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Mainstream economics and the Austrian school: toward reunification

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Abstract: In this paper, I compare the methodology of the Austrian school to two alternative methodologies from the economic mainstream: the ‘orthodox’ and revealed preference methodologies. I argue that Austrian school theorists should stop describing themselves as ‘extreme apriorists’ (or writing suggestively to that effect), and should start giving greater acknowledgement to the importance of empirical work within their research program. The motivation for this dialectical shift is threefold: the approach is more faithful to their actual practices, it better illustrates the underlying similarities between the mainstream and Austrian research paradigms, and it provides a philosophical foundation that is much more plausible in itself.

Keywords: philosophy of economics, Austrian school of economics, economic methodology, epistemology of economics, preferences, Ludwig von Mises

JEL Classification: B40, B41, B52, B53, B59

Among mainstream economists, there is a basic orthodoxy about the philosophical foundations of economic theory. They agree that idealizations about people's psychological preferences serve in some way to ground economic theory, that economic theory can be represented using mathematical functions, and that those functions can guide and be guided by empirical research into a large range of social phenomena. Theorists associated with the Austrian school reject this approach. They think that speaking of a person's ‘preferences’ is just summarizing that person's past observed behavior in a misleading way,

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that formal rationality is empty, and that econometric analysis is of little empirical value.

These differences have led Austrians and mainstream theorists to develop different bodies of theory, with different practical implications. But it is the philosophical disagreements between the two camps that explain why they currently have almost no productive engagement with each other, despite the Austrian school remaining one of the most institutionally established heterodox research programs. Neither side appears to have much interest in reconciliation, but, given the degree of overlap in both approach and ambition, this schism is due for a correction.

The schism has been driven in part by the fact that the standard comparison between those in the mainstream and those in the Austrian school is too abstract to be meaningful. So, I will argue that a serious comparative evaluation of research paradigms requires a discussion cast at a lower level of abstraction. From this perspective, the problem can be more clearly seen to revolve around 'extreme apriorism'. The disappearance of that idea, I argue, is a Pareto improvement for economics.

II. PRELIMINARIES

Before comparing the mainstream and Austrian paradigms, it is necessary to explain at least roughly what they *are* and how they differ. Some of the ways in which differences between these paradigms arise are visible to everyone. The two, for instance, differ in terms of their respective working practices. For mainstream theorists, economics is the enterprise of formal model-building. And sometimes, for the sake of simplicity or tractability, economists will need to include idealizations in their models. Robert Solow acknowledges as much:

[I]f you ask a mainstream economist a question about almost any aspect of economic life, the response will be: suppose we model that situation and see what happens [...]. The idea is to focus on one or two causal or conditioning factors, exclude everything else, and hope to understand how just these aspects of reality work and interact. There are thousands of examples; the point is that modern mainstream economics consists of little else but examples of this process (1997, 43).

Austrian theorists don't view the discipline of economics in this way. They do not see it in terms of building formal models, and they are usually unwilling to accept idealizations or the implications underwritten by them. The more fundamental disagreements between the two camps result from two different understandings of the basic categories in play.

One disagreement among mainstream and Austrian theorists, for instance, concerns the theory of preferences: what preferences *are*, how we can learn about them, and how we should theorize about them. Ludwig von Mises writes of preference rankings (referring to them as “scales of values”):

[O]ne must not forget that the scale of values or wants manifests itself only in the reality of action. These scales have no independent existence apart from the actual behavior of individuals. The only source from which our knowledge concerning these scales is derived is the observation of a man's actions. Every action is always in perfect agreement with the scale of values or wants because these scales are nothing but an instrument for the interpretation of a man's acting (1998, 95).

There are two claims in need of discussion here.¹ One claim is that the only empirical source of information concerning people's preferences is their choice behavior. I will call this claim *epistemological choice exclusivity*, or ECE. The other suggests that theorizing about preferences should not make formal reference to anything other than choices. This claim I will call *methodological choice exclusivity*, or MCE.

Resistance to these two ideas is not universal among mainstream economists. In fact, I believe that nearly everyone accepts ECE. However, a subset of mainstream theorists, *revealed preference* theorists, claims to also accept MCE. The ambition of the revealed preference approach is to justify the key theoretical insights of the mainstream approach

¹ Mises also makes a claim about the *metaphysical status* of preferences, in describing them as having no “independent existence” (1998, 95) apart from behavior, but I think this claim is better left aside here without deeper commentary. It is difficult to characterize the contemporary Austrian school's metaphysical view as either realist or anti-realist. Mäki describes it as “a combination of ontic subjectivism and ontological objectivism” (1990, 336). He argues that “[i]t is ontic subjectivism in that many fundamental objects of economic theory are claimed to be subjective in nature” but “ontological objectivism or realism in the sense that those subjective entities are maintained to have an objective existence, they exist independently of economists' theories of them” (336).

without reference or appeal to anything metaphysically exotic, such as a psychological disposition or mental state (Samuelson 1948). The revealed preference theorist's characteristic objection to the mainstream approach is, in short, grounded in behaviorism.

As I suggested at the beginning of the paper, the approach of the orthodox methodology is to take idealized preferences as primitive, and then to develop a theoretical framework from deductive reasoning about them. But, to whatever extent we have behaviorist sympathies, we may be unsettled by preferences *as* some sort of "primitive characteristic of the individual" (Mas-Colell, Whinston, and Green 1995). That assumption may amount to nothing more than a fancy way of begging the question against MCE. With this in mind, revealed preference theorists try to build the same basic framework from the other "direction"; they take idealized choice behavior as primitive, and then develop of model of rational preferences from deductive reasoning about that choice behavior.

There is a well-known criticism of the revealed preference approach, stemming from its commitment to MCE. As has been discussed by many (Camerer, Loewenstein, and Prelec 2005; Caplin and Schotter 2008; Hausman 2011), the correspondence between a person's preferences and her choices appears to fail in contexts in which she has false beliefs. Daniel Hausman (2011, 27-28) offers the following example, drawn from Shakespeare's *Romeo and Juliet*: since Romeo's alternatives were either to die or to elope with Juliet, and since the choice expressed by his behavior was to die, it appears as if revealed preference theorists are committed to the claim that Romeo preferred to die rather than elope with Juliet. But, on the intuitive interpretation of preference, Romeo had no such preference. Shakespeare intends the viewer of the play to understand that Romeo took his own life *because* he believed falsely that Juliet had died, and so, was unaware that eloping with her was a live option.

As Hausman (2011, 28-29) points out, there is a natural reply to this problem. If we abandon the intuitive understanding we have of preference in favor of simply stipulating that people's preferences are exhaustively defined by their actual choices, the problem disappears. According to that interpretation of preference, it follows from Romeo's choice that he *did* have a preference, so understood, to die rather than to elope with Juliet. Problem solved.

This apparent solution loses track of why we are interested in preferences in the first place. As Hausman argues (2011, 29), if our interest in preferences were merely terminological, this kind of interpretative shift might be justified. But most theorists—and certainly revealed preference theorists—are interested in preferences only insofar as they believe that learning about them is instrumental in learning how to make reliable predictions about people's behavior. This ambition requires facing up to the methodological problems that ECE presents. If we cannot come to know what governs people's behavior solely from information about their choices, then assimilating the concepts of preference and choice cannot carry us very far. To an extent, the dispute between the 'orthodox' and revealed preference methodologies just *is* a dispute about whether ECE entails MCE.

Like all behaviorists, revealed preference theorists avoid explicitly realist commitments at all costs. However, I do not think they manage to actually abandon those commitments. Ken Binmore, for instance, writes:

The theory of revealed preference therefore makes a virtue of assuming *nothing whatever* about the psychological causes of our choice behavior. [...] [It] succeeds in accommodating the infinite variety of the human race within a single theory simply by denying itself the luxury of speculating about what is going on inside someone's head. Instead, it pays attention only to what people do. It assumes that we already know what people choose in some situations, and uses this data to deduce what they will choose in other situations (2011, 8-9, emphasis in original).

This description of the revealed preference methodology carries substantive commitments, at least by implication. According to Binmore, the revealed preference methodology relies on an inference from what people choose in some particular contexts to what they will choose in other contexts. But what could possibly justify an inference of this sort, if not an implicit auxiliary assumption about people's (relatively stable) psychological dispositions? As Binmore argues, revealed preference commitments do not "mean that economists believe that our choice behavior isn't caused by what goes on in our heads" (8). His grievance with a realist interpretation of preferences is not that preferences actually lack such grounding. Rather, his grievance is that a methodology that helps itself to robust realism about whatever it wants, runs the risk of unsound practice. It is hard to disagree with that.

At bottom, revealed preference theorists don't accept MCE, at least not in the strongest sense. They simply can't. If people's preferences don't have any special metaphysical dependency on their choices, and if theorizing about people's preferences is limited to analysis of their actual choices, then theorizing about people's preferences will be impossible (at least in many contexts of interest). We are left with what might be called *predictive nihilism*.

Revealed preference theorists may think that the preceding discussion has presented a caricature of their view, for one reason or another. I hope this is not true, but my purpose here is not to enter the debate between orthodox and revealed preference theorists. I only want to point out a striking convergence in what Austrians and revealed preference theorists *say*. As Murray Rothbard—a student of Mises's—describes it, this convergence belies a more fundamental contrast to be drawn between them:

'Revealed preference'—preference revealed through choice—would have been an apt term for our concept. It has, however, been preempted by Samuelson for a seemingly similar but actually quite different concept of his own. The critical difference is this: Samuelson assumes the existence of an underlying preference scale that forms the basis of a man's actions and that remains constant in the course of his actions over time. Samuelson then uses complex mathematical procedures in an attempt to "map" the individual's preference scale on the basis of his numerous actions. *The prime error here is the assumption that the preference scale remains constant over time. There is no reason whatever for making any such assumption.* All we can say is that an action, at a specific point of time, reveals part of a man's preference scale at that time. There is no warrant for assuming that it remains constant from one point of time to another (2011, 294, emphasis added).

Here, we find the outline of a second sort of problem for revealed preference theorists (and mainstream theorists more generally). What Rothbard is denying is that there's enough stability in a person's values over time to license understanding her choices in terms of some stable set of psychological dispositions. This objection can be traced back to Mises (1998, 103), but it continues to enjoy attention from contemporary Austrians (Block and Barnett 2012), and also hasn't gone unnoticed by the mainstream (Grüne-Yanoff and Hansson 2009).

To be sure, it is very difficult to determine people's 'real' preferences. In some cases, this difficulty arises because of the

instability of people's preferences; what a person wants can change over time or across contexts. This is Rothbard's complaint. In other cases, people's preferences are difficult to assess because their factual *ignorance* opens a chasm between what they want and what they choose. This is the moral of the *Romeo and Juliet* case. There is a third sort of theoretical problem, also involving a chasm between people's dispositions and their behavior. This problem, however, results from people's *irrationality* rather than their ignorance.

Austrians take the concerns about stability and ignorance so seriously that their theory of preferences is structured around them. In contrast, Austrians seem to regard the irrationality problem as a pseudo-problem. Human action *can't* be irrational. Of rationality, Mises writes:

Human action is necessarily rational. [...] When applied to the ultimate ends of action, the terms rational and irrational are inappropriate and meaningless. The ultimate end of action is always the satisfaction of some desires of the acting man. Since nobody is in a position to substitute his own value judgments for those of the acting individual, it is vain to pass judgment on other people's aims and volitions. [...] When applied to the means chosen for the attainment of ends, the terms rational and irrational imply a judgment about the expediency and adequacy of the procedure employed. The critic approves or disapproves of the method from the point of view of whether or not it is best suited to attain the end in question. It is a fact that human reason is not infallible and that man very often errs in selecting and applying means. An action unsuited to the end sought falls short of expectation. It is contrary to purpose, but it is rational, i.e. the outcome of a reasonable—although faulty—deliberation and an attempt—although an ineffectual attempt—to attain a definite goal (1998, 19-20).

This way of understanding rationality is actually quite friendly to the mainstream research program. Mainstream theorists, like Austrians, aim for 'thin' theories of rationality, ones that cast the *content* of preferences as beyond the evaluative reach of economics. Most espouse value subjectivism at least to some extent, because they appear to respect a certain kind of neutrality.

It seems plausible at first to say that, since an act simply is intentional, purposeful behavior, there cannot be action without rationality. But this theory of rationality is so thin that it is practically invisible. It cannot explain why, by all appearances, some intentional

and purposeful actions are *much* more intentional and purposeful than others, while other actions appear to have been done without much intention or purpose at all. Mises considers these implications:

The assertion that there is irrational action is always rooted in an evaluation of a scale of values different from our own. Whoever says that irrationality plays a role in human action is merely saying that his fellow men behave in a way that he does not consider correct [...] When the expressions 'rational' and 'irrational' are applied to the means employed for the attainment of an end, such a usage has significance only from the standpoint of a definite technology. However, the use of means other than those prescribed as 'rational' by this technology can be accounted for in only two possible ways: either the 'rational' means were not known to the actor, or he did not employ them because he wished to attain still other ends—perhaps very foolish ones from the point of view of the observer (2010, 35).

In other words, there is only one way an agent may genuinely err in “selecting and applying means” (1998, 20): if the agent does her best but is incompetent. One can, in other words, commit a ‘competence’ error. As Mises points out, we are not generally inclined to describe such errors as failures of rationality. But the reason the objection seems to arise at all is that everyone knows the feeling of making a ‘performance’ error—a one-off error against a backdrop of general competence—and these errors are the canonical failures of rationality. For Austrians, this is never how we are to think about the world. So, rationality requires not just subjectivism about a person's *ends*, but subjectivism about her *means* as well.

Roderick Long motivates this ultrathin theory of rationality against the problem of intuitively irrational preferences, writing:

What [those who appear to have irrational preferences] are doing seems crazy only because we assume their preferences are like ours, and that their beliefs about how to satisfy those preferences are also like ours. But the very fact that they are behaving so oddly should give us reason to doubt those assumptions. Of course they might assure us verbally, ‘Yes, yes, our beliefs and preferences are just like yours’. But talk is cheap. They might be lying, or confused. For that matter, they might not even be speaking our language. After all, the best evidence we have that their word ‘money’ means the same thing as our word ‘money’ is what they do with what they call money. Meaning cannot be separated from use. Something is money only if

it plays the role in people's actions that constitutes its status as money (2004, 354-355).

This passage may appear to be just an affirmation of Mises's claim that assessments of irrationality always bottom out in differences of value, but Long's claim is stronger than that. Consider a person with an intransitive set of preferences. How do Austrians escape trouble with respect to their evaluation of that person's behavior? At first glance, they do this by denying that we could *know* the person's preferences are wrongheaded by her lights. After all, she can adopt whatever ends she likes, and it is no criticism of her ability to act purposefully that those ends are wrongheaded by *our* lights, even if they leave her bankrupt. Perhaps she sincerely desires to be Dutch-booked, or her tastes change quickly. One never knows for sure.

But the Austrian theory of rationality reaches further. Long also claims:

[Those who appear to have irrational preferences] are not a counterexample to praxeological principles, even if we assume that their coins really are money. And of course the latter assumption too may be questioned. [...] [N]othing counts as buying or selling unless it is in accord with the laws of economics. Hence we are in no danger of encountering irrational prices, for the same reason that we are in no danger of encountering a chess game that consists of tossing a ball back and forth across a net. *That* wouldn't be chess. *Those* wouldn't be prices (2004, 353-355, emphasis in original).

So the Austrian theory of rationality doesn't just deny that the person is being irrational. It denies that the person is making an economic transaction at all.

For Austrians, the theory of preference and the theory of rationality are not foundational. Both are implications of a broad skepticism about empirical social science (Mises 1998, 55-56), which is itself an implication of an anti-reductionist thesis about psychological phenomena called "methodological dualism". In Mises's words:

Methodological dualism refrains from any proposition concerning essences and metaphysical constructs. It merely takes into account the fact that we do not know how external events—physical, chemical, and physiological—affect human thoughts, ideas, and judgments of value. This ignorance splits the realm of knowledge into two separate fields, the realm of external events, commonly

called nature, and the realm of human thought and action (2007, 1-2).

One of Rothbard's criticisms of the revealed preference methodology is that it commits what he calls "the fallacy of psychologizing", which he understands to be "the treatment of preference scales as if they existed as separate entities apart from real action" (2011, 296). From one perspective, this criticism is surprising: revealed preference theorists see their mission precisely as that of dodging metaphysical commitments, yet this is the accusation of the Austrians. But, as I argued earlier, the revealed preference approach ultimately inherits the metaphysics of the orthodox methodology.

In contrast, Austrians are even more methodologically nihilistic about preferences than revealed preference theorists. There are disputes among Austrians about both how metaphysically loaded Mises understood methodological dualism to be, and about how loaded it is in its most plausible formulation.² But in any case, it is what has led them to adopt a different theoretical framework (Wiśniewski 2014). This alternative framework is at once both wider and narrower in ambition. On paper, the framework has two aspects. One is *praxeology*, which relates to the deductive implications of the idea that people act, with intention and purpose; the other is *thymology*, the study of the *causes* that underlie the acts.

The relative importance of the praxeology and thymology within the Austrian theoretical framework is the source of the most basic controversy within the Austrian school (Block 2012). On one side lies a sort of praxeological fundamentalism, which claims to uphold *extreme apriorism* (Rothbard 2011, 103-111) and which deemphasizes thymology. On the other end lie those who, although broadly sympathetic to the Austrian paradigm, favor a more thymologically-informed research agenda. The motivation for this sort of moderatism is the hope that the use of empirical data can somehow be reconciled with the rest of the Misesian theory (Lavoie and Storr 2011). Some Austrians regard the disagreement as internal to the Austrian school; others regard it as the essential difference between two fundamentally different schools of thought (Boettke 2012, xii).

²What underlies Mises's dualism is not settled among Austrians. See Kirzner (1982), Lewis (2010) and Hauwe (2011).

Nothing substantive turns on who ‘counts’ as what, but the schism has been the source of confusion. Without some measure of agreement about what the essential commitments of the Austrian school are, it is meaningless to have a critical discussion about what ‘the Austrian school’ has the conceptual resources to defend. The Austrian school *is* its essential commitments. So, productive engagement with it requires simultaneously exploring both positions in the context of a single argument. If we understand the Austrian school as methodologically defined by extreme apriorism, then the resultant theoretical framework will wind up being obviously inadequate for the purposes of serving as a free-standing research paradigm. So, we should understand the Austrian school as more open to moderatism. This turns out to have other benefits: it is more consistent with some of the most important contemporary ideas produced by the Austrian school, and also more consistent with the mainstream research program.

II. THE INADEQUACY OF EXTREME APRIORISM

Rothbard offers a characterization of extreme apriorism that emphasizes its connection to praxeology. Extreme apriorists hold characteristically that:

- (a) the fundamental axioms and premises of economics are absolutely true; (b) that the theorems and conclusions deduced by the laws of logic from these postulates are therefore absolutely true; (c) that there is consequently no need for empirical ‘testing’, either of the premises or the conclusions; and (d) that the deduced theorems could not be tested even if it were desirable (2011, 103-104).

At bottom, the disagreement between extreme apriorists and everyone else lies in their differing interpretations of the explanatory burden. Extreme apriorists claim that their critics are trying in vain to accomplish the impossible, while their critics argue that extreme apriorists cannot accomplish enough. So, while mainstream economists aspire for economic theory to apply to all interactions, even at the cost of making false assumptions about some of the features of those interactions, extreme apriorists are willing to accept praxeology’s incompleteness in exchange for its deductive soundness. It is on account of this soundness that they often describe praxeology as the more “realistic” approach to evaluating economic interactions (Mises 1998, 34).

But extreme apriorists don't get this soundness for free. The restrictions they place on the domain of discourse impose a high explanatory cost. By itself, praxeology lacks the resources to explain, predict, or even characterize many interactions of interest. To bring this out, I will examine two somewhat familiar *games*. First, one (Rubinstein and Salant 2008, 19; Hausman 2011, 29-31) which admits of representation within the standard formalisms of game theory:

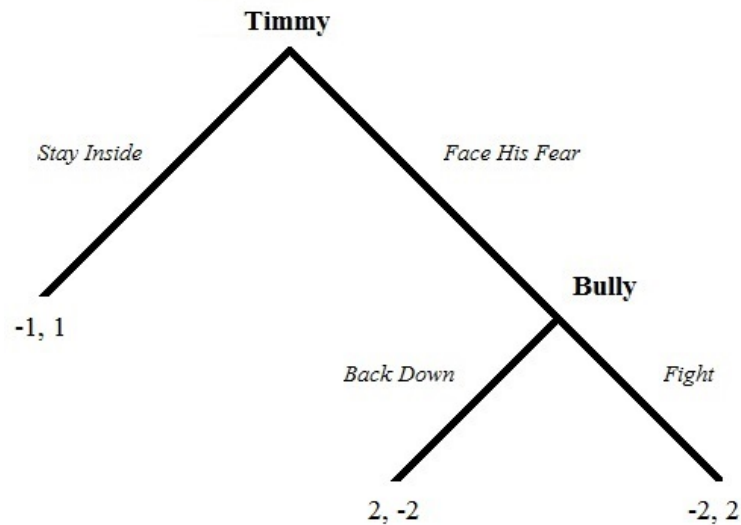


Figure 1: Recess

Recess: Timmy plays first, and has two choices, *Face His Fear* or *Stay Inside*. If he plays *Stay Inside*, he loses 1, Bully receives 1, and the game ends. If Timmy plays *Face His Fear*, Bully can respond either *Back Down* or *Fight*. If Bully plays *Back Down*, then Timmy receives 2 and Bully loses 2. If Bully plays *Fight*, Timmy loses 2 and Bully receives 2.

The payoffs in this game are defined so that Timmy prefers the outcome of (*Face His Fear*, *Back Down*) to (*Stay Inside*) and the outcome of (*Stay Inside*) to (*Face His Fear*, *Fight*), and so that Bully prefers the outcome of (*Face His Fear*, *Fight*) to (*Stay Inside*), and the outcome of (*Stay Inside*) to (*Face His Fear*, *Back Down*). Under the standard interpretation, Bully never has a chance to play at all. This is highly intuitive: if Bully were to get the opportunity to play (i.e., if Timmy were to play *Face His Fear*), Bully would play *Fight*. Knowing this, Timmy always plays *Stay Inside* and the game ends immediately.

To examine this topic using the apparatus of game theory may appear question-begging, but the underlying insights don't really depend on the formalisms. The formal game *Recess* is nothing more than an abstract way of representing a familiar set of collective social dynamics, and all the formalisms do is lend those implications a deductive flavor. The important thing to notice is that in order to even *define* the situation as one of a certain type, one must ascribe to Bully a preference between fighting and backing down, even though he's never given the opportunity to reveal this preference. So how can that preference be understood?

The orthodox methodology has a straightforward answer to this question. According to orthodox theorists, Bully has a real but unrevealed preference for fighting rather than backing down. Bully's preference ranking is just a formal representation of that preference. To explain these results, orthodox theorists need not to say anything evaluative about his dispositions. Naturally enough, revealed preference theorists have more trouble with the case. They cannot define Bully's preference in terms of his choice, because he doesn't act. But they can ease their trouble by relaxing their commitment to MCE, a strategy that does not automatically involve their taking on metaphysical commitments any more substantive than those Binmore accepts when he connects our choice behavior to "what goes on in our heads" (2011, 8).

Extreme apriorists cannot give this response, because Bully's unrevealed dispositions are invisible to praxeology. They cannot give any account of *Recess'* important counterfactuals, and so, cannot examine how things may be expected to change if Bully were absent from school one day, or if Timmy were a little stronger. To extreme apriorists, this sort of counterfactual is categorically off-limits. In fact, they cannot define the game described above at all, let alone offer a nontrivial explanation of its outcome. There is only an inexplicable and unpredictable surface phenomenon: each day, Timmy decides not to go outside for recess.

As far as extreme apriorists are concerned, there is no Bully. Perhaps Timmy is, as Long puts it, "lying, or confused" (2004, 354). If this is the only possibility from the perspective of extreme apriorism, we should hope those who actually monitor schoolchildren playing at recess aren't extreme apriorists!

Games can also be used to draw out problems with the Austrian theory of rationality. Chess is a useful instrument for this particular task, because the errors that chess players make cannot be attributed to their factual ignorance—chess is a *perfect information* game. So, errors in chess *must* be understood in terms of the other sort of mistake, in terms of irrationality. Given our present technology, the game tree of chess is too large for exhaustive computational analysis. However, we can consider particular (relatively simple) chess positions, for which exhaustive game trees (called ‘endgame tablebases’) actually *can* be given.

Consider the following position, one for which there exists a tablebase:

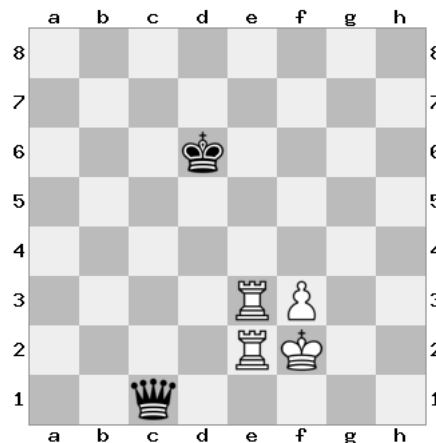


Figure 2: White to play

Suppose that White were to advance her only pawn forward one space (from the space whose coordinates are f3, to f4), or to slide her rook on e3 one space to the left, to d3. (The names of these moves are, respectively, ‘f4’ and ‘Rd3’). These are the moves that strongest human players would play, were they thinking as clearly as they could. However, both of these moves are actually suboptimal: White’s moving her king one square to the right (that is, playing ‘Kg2’) is the first move of a 99-move forced checkmate, and any other move leads to a forced draw. So, playing Kg2 is White’s best move irrespective of how Black responds; it is a *strictly dominant* strategy. Unfortunately, no heuristic

that could be interpreted and understood by human beings would recommend it.³

Mainstream, idealized theories of rationality have trouble with this sort of case. They diagnose playing the suboptimal f4 or Rd3 as ‘irrational’, which seems inappropriately stringent, given that knowing to play the optimal Kg2 requires near-omniscience about the implications of one’s play. But these idealized theories have the resources to offer some kind of indictment of the suboptimal moves (even if the indictment is too heavy-handed). Perhaps playing f4 or Rd3 is forgivable, but there’s an important sense in which anyone who plays f4 or Rd3 in that position could have done better.

Extreme apriorists, meanwhile, have the opposite problem. They cannot offer any indictments of those two moves (or indeed, of *any* moves). Against any move other than Kg2, they can only insist that that player was either incompetent, or abandoned perfect play in service of some other objective. But true incompetence is better exemplified by a move like ‘Rc3’, in which White slides her rook on e3 *two* spaces to the left, to c3. Here, the Rook is ready to be taken by Black’s queen without compensation. Anyone who plays this particular move is either incompetent or pretending to be.

This case provides a nice illustration of Long’s paraphrase of Kant: “Praxeology without thymology is empty; thymology without praxeology is blind” (2004, 359). The theories of rationality that perform best with respect to this sort of case are theories of *bounded rationality* (Simon 1957), which relax the requirements of rationality, for instance, by letting go of the requirement for logical omniscience. And indeed, in this case, a theory of boundedly rational chess play is able to offer the correct general diagnosis: playing Rc3 reflects incompetence, playing f4 or Rd3 reflects rationality at the boundary of human capacity, and playing Kg2 (probably) reflects access to the tablebase. Austrians would recognize such a theory as employing a synthesis of praxeological and thymological theorizing. So, the idea of bounded rationality is clearly useful in this case, but it is also inconsistent with extreme apriorism.

The bounded conception of rationality lends itself to much wider application than do the idealized (mainstream) or empty (extreme apriorist) conceptions of rationality. Consider a casino whose

³ See Zarsky (2011) for an explanation of the distinction between ‘interpretable’ and ‘non-interpretable’ algorithms.

management team wishes to structure their house rules in a profit-maximizing way. Given these motives, which set of rules the casino should adopt depends on certain kinds of facts that are, by their nature, invisible to praxeology. If, for instance, the management team discovers empirically that their customers tend to play dominated strategies to a greater extent than normal when the stakes reach a particular amount or the clock strikes a particular hour, these facts can be incorporated into a model whose agents are boundedly rational in some particular way, which will in turn inform the design of a better rule set. Whatever the details, however, extreme apriorism is inadequate here. At least for their purposes, the team will want, need, and be able to fruitfully infer much more about the players' ends and means than praxeology alone warrants.

Now, praxeology might seem inadequate in this sort of case only because the case implicitly contains substantive thymological features. Within this limited domain of action, it is safe for the casino to assume that most will adopt maximizing their winnings as an end, and will use this end to shape their choices of means to that end. But human action in general is much more teleologically open-ended. So, as those sympathetic to extreme apriorism may observe, no examples of the above sort (indeed, no examples at all) could *falsify* extreme apriorism.

However, the examples do illustrate the general nature of the conflict between extreme apriorists and everyone else. In general, the greater the significance of empirical information and mathematical modeling in generating the results we want, the more limited in scope extreme apriorism appears by comparison. The best strategy for extreme apriorists, then, is to argue that empirical information and the predictive models borne out of that information have little to contribute in the contexts of greatest interest. And on the context of greatest interest—competitive markets—Mises argues:

Within the frame of a market economy competition does not involve antagonism in the sense in which this term is applied to the hostile clash of incompatible interests. [...] Competitors aim at excellence and preeminence in accomplishments within a system of mutual cooperation. The function of competition is to assign to every member of a social system that position in which he can best serve the whole of society and its members. It is a method of selecting the most able man for each performance. Where there is social cooperation, there some variety of selection must be applied. Only where the assignment of various individuals to various tasks is

effected by the dictator's decisions alone and the individuals concerned do not aid the dictator by endeavors to represent their own virtues and abilities in the most favorable light, is there no competition (1998, 116-117).

On what basis would an extreme apriorist be entitled to make the kinds of claims Mises does about the nature of markets or of their participants? Without recourse to thymology, extreme apriorists aren't entitled to make claims about the concepts of cooperation or competitiveness. Praxeology lacks the resources to even characterize those concepts so as to be able to distinguish between them, to say nothing of explaining their general functions in large-scale social settings. Extreme apriorists also aren't entitled to claim that all participants 'aim at excellence' in any nontrivial way, because the Austrian theory of rationality prevents them from giving content to such judgments. Extreme apriorists are not entitled to make claims about the kinds of values, dispositions or attitudes actual participants have in actual markets. In short, they are committed to remaining silent about almost everything actual market participants do. Nothing about the nature of the aims of market participants or the nature of the large-scale social phenomena that emerge from the interaction of those aims follows from the axioms of praxeology. What extreme apriorism grounds is predictive nihilism.

But when praxeology is supplemented by thymology, the prospects for the Austrian school research program improve considerably. Allowing for use of thymology opens up a large buffet of useful tools for Austrian theorists, the most significant of which is game theory itself. Game theorists now work on many topics that are traditionally of interest to Austrians, such as spontaneous order (Axelrod 1984), but for the most part, Austrians haven't internalized game theory's insights, on account of their supposed allegiance to extreme apriorism. Any loss of working efficiency on these grounds is unnecessary.

Thymology also provides the resources to make sense of the chess position from fig. 2. Long explains:

[I]f I am praxeologically mighty but thymologically weak, I might be able to write hefty tomes on, say, monetary theory, and yet be woefully unable to recognise monetary exchanges in real life—in which case I would be helpless in trying to explain historical events like depressions and hyperinflations. It may thus appear that praxeology is useless in explaining anything unless it is

supplemented by thymology, which in turn seems to require some special knack of intuition whose presence or absence seems more a matter of luck than of scientific insight. [But] we don't count as possessing a concept unless we are—not perfectly reliable, but—reasonably reliable at applying it. It follows that the just-imagined scenario of praxeological proficiency combined with thymological ineptitude is not a real possibility; we don't count as possessing praxeological concepts except insofar as we are generally able to apply them accurately (2006, 42).

So, praxeology by itself lacks the resources to even recognize figure 2 as a chess position. One may, in other words, run across two persons seated across the table from each other, a chessboard with that position prepared, and praxeology is not only silent about what either person should do, it is silent about what the two are doing. It is only thymology that allow us to categorize this state of affairs as that of two people playing chess, and thus, only thymology that allows us to apply to that situation a theory of boundedly rational chess play.

I don't mean to suggest that all of the various challenges to the Austrian research program can be met by jettisoning a few incidental commitments, or by reinterpreting a few inopportune passages of Mises's writing.⁴ To accept the explanatory benefits of empiricism is at the same time to accept its problems.

How, for instance, does methodological dualism square with this newfound empirical optimism? If methodological dualism truly prevents using the methods of natural science to examine human action, how can thymology ever get off the ground? And what exactly are the standards of Long's "reasonable reliability" (2006), either in the particular case of chess or in a systematically more general sense? And however we understand it, how could we ever *know* that we have it? And even if we set aside those ontological and epistemological questions, how *in practice* could we distinguish, say, a chess game from some other kind of structured activity that is played by chess players at a chessboard but with different payoffs? The introduction of thymological considerations isn't in itself a panacea which lacks a need for further consideration. It immediately raises several standard philosophical issues. But this isn't

⁴ Scott Scheall (forthcoming) argues that the problems associated with extreme apriorism aren't somehow undercut or explained away by the fact that the scope of praxeology within economic theory is narrow.

always an indication of a methodological misstep. And here, I think it helps reorient Austrians in a more productive direction.

In fact, at a lofty enough level of abstraction, the practical difference between the moderate Austrian and mainstream research programs seems to run thin. Peter Boettke (2012) offers a set of three commitments he regards as characteristic of the contemporary Austrian school. This set includes (1) *methodological individualism*, the idea that social phenomena are explained in terms of how they result from the behavior of individuals; (2) the methodological priority of exchange over allocation, the idea that economics is primarily about “exchange behavior and the institutions within which exchanges take place” (xii) rather than mere allocative concerns; and (3) methodological dualism.

Each of these three commitments has defenders inside the mainstream, at least outside the Austrian school. Methodological individualism, for instance, is an axiomatic assumption of non-evolutionary game-theoretic analysis, and there has been consistent interest in the idea itself since Weber, including a renaissance brought about by Jon Elster (1982). The ‘institutional’ and ‘new institutional’ approaches to economic theory likewise suggest understanding the study of economics in terms of exchange rather than optimal allocation. Even methodological dualism, the least mainstream of the three commitments, continues to enjoy serious discussion (Chomsky and Smith 2000) as an idea. Overall, mainstream and Austrian theorists seem to share a similar broad ambition: to understand the nature and mechanics of intentional action, according to a methodology constructed from the subjective preferences of individuals.

It is important not to undercut the importance differences between the two camps. Both, for instance, claim to accept value subjectivism, but the sort of subjectivism Austrians accept (Lachmann 1986) is often much more comprehensive in nature. But the broad point stands apart from these issues. As more and more Austrians conduct empirical work,⁵ and as more and more mainstream theorists work on topics that were traditionally of interest to Austrians,⁶ the value of methodological reconciliation between the two camps comes into view.

⁵ See Stringham (2003), Chamlee-Wright and Storr (2010), and Selgin, Lastrapes, and White (2012).

⁶ For work on subjectivism, coordination, institutions, and entrepreneurship, see Foss (1999). For work on complexity, see Rosser (2012), and for work on spontaneous order, see Leeson (2014).

So, one might wonder, who are the extreme apriorists? Was Mises? He's not especially clear on this issue (1998, 858; 2010, 13-18), but I think the answer is no. He writes: "For lack of any better tool, we must take recourse to thymology if we want to anticipate other people's future attitudes and actions. Out of our general thymological experience [...] we try to form an opinion about their future conduct" (2007, 313).⁷ Mises does not regard this sort of 'opining' as the practicing of economics proper. But insofar such opining as a superior substitute for what mainstream economists just call 'economics', this is a terminological quibble.

I think that the idea of free-standing extreme apriorism is best understood as a relic of Mises's hyperbolic way of speaking. Even Rothbard, the author of a paper entitled *In defense of extreme apriorism*, describes the basic assumptions of praxeology as "derived from the experience of reality and [...] therefore in the broadest sense empirical" (2011, 65). And almost all contemporary Austrians have retreated even further. Boettke, for instance, writes:

The epistemological issue Mises sought to address with his insistence on apriorism, while more exotic in its philosophic treatment than his predecessors, boils down to the claim that theory comes prior to observation. We use theory to make sense of the economic world around us. The choice for the analyst is never theory or no theory, but instead always theory that has been articulated and defended or theory that remains inarticulate and hidden from critical examination. The analyst does not confront the 'data' pure and simple. [...] [I]t is a mistake to believe that these arguments either claimed that the entire field of economics was a priori or that economics is completely insulated from criticism of an empirical nature (2012, 161).

I think this passage is most charitably understood methodologically: as a way of distilling Mises's project into a more palatable Popperian insight about the appropriate relationship between theory and data in the social sciences (Iorio 2015). It is not that there is no such thing as economic data, or that such data cannot make contact with economic theory in virtue of the kind of thing it is, as Mises himself sometimes suggests. Rather, economic theory should be regarded as prior to data,

⁷ For more on this way of defending Mises, see Long (2004), Boettke and Leeson (2006), Long (2006), and Lavoie and Storr (2011).

and that this priority is worth understanding and taking seriously. I agree.

I do not want to be misread as admitting to having been attacking a triviality. Even if extreme apriorism doesn't endure in content—and so, no one is to *blame* for its persistence—it still endures in spirit. It should be eliminated. Once it is gone, the mainstream will be free to share its data with the Austrian school, and the Austrians will be free to share their theory in return.

III. CONCLUSION

I think there are important lessons to be learned here, both for those broadly sympathetic to the Austrian agenda and for those not.

As Solow noted, the interests of the Austrian school—to examine the nature and dynamics of human action—are far more comprehensive and metatheoretical than nearly all of what goes by 'economics' in mainstream circles. But why let methodological allegiances interfere with collaboration, 'interdisciplinary' or not? The Austrians are good at (and have always been good at) shedding light on what the economic mainstream could do better. Mainstream theorists would be well-served to develop a greater appreciation for coherent metatheory, the theoretical complexity of the topics of interest, and the limits of empiricism. And taking note of what the Austrians have done poorly—for instance, embracing the synthetic a priori and their own heterodox status to a self-destructive extent—is also instructive. It offers a good explanation of why certain philosophical ideas are better left aside.

For those with Austrian sympathies, the lesson is less cautionary. Austrian theorists should allow their theoretical frameworks to further cozy up to empiricism. It is time, in other words, for a 'thymological turn'. Contemporary Austrians are increasingly receptive to this movement, and any further developments along those lines will serve to move economics even further toward reunification between Austrian theorists and their critics. In the end, the most important lesson for everyone is that, upon careful examination, the two camps have less to disagree about than either one thinks.

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