Critical Realism vs Economic Rhetoric

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Discussing 'realism' has been a growth industry lately. Where once we might have heard philosophers argue over realism vs idealism or vs instrumentalism, today the discussion concerns various types of realism. According to methodologists such as Uskali Mäki and Tony Lawson, there many types: critical, commonsense, empirical, ontological, scientific, scholastic, social, structural, transcendental, transfactual, etc. I am not sure I understand the need for all these distinctions. Given my Popperian background, I guess I have always taken critical realism for granted. I would think any concern for critical realism is either an obvious necessity or it is mere rhetoric. For me, the question of critical realism only concerns the methodology of model building in economics. Basically, the main question is: do the model's assumptions represent reality, that is, represent the real, objective world?

In this paper I am going to apply critical realism to two aspects of the methodology of model building. In Section 1, I will criticize some of the common excuses given for accepting less than desirable realism in economic models. In Section 2, I will discuss various obstacles to obtaining realistic economic models.

1. Excuses for accepting unrealistic models

It is not always clear what economic model builders think they are doing. Today more than ever it would seem that they are less interested in whether their models represent observable reality and more interested in whether their models are novel applications of the latest fad in model building methodology. Over the last thirty years whenever I pressed my colleagues about the unrealism of some of their assumptions, I was usually told in effect that 'absolute' truth is unattainable and thus that my criticisms were not appreciated. Early on I learned that it is always counter-productive to ask whether they thought their opinion of the unattainableness of 'absolute' truth is itself absolutely true. So, now I just ask why they think one should not expect one's models to be absolutely true.

Excuses for stopping short of the pursuit of absolute realism are based on the conventionalist philosophy of science. Conventionalism declares that theories are neither true nor false but only better or worse. According to conventionalism, theories are mere filing systems or catalogues to be used to describe observed data. That is, theories are not intended to be representations of the world that generates the data. Theories are to be judged only on whether they are efficient at cataloging the data. Some theories are better than others at this task.

The only philosophical problem that needs ever be to considered is the problem of 'theory choice'. That is, how can we go about choosing the best theory? The presumption of conventionalism is that for a theory to be absolutely true it must be possible to prove its truth status. Moreover, the only acceptable proof must be based solely on indisputable facts, that is, all proofs need to be inductive. But as is widely accepted today, there are no indisputable facts, only theory-laden facts. Thus, at best, one's proof would be circular.

Conventionalism is the nineteenth-century philosophical response to the recognition that inductive proofs are impossible. Hence, according to conventionalism, we

can never claim that our theory is true. Nevertheless, theories abound both in science and everyday life. To meet the needs of science and everyday life, conventionalism recommends that the best we can do to meet the needs of a proof is to accept analytical proofs. Thus, theories are not to be proven inductively, based only of objective facts, but proven analytically by showing that the theories are logically consistent with the current conventions concerning truth status. In economics, such conventions flourish in the field of applied econometrics.

Conventionalism is then a denial of realism. In the place of realism, conventionalism in found in two forms. An optimistic form often called approximationism and a pessimistic form usually known as relativism. Let me discuss these in turn with respect to how they provide excuses for accepting unrealistic representative economic models.

A. Approximationism vs realism

Approximationism is the most common form of conventionalism. It seems to appeal to commonsense. It simply says that while we might like our assumptions to be true, that is, realistic, as a practical matter true representative models would be too complex and thus intractable. Friedman's instrumentalism agrees with this starting point but departs by immediately accepting simple and obviously false assumptions so as to push on with dealing with practical problems on the grounds that for practical problems theories do not need to be true. The obvious supporting example for instrumentalism is engineering which never claims to deal with true assumptions yet addresses practical problems. It does so in a simple manner, it employs safety factors and recognizes degrees of measurement tolerance. Conventionalism, however, is interested in more than immediate practical problems. For this reason, the criteria to be used in the problem of theory choice are central.

According to conventionalism, science is distinguished from engineering in that science transcends immediate practical problems by being concerned with general understanding. In the hands of mathematical economists this has led to the view that the more general the model, the better the model. General equilibrium theory is the obvious example. Rather than explain one product's price, we should develop models that apply to the explanation of all prices. In the extreme, generalist models have minimal indications that they are about economics.

Everyday economics does not go to such extremes. Instead, we know that we must make assumptions, but there is no need to be any more general than the problem at hand requires. For example, while two-dimensional diagrams might not be an adequate basis for a general proof, they can approximate the essential notions of economics. Specifically, two-by-two-by-two Edgeworth-Bowley boxes can be used to describe all of the necessary conditions for general equilibrium. In the equilibrium between any two factors producing two different goods, the marginal rate of technical substitution (i.e., the slopes of the isoquants shown within the Edgeworth-Bowley box which has the available labor and capital as its dimensions) must equal the ratio of the respective factor prices. Similarly, between any two people consuming those two goods, in equilibrium their marginal rates of substitution (i.e., the slopes of the indifference curves shown within the Edgeworth-Bowley box which has the produced quantities of the two goods as its dimensions) must equal the ratio of the prices of those two goods. Although most textbooks fail to mention it, the

necessary condition for income distribution can also be shown in this latter equilibrium Edgeworth-Bowley box.

Apart from the assertion that two-dimensional diagrams adequately approximate the conditions of general equilibrium, there are numerous other assumptions that are claimed to approximate the reality that textbook models claim to represent. Before I examine some of them that might be no more problematic than two-dimensional diagrams, I wish to add some brief comments on the pessimistic form of conventionalism that has been gaining ground in recent years.

B. Relativism vs realism

The most recent challenge to realism comes from those who are advocating a more pessimistic view of the problem of theory choice. According to this pessimistic conventionalism, since any choice could never be proven absolutely, one should not try to chose just one theory, one model or even one paradigm. At best, it is claimed that any choice will always be relative to the basic criteria approved in one's cultural environment. The code words for this view in economic methodology is 'pluralism' and 'rhetoric'. Pluralism is the mild form of pessimistic conventionalism and rhetoric is the extreme form.

Pluralism is the feel-good form of pessimistic conventionalism. It says that since we cannot ever (inductively) prove that our chosen theory is the true theory, one must be tolerant of others who have chosen other theories. Thus, this mild form is not an outright ban of theory choice, but only a recognition that we should limit our claims for our choice to those which would appeal to someone who accepts our conventionalist criteria of choice. Moreover, it is all too easy to see that one's acceptance of particular criteria cannot be justified beyond their consistency relationship to the chosen theory. While philosophers worry a lot about the circularity of this form of pessimistic conventionalism, economic methodologists just take it for granted. And since they take it for granted, they are easy targets for proponents of the extreme form of pessimistic conventionalism.

Rhetoric technically is concerned with how one goes about convincing an audience that one's proposition is true or otherwise correct. The eighteenth-century philosopher's advocating logic as the only acceptable means of convincing an audience is merely one example of such advocacy but not one that is discussed by the rhetoric of economics advocates today. Today, even eighteenth-century rationalism is the subject of criticism and ridicule by pessimistic conventionalists. By whose standards of rationality are we able to convince an audience? Well, obviously, only by the standards accepted by the audience. This will obviously be different for different audiences. Standards of rationality are culturally dependent. An argument that might convince someone educated in a Marxist environment is not likely to be convincing in a meeting of neoclassical economists and vice versa. Understanding and exploring the relativity of rhetoric is the celebrated cause promoted by the advocates of the rhetoric of economics such as Dierdre McCloskey and Arjo Klamer. For such relativists, there is no absolute reality, reality is only in the eyes of the beholder. Thus, given their understanding of optimistic conventionalism, advocates of the rhetoric of economics have much fun ridiculing those methodologists who would ever claim to have conventionalist criteria that might be considered universal.

At best, the published disputes between pluralists and advocates of the rhetoric of economics are mere family disputes. Both reject realism. Both advocate some form of

relativism. Both violate their own principles by asserting that theirs is the true methodology – even though, of course, rhetoric is claimed to be an alternative to methodology (this is merely an expression of the family dispute). Both say that it is illegitimate to argue about the truth status of the basic or fundamental assumptions of neoclassical economics. As the philosopher Joseph Agassi [1992] has pointed out, arguing over basic, fundamental principles is universally forbidden by relativists, particularly, feel-good relativists.

2. Obstacles to representative realism

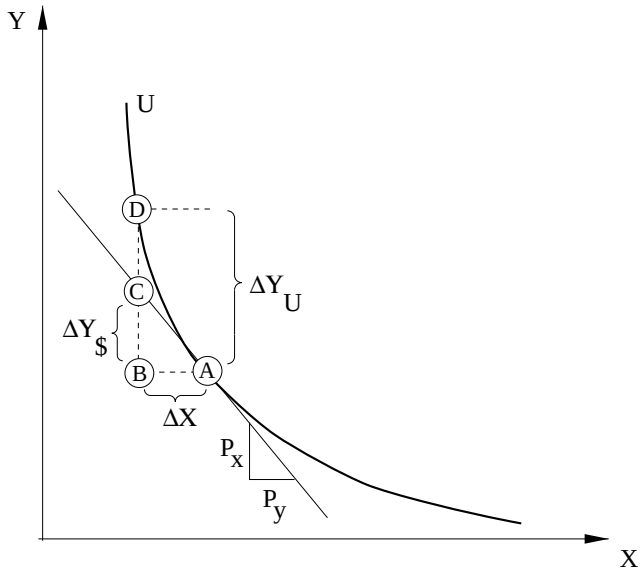
Realism in mainstream economics is concerned with the realism of assumptions used in building economic models. Those advocating rhetoric of economics will usually ridicule the mainstream interest in model building so there is no need to discuss them further. In the remainder of this paper I will discuss five obstacles to building realistic representative models in economics. These five obstacles lie at the foundation of neoclassical economics and thus are not likely to have been the subject of critical consideration by model builders who rely on some form of conventionalism to provide their guiding methodological principles. According to conventionalist principles, we are told that we should not argue over the truth of one's theory or model since ultimately one would have to prove that one's basic principles are true. And since such proofs could only ultimately succeed inductively, that is when they are shown to be based on indisputable facts, as noted above, this is precluded by the recognition of theory-laden facts.

The five obstacles I will discuss are (1) the use of the idea of infinity or equivalently of the infinitesimal, (2) the usual presumption of an inductive basis for knowledge assumptions in economic models, (3) the inconsistency between any explanation of the process of reaching an equilibrium and the conditions necessary for equilibrium, (4) the inconsistency between model assumptions and the ideas being modelled, and (5) confusing unrealistic mathematical assumptions with objects in the real world.

A. Infinity and infinitesimals

Before criticizing the notions of infinity and the infinitesimal, let us ask a simple question. Where would neoclassical economics be if one were denied the use of these notions? Obviously, all of calculus would be ruled out. Once the use of calculus is denied, the conditions for maximization cannot be proven. To say whenever utility is maximized in the choice of quantities of two goods, the necessary equality between the marginal rate of substitution and the ratio of their prices may not refer to an actual event. To prove or even achieve the equality between the marginal rate of substitution and the ratio of the prices is usually accomplished by invoking the idea of an infinitesimal adjustment in one's choice between the two goods. Namely, the marginal rate of substitution (MRS) between goods X and Y is defined as the extra amount of Y, ΔY, that it would take to exactly compensate for a specified reduction in the consumption of X by ΔX. That is, $MRS \equiv \Delta Y / \Delta X$. If utility is maximized, then $P_x / P_y = \Delta Y / \Delta X$. The problem here is that while it is easy to calculate the price ratio, calculating the MRS is not so easy. Consider Figure 1.

Figure 1. *Utility maximization*



In Figure 1 the consumer is thought to be considering reducing the consumption of X by ΔX, that is, to first move from point A to point B. Then at point B, the consumer is able to spend the money released by the reduction in the purchase of X (as represented by the horizontal distance between points A and B) and use it to buy $\Delta Y_{\$}$ (as represented by the vertical distance between point B and C), that is to move to point C which is on the budget line. However, to replenish the utility lost by the reduction in the purchase of X the consumer would have to purchase ΔY_U (as represented by the vertical distance between point B and D), that is, to reach point D. As the diagram shows, ΔY_U exceeds $\Delta Y_{\$}$, and thus the consumer would not choose to reduce the purchase of X. The behavior presumed in the textbook consumer theory is that the individual would find that any such substitution of ΔX for ΔY would reduce utility. But how does the consumer know when the utility-maximizing point A has been found? Supposedly, the consumer would know utility is maximized at A by considering smaller and smaller potential reductions in the consumption of X. As ΔX is smaller and smaller, the difference between ΔY_U and $\Delta Y_{\$}$ is less and less. When ΔX is reduced to zero, then the consumer is at A and the equation $P_x / P_y = \Delta Y / \Delta X$ is claimed to hold. Most importantly, the consumer is said to know that he or she is maximizing utility whenever that equation holds. And this knowledge is based on independent calculations of both that price ratio and the MRS represented by the right-hand side.

Nobody would question what I have said so far. Nevertheless, it is nonsense. While the ratio P_x / P_y can be calculated to equal an easily understood scalar number, the value of the ratio $\Delta Y / \Delta X$ is not so easily calculated because at point A ΔX supposedly equals zero. The question is, even if ΔY is a non-zero number, what number do you get when you divide by zero? Stated another way, how many times will zero go into a finite non-zero number such as two or ten? The correct answer is infinitely many! I repeat, this equation is nonsense. And it is nonsense even if we define the value of the right-hand side to be what you get when you think of successive reductions in ΔX as an infinite series of calculable values for the MRS

such that this infinite series converges in the limit to P_x / P_y. There are two problems with this, the notion of an infinite series and the notion of an infinitesimal.

Avoiding for the moment the notion of an infinite series, the calculation of the *MRS* is supposedly done by considering the ΔX to be an infinitesimal. An infinitesimal is an alleged number that simultaneously has the value of zero and non-zero. The non-zero value is needed to calculate the ratio as a finite number. The zero value is needed to assure that $\Delta Y_U = \Delta Y_\$$. In any other logical argument this would constitute a contradiction and thus be inadmissible. While it may in some sense be permissible for instrumentalists to accept such a false equation and push on, conventionalist economists usually claim to oppose Friedman's instrumentalism. Nevertheless, any attempt to dismiss this contradiction on the grounds of its being an approximation is open to question. So, I ask, an approximation of what? A contradiction is a contradiction. There is no such thing as a partial contradiction, acceptable or otherwise.

The notion of an infinite series fares even worse. The usual claim that the needed equation holds in the limit and that one can prove this equation holds at the end of an infinite series stretches the notion of a proof at best into the territory of science fiction and at worse into the realm of contradictions. The contradiction here is simple. Infinity by definition is an unreal number, an impossible quantity. Any proof which requires an impossible number is an impossible proof. An impossible proof is no proof at all.

So, if calculus proofs of the necessary conditions for maximization are impossible, how can we use consumer theory to explain the demand in the market? Some readers will say that we learned long ago to use set theory instead of calculus for just these reasons. But, as I have argued in my 1986 book and elsewhere, the set-theory proof would have to be based on the assumption of a strictly convex 'better than' set (the set of points above and bounded by the indifference curve) and such an assumption makes the set-theory version and the calculus version logically equivalent.

The problem is not calculus but the use of calculus to prove notions that cannot be proved by calculus. Thus, if realistic models are to be created, calculus proofs (and their logical equivalent, set-theory proofs) must be avoided.

B. Inductive basis for knowledge assumptions

In my discussion of infinitesimals, I raised a related question. How does the consumer know he or she is maximizing utility? What knowledge is required to prove one is maximizing utility? Obviously, one must know the prices and one's income. Since these are both objective and easily calculated, it is easy to assume they are known by the consumer. But, what about the utility function? The utility function is supposed to tell us how many utils an individual would obtain for *any* bundle of goods that might be consumed. 'Any' bundle means that the utility function is able to report on an infinity of conceivable bundles. How does the individual know the utils provided by an infinity of bundles given that infinity is an impossible quantity? That is, no living person could ever consider in real time an infinity of bundles. So just how do the individuals know their utility functions? This problem was addressed forty-five years ago under the name of the integrability problem. Even if one could quiz an individual about the *MRS* for each of a finite set of bundles, one could never deduce the utility function (or equivalent indifference

map) that would yield that set of observations. Moreover, as I have argued above, it is not even clear that the individual knows the *MRS* for any one point let alone all of the infinity of points.

The more general problem is that when in doubt, theorists, realizing that one must make assumptions about needed knowledge on the part of the individual decision makers, simply assume that the individual's knowledge is acquired inductively. That is, the individual acquires the needed knowledge simply by making observations and inducing the general principles embodied in that knowledge. How does the individual know his or her inductive knowledge is true? Inductive proofs require an infinity of observations, a clear impossibility. Or they require that the observations were made instantaneously, that is, infinitely fast. Either way, people cannot acquire their needed knowledge inductively. Any theory that presumes so cannot be considered a mere approximation since it is an impossibility.

C. Disequilibrium process vs equilibrium attainment

Many critics of neoclassical economics might see these observations as criticisms of equilibrium economics. In one sense, this will be true since any market equilibrium implies simultaneous maximization by all demanders and all suppliers. The demand curve is the locus of price-quantity combinations at which all the demanders are maximizing. That is, the quantity demanded in the market is the sum of the quantities demanded by each individual for the given price. The supply curve is defined in a similar way. Thus, if demand does not equal supply at the going price, either one or more of the demanders is not maximizing, or one or more of the suppliers (or both).

With this in mind, Kenneth Arrow [1959] asked a simple question: who sets the given price? And, if at that going price demand and supply are not equal, who changes the price? That is, what is our theory of price adjustment?

It would be tempting to ask who knows that demand and supply are not equal. But, this question is easy to answer if we keep in mind the definitions of demand and supply curves. If the price is above the market clearing price then supply will exceed the demand. This would mean that at least one supplier is not able to maximize profit at the going price. So in this sense, we can easily explain who knows that there is a disequilibrium. Moreover, we also know who will offer to change the price, namely, the non-maximizing supplier. The non-maximizing supplier tries to compete with other suppliers by offering to sell for a lesser price (thereby inducing some buyer to switch suppliers). So long as the price is still above the market clearing price, such competitive behavior will continue to cause the price to fall. The price-adjustment behavior stops when the market clearing price is reached. But a question remains, how does the non-maximizing supplier know how much to lower the price? And worse, the definition of demand and supply curves are based on the notion of decision makers as price takers. We have an inconsistency here between the behavior of demanders and suppliers when the market clears – that is, when all can be price-taking maximizers – and when the market is not clearing – that is, when at least one ceases to be a price taker and instead chooses to offer a price other than the given price.

One suggestion Arrow makes is to recognize that the theory of a monopolist has the monopolist deciding what the price is. At first blush, it sounds a lot like a theory of price adjustment. But, what is this theory? The price-setting monopolist would set

the price where marginal revenue equals marginal cost. This means that by not being a perfect competitor, this firm faces a downward sloping demand curve which in turn means that marginal revenue is always less than the price. Thus, the price set by the monopolist is never the one where price equals marginal cost as is the case with the perfectly competitive profit maximizer.

The result of this is a simple dichotomy. If the market clears, and thus everyone is facing the market clearing price, all firms can be perfect competitors. But if the market is not clearing, we have to have a non-perfectly competitive theory of the prices. This is a clear inconsistency. We cannot have one theory to explain prices if equilibrium suggests one theory and disequilibrium requires another. Moreover, these two theories are inconsistent. In equilibrium, everyone sets the price at the marginal cost but in disequilibrium, the price is set where marginal cost equals a marginal revenue that is not equal to the price.

Again, the notion that our simple assumption that the market determines the price in a competitive manner is an adequate approximation will not do if it involves, as I am arguing, an inherent contradiction. But another question might be raised if we choose to explain all price behavior with one price adjustment theory, the monopolist. There would be no problem if we were also to assume that the monopolists have certain knowledge about the demand curves facing them. However, if, as is more likely, they are ignorant about their demand curves, then we cannot be guaranteed that the resulting equilibrium is the one where everyone is maximizing. Robert Clower [1959] considered the plight of an ignorant monopolist. He showed that an ignorant monopolist must make assumptions about the demand curve and there are no obvious assumptions to make. Clower then showed that even with plausible assumptions if they are false we can be led to a market equilibrium where the monopolist may think he or she is maximizing profit, but actually is not maximizing profit and possibly not so by a wide margin. In this case, in the ignorant monopolist's market, market clearance does not imply universal maximization. So, even Arrow's optimistic approach to price adjustment seems to lack promise. And again, this is not a matter of approximation but a broader question of how the monopolistic price adjuster could be assumed to have perfect knowledge. As Clower's ignorant monopolist demonstrates, there is no reason to think that the monopolist's deviation from the true maximizing output is a mere matter of acceptable approximation.

D. Inconsistent model assumptions

The econometrics theorist David Hendry has for several years pointed out problems that he thinks econometric model builders cause by ignoring inconsistencies between the modelling assumptions they make and the econometric theorems they apply. His complaints have to do with the technical mathematical forms of the models constructed. Here I wish to consider some less technical contradictions that are even more fundamental. Consider the common acceptance of Bayesian econometrics. According to this version of econometrics one is to think of the truth status of any assumption of a model as having a probability. As data are collected, the probability of the truth status changes in accordance with Bayes' theorem. While it is obviously possible to build plausible models based on Bayes' theorem, the question never considered is whether this theorem is consistent with the principles of logic that are used to prove or establish other aspects of the models. Without going into detail

about the nature of Bayes' theorem, it is enough to note that it presumes that the truth status of any proposition is a probability. That is, the truth status is alleged to be between 0 and 1 and rarely is it either 0 or 1.

Before examining the meaning of this for economics, let us consider some non-formalist views of logic. As I have argued before, at root logic is about admissibility of assumptions into logical arguments. According to Aristotle, three conditions must be met. First, we must not change the meanings of the basic words (i.e., one cannot simply cross out the word frog and write in horse in order to use a statement concerning frogs in a argument about horses). Second, statements in a logically valid argument cannot be simultaneously true and false (this is called the axiom of non-contradiction). And third, there are only two possible values of truth status of an admissible assumption, true or false (this is called the axiom of the excluded middle). Any indirect proof requires all three axioms. An indirect proof starts with the statement to be proved using the assumptions of the argument, then assumes the statement is false. The indirect proof shows that if we accept all of the assumptions as true then our assuming the statement in question is false constitutes a contradiction. Since contradictions are excluded, the statement must not be false. And by the axiom of excluded middle, since there are only two possible values of truth status (true or false), not being false leaves only being true. Thus, indirectly one has proven the truth of the statement in question.

Bayes' theorem would have us violate the axiom of excluded middle. That is, attaching any value other than 0 or 1 as the statement's truth status would put a value that says the statement is neither true nor false. Now, this is not necessarily a problem so long as one is willing to avoid indirect proofs. Who among the Bayesian econometricians is willing to show that absolutely none of the mathematical theorems they use in their econometric theory employs an indirect proof. One suspects that even if they could, which I strongly doubt is possible, there still remains all the problems surrounding the use of infinity and infinitesimals discussed above. The question here however is whether this is merely a matter of approximation.

Clearly, one cannot approximate the axioms of logic. Trygve Haavelmo [1944] made this point more than fifty years ago. In his day (and for some, even today) it was thought that one could use probabilities in place of absolute 0 and 1 values of truth status such that the conclusions reached in a logical argument would simply inherit the same values. Such is not the case. If, say, all the assumptions of a model are true with a probability of 0.5, the logically valid conclusion will usually carry a probability much below 0.5. For this reason, Haavelmo advocated that econometricians should give up on the hope that they could build probabilistic algebraic models to estimate the values of the coefficients of the exact model and then substitute the estimated values into the exact algebraic model to finish deducing various propositions about the economy. He said that instead one needs to enter the world of probabilistic models and never leave. While I agree with his warning concerning the misuse of logic, I do not think the recommendation to remain within the world of probabilistic models avoids the unrealism of probability-based explanations.

E. Unrealistic mathematical objects

Some non-probabilistic models are also open to question when it comes to the question of realism. Here I will discuss two mathematical objects that are problematic. One is the ubiquitous Lagrange multiplier and the other is the Keynesian marginal propensity to consume. I discuss the marginal propensity to consume only because it is an obvious example, but not the only example.

To begin, let me clearly define what I mean by the Lagrange multiplier. Consider the problem of maximizing utility given prices, budget (B) and a given utility function, $U(X, Y)$ – that is, the problem of constrained maximization. Supposedly, the problem here is that the consumer needs to juggle two different evaluations as evident in the discussion of Figure 1. On the one hand, a consumer must evaluate choices in terms of his or her utility implications. That is, what utility value is indicated by the given utility function, $U(X, Y)$. On the other hand, he or she must evaluate the cost implications of differing choices. That is, comparing the cost, $P_x X + P_y Y$ with the budget, B. Clever mathematicians will point out that one can deal with these two different evaluations simultaneously by creating a Lagrange multiplier. Specifically, the two separate evaluations can be combined into a variable V that represents the value of a single function to be maximized, $V(X, Y)$ as follows:

$$V = V(X, Y) = U(X, Y) + \lambda (B - P_x X - P_y Y)$$

Note that this requires the introduction of a new variable, the Lagrange multiplier λ. The role of this variable is solely to translate differing dimensions of the two evaluations. The function $U(X, Y)$ is measured in units of utils. The budget and the expenditures are both measured in monetary units. Thus the role of the λ is to translate monetary units into utils so that we are not adding apples and oranges. The point here is that the λ is an artifact of the mathematics of creating the combined function and nothing more.

Unfortunately, many model builders lose sight of the artificiality of the Lagrange multiplier. Instead they wish to interpret the λ as the 'marginal utility of money'. This is amazing since money is not an argument in the utility function, $U(X, Y)$. Only X and Y are. Is this merely a matter of approximation? I cannot see how it could be. Either B is an argument in the utility function or it is not. This is clearly not a matter of approximation.

Let us turn to a more elementary issue. Consider a simple-minded Keynesian consumption function, $C = \alpha + \beta Y$. It is commonplace to call β the marginal propensity to consume as if it is a natural parameter characterizing the real world of consumers. Is this an object in the real world? Or, is this merely an artifact of the assumption that consumption C is a linear function of income Y? Unfortunately for realists, it does not necessarily correspond to anything in the real world. Consider what we would face had we assumed a quadratic rather than a linear function, namely, had we assumed that $C = \alpha + \beta Y + \gamma Y^2$. Now there is no single parameter representing the 'marginal propensity to consume'. Instead, the marginal propensity to consume is $\beta + 2\gamma Y$ and thus not a natural constant as implied by the notion of the textbook's marginal propensity to consume. Again, the difference between $\beta + 2\gamma Y$ and β is not a matter of approximation.

Reading real-world significance into artifacts of arbitrary mathematical assumptions is not uncommon. Nevertheless, representable realism demands that the parameters of models should represent real world, autonomous phenomena.

That is, the phenomena come first and the model second, not the other way around. Of course, one could be claiming that there is a constant real world marginal propensity to consume, but just how would one test for this? For that matter, how do we even know that the other parameter, α, is a constant? Put another way, is the assumption of linearity a matter of mathematical convenience or an empirical assertion about the nature of the real world? I suspect the former.

3. Concluding lessons

As I mentioned earlier, realism was never an issue in my reading of methodology. For the first time about ten years ago I listened to a historian of economic thought exhorting us to 'put more realism in our models'. The philosopher Ian Hacking was called upon as an authority to support this exhortation. This I found shocking. For one reason it made realism a commodity that one could simply pour into our models. For another, it seemed to be saying we are all dummies incapable of seeing that the solution to all of our problems is simply to use more realism. Moreover, what we really needed was to kowtow to someone's favorite philosopher. Either way I found it offensive. Nevertheless, realism is important, as I have explained in this paper.

Realism, however, directly conflicts with every form of conventionalist methodology that is advocated today. Those methodologists who are currently advocating some form of philosophical realism need to examine why they are doing so. Is it because realism is seen to be a useful avenue to criticize mainstream economics? Perhaps it is, but one must be careful to not advocate that mainstream economics needs more realism while simultaneously arguing for the methodological principles provided only by some form of conventionalism.

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