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Mathematics in Economics: An Austrian Methodological Critique

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I. Introduction

The old joke of the drunk searching for his keys near the street light is an appropriate metaphor for the economist placing mathematics on a pedestal, and in effect worshipping it, while eschewing the legitimate tools of his own trade.

In this article we discuss several classic illustrations of this habit, where the mainstream economist must abandon common sense because his favoured mathematical tools demand total loyalty, even in the face of doing violence to the underlying economic realities.

The first example of mathematical misuse, section II, comes from a recent challenge to Austrian economics from its frequent foe, Bryan Caplan.[1] The next illustration, in section III, involves economics professor and *Slate* columnist Steven Landsburg's criticism of people who donate to multiple charities. Section IV is devoted to the criticism made by many neoclassical economists of the imperfectly competitive model: it misallocates resources, since a downward sloping demand curve must necessarily be tangent to a U-shaped average cost curve at a quantity less than its minimum cost point. We introduce our methodological critique in section IV.

Sections V, VI, and VII are devoted to, respectively, methodology, equilibrium and capital. We conclude in section VIII.

As we shall see in our examples, these orthodox economists go wrong because they desperately want to deploy their mathematical models to

1. For this author's criticisms of Austrian economics, see Caplan (2003); Austrian rejoinders include Block (2007), Callahan (2003), Carilli and Dempster (2003); Hoppe (2005); Hülsmann (1999); Machaj (2007).

capture economic realities.[2] They demonstrate why the Austrian approach – often derided as "unscientific" and "verbal" – is an excellent foil to guard against such neoclassical slips.

II. Bryan Caplan Asks Austrians an Impossible Question

Caplan (2008) poses a challenge to Austrians:

> Austrian economists often attack the mainstream for ignoring something they call "radical uncertainty," "sheer ignorance," or sometimes "Knightian uncertainty." A common Austrian slogan is that "Neoclassical economists study only cases where people *know* that they don't know; we study cases where people *don*'t *know* that they don't know.
>
> All of this sounds plausible until you press the Austrian to do one of two things:
>
> 1. Explain his point using standard probability language. What probability does "don't know that you don't know" correspond to? Zero? But if people really assigned $p = 0$ to an event, then the arrival of counter-evidence should make them think that they are delusional, not [that] a $p = 0$ event has [occurred].
> 2. Give a good concrete example. I've heard Israel Kirzner give examples involving library books, pay phones, and bumps on the head, but none of them [makes] any sense to me.

Before pointing out the absurdity in Caplan's request, we should provide some background. In the early and mid-20th century, standard mainstream models assumed that agents in the economy had perfect information. In such a setting, it was easy enough to "prove" that advertising was wasteful, that cleverly designed mechanisms could allow central planners to replicate the outcomes of a market and that economic theorists could safely neglect entrepreneurship.

Naturally, more and more critics lambasted such an unrealistic approach. In response, the top mainstream economists incorporated "uncertainty" into their models, but only in a very limited way. Now, instead of knowing (say) the spot price of oil at every hour, from now until the year 2050, the cutting-edge general-equilibrium models would instead assume that agents only know the *probability distribution* of spot oil prices at each moment in time for the rest of eternity. Thus, the modellers

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2. Anderson (2001; 2002), Barnett (2004), Herbener (1996), Jablecki (2007), Leoni and Frola (1977); Mises (1977; 1998/1949); Murphy (2008); Rizzo (1979); Rothbard (1988; 2004/1962); Shostak (2002).

thought they had adequately dealt with the objection, for now the agent in the model really does not know what the price of oil will be tomorrow.

With this backdrop, we can understand the debate to which Caplan alludes. Austrian economists, most notably Israel Kirzner, have stressed that this is not *really* incorporating genuine uncertainty – versus actuarial risk – into the mainstream approach. Agents still know the structure of the world with certainty; there is no possibility, say, that an agent mistakenly thinks oil prices are distributed normally with a mean of $120, when in reality the true mean is $121. This type of situation is ruled out as impossible by neoclassical economics.

In a mainstream model – even one that nominally includes "uncertainty" – Jim the Speculator cannot outperform Joe the Speculator in the oil futures markets because of his better foresight. Nobody can ever make a *true* mistake. Yes, investors might later regret their actions once the random variables have been realised, but they would only do so the way a blackjack player might "regret" doubling down on an 11 against a dealer's 6. Even after losing, the blackjack player would rightly say "I didn't really make a mistake, because my move was optimal given the information I had at the time of my decision." This immunity to actual error and surprise is still true of all formal mainstream models, because it is very difficult to formally (but nonarbitrarily) model mistakes.

The Austrians, on the other hand, are perfectly free to explain that some entrepreneurs do better than others in the real world because they adapted to changing conditions more quickly than their peers and hence earned economic profits. Austrians can do this because they are not wed to formal mathematical models. This freedom in their verbal explanation is not a sign of sloppiness but rather a more accurate description of what is really going on in the world.

Let us sum up the debate: Austrians say that standard probability tools are inapplicable in a world of open-ended uncertainty, where it is impossible to assign a numerical probability to all possible future outcomes. If for no other reason than limited mental powers of computation, real people simply do not act this way.

Recall now Caplan's request: he wants the Austrian to paraphrase this insight "using standard probability language!" It reminds us of an episode in grad school that occurred to one of us (Murphy) when he was working on a paper explaining that "radical uncertainty" posed serious problems for the neoclassical approach to interest (Murphy 2003). One of his advisers listened to him explain the problems for five minutes in the hall and then said, "Hmm, put all of that into a simple two-period model."

The point is, Caplan's challenge is misbegotten. It is *impossible* to utilise mainstream or "standard probability language" because this Austrian insight constitutes an *attack* on this very methodology. What Caplan is asking for is, in effect, a square circle: Austrian theory couched in terms that contradict its very assumptions.

III. Landsburg Thinks Most People Giving to Charity Are Selfish

Landsburg (1997) argues that if they were truly motivated by altruism for the recipients, then people should concentrate their charitable giving on a single target:

> People constantly ignore my good advice by contributing to the American Heart Association, the American Cancer Society, CARE, and public radio all in the same year – as if they were thinking, "OK, I think I've pretty much wrapped up the problem of heart disease; now let's see what I can do about cancer." But such delusions of grandeur can't be very common. So there has to be some other reason why people diversify their giving.
>
> I think I know what that reason is. You give to charity because you care about the recipients, or you give to charity because it makes you feel good to give. If you care about the recipients, you'll pick the worthiest and "bullet" (concentrate) your efforts. But if you care about your own sense of satisfaction, you'll enjoy pointing to 10 different charities and saying, "I gave to *all* those!"

In the first place, even on his own terms, Landsburg's criticism does not make much sense. Even if people are really donating merely in order to feel good about themselves, why would they care about something irrelevant, such as the number of organisations to which they donated? For example, if a person pats himself on the back for sending $500 each to four different charities, why would that person not feel equally smug by sending $2000 to one charity? (Or for that matter, why not $2 each to one thousand different charities?)

As so often happens in these matters, the problem is that Landsburg needs to model the situation using formal mathematical tools, and once he goes down this path, out pops the "optimal" answer that a person should concentrate all of his giving into the single charity that he considers most important. We are not putting words in his mouth; here is Landsburg (1997) himself:

> Early in this century, the eminent economist Alfred Marshall offered this advice to his colleagues: When confronted with an economic problem,

> first translate into mathematics, then solve the problem, then translate back into English and burn the mathematics. I am a devotee of Marshall's and frequently follow his advice. But in this instance, I want to experiment with a slight deviation: Rather than burn the mathematics, I will make it available as a link.
>
> I propose to establish the following proposition: If your charitable contributions are small relative to the size of the charities, and if you care only about the recipients (as opposed to caring, say, about how many accolades you receive), then you will bullet all your contributions on a single charity. That's basically a mathematical proposition, which I have translated into English in this column.

There are all sorts of motivations that could acquit philanthropists of Landsburg's charge, namely that they are either irrational or secretly donating to charity just to win public approval. For example, someone might want to build up a level of trust with an organisation over time and see how it spends smaller donations before ramping up the contributions. Or someone might think that an organisation can only maintain its independence if it relies on numerous small contributions rather than a few large donations from wealthy individuals.

The point here is not to offer a rival theory. Rather, our point is that people *do not approach charitable giving* the way Landsburg assumes in his model. Since he is wrong at step 1, it should not surprise us that he reaches a conclusion that not only strikes most people as counterintuitive but actually fails to explain human action, motivation and purposes.

IV. Downward Sloping Demand Curve, Tangency, U-Shaped Average Cost Curve

Anti-trust law has done great harm to our economy. The geometry in support of it furnishes yet another example of misplaced worship at the altar of mathematics on the part of dismal scientists. Figure 1 depicts the traditional U-shaped cost curve, along with a downward sloping demand curve. Naturally, the tangency cannot take place at point A, the quantity at which costs are minimised. Instead, the demand curve can only be tangent to the cost curve at point B, upward, and to the left of A, an "inefficient" price and quantity. This is neither the time nor the place for a full-blown attack on this particular neoclassical model.[3] Suffice it to say for our

3. See on this Anderson *et al.* (2001); Block (1994); DiLorenzo and High (1988); High (1984–1985); McChesney (1991); Rothbard (2004/1962); Shugart (1987); Smith (1983); Tucker (1998a; 1998b).

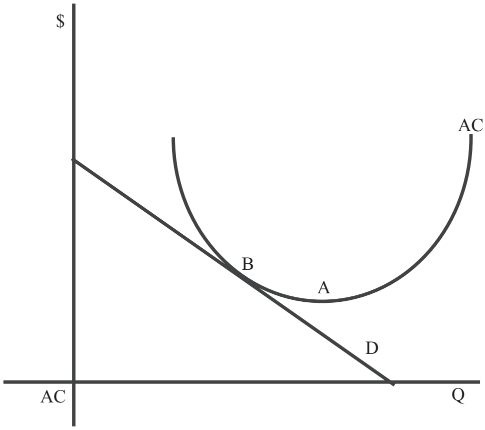

Figure 1 smooth U shaped AC curve, straight line downward sloping demand curve

present purposes that this conclusion of theirs stems, solely, from mathematical considerations, not economic ones.

One indication of this is provided by Rothbard (1970, 644, figure 72), which we reproduce as Figure 2.[4] Here, as can be seen, the demand curve and the cost curve intersect precisely at point A, again, supposedly the most efficient solution. There is an intersection at this point, not a tangency, since the U-shaped cost curve in this figure is not a smooth one.

Why oh why do mainstream economists attempt to make their case in such a manner? Why the blinders? Why the insistence on smooth curves? This is because of their adherence to mathematical techniques. If integration and differentiation are to take place, then the functions must admit of infinitesimally small changes. Of course, human action never takes place on such a basis. Rather, human beings act in a discrete manner, not compatible with calculus and differentiation. But for economists who place the requirements of mathematics on a higher plane than the actions of the human beings they are supposedly trying to explain, this makes no never mind. Let us have smooth curves by all means and allow the economics to fend for itself.

4. For an alternative depiction of this phenomenon, see also Barnett and Block (2006, 62, figure 1).

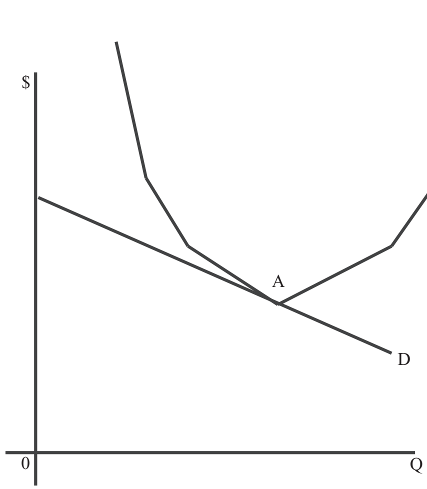

Figure 2 kinked U shaped AC curve, straight line downward sloping demand curve

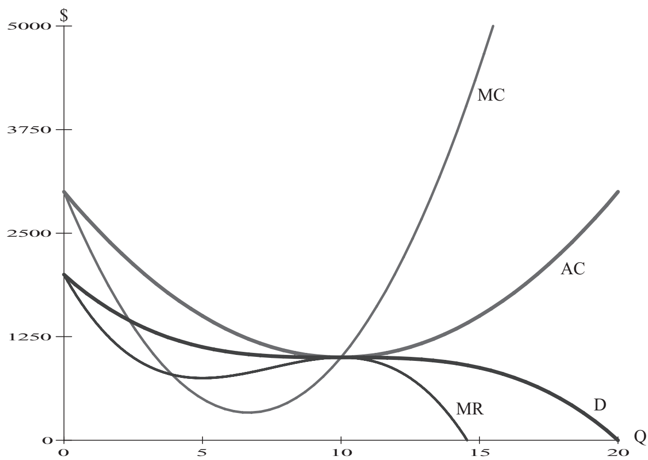

Figure 3 smooth U shaped AC curve, curved downward sloping demand curve

But as Figures 2 and 3 make clear, the "inefficiency" of the downward sloping curve stems, solely, from the smooth curved assumption. Without it, with a U-shaped average cost curve as indicated in these figures, point A in Figure 1 may be attained. The presumed inefficiency, then, is a product of mischievous and erroneous mathematical assumptions and has nothing to do with the economics of the matter.

There is yet another way of illustrating the fallaciousness of the mainstream conclusion about the inefficiency of downward sloping demand curves. This is offered by Barnett and Block (2006a, figure 3), which we replicate as Figure 3. Here, there is indeed a tangency between the downward sloping demand curve and the *smooth* U-shaped average cost curve. However, as can be seen, the demand curve is no longer a straight line.

Strictly speaking, this is no longer a case of the mathematical tail wagging the economic dog, as in the previous examples. There is certainly no mathematical reason why the demand curve must be a straight line. Yet if it is not, then it is entirely possible for it to be tangent to the smoothly drawn U-shaped average cost curve at the bottom point of the latter, as illustrated in our Figure 3. This puts paid to the notion that there is something necessarily inefficient about the non-flat demand curve of imperfectly competitive models.

V. Methodology

The implicit goal of mainstream methodology[5] is to measure and determine the relations between economic variables. Such a goal presupposes the existence of at least some constant variables making measurement and calculation possible.[6] It is believed that the achievability of this goal is only circumscribed by the bewildering number of affecting factors of economic activity rendering laboratory type experimentation in economics infeasible.[7] If experimentation was possible, it is widely believed that

5. For the purposes of this paper, mainstream methodology is defined as the scientific empirical method based on positivism as encountered in economics textbooks and generally taught at universities. See on this Friedman (1953); for a rejoinder, Long (2006).

6. For example, to solve for a set of simultaneous equations requires a minimum number of exogenous variables, without which a solution would not be possible. In the natural sciences fixed relations of measure exist, such as the expansion of mercury in a thermometer or water always boiling at 100 degrees Celsius at sea level. In fact it is these fixed points of events (boiling and freezing in the case of water) that have been used to define measure. There are no such known constants in economics. For example, Solow growth models based on Cobb–Douglas production functions cannot account for differences in per capita capital and income over time or countries within any small margin of error or probability so as to prove useful for applications as in the natural sciences.

7. Where experiments have been made, such as in behavioural economics, the results reveal explanatory power but little quantitative or predictive power, as the results are time and place dependent (unlike natural science experiments, which have far broader applications because of time and place independence).

economics would be as practical and as successful as the natural sciences have been for the advancement of technology. This belief sustains mainstream methodology.

In contrast, the implicit goal of Austrian methodology is the derivation of exact or universal laws of human action independent of time and place. Measurability and the existence of constant variables are denied. They are incompatible with the existence of free will and human choice.[8] Money, prices, profit, and loss all arise as a result of the human mind and would not exist without it. Prices are not *measured* in terms of money; prices *consist in* money, e.g. monetary terms (Mises 1977). Purposeful behaviour involves attributing meaning to reality. Human action also has the power to lift man above biological and natural necessities. Man is capable of interfering with the course of nature, of adjusting his behaviour to greater advantage that can be achieved by means of cooperation through the division of labour. The influence of man's animal instincts and his environment makes him subject to psychological dispositions. Human consciousness, however, places him in the unique position of being able to order and control his dispositions and shape his destiny.

The objects of reality are shaped or unpacked and used in very different ways by the two methodologies. In the mainstream paradigm the objects of reality are essentially shaped into a formamenable to quantitative analysis, thereby trying to establish their measurable content whether real or in abstract terms.

By conflating action with the content of the related objects – such as the act of preference with the objects of preference – mainstream theory remains time and place bound. The conclusions of mainstream economics are therefore only ever "probable" ones, that probability being based on past relationships continuing to hold or on guess estimates of future conditions. For this reason its theories are always subject to external conditions and therefore falsifiable. No attempt is and can in fact be made within the mainstream paradigm at the systematic derivation of universal truths.

The conclusions of Austrian economics are necessarily qualitative. However, the qualitative nature of Austrian economics differs from the probable or ad hoc time- and place-dependent kind one encounters under the scientific method of the mainstream paradigm. Qualitative conclusions are therefore less satisfying from a mainstream perspective. Whereas mainstream economics has therefore adopted the application of quantitative

8. Anthropomorphism is condemned in neoclassical economics but is a basic element of Austrianism.

techniques as central to its exposition, Austrian theory has adopted the very *form* of mathematics or logic (to be understood in their pure a priori constructed forms) as central to its theory. Like mathematics and logic, the qualitative conclusions of Austrian economics represent exact laws that can be applied to reality.

Whereas there are many further aspects[9] that flesh out Austrian economics, we are now concerned only about the core structure of Austrian economics, as developed by the most prominent teachers of the Austrian school, Carl Menger (1950/1871) and Ludwig von Mises (1998/1949). Our focus under the "Austrian methodological framework" is therefore on praxeology.

What of the relationship between methodology and mathematics? Callahan (2005) states:

> Today, the prevailing belief is that any real science must be composed of mathematical models, models which yield quantitative predictions about some class of events based on particular, initial conditions, also specified numerically. Once again, the currently popular methodology has been imposed on diverse disciplines with little regard to whether it is suitable to their subject matter, but simply because it is thought to be the only respectable way to do science. The philosopher John Dupré calls this "scientific imperialism," meaning "the tendency for a successful scientific idea to be applied far beyond its original home, and generally with decreasing success the more its application is expanded."
>
> Once again, we see a frantic effort to generate models fitting the accepted paradigm, with little regard for the realism of the assumptions and mechanisms from which they are constructed.

This mainstream philosophy was further developed and debated by groups known as the Vienna Circle. The central principle of logical positivism was that all empirical statements about the world must be testable. If a statement cannot be tested, then it has no meaning. Consequently all necessarily true statements are vacuous and the existence of logically necessary truths is therefore to be dismissed as tautologies that cannot convey any new information about the world. After the German Anschluss of 1938, the Logical Positivists and those attending meetings of the Vienna Circle (including non-Logical Positivists such as Karl Popper) were dispersed in

9. For example, the use of models in a purely abstract sense as mental constructs to aid understanding, such as Mises's "evenly rotating economy" for the determination of the nature of interest derived from time preferences. The use of such models is subsidiary to and does not enter praxeology as exogenous variables. Other aspects fleshed out by particular individuals include Hayek's evolutionary theories of knowledge and the spontaneous order of society and Kirzner's (1973) theories of entrepreneurial action.

educational institutions around the English-speaking world. Their philosophy was readily integrated with that of the empiricists as both philosophies implicitly shared the same basis: material determinism, i.e. that social phenomena will one day be capable of being explained by physiological and chemical processes affecting our behaviour and our actions and thus determinable solely on the basis of experience. Their influence at American universities was to reject Mises's praxeology as archaic and Scholastic (Gordon 1996).

Karl Popper, who can be regarded as having been most influential on mainstream economic methodology as it is practised today, adapted the principle of verifiability of the Logical Positivists, replacing it with his principle of "falsifiability" serving as the distinction between science and non-science.

Thus necessarily true statements such as the a priori statements of mathematics and logic are cast into the realm of non-science, leaving only falsifiable statements such as the hypotheses of economics within the realm of science. Praxeology, which adopts the same a priori "non-verifiable" form as mathematics and logic, thus also becomes non-scientific.[10] Popper also maintained that verifying a model does not increase its chances of being true. No matter how many times a demand curve has been found to be sloping downwards and to the right, the probability that this statement is true has not gone up (Gordon 1996).

Whereas mainstream economic methodology is rigorous with regard to the use of mathematics, Austrian methodology is rigorous with regard to logic and its link to reality. For this purpose, Austrian economics has developed its methodology based on praxeology, otherwise defined as the "logic of human action," the "logic of choice" or "the general theory of human action" (Mises 1998/1949, 3). Instead of developing hypotheses, whether on the basis of mathematics or logic, praxeology is itself a branch of logic and as such is cast in the same *form* of mathematics and logic, i.e. as a pure a priori science, where conceptual truths or essences play the role as numbers do for mathematics.

This gives rise to something of an anomaly. Austrians, on the one hand, eschew what they see as the over-mathematicalisation of economics. However, their methodology is completely within the realm of mathematics, geometry, and symbolic logic in that the truths it unearths are akin to

10. Popper disclaims association with the Logical Positivists by stressing that his principle of falsification is a test for scientific statements and not as a criterion of meaning (Gordon 1996).

the Pythagorean theorem: synthetic a priori statements, which are necessarily true and yet help us understand and explain economic reality. Neoclassical economists, on the other hand, embrace the mathematicalisation of economics. And yet they accept only tentative hypotheses, something that is incompatible with the axioms of mathematics, geometry, logic, etc.

Mainstream economics is concerned with the correct specification of mathematically tractable models. A mainstream theory consists of a set of definitions of the variables to be employed, a set of assumptions under which the theory is meant to apply and a set of hypotheses about how things behave (Lipsey *et al.* 1990).

All parts of the model are to be shaped into mathematical form. So for example, variables are numerical and deemed measurable even where these cannot be directly observed, such as in the case of utility. Even where utility has been recognised as ordinal in nature, sophisticated attempts have been made to cardinalise utility (Barnett 2003).

Assumptions may be qualitatively stated, such as perfect competition or complete information, but their importance is in isolating an aspect of reality that is amenable to mathematical modelling. Behaviour, such as optimising behaviour, can be captured in production, marginal benefit and marginal cost functions even though such functions are indeterminable in practice.[11]

The application of mathematics for any model requires the determination of exogenous variables. Without exogenous or fixed variables, there would be no solutions to any quantitative model. The number of endogenous variables capable of being supported by any model is based on the

11. This is because dimensional analyses, which always accompany mathematical applications in physics in order to ensure the consistency and validity of formulas (i.e. to ensure that dimensions such as metres on one side of an equation are equal to metres on the other and not centimetres, litres or metres/second), is not performed in economics, where dimensional analyses would generally not hold. See Barnett (2004), who performs dimensional analysis on the Cobb Douglass production function and a household macroeconomic model with micro foundations. This is because mathematics is applied at a higher level of abstraction in economics than in physics. Mainstream economics can aptly be defined as "relational science" where what counts is the relation between variables, be they between incommensurables such as prices and quantities, and not a formula necessary for an output in the form of some technology, where dimensional analyses would be imperative. This higher level of abstraction, however, renders direct application to the real world impossible and is to be regarded as merely explanatory of optimising behaviour. Only in the case where there are missing markets, such as in environmental economics, are marginal cost and marginal benefit computations employed in practice. The implication is that one first has to have missing markets before one can do the computation – which only brings into question the nature of the assumptions underlying the derivation of the formulas or values used.

number of equations to be incorporated in it and can be determined by matrix algebra techniques such as the computing of matrices in "row echelon form."

A model theory needs to be subjected to hypothesis testing, as the assumptions are not self-evident and often abstract. The assumptions are also always time and place dependent. The model is therefore only ever as good as the essential features captured by it remain the essential features within the complex of reality modelled. For example, econometric models hold under the assumption that the historical conditions and relationships continue to hold. Adjustments for possible changes in exogenous conditions then become guess estimates.

In this manner, mainstream economists are trying to directly solve for the problems of entrepreneurs or governments – which provide them with their funding – as if the process of speculation itself could ultimately be done away with making the process of speculation effectively redundant for economics.

Friedman (1953) provides many examples of the usefulness of unrealistic assumptions as applied by the physical sciences. Long (2006) rebuts many of Friedman's examples by showing that the physical sciences do not violate principles of realistic abstraction once we become familiar with Aristotelian principles of abstraction. Long sets the foundations for understanding how the failure of economists to distinguish between platonic forms of abstraction and essential isolations of reality leads mainstream theorists astray in their modelling. Both forms of abstraction are useful, but they are to be used in different ways. For example, platonic abstractions such as the assumption of complete information, which cannot conceivably exist in the real world (there will always be uncertainty and change), can only be used as a mental construct in aiding our understanding, whereas the assumption of perfect competition, which is conceivable as an existent part of reality, could be validly modelled as a representation of reality if that is what is regarded as the essential feature of that part of reality that is to be explained.

For Austrians, "what we know about our action under given conditions is derived not from experience, but from reason. What we know about the fundamental categories of human action – action, economizing, preferring, the relationship of means and ends, and everything else that, together with these, constitutes the system of human action – is not derived from experience. We conceive all this from within, just as we conceive logical and mathematical truths, a priori, without reference to any experience" (Long 2001, 15, referring to Mises's "Epistemological Problems of Economics" I.1.6)

Mises presents a radical anti-positivist approach[12] that is just about inconceivable or ludicrous for application by the mainstream. Blaug, in his critique of Austrian economics, offers this view of such a position:

> Mises' statements of radical apriorism are so uncompromising that they have to be read to be believed"; they "smack of an anti-empirical undertone . . . that is wholly alien to the very spirit of science," and are "so idiosyncratically and dogmatically stated that we can only wonder that they have been taken seriously by anyone" (Long 2001, 3, referring to Blaug).

Mises's insistence on the non-empirical nature of economics appears as extreme, particularly when viewed from the perspective of Humean scepticism or its positivist variants. Thus, Blaug interprets him as anti-empirical.

As a causal process theory whereby "causal processes are self-determined in that they are not parasitic upon causal influences exterior to the process itself" (Mäki 1992, 40), Austrian economics is exactly *like* mathematics and logic. Mathematics and logic represent internally consistent systems whose rules (such as the rules of differentiation and integration) are derived a priori from central axioms and principles, which themselves are not dependent or contingent on any exogenous variables. In this sense, mathematics is not *dependent* on experience; it is *applied* to experience. "Experience makes it possible for us to know the particular conditions of action in their concrete form" (Long 2001, 15, referring to Mises's "Epistemological Problems of Economics" I.1.6) – for example, when wanting to count something, we apply rules of addition to that something.[13] Thus only once we understand the *form* of economics that Mises is talking about can we begin to understand the relevance of what he is saying.

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12. Ludwig von Mises (1978; 1998/1949) was indeed opposed to logical positivism. But of course, there were "positive" aspects to his work. For example he made a radical distinction between fact and value, between the normative and the positive (we are grateful to a referee of this journal for pointing out this fact). The reconciliation between these seemingly incompatible statements is that there is a world of difference between (a rejection of) logical positivism, and support of the distinction between fact and value, between normative and positive economics.

13. This sense in which mathematics is not dependent on experience but applied to experience can be illustrated by a quote from Long (2001), who quotes from Wittgenstein: "[i]f 2 and 2 apples add up to 3 apples, i.e. if there are 3 apples there after I have put down two and again two, I don't say: 'So after all 2 + 2 are not always 4'; but 'Somehow one must have gone.' " In other words: mathematical concepts are applied in such a way that *nothing counts* as a falsification of mathematical law. We may *illustrate* mathematical claims by means of empirical experiments, but if the experiment goes wrong we revise not the mathematical claim, but rather the choice of illustration" (Long 2001, 38).

In the same way that the rules of differentiation and integration are derived a priori from reason, so too the laws of praxeology are derived from this same source. We do not create mathematics on the basis of empirical evidence (for example, rules of addition and subtraction are independent of singular experiences); rather, we need mathematics in the application to make sense of empirical evidence in the first place. When people say that mathematics is based on experience, they can only mean so in the sense that a reasoning mind conceives of the usefulness of developing a method of counting or differentiating *for* experience or making sense thereof or providing order thereto. The distinction between what we *learn* from experience and what we *use to make sense of* experience is important if we are to understand what mathematics, logic, and praxeology are about. It is in this sense that praxeology is, like mathematics and logic, non-empirical. To say of mathematics that it is anti-empirical would mean that it has no application *to any objects*.

Whether something is to be regarded as a priori or empirical can only be determined with regard to its dependence upon experience. All claims can be derived by reference to experience, but only a priori claims are independent of experience, i.e. apply to all related experiences and are therefore to be regarded as prior to experience. The following statements about which empirical claims could be made will help illustrate this point:

A The sum of the angles of a triangle equals 180 degrees.
B The propensity to consume of a society is x per cent at a particular time and place.
C Humans have time preference.

Each of these statements can be derived by reference to experience. However, A is a synthetic a priori statement as it is independent of experience. Only reason allows us to determine that A is a priori true and unlike model hypotheses is to be regarded as universal of all triangles without the need to keep testing all various shapes of triangles to establish this fact. And yet, A certainly applies to reality, unlike a mere tautology. B, on the other hand, is an empirical statement that needs to be constantly validated by experience and is therefore entirely dependent on experience. C, unlike A, has a social dimension to it as it is concerned with subjective human experiences.

However, like A, C as stated above is universally valid and is in this sense a priori and to be regarded as before experience. Only the extent of a particular person's time preference in its concrete form is an empirical matter which is dependent on the time and place of the individual.

Austrian economics is foremost concerned with the universal aspect of time preference and the implications of an increase and decrease in time preferences in the same way that mathematics is concerned with the implications of increasing and decreasing functions on, for example, the signs of various orders of differentiation for the determination of positive or negative semi-definiteness, i.e. local minima or maxima. Praxeology is not concerned with the actual measure of time preference, which is empirical with regard to the past and speculative with regard to future uncertainty inherent in nature.

VI. Equilibrium

Walrasian economics, the basis of most modern versions of this discipline, has been predominantly concerned with equilibrium end-states demonstrating under which conditions efficiency in the allocation of economic goods is to be obtained. The Walrasian perspective therefore also teaches us that the market cannot be efficient because the conditions are seldom, if ever, met in the real world (complete information and perfect competition, for example).

Austrian economics, on the other hand, is concerned with market processes, i.e. the mechanisms by which information is transmitted and the allocation of goods occurs. The existence of steady states of equilibrium, which can only be made under the assumption of complete information, makes such processes redundant. A focus on such states cannot therefore show us what these processes are, nor can they teach us anything about what a more efficient process would look like or what happens with regard to the process when policy makers try to impose the mathematically determined Pareto optimal conditions thought to lead to a more efficient outcome. As Mäki (1992) points out, the assumption of full information subsumed under general equilibrium leaves "no room and no need for the notion of the market process as a learning or discovery process" (Kirzner 1973; Mäki 1992, 46).

The assumption that all exchanges take place at one equilibrium price also precludes the idea of process. "In the Austrian theory it is vital that the market envisage a multiplicity of prices for one good. It is these price differentials that provide entrepreneurs with opportunities for arbitrage gain and thus stimulate them to generate the market process" (Mäki 1992, 46).

Increased similarities have recently emerged between behavioural economics and Austrian economics. For example, Bowles (2004, 63) states that "taking *explicit account of out-of-equilibrium dynamics* is a . . . characteristic of evolutionary approaches," and the notion of "path dependence" would be familiar territories for Austrian economists who have always eschewed the general or partial equilibrium or time simultaneity approaches of mainstream economics. These similarities, however, are only on the surface, as the two methodologies remain distinct. The methods of behavioural economics can be shown to represent an evolution of the mainstream paradigm based on advances in cognitive psychology, simulation programming techniques and the application of probability theory (as in game theory). This evolution of mainstream methodology can be contrasted to the methodology of Austrian economics. This comparison is particularly stark as these advances leave Austrian economics unchanged.

Aspects of reality, such as the role of the entrepreneur, which up to now at least eludes amenability to mathematical techniques, are largely ignored. Modelling what are considered to be the essential features of economic reality therefore comes with the proviso that it be amenable to quantification.

VII. Capital

For mainstream economists, capital also has to be homogenous in order to facilitate their aggregation and mathematical tractability. In the typical macroeconomic treatment of the economy, capital is incorporated into the analysis as a simple k, which enters into the production function. There is no doubt that this facilitates calculation.

However, as the Austrians see economic reality, matters are not at all so simple. Rather, capital consists of a delicate lattice work of raw materials, semi-finished products, factories, machines, fuel, hotels and forests, in bewildering complexity. To shovel all of these factors of production into one category, k, merely for the sake of mathematical ease is surely to do violence to economic reality.

Of course, it cannot be denied, all of these disparate elements of what counts as capital can indeed be summed up into one expression. They each, at least in the free society, have a market price, and this can be added together. But this assumes that the economy is in equilibrium, e.g. that all of these different aspects of capital are perfectly integrated with one another. It simply will not suffice for any real-world economy.[14]

14. As seen, supra, we do not accept Friedman's (1953) notion that assumptions in economic models may properly play fast and loose with the truth.

Typically, the Austrians characterise capital not as a homogeneous blob, k, but rather in the form of a triangle (Garrison 1994; 2001; Hayek 1931; Rothbard 2004/1962).[15] This highlights the fact that there is a time dimension involved in this sector of the economy. It allows contemplation of the difference between capital widening and deepening. It enables the analyst to bring to bear on macroeconomic issues the concept of time preference. Thus, ABCT is a far more enriched enterprise than are neoclassical and Keynesian perspectives that give short shrift to capital. For another illustration of the danger of economic theory being sacrificed for mathematical tractability, consider the famous Cambridge Capital Controversy. Here, the "simplifying" assumption of a homogeneous capital stock fostered the dubious conclusion that interest is a return to the productivity of capital. The serious problems with this (still standard) notion allowed the UK critics to challenge marginal distribution theory itself, when in fact it was the one-good model and reliance on steady states that were to blame for the confusion (Murphy 2005).

To enlarge on these issues would take us too far from the thesis of the present paper: the excessive role that mathematics plays in modern-day economics. Suffice it to say that if mathematicalisation is not the prime explanation for this phenomenon, it is certainly compatible with it.

VIII. Conclusion

The methodology of mainstream economics remains time and place bound. The predictions of its models are dependent on future relations remaining the same as past ones. Its explanations too are only ever probable ones as its specifications cannot be comprehensive enough. Because of its implicit belief in measurability, a belief that correct model specification is ultimately possible, the mainstream economist believes that he or she only needs to overcome the difficulties presented in obtaining the requisite measures of data. If a prediction or explanation turns out to be false, the mainstream economist invariably points the finger to the specification of the model. He has not considered that if reality is not about measure but about expression, then all exercises in model specification are exercises in alchemy – attempts to transform base metals into gold.

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15. Barnett and Block (2006b) criticise the use of the triangle in Austrian business cycle theory (ABCT) and offer, instead, interest sensitivity as an alternative.

The possibility that universal knowledge can be obtained is overlooked by a mainstream paradigm based on the philosophy of positivism and which has adopted mathematical methods – which have otherwise proved so successful in the natural sciences where measures are readily found – as its foundation. The strength of mathematics, however, lies in its a priori methodological structure. That strength is weakened the further one abstracts away from reality in the quest for elusive measures.[16]

In contrast, Austrian economics has not adopted the methods of mathematics as foundational but as its a priori form in its methodology of praxeology. On the basis of necessarily true statements, which can be identified after a re-description of outwardly observable phenomena in terms of attributed meaning, Austrian economics is able to obtain a comprehension of the "endogenous" market process. Like mathematics, praxeology can be applied to reality. Its qualitative explanations and predictions are independent of time and place.

The Misesian approach to economics does not rely on unrealistic formal models. Rather, praxeology – the science of human action – deduces necessarily true conclusions from the fact that people act.

It is true, the Misesian can often present his entire analysis in purely verbal terms, and he cannot make any guaranteed quantitative predictions about the economy. But as the joke about the drunk looking for his keys illustrates so well, scientists need to look for answers where they actually may lie rather than redefining the problem into one that our favourite tools can handle.

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16. See, for example, Ross (1999), who writes about the development of utility theory and welfare economics from Bentham to game theory: "[w]e learned that piling increasingly elegant mathematical proofs, conducted in a spirit of stubborn refusal to abandon the simplifying assumptions that both made it possible and *also* tended to make them impossible to usefully apply, was an enterprise of diminishing returns." Ross, however, does not abandon mainstream methodology; he believes that the way forward is to fashion new and better tools (presumably also quantitative ones).

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