agent is confronted with a choice set of commodity bundles that is partly determined by his endowment and income and partly by availability; b) he compares this choice set to his indifference map; and c) he chooses the bundle that lies on the highest indifference curve.

Thus, indifference maps are taken to be a partial *cause* of the observed behavior, qua *economic* behavior. Alternatively, since well-behaved preference maps are fully represented by ordinal utility functions, we might substitute for b) and c): b′) He plugs this information into his utility function, and c′) chooses the bundle that is calculated to give the greatest utility. Hence, the name. It is quite certain that little of the "comparing" or "calculating" referred to is done consciously, but that is to be expected. Many complicated psychological tasks involve substantial amounts of cognition that does not take place at the conscious level. The understanding and production of language is an example. In what follows, I shall argue that the Utility-as-Explanatory interpretation is superior to Utility-as-Revealed.

The crucial differences between Utility-as-Revealed and Utility-as-Explanatory can be brought out by familiar episodes from the history of other sciences in which analogous distinctions have been important. Consider the mathematical expression for the Balmer series of the hydrogen atom. Before scientists discovered how to derive this expression from the Bohr model of the atom, it appeared to be a fortuitous piece of numerology concocted to conform to existing data. The fact that it gave excellent empirical results and was eventually shown to predict even the frequencies of previously unobserved spectral lines contributed to scientists' confidence in the relationship, but Balmer's formula did not *explain* the phenomena that it described. In this case, an explanation of the frequencies required some account of what *causes* the lines to come out the way that they do. The explanation was provided only after the appearance of spectral lines was understood as resulting from differences in the energies of various excitation states of the atom's electrons.

According to Utility-as-Revealed, utility functions must have the status that Balmer's formula had before physicists knew how to derive it from more fundamental laws. Utility functions are concocted to fit the available data, and we hope that they are able to correctly predict previously unobserved phenomena. It is not, according to Utility-as-Revealed, in order to inquire as to *why* a certain utility function accounts for the data. Economists who believe in Utility-as-

Revealed are not interested in deriving the utility functions from anything more fundamental or more general, nor are they interested in trying to understand how a utility function might be connected with the causes of the behavior that it describes. But according to Utility-as-Explanatory, theoretical economic entities like utility functions ought to explain phenomena by providing a story about their causation. Accepting Utility-as-Explanatory commits one to attributing a causal role to the facts about human economic agents described by utility functions. Analogously, we now understand Balmer's formula (or Balmer's law, as it appears to us now in light of its derivation) to describe causally significant properties of entities such as atoms, electrons, and photons.

If a causal role cannot be found for utility functions, then there is some danger that they are not like Balmer's law as much as they are like Bode's law. Bode's law (which is really not a *law* at all) was a mathematical expression concocted to give the distance of the planets, Mercury through Saturn, from the Sun. It turned out, like Balmer's law, to be an astonishingly accurate predictor. The mean orbital radii of the asteroid belt and Uranus fit Bode's law almost perfectly. Even its predictions for Neptune and Pluto were within reason.[17] But Bode's law is obviously only a historical curiosity. Unlike Balmer's law, it is not recognized as a law, even though it has never been convincingly disconfirmed.[18]

According to Utility-as-Revealed, economic descriptions of choice behavior are more like the description given by Bode's law than the one given by Balmer's law. It says that utility functions are supposed to do no more than describe actual choices, thus their variables and parameters are specified post facto so that they necessarily yield the correct results. There is no presumption about the nature of any underlying causal mechanism. It may then be hoped that they are also useful for prediction. Many commentators have pointed out that this practice can reduce utility functions to tautologies. It is possible to concoct a utility function that can give an empty account of almost any behavior. This means, moreover, that if at time T we assign utility function U_i to agent i and her behavior at T + t disconfirms U_i, it is a similarly trivial matter to modify U_i to account for the new behavior as well. The result of this uninteresting process is clearly a mere description and not a scientific theory at all. The situation is directly analogous to, say, providing neo-Ptolemaic derivations of all ephemeridical data through today's date. With enough circles and epicycles, there is little question that it is possible to do this, but no one would care much about the results. Providing Ptolemaic derivations today would be little more than an exercise, because the results could not afford us with an explanation of any astronomical phenomena. The Ptolemaic setup has no chance of being even approximately true, or true *ceteris paribus*, given our current understanding of astronomy.

The problem with unsatisfactory theories like Ptolemaic positional astronomy or Bode's law is not only that they can be manipulated to account very accurately for a large number of observations. Reasonable theories like Newtonian

mechanics can likewise account for any collection of celestial motions by simply postulating sufficient ad hoc forces. Instead, theories like Ptolemy's fail primarily because of the severely limited range of phenomena that they can account for; Ptolemaic theory and Aristotelian spheres apply only to the motion of the Sun, Moon, and six planets, while Newtonian mechanics applies to countless kinds of motions. What makes contemporary Ptolemaic theory trivial is the a priori restriction on the range of phenomena that have evidential bearing on it. It is not possible to use unrelated, well-confirmed theories to get access to any other information that could independently confirm the problematic theory. In contrast, the Newtonian account of celestial motions can be independently corroborated by terrestrial motions. The Newtonian formulas fitted to celestial motions are constrained by phenomena other than the ones they are devised to describe. There is an analogous difference between Utility-as-Revealed and Utility-as-Explanatory. In the former there is, by stipulation, no observational or experimental access to utility functions independent of the instances of behavior that they describe.

In Utility-as-Explanatory, however, the whole point of mentioning utility is to give an insightful characterization of how and why the observed behavior comes about. Moreover, since the maximization of utility is given a part in a causal story, it has real ontological standing. This means that there are, in principle, ways of finding out about it aside from observing its effects on overt economic exchanging behavior. This is important. Seventeenth-century natural philosophers would have been much more suspicious of universal gravitation if it were introduced only as a means of accounting for celestial motions. It was a great advantage that the same force postulated to account for those phenomena had already proven its worth in (what seemed to be) an entirely different domain—terrestrial motions. In another article (Nelson 1986), there is a fairly detailed treatment of how this might work in economics. I can only briefly summarize the results here.

The Utility-as-Explanatory interpretation eventually requires experimental investigation of individual economic behavior. One basic, but powerful consideration in favor of this position is that once preferences are assigned a causal role, they must be treated as things or entities in the strongest sense. And when one is scientifically investigating *things*, one does not deliberately ignore any available information about these things without compelling reasons to do so. An excellent source of information about individuals' preferences, perhaps the best source, is the actual economic behavior of the individual in question. Attending primarily to individuals does not have the terrible result that interesting generalizations cannot be made across individuals. Economics would not be of much interest if it produced a separate theory for every agent in an economy. Even if consumers differ in their relative appreciation of avocados and bananas, however, their utility functions may be represented by similar mathematical forms. One

consumer's preferences might, for example, be of the form (3 × number of avocados + 2 × number of bananas) while another's might be of the form (number of avocados + 5 × number of bananas), but both of these consumers have utilities that are linear in avocados and bananas. What needs to be done to determine what particular individuals' utility functions look like is some experimental investigation in either the laboratory or the marketplace.[19]

V

So far, the argument for preferring Utility-as-Explanatory to Utility-as-Revealed has relied on considerations familiar to philosophers of science. While these are, I think, quite convincing in their own right, it is initially worrisome that many, perhaps most, economists who have considered the matter say that they prefer Utility-as-Revealed. If there were reasons deeply rooted in the necessities of economic practice for their stated preference, we would have a very perplexing problem about the foundations of economics. Therefore, let us continue by examining not what economists say about how the theory should be interpreted, but instead the interpretation that emerges from their own discussions of economic, and not philosophical problems.

In a very revealing passage in an important textbook, J. Henderson and R. Quandt write,

> At the beginning of this chapter, the cardinal approach to utility theory was rejected on the grounds that there is no reason to assume that the consumer possesses a cardinal measure of utility. By the same token one could question whether she even possesses an indifference map. It can fortunately be proved that a consumer who always conforms to the axioms of revealed preference must possess an indifference map. . . . If the consumer does not conform to the axioms, she is said to be "irrational". Her inconsistent actions mean that she does not possess an indifference map, *and the shape of her utility function cannot be determined by observing her behavior*. (1980, 46, emphasis added)

Is this the correct way to handle inconsistent actions (e.g., actions that seem to reflect intransitive preferences) according to Utility-as-Revealed? It seems strange to say that in these circumstances the consumer "does not possess an indifference map"; Utility-as-Revealed says that descriptions of the indifference maps are constructed by simply recording the choices that the consumer would make in given circumstances (or, according to Revealed Preference, recording the actual choices and smoothly interpolating the missing points on the map). This can be done no matter what the choices turn out to be. The strangeness can be resolved by remembering that 'indifference map' is shorthand for 'indifference map conforming to some basic assumptions'. Thus, in standard economic usage,

what constitutes an indifference map cannot be just any batch of points in commodity space.

So we are supposed to say that the irrationally behaving consumer does not possess an indifference map. This is in accord with the teaching of the Revealed Preference Interpretation. But Henderson and Quandt go on to conclude that, "the shape of her utility function cannot be determined by observing her behavior." That clearly implies that a utility function does exist and that it does conform to the standard restrictive assumptions, but in this case the consumer does not "pay attention" to it while making some economic choices. This implication is inconsistent with Utility-as-Revealed. It is a mathematical fact (and true on any interpretation of the theory) that ordinal utility functions are equivalent to well-behaved indifference maps. Therefore, if we are going to say that the errant consumer possesses no indifference maps, then we must also say that she possesses no utility function. The only alternative is to maintain that she does have a utility function or the equivalent indifference map, but (sometimes) does not pay attention to it, or does not use it, when choosing. But Utility-as-Revealed says that indifference maps must be understood as being mere constructs of choices. The internal inconsistency is unavoidable.

I think that this confusion finds its way into the quoted passage because Utility-as-Explanatory is so reasonable that one must be extremely vigilant if one wishes to avoid believing it at all costs. A very neat account of the errant consumer's behavior has already been hinted at. She has a utility function. How is it then that she goes astray while making an economic choice? It is because something has interfered with her accurately maximizing her utility function. Perhaps her knowledge of the choice that she was making was deficient, or perhaps she was nervous or short of time so that she made an error, or perhaps the choice was so complicated that it was beyond her mathematical abilities to make the maximizing choice, and so forth. If we are to come up with *economic* explanations, it is probably wise to avoid trying to account for every conceivable type of interference with purely economic phenomena.

This is probably what Henderson and Quandt meant, but if one is committed to Utility-as-Revealed, it is quite difficult to say what one means. If a utility function is merely a description of an agent's behavior, and we do not want to count behavior that has been interfered with in ways that obscure the economically relevant features of the behavior, then how can the function be characterized? We must say something like this: a utility function describes the choices an agent *would* make *if* the choices that *were* to be made were not interfered with in *certain* ways. The counterfactuals in this characterization must evidently be cashed out psychologically, but Utility-as-Revealed proposes to ignore this resource. So we are returned to the fact that Utility-as-Revealed precludes actually obtaining an indifference map for an actual individual economic agent. That task would involve either dabbling in psychology or employing aggregate data.[20]

Another context in which economists seem to be committed to Utility-as-Explanatory in spite of themselves is in discussions of what commodities ought to appear as the independent variables (or "arguments") of utility functions. This is illustrated by microeconomic treatments of money (and some macroeconomic treatments of wealth that have microfoundations in GET). It is overwhelmingly obvious that one thing actual economic agents enjoy getting their hands on is money. This is true even when the stuff that serves as money in a particular economy has no other important practical use. Rational agents are just as anxious to acquire bits of paper printed by governments or banks whose only value lies in that fact that they are, for whatever reasons, considered to be money. They will prefer a ten-dollar bill to nine dollars' worth of gold, even though the gold can be useful and valuable for purposes other than exchange. This kind of intrinsically useless money is called "fiat money."

The point emerges when we consider a revealed preference experiment. A rational agent is asked to choose between a bucket of dollar bills and an avocado. She is then asked to choose between a dime and a bucket of avocados, and so forth for a very large number of combinations of avocados and U.S. currency. After completing the experiment, it is an easy matter to construct an indifference map for this agent between avocados and fiat money, and therefore partially to construct an ordinal utility function that includes avocados and fiat money as dependent variables. It is significant that economists are extremely reluctant to do this. Neil Wallace writes,

> The principal way of abandoning intrinsic uselessness [the thesis that fiat money is never wanted for its own sake] is to make money an argument of utility functions or engineering production functions. But this begs too many questions. Is it fiat money or commodity money that appears in these functions? What if there are several fiat moneys, those of different countries? Do all appear, and if so, how? Does Robinson Crusoe have fiat money as an argument of his utility function? And what about other pieces of paper? . . . All of this is to say that theories that abandon intrinsic uselessness will be almost devoid of implications. (1980, 49)

James Tobin is more blunt,

> Clearly enough, the value of paper money does not derive from the beauty of the engravings; the practice of putting money stocks in utility functions is reprehensible. (1980, 86)

Why is it "reprehensible" to include certain dependent variables in utility functions if the functions that result are the best at saving the phenomena? Why does including some extra dependent variables render a utility function "devoid of implications"? It is especially surprising to see a greatly respected economist wor-

ried about begging empirical questions. Is it fiat money or commodity money? Why not obtain the answer by repeating the revealed preference experiment I suggested, substituting a commodity money for avocados? Is it U.S. money or U.K. money? Why not find out by offering combinations of buckets of each variety? Which, if any, does Robinson Crusoe have in his utility function? If we can find him, we can experiment on him. These utility functions are empirically very rich and not at all "almost devoid of implications."

I conclude that the powerful sentiments against including money in utility functions stem from the idea that utility is a measure of something real. Perhaps it is not very plain what the "something" is supposed to be, whether it is a measure of psychic satisfaction, or literally something that has utility as a property, or something else. Anyway it appears that according to these and many other extremely influential economists, a piece of paper cannot give one this something (except in small quantities when it is used as wallpaper or to light a cigarette); fiat money can only be exchanged for another commodity, another *real* commodity, that does confer the mysterious something on a agent. But any theoretical entity that is separable in this way from the observation of actual choices of economic agents cannot be given a meaningful interpretation by Utility-as-Revealed. If utility is going to be considered an objective psychological construct with causal efficacy, then any economic theory that is going to be a theory of this construct requires an interpretation something like Utility-as-Explanatory. Therefore, the popular view of money expressed in the above quotations is committed to a denial of Utility-as-Revealed.[21]

A third example of the discomfiture that can be caused by adherence to Utility-as-Revealed is provided by Milton Friedman's analysis of demand (1976). He writes,

> . . . [we] shall suppose that the individual in making these decisions acts *as if* he were pursuing and attempting to maximize a single end. This implies that different goods have some common characteristic that makes comparisons among them possible. This common characteristic is usually called *utility*. . . . We observe that people choose; if this is to be regarded as a deliberative act, it must be supposed that the various things among which choice is made can be compared; to be compared, they must have something in common.
>
> Let X, Y, Z, etc., stand for the quantities of various commodities. Then the notion that these commodities have some element in common and that the magnitude of this common element, utility, depends on the amounts of the various commodities can be expressed by writing utility as a function of X, Y, Z. . . . (35–36)

The most striking thing about this treatment is that it makes utility a property of the commodities themselves. It is more common to treat utility as a representation

or index of the consumer's preferences. The commoner treatment, then, makes the amount of utility associated with any commodity object essentially relative to the consumer who consumes it. Each consumer has his own, perhaps unique, utility function that determines how much utility he "gets" from a given bundle of commodities. But according to Friedman, the total amount of utility from a commodity bundle is obtained by combining the amounts of utility inherent in each commodity in the bundle. Facts about the consumer, therefore, are irrelevant to determining the utility level.

Friedman's motivation for this nonstandard approach is clear. The already sparse ontological requirements of Utility-as-Revealed appear to have been further purified. Utility-as-Revealed allows a specially defined property of utility to be *attributed to* individual consumers, or at least to an idealized average individual. Friedman does not allow even this hint of economic cognition on the part of consumers; utility is a completely objective property of physical objects. Consumers use information about this utility to make otherwise inscrutable deliberations resulting in observable choice.

The proposal is difficult to interpret. It apparently entails that there is only one utility function, because utility is a "common element" in commodities, and a utility function simply gives the total magnitude of the common element that results from combining commodities (and hence, utility) in bundles. If follows that consumers' deliberations are all based on the same central economic fact—the amount of utility in the bundles under consideration. This means that if two consumers are weighing the choice between a bowl of beans and a bowl of rice, the only economically relevant difference in their deliberations will be their incomes. So if they have identical incomes, any difference in observed choice must result from economically inscrutable aspects of their deliberations. To most economists, this result will seem daft. If one consumer chooses the beans and the other the rice, microeconomics ought to say that the first *prefers* rice to beans, and the second prefers beans to rice. Or, equivalently, one *gets* more utility from rice than from beans, and the other more utility from beans than from rice. According to Friedman, microeconomics must say that one deliberated differently from the other, and the whole explanation of this must be found in another science, like psychology.

Another way of expressing this position is to say that, from an economic point of view, consumers are identical up to their budget constraints. Instead of identifying consumers by *their* utility functions, all economically explainable behavioral differences are to be attributed to diverse budget constraints resulting from differences in incomes or prices. This consequence of Friedman's view has seemed attractive to a few other notable economists (though for different reasons), but it has some grave flaws. Perhaps the worst of these is that it is easy to construct counterfactual economic situations that ought to be explainable by any account, but are not by this one. Consider again the two consumers with iden-

tical incomes who, when faced with the same choice between a bowl of beans and a bowl of rice, make different choices. There is a natural and satisfying explanation of this phenomenon in the Utility-as-Explanatory interpretation of microeconomics, and there may be at least a pretense of explanation in Utility-as-Revealed. The Utility-as-Element-of-Commodity interpretation however, like the hapless anti-Copernican who denied the responsibility of explaining planetary phases, arbitrarily and unnecessarily restricts the scope of the theory. Since there are no mitigating circumstances, the rejection of the interpretation is indicated.

Both an investigation of economist's actual practice and independent methodological considerations lead us to the conclusion that the best interpretation of utility theory requires us to think of concepts such as utility as robustly real. The reality of these concepts does not derive its significance by extending economic theory to additional results about economic markets; the theory's usefulness for dealing with markets is unaffected by (sensible) methodological interpretation. The Utility-as-Explanatory interpretation does have some important empirical implications for psychology (utility is to be psychologically real; see Nelson, 1986) and also for the economic behavior of individuals. In the next section, I shall argue that there are further implications of considerable philosophical interest. One might, however, object at this point that these conclusions are too strong. From an examination of economic theorizing and some basic lessons from the history of science, I seem to have concluded that some kind of scientific realism is true, and instrumentalism and other anti-realisms are false. Surely such a conclusion would require more purely philosophical reasoning.

But my conclusions do not involve any controversial philosophical doctrines about realism and instrumentalism. I have argued that Utility-as-Explanatory opens the possibility that important facts about utility functions can be inferred from observations of individual economic behavior and, perhaps, even from some kinds of noneconomic behavior of interest to psychologists. There is no sensible scientific instrumentalism of the general sort that is inconsistent with this. Sophisticated instrumentalists say that we have no warrant to regard unobservable theoretical constructs as ordinary entities.[22] This is a purely *philosophical* stance to adopt towards a scientific theory; it does not have any implications for how the science is to be conducted. In particular, it obviously does not proscribe any means of investigating the properties of the theoretical constructs. Proper scientific method, according to the sophisticated instrumentalist, requires us to behave *as if* the objects apparently referred to by the theory exist. It may even be psychologically helpful for the scientist to actively pretend that they exist.

But this is all that is required by the arguments I have given for Utility-as-Explanatory. This interpretation does not require any particular philosophical stance on the precise ontological standing of theoretical constructs that are, in some sense, unobservable. So, for example, if we are to pretend that utility functions exist, the pretense commits us to recognizing the potential relevance and im-

portance of experiments done in psychology laboratories that (pretend to) investigate them. Neither Utility-as-Explanatory nor sophisticated instrumentalism permits us to ignore psychological effects of utility functions on the grounds that they do not really exist. Similarly, an instrumentalist does not argue against the construction and use of electron microscopes on the grounds that electrons do not really exist. It is enough that electrons seem to exist. Thus, the argument of this paper has no direct bearing on realism/anti-realism issues. In what follows, I shall take this for granted and speak of utility functions, commodities, and the like as "existing."

VI

I have argued at some length for the Utility-as-Explanatory interpretation. This interpretation reinforces the plausible idea that the concepts of utility and preference as they appear in GET are more than convenient fictions. They seem instead to be closely related to corresponding putative natural kinds in the world. This has important implications for our initial problem about the degree of empirical success of economic theory. I have already described how Utility-as-Explanatory places potential empirical constraints on economics, constraints that come from psychology, neurology, or some other science.

One obvious explanation of the empirical difficulties of economics would be that it is a false theory precisely because these constraints are not met. It may be that experimental studies of the psychology of economic behavior, or of neurology, etc., will show that nothing that can be sensibly represented by preferences or maximizing goes on when humans behave economically. It is very unfortunate that very little of this kind of experimental study of economic behavior is being done. We are beginning to see a good deal of data on overt economic behavior under laboratory conditions (there is the kind of work initiated by Vernon Smith and his associates, see e.g., Smith 1982) and some data on animals like pigeons and rats. We will need to wait quite a long time before we have enough results from investigations of the right kinds of "nonovert," "internal" economic phenomena to be able to see whether economics actually meets the relevant constraints (again, see Nelson 1986 for more discussion).

If I am right, then interpreters of economics are faced with a dilemma. They must either adopt the mistaken Utility-as-Revealed interpretation or sit and wait for the verdict of currently unpracticed experimental procedure. In what follows, I shall be examining a potential way out of this dilemma. Those who fear that economics might be unfairly convicted by constraints imposed by arguments concerning a difficult theoretical notion such as utility will welcome the possibility of this way out.

The position is developed by shifting our focus from the concepts of utility and preference back to the other central concept, the commodity. Because of the close

connection between *utility* and *commodity*, the adoption of Utility-as-Explanatory yields a parallel interpretation of *commodity*. Commodities, like utility functions, are more than a picturesque aid to describing actual choices. They are the *things* that real agents choose to exchange and produce; hence, in GET they are an essential part of the explanation of economic behavior. This follows from Utility-as-Explanatory and the fact that utility functions can be *defined* as mappings from quantities of commodities to levels of utility.

The case for a robust interpretation of commodities may be even stronger than the case for so interpreting utility. Utility is an indispensible link in the explanatory chain, and there is potentially a variety of means by which its effects might be observed. Still, we never directly observe a utility function; we infer their existence from direct observations of their various effects. Commodities seem different in this respect. Although the theoretical notion of a commodity is central to the very abstract GET, we are inclined to think that modern people have a clear pretheoretical notion of what a commodity is. Utility functions, in contrast, would probably not occur to a layperson trying to grapple with interesting economic phenomena—a "folk economist." Some things that turn out to count as commodities in the theory are admittedly somewhat surprising: shares of stock in financial institutions and currency futures, for example. But the paradigm cases of actual commodities are such familiar items as avocados, shoes, and theater tickets. Even sophisticated treatments take it for granted that GET's *commodities* are to be closely related to, or even identified with, what we pretheoretically think of as commodities:[23] "A general equilibrium theory is a theory *about* both the quantities and the prices of all commodities" (Arrow and Hahn 1971, 2, emphasis added). Most economists, if asked to describe what phenomena they are theorizing about, would reply with facts about ordinary objects and their aggregates. The price of bananas at the supermarket, the output of the auto industry, the wages of steelworkers, and the glut of avocados. These seem to be the natural economic kinds that we want to have a theory of; we do not need a theory to tell us what they are. Similarly, it is good to have a theoretical understanding of water, but we are inclined to think that we know much about water before we learn any science. We want to say it is manifestly a natural kind.

So it seems extremely plausible to think that the central concepts of economics, the natural-kind *terms* of the theory, do pick out natural economic kinds in the world. And, one might think, we do not need to worry about the interpretive dilemma posed by the analysis of utility. Even if we became convinced that GET and related theories were simply bad and ought to be rejected, it would be plausible to think that we had bad theories *of* the kinds of things these theories are about. We would seek better theories of the same things: the purchases of consumers, fluctuations in banana prices, the relative wage rates of Wall Street analysts and professors, and so on. These are the kinds of things that are salient

in our economic life, and we want a scientific theory to make events involving them explainable, predictable, and intelligible.

Here we can draw an interesting analogy with the philosophy of psychology. Many think that there is something called *folk psychology*, an informal theory that we all learn without formal study.[24] This theory, or prototheory, is relied upon in our everyday lives to explain intentional behavior as resulting from our acting on our beliefs to fulfill our desires. Folk psychology may be further articulated to deal with other kinds of mental states like worries, suspicions, imaginings, and rememberings. Some philosophers say that folk psychology is almost useless to the modern scientific psychologist. They think that, as a matter of empirical fact, the vocabulary of folk psychology as it is presently understood does not refer to natural kinds. They expect that the advance of cognitive psychology and neuroscience will force us to either eliminate concepts like *belief* and *desire* from a true scientific theory of behavior, or to heavily revise our folk-theoretic understanding of them. There is, however, a second view of the status of folk psychology. The proponents of the second view argue that pretheoretic concepts like *belief* are not subject to revision by science. If some scientific theory tells us that there is no such thing as beliefs, then this is ipso facto not a theory *about* intentional behavior. It might be a fine theory for what it is about, but it would be quite irrelevant to a range of phenomena we cannot help but regard as centrally important. Similarly, if some biological theory were to tell us there is nothing scientific to be said about *human beings* because the important generalizations are over some other kinds, then we would stubbornly look for another theory to tell us what we want to know about humans—humans are inescapably interesting.

If this second view is correct, then there are some domains for which we know what some of the natural kinds are before we do any real science. Before we do any psychology, for example, we are supposed to know what we want psychology to do for us: explain how our beliefs interact with our desires to contribute to the production of our behavior. Of course, the second view is not correct for all sciences. People used to mistakenly think that Earth was essentially different from other planets. A science whose development was constrained by the necessity of respecting pretheoretical beliefs about the Earth would not have turned out (did not turn out) very well.[25] It might be true, nevertheless, that we are not always prepared to let science have the first cuts when we carve up the world into intelligible slices.

Perhaps an analogue of this second view about folk-psychological concepts is appropriate for economic concepts as well. *Commodities*, *preferences* for commodities, *prices* of commodities, and so forth, may be so deeply ingrained in our pretheoretical thinking about economic life, in our folk economics, that we shall insist that a fully adequate scientific theory of economic phenomena be *about* them. Another theory that dealt with some aspects of economic life, but did not employ these concepts simply wouldn't be *economics*. If this is right, then it is

no surprise that a highly coherent theory employing exactly these concepts should have developed. Perhaps I need not have produced a long argument to establish what the natural economic kinds are; instead, the argument might be viewed as a checkup on the health of the theory. Had the theory been built around other concepts, it would be unhealthy economics. Is this optimistic, conservative outlook justified?

To answer this question it is helpful to begin by reconsidering the nonconservative view of the scientific viability of folk psychology. On what ground might nonconservativism be justified? The most obvious would be the clear prospect of a successful empirical theory of human behavior that made no use of folk-psychological terms as they are ordinarily conceived. But psychologists have not yet found anything like this; the best we have are some arguments that research programs that are not committed to folk-psychological concepts are more promising. Another convincing though less powerful ground for nonconservativism would be a demonstration that scientific psychology based on folk concepts was quite hopelessly stagnant. In this case, we might abandon conservative psychology even in the absence of a competitor; we might search for a replacement *in vacuo* or even conclude that scientific psychology is impossible. Again, as a matter of empirical fact, this ground does not obtain.

Do either of these considerations, that is, the prospect of a better nonconservative theory or a demonstration of conservative stagnation, come to bear in the case of economics? We might be persuaded that there are serious competitors to economics in the image of GET, but relatively few economists will claim that we are in possession of a *better* theory. There is, however, some consensus that GET is in some danger of stagnation. Whether the theory itself is stagnating is controversial. The theory is constantly being beautifully articulated, but some would argue that the details of existing theory are being highly polished without any fundamental advances taking place. But it is uncontroversial that our ability to *apply* the theory to predict, control, or even explain actual phenomena is stagnating. As noted at the start of this essay, many prominent economists have argued that economics is almost a paradigm case of a stagnating science.

The question whether we should revise or abandon our folk-psychological concepts will not have much urgency until we see some decisive empirical developments. But in economics, the argument from stagnation is strong enough—I shall assume this in this essay—to make us take seriously the possibility that the root of the problem with GET-based economics is its conservative nature. The argument from stagnation is apparently reinforced by some examples of progress in the physical sciences. In physical science, the starting point for the development of a theory is often a pretheoretical notion—a part of folk science. For example, the untutored human intellect tends to see mechanical phenomena in a rather Aristotelian impetus framework. Bodies suffering impacts gain something, impetus, that propels them into motion and sustains motion until it is depleted.[26]

Other examples: it used to be thought that the Earth was uniquely distinguished from the celestial bodies and that it, therefore, was governed by a wholly different set of physical principles. Also, it was very widely thought that the heart was the center of most bodily functions and of life itself, and that electricity was a kind of fluid—it is easy to go on.

In each of these cases, beginning with scientific concepts picked out by terms in the vernacular, that is with folk-scientific concepts, led to empirically bad scientific theory. One might even think that it is partly *characteristic* of scientific inquiry (as opposed to other ways of finding out about the world) that our inherently uninformed folk concepts be rejected in favor of those that allow us to increase the precision and intelligibility of our predictions and explanations. In light of the considerations raised by historical cases, we might judge it an error to fixate on folk concepts when doing science.

Could this be the root of the persistent empirical difficulties facing economics? Is it the case that economics has uncritically taken folk notions and built a vast theoretical edifice upon such unsubstantial foundations? If *commodity* is inescapably a vernacular kind, a folk concept, and hence not a natural kind, and if, moreover, we require that economics be about actual commodities, then the beginning of an apparent explanation of the theory's shortcomings presents itself.

It can be shown, however, that this is not a good explanation. There is, I shall argue, at least one imposing example of a very successful scientific theory that was founded on a folk concept. I have in mind Newtonian mechanics—the theory as it appears in Newton's *Principia* and not later elaborations and improvements of the theory. Just as modern economics is founded on the *commodity*, Newtonian mechanics is founded on the *body*.

I first need to produce some evidence for this idea. There appear to be competitors for the status of foundational concept in Newtonian mechanics, or it appears, at least, that there is no single central-kind term. There are *motions* (and this even seems to be a folk concept), both uniform and accelerated; there are *forces*; and there are *interactions*. I shall first argue that the concept of *body* is the folk concept. I shall then show how the other important kinds in the theory are derivative in the same way that important economic kinds can be derived from *commodity*.

The word *body* (*corporis* in Latin)[27] first appears in the *Principia* in Newton's own preface to the first edition. There he says, "But since the manual arts are chiefly employed in the moving of *bodies*, it happens that geometry is commonly referred to their magnitude, and mechanics to their motion" (C xvii, emphasis added). Manual arts, of course, deal with what we prescientifically take to be bodies, or "physical objects." So mechanics is to be *about* the motions of ordinary bodies. But after reading this prefatory remark, one might still wonder whether the *theory* deals with these motions in virtue of dealing with some idealized kind of body that only resembles or represents actual, ordinary bodies. Such is not the

case. The famous first law of motion says, "Every body continues in its state of rest . . . ", and the elucidation goes on to say:

> Projectiles continue in their motions, so far as they are not retarded by the resistance of air. . . . A top . . . does not cease its rotation, otherwise than it is retarded by the air. The greater bodies of the planets and comets . . . preserve their motions both progressive and circular for a much longer time. (C 13)

There is clearly nothing of an ideal or representational character about "projectiles retarded by air," "tops retarded by air," and "planets and comets." All of these bodies are precisely the kinds of things we are familiar with before any investigations in the science of mechanics (though *recognizing* the celestial bodies as bodies takes some science).

Futhermore, there is never any analysis of *body* in the *Principia* to suggest that in the theory the concept has some complexity not present in the common concept. When, in the Scholium to the Definitions (C 6–12), we are given clarifying definitions of some fundamental terms that are "well known to all" such as 'time', 'space', and 'motion', the term 'body' is conspicuously absent. We do learn that there are "particles of bodies" (C xviii) that compose them, but since at least some particles are themselves divisible and not essentially atomic, and since some bodies are so small as to be insensible (C 399), it seems that the things Newton calls particles are themselves bodies. So bodies are made up of smaller bodies, quite in keeping with the common, pretheoretical concept.

Finally, most other apparently fundamental and apparently theoretical terms are straightforwardly definable in terms of *body*. *Motion*, both relative and absolute, is simply "the translation of a body from one absolute/relative place to another" (C 7). *Place* is "part of a space which a body takes up" (C 6). All that remain are *space*, *time*, and *forces*. These, like body, are unanalyzable, though Newton's space and time are probably not pretheoretical concepts, and forces certainly are not. But unlike space and time, forces, both inertial and impressed, cannot be understood apart from bodies. "The innate force of matter [subsequently called *vis inertiae* or inertia] is a power of resisting by which every body . . . continues in its present state. . . . " and "An impressed force is an action exerted upon a body. . . . " (C 2). So inertial forces are essentially connected to bodies, and impressed forces are necessarily forces on bodies. Moreover, any force impressed on a body somehow originates with some other body or bodies; consider gravitational force, magnetic force, tensions, etc. Similarly, a body's inertial force will not be exerted unless there is a force impressed on it. Therefore, a perfectly isolated body would neither exert nor feel any forces, so forces do not exist except in the presence of more than one body. Moreover, once we have bodies and the forces that act on them, we have everything. Newton says the whole burden of philosophy is, " . . . from the phenomena of motions to investigate

the forces of nature, and then from these forces to demonstrate the other phenomena . . ." (C xvii–xviii).

Thus the only things that are, in a sense, ontologically independent in Newtonian mechanics are space, time, and body. But Newton did not think that his theory was in any way *about* space and time (though today we may be inclined to disagree with him about this). It is about bodies *in* space and time. It did not seem strange to Newton to accord space and time this peculiar status; for him they are charged with theological significance (C 545). I conclude that the whole of Newtonian mechanics, as it is expressed in the *Principia*, rests on the concept of *body*; bodies are the *kinds* of things that the theory is about. And these bodies are understood prescientifically – the extension of the vernacular term 'body', one is tempted to say.[28]

The theory of Newton's *Principia* has to its credit many empirical successes, many of these described in the same pages by Newton himself. Moreover, the folk-theoretical kind *body* plays a ubiquitous role in the development of the science. Thus, it cannot be concluded that it is generally true that successful science cannot be based on kinds that are really accessible to us prior to theoretical articulation. Let us return to the example of economics. It now seems most unlikely that its difficulties in satisfactorily connecting up with observed phenomena can be blamed on the centrality of the kind *commodity*. The example from physics shows that there is nothing wrong in principle with having a science *of* some things or phenomena that are individuated prescientifically. We still seem to be left with a puzzle about why the impressive theoretical framework of economics is so difficult to apply to the world successfully.

It might be objected at this point that my treatment of *body* in the *Principia* was too hasty and that the conclusions drawn are too strong. Perhaps Newton does *begin* with a folk-theoretic kind, but in the course of developing his theory, it might be that the concept *body* also develops. Perhaps, by the end, the characteristic scientific enterprises of generalizing and systematizing leave behind a concept that is at some remove from the common notion of body. Perhaps when we look at the entire *Principia* in context, and not just at isolated passages as I have done above, we can discover ways in which *body* evolves from a folk-scientific concept into a concept that is almost as plainly theoretical as *quark* is.

If so, we should consider the possibility that the source of the empirical differences between Newtonian mechanics and economics can be located here. Newtonian mechanics may owe its predictive and explanatory successes with bodies ordinarily conceived to its employment of a more highly developed, idealized version of the folk concept to model or represent the actual objects. I shall call such an idealized concept a *refinement* of a folk-theoretic or pretheoretic concept.[29] the refined theoretical concepts often turn out to be surprising or counterintuitive to those not sufficiently familiar with the theories and the problems they were meant to solve. The objects and properties that these new concepts in-

volve are also usually further removed from straightforward observability than those involved by folk concepts. For instance, *impetus* and *heaviness*, almost sensually determined kinds, are refined into *momentum* and *mass*. On the face of it, scientific progress is characterized by improvements in prediction, explanation, and intelligibility that are all made possible by the refinement first of folk-scientific concepts, and then by the successive refinement of more sophisticated concepts.

If this is right, then the symmetry between *body* and *commodity* would be broken if Newtonian mechanics provided a refinement of *body*, but economics failed to refine the folk theoretic *commodity*. I can briefly indicate to what extent *body* gets refined in the *Principia*; a complete treatment is difficult because Newton himself seems to have been confused about this aspect of the foundation of his theory. What is important for this essay, however, is that it is quite clear that, contrary to what I have been heretofore supposing, *commodity* also undergoes some surprising refinement in economic theory. The relevant "structural" symmetry between physics and economics that I have been exploiting is not, therefore, broken by considerations of refinement.

The most significant refinement of *body* first explicitly occurs in Corollary IV to the famous third law of motion, with the introduction of the concept of the *center of gravity* common to two or more bodies (C 19). When applying the theory to determine actual trajectories, tensions, and so on, almost all calculations are performed using the device of centers of gravity (today we quite properly prefer to speak of centers of mass) and not with the surfaces of the bodies themselves. When calculating we often treat systems of bodies as single bodies, all of whose mass is concentrated in a single point of space, the center of gravity of the system. The "body" so obtained is plainly not a real body at all in the ordinary sense. Even the center of gravity of a single body (what modern texts call a point mass) is not an ordinary body—it occupies only one dimensionless point of space.[30] Without at least this refinement of *body*, it is hard to see how Newtonian mechanics could yield any empirically useful results at all.

If economics were unable to offer an analogous refinement of its central-kind term, we might naturally suspect that this was the root of the difficulties with obtaining good quantitative results. But there does seem to be a strong analogy between economics and Newtonian mechanics with respect to refinements. Just as powerful mathematical techniques cannot be applied directly to unrefined features of bodies, the technical apparatus of economics (the most powerful being associated with GET) cannot cope with the complexity of the unrefined concept of commodity. One complexity that can be made to appear obvious has to do with the overwhelming variety of commodities in any modern economy. Part of our economic behavior as consumers involves making choices among avocados, bananas, cucumbers, dates, eggplants, figs, etc., and that is only for produce. Then there are economic decisions among airline tickets, boat cruise tickets, ca-

noe rentals, dog sled rentals, elevated train tokens, etc., for transportation. These lists can be extended almost indefinitely, and there are almost indefinitely many more lists to be constructed. Even if a consumer did have a well-defined utility function for this vast array of commodities, there would be no practical hope of discovering what it was. If we experimented on a particular person for his entire lifetime, we would not have enough data to infer even approximately how his choices among bundles containing indefinitely many commodities were structured. But there are methods for coping with this situation.

In the first place, economists almost never attempt to predict or explain the behavior of particular consumers. Instead, the unit of analysis is a "representative" or "average" consumer, whose properties are constructed from data about entire markets that is often plentiful and reliable. Since such a representative individual (call him R) is a fiction, we must think of what he consumes as also something less than fully tangible. This is already beginning the process of refinement. A second sort of refinement takes place when we begin to study the hypothetical behavior of R. If R's utility function is to be subject to standard mathematical maximization techniques, its potentially indefinite list of variables (commodities) need to be *aggregated* into a manageably small list. We might, for example, take R's utility to be a function of three variables: food, shelter, and entertainment. Each of the three categories is understood to comprise the appropriate specific commodities that R is, or might be, confronted with, like avocados, apartment rental, and computers.

Commodities can be aggregated in various ways into variously constructed aggregates. The skillful economist will choose aggregates that make problems of interest mathematically tractable. For example, if we are interested in market behavior concerning facsimiles of the manuscript of Brahms's Fourth Symphony, we may wish to suppose that R's utility is a function of two variables: 1) copies of the facsimiles, and 2) everything else that R might choose. If we are interested in R's choices of investments, we may aggregate all commodities into these two: 1) things consumed in the first half of R's life, and 2) things consumed in the second half of R's life. There is some well-developed theory regulating the conditions under which it is permissible to aggregate commodities. There is, for example, Hick's composite commodity theorem that states, " . . . if a group of prices move in parallel, then the corresponding group of commodities can be treated as a single good [commodity]" (Deaton and Muellbauer 1980, 121). In practice, of course, we can only hope that our aggregates do not violate the restrictions imposed by Hick's theorem, because we do not have dependable data on the pairwise price movements for the whole indefinitely long list of actual commodities. But I am presently interested in theoretical refinement.

The examples I have presented from physics and economics are much more complex and subtle than I can indicate here, but I think that these very brief sketches suffice to bring out a significant point. In Newton's theory, the ordinary

concept of body was refined into something unrecognizable to the untrained intellect—the center of gravity. In GET, the ordinary concept of commodity is similarly refined, for the "commodities" arising from aggregation are surprisingly unfamiliar objects. What manner of *thing* is *everything consumed in the first half of a lifetime*, or *everything except facsimiles of the manuscript of Brahms's Fourth Symphony*? And it is notoriously difficult to think of a person's generalized capacity to labor as a commodity, something to be bought and sold as though it were in principle completely alienable from the laborer. No one would normally have occasion to think of such things as these except in the context of a sophisticated scientific theory.[31] I mentioned a related refinement earlier, when characterizing GET. In GET, a *consumer* is a thing with a utility function that exchanges, annihilates (i.e., consumes), and helps create (i.e., by supplying labor) *commodities*. We can think of GET's *consumer* as an extreme refinement of the pretheoretic concept of a person or the folk-economic concept of a consumer. It is indeed extreme; GET's consumers are paradigms of what we would ordinarily apply the vernacular term 'inhuman' to! Similarly, centers of gravity that are at single points of space are good candidates for the vernacular terms 'incorporeal' and 'immaterial'.

VII

In the absence of further arguments, it is reasonable to conclude that Newtonian mechanics and GET both begin with folk-theoretic kinds and refine these in strikingly analogous ways. The difference in empirical applicability cannot be accounted for by differences in the type of refinements the important kinds undergo. In this section I want to examine the radical idea that the kind terms of economics do not pick out natural kinds in the world. This idea is radical because it suggests that economics is essentially different from natural sciences. Successful natural sciences produce reliable laws and formulas that can be thought of as expressing relationships among natural kinds or relationships among the properties of a single natural kind. If there are no economic kinds that are natural, economics cannot do this. This way of formulating the problem is not new. A. Rosenberg (1983) has written,

> Philosophers have shown that the terms in which ordinary thought and the behavioral sciences describe the causes and effects of human action do not describe "natural kinds," they do not divide nature at the joints. (301–2)
>
> The predictive weakness of theories couched in intentional vocabulary reflects the fact that the terms of this vocabulary do not correlate in a manageable way the vocabulary of other successful scientific theories; they don't divide nature at the joints. . . . [Some] insist that we must jettison "folk psychology" and its intentional idiom if we are to hit upon an improvable theory in the science of psychology. This choice extends, of course, beyond psychol-

> ogy to all the other intentions sciences, of which economics, with its reliance on expectation and preference, is certainly one. (303)

In this essay, I want to remain neutral as to what philosophers have or have not shown about the existence of intentional natural kinds and offer another sort of diagnosis of the failure of *commodity* to pick out a natural kind. Let us return to the fact that there is a pretheoretical concept of a body or object and to the assumption (I shall question this assumption in what follows) that there is a pretheoretical concept of a commodity.

Why is there the pretheoretical concept of a body? It is tempting to answer by saying it is because there *are* bodies, and human beings have evolved to be extremely good detectors of bodies that are roughly their own size. It seems safe to conjecture that any mature normal human being will have a concept of body. We need to be good at recognizing things that move, can be grasped and thrown, can be eaten and mated with, and can forcibly collide with us. One could also approach the question from the other direction and say with Kant that it is the fact of appearances of bodies as spatiotemporal that yields the transcendental ideality of space and time as founding the possibility of these appearances. Either way, the result is that humans inescapably perceive the world as containing bodies, and that fact itself is some kind of evidence for the veracity of these perceptions. Newtonian mechanics, therefore, has helped itself to a pretheoretical concept that is very well suited, perhaps uniquely well suited, to scientific refinement. Now let us ask the same question for economics.

Why is there the pretheoretical concept of a commodity? In one sense, there simply isn't one, because it is very unlikely that this concept is universally possessed in the same way that the concept of *body* is. We know that there have been some societies in which the practice of private ownership is extremely attenuated for most items that we would consider commodities. In such a society, these items would not be conceived as objects of production or even of exchange. It is even unlikely that anything except food would be thought of as being *consumed*, since the practice of leasing whereby time-slices of durable goods are consumed requires a specialized system of strong property rights. Anthropologists could probably identify the concept of a 'potentially useful portable object', but that is simply a *body*—commodities are not kinds of things for such a society and, a fortiori, not natural kinds. It is, I think, questionable whether the concept is *pre*theoretical even in advanced capitalistic economies. By the time one has reached the age at which things like paper money, salaries, rents, banking and investment, and so on are somewhat comprehensible, one is surely already in possession of a fairly substantial economic folk theory. One understands that higher prices mean fewer sales, inflation is good for debtors and bad for those with fixed incomes, depressed stock prices are bad for pension funds, and so forth. It is not important for present purposes to draw a sharp distinction between what is pretheoretical

and what is folk theoretical; I merely want to stress the point that whether people can recognize commodities as kinds of things, or even individuate them at all, is determined by the society and culture that they live in. This does distinguish economics from Newtonian mechanics. Economics did not begin, as physics did, by helping itself to a ready-made pretheoretical central concept that lent itself to successful refinement.

Now it does not *follow* from the fact that *commodity* is not a universally recognized pretheoretic kind that it is not a natural kind after all. In general, it is entirely possible that people not have the right kind of mental equipment to recognize real kinds. In like manner, it does not follow from the simple fact that there is a pretheoretic concept of body, that *body* is a natural kind. But it is. We can see this from the fact that lawlike relationships obtain among bodies. Furthermore, the universality of the experience of bodies seems closely related to their constituting a natural kind. This property of universality might be a key to understanding the status of *commodity*.

Suppose that the reason commodities are not universally recognized is that there just aren't any of them in some nonpathological human societies. Suppose, that is, that insofar as commodities do exist, they are brought into existence by virtue of some distinctive properties of some societies. (I shall consider some points that may count against these suppositions below.) Now consider that an intuitively extremely plausible condition on something's being a scientific law is that it be universal, that it apply to all the things in its domain.[32] Newtonian mechanics applies to all bodies, electrochemistry applies to all solutions, and so on. Does GET-based economics have this kind of universality? This question cannot be answered until it is determined what the domain of the theory is supposed to be. If we think of economics as a *social* science, a science dealing with the economic aspects of life in society, then economics is not universal, for we are supposing that there are societies to which it does not apply in virtue of their not having any commodities. This is close to the conclusion that economics is not a science in the same sense that natural sciences are.

If economics is taken to have a more modest domain, the economies of modern capitalistic societies, for example, then the level of empirical success that the theory enjoys becomes important to the investigation of its foundations. If the theory were an extremely good predictor and explainer for the limited domain, then we would be inclined to say that modern capitalistic societies are essentially different from any others to which the theory does not apply, and that this difference is part of what makes them appropriate objects of scientific economic inquiry. If, however, we are prepared to admit that the theory cannot be applied with great success even to a specially chosen domain, then I think we should be less inclined to see a deep divide between modern capitalistic societies and other human societies. If there is no such divide, and if all the other suppositions I have been enter-

taining are well founded, then economics is not universal and does not express relations among natural kinds and their properties. This is stronger than claiming that economics sometimes gets it more or less right (when applied to modern capitalistic societies) and sometimes gets it wrong (when applied to other societies). It is claiming that economics *never* gets it really right because *commodity* is not a natural kind. If commodities are not natural kinds in *any* society, there cannot be an empirical science about them. If I am right, we should not think of economics as a false theory about things that are in the world; its lack of success is, instead, inevitable because the things that it is supposed to be dealing with are not there.

This powerful conclusion is arrived at with the help of a few assumptions. I shall conclude by considering two kinds of interesting objections to the assumptions I have made. First, however, I want to point out an objection that won't be discussed. It is often asserted (more commonly in conversation than in print) that economics is terrifically good at prediction and explanation after all. I think that most scholars, even most economists, believe that this is wrong. Applications passed off as great successes usually turn out to be either post facto reconstructions of aggregate data from properties of imaginary representative individuals (see Nelson, 1989), or else things readily available to folk economists such as, "people buy smaller automobiles when gasoline prices are higher." It is not clear how this disagreement can be resolved.

A more interesting objection comes from combining the assertions that economics does tolerably well for modern capitalistic societies, that in principle it can do extremely well for these, *and* that it can, in principle, do extremely well for other kinds of societies as well. Some economists, following the prominent example of G. Becker, think that virtually all individual, and hence all societal, human behavior can be understood by means of GET-based economics. They presumably think that this is the case even when the members of the society do not conceive their actions within this framework. Empirical work on precapitalistic societies has been initiated, and some of it is very interesting even though it is far from convincing. (C. Dahlman's 1981 treatment of the feudal English open field system is a good example). As with economic studies of contemporary societies, we cannot determine a priori that surprisingly good empirical results are not forthcoming at some future date. We seem to be in store for a long wait.

Another related, but stronger objection could be mounted if we could discover some naturalistic foundation for *commodity*. Such a foundation might be provided by psychology in the way indicated above on pp. 108–9. (See Nelson 1986 for some details.) It might be true of the cognitive faculties of human beings that they include something representable by a utility function and that economic behavior (perhaps narrowly, perhaps broadly construed) is partially caused by a process representable as the maximization of this function. Another way to demonstrate

that commodities are natural kinds, despite the argument of this essay, would be to bypass psychology and go straight to biology. It is undeniable that human beings have in common certain biological needs, like nutrition and shelter, and that there are various choices among strategies to be implemented in attempting to meet these needs. In response to the selectional pressures these needs place upon the species, humans might have evolved specialized neurological structures that compute what the efficient courses of need-satisfying action are. These neurological structures might turn out to have accurate descriptions in the language of economics; they might realize utility functions, for example. In this way, economic theory might turn out to be in our genes. One striking thing about both of these proposals is that the *utility function* or the *preferences* replace *commodity* as the fundamental concept in the theory. If there is a sense in which human minds or brains actually unconsciously compute utility maxima, then this phenomenon can be directly studied with the help of psychology or neurology. What commodities there actually are becomes a practical, not a theoretical issue. Of course, there may be a further "transcendental" argument from our having utility functions to there being *commodities*, like the argument from the appearances we have to the existence of bodies. This would give *commodities* a sort of ontological priority to *utility*, but the investigation of this connection between *commodity* and *utility* would not be part of economics, it would be part of biology.

It must be conceded that these possibilities that *commodity* would turn out to be a natural kind are remote. And few economists show any interest in pursuing them. Still, careful scientific investigation might reveal one of these possibilities to be the truth. It is, however, sometimes open to philosophers to argue about what is and is not possible given what we presently know.

In view of the extreme claims made on all sides about economics, I think that the position I have argued for in this essay can be considered moderate. I have tried to show that there is nothing incoherent or wrongheaded about thinking that the central-kind terms of economics pick out natural kinds. But if they are, then we are committed to an interpretation of the concepts connected with the kind terms as having explanatory force. We are committed to what I called the Utility-as-Explanatory interpretation. When conjoined with the fact that economics has a poor empirical record, however, this interpretation has the consequence that *commodity*, and along with it the other key concepts in the theory, is not a natural kind. Since physics, chemistry, biology, and closely related disciplines contain lawlike relationships among natural kinds and their properties, this fact about economics distinguishes it from these "natural" sciences. After studying other social sciences and other approaches to economics in the way that GET-based economics has been studied in this essay, we might arrive at the same conclusion about them. If so, we would be closer to understanding the peculiar scientific status of the social sciences.

Notes

1. My own, apparently unpopular, view is that insofar as vernacular "natural"-kind terms have a class of referents tidy enough to merit the title "extension" (quite unlikely, I think), this extension is very different from the extension of the corresponding scientific term. For example, I think that the semantic properties of the vernacular term 'water' differ considerably from the chemical term 'water'. These positions are strongly argued for in Donnellan (1983, 85ff.):

> . . . the terms obviously are not borrowed from the vocabulary of science and were part of English long before the advent of modern science. . . . [a]lthough one might suppose that if terms for natural kinds are to be found anywhere the language of science would be replete with them, it is not obvious that the Kripke-Putnam theory is applicable to kind terms in science. Nor is it obvious that it will apply to terms which the vernacular has borrowed from the language of science. . . .

I shall just assume in this essay that there are natural kinds of things in the world, but I really only need the notion as a tool for analyzing particular scientific theories. Little depends on the metaphysical stance adopted toward the kinds. In particular, the argument does not require that natural kinds are ontologically prior to scientific laws.

2. I shall not argue here that economics is not as empirically successful as physics is. The claim seems to require enumeration of cases more than argumentation anyway. For the view of an important economist see Leontief (1971). A philosophical treatment of the relative success of economics and the physical sciences is in Rosenberg (1989).

3. Anything that cannot be put into the GET framework is usually regarded as suspect economics. (Economists disagree among themselves about this to some extent.) The situation is similar to that in classical mechanics in the nineteenth century. Any physical theory that was not somehow a part of, or reducible to, classical mechanics did not clearly count as physics. Even a "nonmechanical" domain like electromagnetism is subsumed by virtue of the forces it describes figuring as components in $F=ma$. A good example of the generality of GET is it applicability to parts of Marxian economics in Roemer (1981). For a discussion of the problem of fitting macroeconomics into GET see Weintraub (1979).

4. My exposition of this material is unusual, but its economic content is based on Arrow and Hahn (1971), the book that H. Varian calls one of the two "definitive modern treatments" and "the most up to date treatment" of systematic GET (1984, 210, 211).

5. The intuitive idea is that different bundles of commodities are ordered according to preferences the consumer has for one bundle over another: Arrow and Hahn write of "levels of satisfaction" (1971, 80), and Varian uses the phrase "the consumer thinks that the bundle $\mathbf{x}$ is at least as good as the bundle $\mathbf{y}$" (1984, 80). But economists often wish to dispense with such psychological terminology as I explain in what follows.

6. It is often convenient (and will be in this essay) to think of the order given by the consumer's preferences as inducing an *indifference map*. An indifference map is a set of *indifference curves* in R^n (where n is the number of commodities in the economy), each of which contains points in the space of commodity bundles that "occupy the same place in the preference order"—the consumer is "indifferent" to any two points on the same indifference curve.

7. Any set of consistent preferences, preferences that are transitive, can be represented by some utility function—by brute force if necessary. Not every consistent preference map will be representable by a *well-behaved* function. Lexicographic preferences, for example, cannot be represented by a continuous utility function.

8. This is the place where most of the ideological, noneconomic content of the theory is introduced. The concept of an endowment entails a far-reaching theory of property rights. I shall have more to say about this later.

9. See Stigler (1965, 84–144) for a summary of the relevant history.

10. See Coats (1976, 50–59), Schumpeter (1954, 1056–61), and Stigler (1965, 144–48, 151–55) for a summary of the relevant history.

11. As is usual in the history of science, the first fully satisfactory treatment was prefigured in various ways; Allen and Hicks did not invent ordinal utility theory, but they found a statement of it that made a clean break with the cardinal theory possible. For some historical details, see Schumpeter (1954, 1062–66) and Samuelson (1974, especially the appendix).

12. SARP requires, roughly, that the consumer's choices not violate a transitivity condition. For details, see Samuelson (1938b and 1950).

13. It is only a suggestion. It would not be inconsistent to adopt the formalism of revealed preference for its formal virtues and still interpret the theory as being about cardinal utilities.

14. The import of this historical episode has various interpretations. Rosenberg (1981) argues that psychological facts disconfirmed the economic theory and that subsequent developments were unjustifiable, ad hoc attempts to save the theory. Cooter and Rappoport (1984) think that economists' interests shifted away from the welfare economics that required interpersonal comparisons towards more theoretical concerns.

15. Samuelson here refers to marginal rates of substitution.

16. In particular, this interpretation tells us to ignore the (disastrous) possibility that preferences change over time. A consumer who behaves as if his preferences change is considered irrational and, therefore, not subject to the theory.

17. Given the additional, not terribly ad hoc, assumption that Pluto was once a satellite of Neptune's. For a brief account of the history of Bode's law see Holton and Roller (1958, 198–201). For Balmer's formula and the Bohr model, see Holton and Roller (607–33).

18. Bode's law does not fail to be a real law because of a conceptual flaw; it seems that we can imagine a universe constructed along Keplerian lines in which it falls out of other cosmological facts. Its success is simply accidental. Accidental successes of this magnitude demand searches for underlying mechanisms; in this case the search was soon shown to be futile.

19. For more details and a defense of this kind of procedure against methodologically inspired objections, Nelson (1986) must be consulted.

20. Both alternatives involve surprising complications. For a discussion of the former, see Nelson (1986); for the latter, Nelson (1984, and 1989).

21. An analogous situation arises in the theory of the firm. Production functions contain factors of production as independent variables. Should money be included as a factor of production? One might think not because the physical money, the bank notes and coins, do not help produce output. One might shovel notes into the furnace along with a genuine factor of production like coal, but this would be similar to a consumer's using the notes as wallpaper to get utility. If, however, one thinks that production functions, like utility functions, are descriptive and not explanatory, then it seems that economists should use dependent variables resulting in the functions that best fit observed phenomena—and these probably will include money. See Sinai and Stokes (1981) and the references listed there for a sample of the literature.

22. See, for example, Van Fraassen (1980).

23. Later, I shall question whether there really is a good pretheoretical notion of a commodity.

24. For discussion, see Stich (1983).

25. Experimental studies show that modern, well-educated people have generally poor physical intuitions about basic mechanical processes (McCloskey 1983). We do need a theory to tell us what are natural kinds of motions.

26. Again, see McCloskey (1983) for a review of the old physics and for some psychological experiments supporting the claim that this is indeed the folk physics of mechanics.

27. *Corpore* is used in two ways by Newton. It usually means some particular body, but it can also mean matter or what makes up bodies. It appears in both senses in Definition I, one line apart!

"It is this quantity that I mean hereafter everywhere under the name of body [*corporis*] or mass. And the same is known by the weight of each body [*corporis*] . . . " (Cajori 1934 [1726], 1 [I shall refer to this book as "C"]; Koyre and Cohen 1972 [1726], 1:40). The first sense predominates and I won't discuss the second here. M. Jammer seems to suggest that Galileo struggled with the distinction (1961, 52).

28. The primary vernacular sense is, of course, as in the bodies of animate creatures (*corpore* in Latin). The *Oxford English Dictionary*, however, suggests that this sense is very close to the sense of "material object." Samuel Clarke, writing to Leibniz more or less on behalf of Newton, suggests that we understand bodies and their interactions on the model of the interaction between our own bodies and our minds (or God). (Alexander, ed., 1956 [1717], 116–17).

29. It is not necessary here to give an analysis of what I am calling refinement. It is, perhaps, related to Toulmin's account of the evolution of concepts (1972, 200–36).

30. So the folk concept *body* is implicitly refined in the very first corollary, "A body, acted on by two forces simultaneously, will describe the diagonal of a parallelogram . . . " (C 14) because a parallelogram and its diagonal are mathematical and not physical figures. Only a *center* of gravity could describe a line; an actual body would describe a solid wormlike figure. I think that this lack of expository precision on Newton's part indicates that he was not clear in his own mind about the distinction between the folk notion *body* and its refinement. It is interesting to note that the same refinement is employed when Newton discusses the *arcs* that planets describe.

31. There are, of course examples of more familiar aggregates, such as *food*, but if a utility function has many arguments at this familiar level of aggregation, calculation again becomes impossible.

32. The intuitive idea seems reasonably clear, although it is notoriously difficult to state clearly. "Applying to everything in its domain" is different from being "exceptionless." A law has an exception if it applies to something, but gets at least one thing wrong about *it*. *Really wanting ice cream* might be lawfully related to *trying to get ice cream*, although there are many exceptions—some external phenomenon may intervene before the wanter manages to try. But we might agree that *everyone* who is a wanter will be a tryer *unless* there is intervention. So the relation might be universal without being exceptionless.

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