

Recognizing Knowledge: Prescription vs. Explanation

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Critics of neoclassical economics have claimed for decades that the achievement of a general equilibrium requires perfect knowledge on the part of all participants in the economy. The critics are wrong – at least to the extent that they think ‘perfect knowledge’ is a *necessary* condition for the achievement of a general equilibrium. However, while it is not necessary, perfect knowledge is *sufficient* for the achievement of universal maximization and hence for general equilibrium. Moreover, failure to maximize would be difficult to explain in a neoclassical model when every decision maker is presumed to have true knowledge. This presents a problem for neoclassical theorists who wish to address a long-standing need to explain the stability of any presumed equilibrium. That is, to address stability questions, one has to recognize a state of disequilibrium but such a state is inconsistent with the neoclassical assumption of universal maximization. Unless one can see maximization as a process rather than a static state, stability analysis is impossible. And, as recognized long-ago by Paul Samuelson [1947/65] and Kenneth Arrow [1959], without an adequate stability analysis, neoclassical theory is empirically empty.

The ‘ignorant monopolist’ that Robert Clower [1959] identified was an explicit refutation of the claim that perfect knowledge is necessary for a neoclassical state of equilibrium. Specifically, Clower showed that a decision maker could hold a false theory about the market and still reach a state of apparent equilibrium since there would be no reason for change. The only reason for a neoclassical firm to ever change its supply quantity is that it *thinks* it is *not* maximizing with its current supply decision – but, in Clower’s model, his ignorant monopolist (erroneously) thinks profit is being maximized. Perhaps Clower’s ignorant monopolist is an example that fits Plato’s Allegory of the Cave which Pablo Garcia discusses in his comment on my article.¹ In any case, so long as any neoclassical decision maker thinks he or she is maximizing, there would be no reason to alter the decision currently being made (e.g., the decision about how much to supply to the market).

The purpose for discussing Clower’s ignorant monopolist in my 2002 article is that it clearly illustrates how a decision maker’s knowledge plays a significant role in the explanation of the decision maker’s behaviour and importantly, it shows how a decision maker’s false knowledge can lead to results that challenge neoclassical theory. But, even more important, it demonstrates how to employ Friedrich Hayek’s 1937 view that recognizing a role for knowledge is essential if one wants to see maximization (and equilibrium) as a process rather than just a state of affairs – particularly, if one wishes to address the long-standing problem of stability assurance needed for any neoclassical equilibrium explanation of observed events.

So, with the combination of Clower and Hayek, we have a sophisticated challenge to any neoclassical equilibrium explanation of an observable economy – a challenge which puts knowledge as a mediator between an assumption of maximization and the need to recognize change in order to address questions of equilibrium stability. By adding to this mix of ideas I have tried to show that such an approach to overcoming claimed knowledge shortcomings of neoclassical economics necessitates the additional

recognition of the *methodology* of the successful utility-maximizing or profit-maximizing decision maker in any state of general equilibrium. But, there is more to this than just the questionable matter of an appropriate neoclassical equilibrium methodology, there is also – as Hayek recognized – the matter of the *theorist's* recognition of the decision maker's learning methodology that is implicit in any *process* of maximization.

In other words, methodology must be considered at two different levels: there is the methodology of the neoclassical theorists who go about explaining an individual's choice in a market and there is the methodology that the individual must be presumed to have in order to explain how he or she makes his or her choice. This dual consideration can be confusing and one must be careful to keep them separate. It would seem that in my 2002 article I was not completely successful in keeping them separate.

Gustavo Marques has identified a problem that might arise should anyone not see the distinction between these two levels of consideration. Specifically, Professor Marques thinks that by my saying that I favour what I call the Socratic-Popper theory of learning (viz., one learns only by discovering one's errors), I must be arguing that this is the only theory of learning (and knowledge) we should consider when we construct any neoclassical explanation of an observed decision. He says that if everyone were engaged in Socratic learning, equilibrium would be impossible. And thus, he claims, repairing the neoclassical program for equilibrium-based explanations would also be impossible.

In what follows I will argue the opposite, namely, if decision makers employed a Socratic-Popper theory of learning, true equilibrium would be more likely than a false equilibrium (which I will explain). But first, I need to clarify that by suggesting that the decision makers' knowledge and methodology be recognized, I am not advocating that the only methodology or theory of learning that decision makers should be allowed to have is the Socratic-Popper one. To the contrary, my primary purpose for discussing the Socratic-Popper theory is to have it recognized as one of many possible theories of learning and methodology that a decision maker might employ in his or her decision process.

As I noted in footnote 3 of my 2002 article, years ago I already discussed some alternative assumptions one might make concerning possible theories of learning that a decision maker may employ when utilizing the information provided by a market. That is, for example [Boland 1992, Chapter 11], a decision maker could deal with information as an inductivist (sophisticated or not), an apriorist, a conventionalist, a scepticist or an instrumentalist. Actually, in that discussion I never considered neoclassical theorists presuming that the decision maker might employ the Socratic-Popper theory of learning. But, Professor Marques thinks that if one did presume every (or maybe even just one) decision maker were following Popper's lead, then equilibrium would be impossible. The reason is simply that the Popperian decision maker would supposedly be testing his or her knowledge of the situation and thus be constantly disrupting any state of equilibrium.²

Now, behind the scene is an unspoken problem that neither of us discussed.³ The problem is that neoclassical economists commonly confuse 'equilibrium' with 'balance'. When it is said that the equality of demand and supply constitutes an equilibrium, this is misleading. The equality of demand and supply is merely a matter of balance. Equilibrium is a state of *stable* balance. For example, a coin balanced on its edge is not in a state of equilibrium since any slight disturbance causes the coin to fall over. For a coin to be in a stable balance on its edge there would have to be a reason for it to always return

to its upright state. Similarly, for the equality of demand and supply to be a state of equilibrium, there must be a reason for why there is a stable balance between demand and supply. That is, there must be an explicit reason for why any disruption of that equality would be corrected by a return to the balance between demand and supply. A theory of the stability of market equilibria would provide such a reason.

If one gets away from thinking of an equilibrium as a static balance and instead recognizes that it must involve some dynamic considerations involving reasons for why any perturbations of the balance will be corrected, then I think the door will be open for the consideration of the issues raised by both Clower and Hayek. But I go further. For example, my 1986 book is mainly about how to solve the main *theoretical* problem facing neoclassical economics: how are we to be *assured* that any balance between demand and supply is also a market equilibrium?⁴ Specifically, since the market demand curve is logically the locus of price-quantity points where the quantity demanded is the one where every demander is maximizing his or her utility at the given price (and the market supply curve is similarly the locus of profit maximizing points) whenever demand equals supply, everyone is maximizing. And thus, whenever demand does equal supply, there is no individual who would wish to change his or her quantity demand or supplied (since, of course, everyone is maximizing!). Surprisingly, such a balance is *conceivable* even when the market demand curve is positively sloped (e.g., when the good is a Giffen good) and/or the supply curve is negatively sloped (e.g., when all suppliers engage in quantity-discount selling), as long as the price is exactly the one at which demand equals supply, there would be an apparent equilibrium (i.e., no change occurring).⁵ It is important here to recognize that in such a conceivable market the assumption of equilibrium (i.e., stable balance) would be false yet the assumption of universal maximization would still be true.

While market stability may seem to be only an arcane concern of theoretical model builders, I raise this because it matters for anyone today who might think the market solves all social problems. In particular, without an explicit theory which assures that every market balance is a stable equilibrium, all proponents of privatization – or anyone who believes markets always provide the necessary dynamics to assure universal maximization – would be assuming what they wish to prove. This is because any disruption of the market balance in the *conceivable* market I have identified would actually cause chaos: either the price jumps above the one yielding a balance and thus causes excess demand and a price rising indefinitely or it falls and causes a collapse in the price.

In this conceivable market (that is, one not ruled out by the logic of neoclassical theory), if any of the decision makers were followers of the Socratic-Popper theory of learning, Professor Marques would be right simply because anyone testing his or her maximizing choice would cause a disequilibrium and thus cause chaos. But, a market of this conceivable type could never be confused with a stable market. The reason is that such a decision maker would have long ago caused chaos. So, any market we see that is not collapsing or exploding can be a market where such Popperian decision makers can safely exist. Moreover, if the market really is a stable market, such behaviour (constantly testing to see if one's choice is a maximizing one) will not necessarily be disruptive. All that is required here is that there be no reason for why *all* decision makers would be deviating in the *same* direction (e.g., to test whether maximization is actually being

achieved, they all buy a little more of a good). If this is not the case, it is always possible for such deviations to cancel out. And, when all consumers are satisfied that they are indeed maximizing, there would be no more disruptions of the balance.

In his comment on my article (and implicitly on my 1982 book), Professor Marques expressed a concern for a possible contradiction in my article – namely, between the Socratic-Popper theory of learning that I might prescribe for theorists (and possibly consumers) and the *possibility* of an equilibrium (i.e., a stable balance). However, for the above reasons, I do not think there is necessarily a contradiction. Moreover, the issue is not what a decision maker *should* assume regarding his or her learning methodology, but what theorists *do* assume about any decision maker's presumed methodology whenever they think they are explaining the decision. That is, whenever they explain a consumer's choice of a particular bundle of goods as the one which maximizes his or her utility, it must also be recognized that maximization implies several necessary conditions. If utility is being maximized then we know, of course, that marginal utility is both zero and falling with the quantity consumed. However, as I have argued elsewhere [Boland 1981 and 2003], there is a commonly unrecognized third necessary condition: the consumer must have sufficient knowledge of his or her choice situation.

Critics of neoclassical economics seem to claim that there is a needed 'perfect' knowledge which involves more than this. But a neoclassical theorist might say that the only 'perfect' knowledge required is that which concerns the given market prices and this is a lot less demanding assumption than what the critics seem to think is needed for a consumer to make a decision. But, even if the critics are wrong about this, knowledge of just the going prices would not be enough. That is, the consumer must know his or her utility function (or indifference map) as well as the going prices. And in this case, the neoclassical theorist still must explain how the consumer knows what needs to be known. Moreover, in the absence of perfect knowledge, the neoclassical theorist must explain how the consumer *adjusts* to inevitable failed plans (plans which, as Clower and Hayek note, necessarily involve forming possibly false expectations about future prices and future demand or availability⁶). As Arrow [1959] shows, such an adjustment explanation is necessary for any stability analysis.

Let me conclude by summarizing my response to the challenging observations of Professor Marques. There is nothing to prevent a *theorist* from practicing the Socratic-Popper theory of learning and at the same time assuming that the *decision maker* is engaging in typical conventionalist methodology (which Leandro Gorno correctly observes is the methodology presumed in the Rational Expectations Hypothesis of the 1970s that I criticized in my 1982 book⁷). My concern is not with what neoclassical theorists should assume about the methodology that a decision maker must be employing in order to explain the observed consumer behaviour, but instead my concern is first that they must recognize that a decision maker must employ (explicitly or implicitly) some sort of theory of learning. And, once this is recognized, the only question then is whether the assumed theory of learning that *the theorist assumes the decision maker is assuming* is adequate to explain the observed behaviour. Adequacy here is a question of logic and the truth status of the explanation is a matter of empirical observation.

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Notes

- ¹ Professor Garcia obviously knows more about Plato than I do so I cannot say much in response. For my purposes, my references to Socrates are always about the Socrates that appears in Plato’s early dialogues which except for ‘Euthyphro’ are thought to be accurate recordings of what real Socrates said. The Socrates of the later dialogues – such as ‘The Republic’ where the cave is discussed – is a figment of Plato’s imagination [see Popper 1945/66, vol. 1, 194–7].
- ² There may be a confusion here between what I called a ‘scepticist’ consumer (one who forever doubts that he or she is maximizing with the current choice) and what would be called a Popperian consumer (one who might occasionally test to see if the maximizing choice is being made). The former is constantly making changes but the latter need not be.
- ³ However, I have subsequently discussed this in Chapter 7 of my recent book [Boland 2003].
- ⁴ In the end I discussed three separate ways the might be pursued but, as I showed in this book (see Chapters 9 through 11), all have problems.
- ⁵ See the discussion of neoclassical equilibrium methodology and the role of knowledge in Chapter 7 of my 1986 book.
- ⁶ For more on this, see Chapter 7 of Boland [1986], and Chapter 8 of [2003].
- ⁷ I agree with most of what Mr. Gorno says in his comment on my article but it must be recognized that few neoclassical economists today are willing to openly invoke the Rational Expectations Hypothesis. For this reason, in the second edition of my 1982 book I had to eliminate most of the discussion of this hypothesis in favour of a more general discussion of the common notions of imperfect knowledge employed by game theorists today (see Chapter 8 of Boland [2003]).
Note that currently the first edition of my 1982 book (as well as the 1986 and 1992 books) can be downloaded from my web page (www.sfu.ca/~boland).