TESTING ECONOMIC THEORY

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Two years ago, Bob Mulligan and I empirically tested whether the Bank of Amsterdam, a prototypical central bank, had caused a boom-bust cycle in the Amsterdam commodities markets in the 1780s owing to the bank's sudden initiation of low-fractional-re-serve banking (Guzelian & Mulligan 2015).[1] Widespread criticism came quickly after we presented our data findings at that year's Austrian Economic Research Conference. Walter Block representa-tively responded: «as an Austrian, I maintain you cannot «test» apodictic theories, you can only illustrate them».[2]

Non-Austrian, so-called «empirical» economists typically have no problem with data-driven, inductive research. But Austrians have always objected strenuously on ontological and epistemolog-ical grounds that such studies do not produce real knowledge (Mises 1998, 113-115; Mises 2007). Camps of economists are talking past each other in respective uses of the words «testing» and «eco-nomic theory». There is a vital distinction between «testing» (1) an economic proposition, praxeologically derived, and (2) the *rele-vance* of an economic proposition, praxeologically derived. The

1 A second paper provides additional support for the original paper's empirical conclusions, even after controlling for a concurrent Dutch war's effects. (Guzelian, Mulligan & Zelmanovitz 2018).

2 In the same breath, Dr. Block helpfully sent me his bibliography (on file) of around twenty empirical articles on Austrian Business Cycle Theory.

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former is nonsensical; the latter may be necessary to acquire eco-nomic theory and knowledge. Clearing up this confusion is this note's goal.

Rothbard (1951) represents praxeology as the indispensible method for gaining economic knowledge. Starting with a Aristote-lian/Misesian axiom «humans act» or a Hayekian axiom of «humans think», a voluminous collection of logico-deductive eco-nomic propositions («theorems») follows, including theorems as sophisticated and perhaps unintuitive as the one Mulligan and I examined: low-fractional-reserve banking causes economic cycles.

There is an ontological and epistemological analog between Austrian praxeology and mathematics. Much like praxeology, we «know» mathematics to be «true» because it is axiomatic and deductive. By starting with Peano Axioms, mathematicians are able by a long process of creative deduction, to establish the real number system, or that for the equation $a_n + b_n = c_n$, there are no integers a, b, c that satisfy the equation for any integer value of n greater than 2 (Fermat's Last Theorem).

But what do mathematicians mean when they then say they have mathematical knowledge, or that they have proven some-thing «true»? Is there an infinite set of rational numbers floating somewhere in the physical universe? Naturally no. Mathemati-cians mean that they have discovered an apodictic truth — some-thing unchangeably true without reference to physical reality because that truth is *a priori*.

When using mathematics in *physics*, there is then a necessary epistemological leap because physics involves measurements of and knowledge about the real universe. Thus, to move from the notional (mathematics) to the practical (physics) requires some bridge between apodictic mathematical propositions and theo-rems and humans' mathematical representations of physical real-ity. Philosopher John Foster in a well-regarded article explained this is done using a form of partial induction (Foster 1983).[3]

[3] One of the criticisms of string theory is that it is purely a theoretical mathemat-ical exercise that by the very conditions of the mathematics, admits no testable predic-tions about the universe. (Woit 2007).

Mises (1998, 17-18) similarly sought to bridge the divide between «two separate economic realms: the external world of physical, chemical, and physiological phenomena and the internal world of thought, feeling, valuation and purposeful action». He concluded that although someday science may locate the source of human action in a reductionist explanation of mental synapses and so forth,[4] currently economists can regard only one thing as knowl-edge: human action, which he called the «Ultimate Given» — very much like Peano Axioms in mathematics. In other words, Mises regarded human action, the interplay between «external facts… [that] produce in a human mind definite thoughts and volitions resulting in concrete acts», as the apodictic beginning of all eco-nomic inquiry (Mises 1998, 17).

Mises also praised praxeology whose «statements and proposi-tions are not derived from experience, [but] are like those of logic and mathematics, a priori» as the only method by which economic knowledge is gained. (1998, 30-32). Moreover, he specifically excluded from economics the use of empirical data, stating, «it is impossible to reform the sciences of human action according to the pattern of physics and the other natural sciences» (1998, 31). Mises contended the difference between physics and economics is that economics is non-repeating and non-experimental (apologies to Nobelist Vernon Smith), whereas physics is experimental and repeatable (*id.*.). To Austrians, praxeology can *«explain»* particular historical data, but conversely, data cannot inform the truth or fal-sity of apodictic economic theorems/propositions, nor lead to the discovery of additional economic theorems. To Austrians, only praxeology produces economic theory and knowledge.[5]

The foregoing reasoning is why Austrians reflexively dismiss «testing economic theory». After all, how can one «test» what is

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4 One Soviet-era Christian economist contended that such a scientific break-through will never happen because «Sophia» — the invisible Spirit of Knowledge and Wisdom — pervades both nature and mankind's internal life and is what alone ena-bles human identification of truth. Bulgakov (2000).

5 Logician Kurt Gödel proved that within any formal system of mathematics, there are propositions that can be neither proven nor disproven. (Goldstein 2006). One might interestingly examine whether, in the realm of praxeology, corresponding eco-nomic theorems exist.

apodictic? One cannot. And if all «economic theory» is deductively derived and apodictic, how could it be wrong unless incorrectly deduced? Again, one can agree. It could not be wrong otherwise. But the conclusion that there is no appropriate empirical test for economic theory rests entirely on the false Misesian belief that there is no economic analog to the mapping of mathematics onto physics. Economics *does* offer a way of epistemologically spanning praxeological theorems (the apodictic «mathematics» of econom-ics) to the real world of economic data (the empirical «physics» of economics). Economics does so by assessing ***relevance***.

What empirical studies of relevance yield *is* economic theory, but it is not «economic theory» as Austrians understand. By «eco-nomic theory», Austrians usually mean the collective body of apo-dictic, deduced theorems starting with Mises's «human action» axiom. But as lawyers like myself (and a fair number of non-Aus-trian economists) describe «economic theory», it is more like what Mises called *Verstehen* («Understanding») — integrated knowledge that is in part deductive and in part a derivation of relevance. In a very real sense, it is more robust and «truer» knowledge than «the-ory» that either induction or deduction alone yields.

I have written more extensively about relevance elsewhere (Guzelian 2016) and will not recreate that article herein. However, in the box below are four characteristics about relevance that are true and noteworthy. Each is discussed in turn.

I
ECONOMICALLY IMPORTANT PRINCIPLES OF RELEVANCE

1. The bridge between economic theorems, praxeologically derived, and practical economic theory is *relevance*[6]

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6 One anonymous reviewer commented about these four claims, and the first in particular, that:

> «This is not an empirical statement that can be falsified with empirical testing; it is a synthetic a priori statement. Thus, an empiricist must argue it is not knowledge about the real world. … The author wants to synthesize praxeology and empiri-cism, but this is impossible. Praxeology and empiricism are logically incompati-ble. One accepts and one rejects synthetic a priori knowledge. The author must

2. Scientific inquiries into relevance are usually best conducted quantitatively.
3. Relevance is not subjective, but not wholly objective, either. It is an amalgam of science and art.
4. Relevance has two aspects. One aspect is *specific* vs. *general* relevance. The other is *historical* vs. *future* relevance.

1. Relevance: The bridge between notional and historical reality. A correct judgment of relevance enables an economist to state whether a specific theorem (e.g. low-fractional-reserve

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choose between praxeology and empiricism; there is no middle way. If he chooses empiricism, he must abandon his claims about relevance because they are not empirically falsifiable. If he chooses praxeology, his discussion of relevance is unnecessary».

The peer reviewer, however, is incorrect that these four statements about rele-vance are purely synthetic, *a priori* statements. They are gained through a blend of empirical experience and a priori thought, Foster (1983). To wit: imagine a child who does not introspectively realize it is relevant to self-preservation to avoid touching a hot stove. A parent can give the child a warning to jar his thinking. (The parents' warning may stem from personal empirical experience or introspection about how heat transfers, or both).

A child may disregard the parents' warning and touch the stove. If he does so enough times, then it is through painful empirical experience, *coupled with* introspec-tive reasoning (that is: introspectively contemplating heat transfer), that a sense of the relevance of avoiding hot stoves to self-preservation is formed within him. If the child continues to think the proximity of his body to the heat of the stove is generally irrel-evant to his well-being, would anyone claim otherwise that he is apt to lose a finger? If instead he finds keeping his fingers from a hot stove is generally relevant to self-preservation, would we not say that he may survive to adulthood? And do we not say that *wisdom* — that blend of empirical experience and introspection — is well-formed more commonly in adults than in children because there is a «better» sense of relevance about proximity to dangerous heat? Therefore, through partial induction Foster (1983), it is permissible *both empirically and introspectively* for us to say avoiding hot stoves is relevant to self-preservation.

All that the peer reviewer has done, then, is to create a *recursive* objection by say-ing that I have only introspectively derived «the relevance of relevance». I have shown through this example that there exists at least one circumstance — fingers and hot stoves — where relevance bridges the gap between introspection and empiricism. I did not use pure introspection to reach this conclusion, but also partial empirical induction (I have touched a hot stove at least once). The question then is: does my same understanding about relevance apply across all problems of relevance, or only to this one (i.e. hot stoves and staying alive)? I contend that, *both through introspection and empirical testing*, I have come to the conclusion that relevance is my *generally relevant* bridge between the synthetic and the empirical (see Principle #4).

banking causes boom-bust cycles) is not just «true» in the apodic-tic Austrian sense, but also that it is relevant to (in lawyers' lan-guage, a «proximate cause» in) the real world. Austrians are fond of saying that praxeology is the only method for «explaining» his-torical results (and there is decidedly ambiguity as to what the word «explains» means). But explanation is not the same as *knowl-edge*, or as *Understanding* (*«Verstehen»*). And ultimately, economic theory is not built on «explanation», but on «relevance» and, con-sequently, «knowledge». As Guzelian & Mulligan (2015) notes,

> «Per Mises, consider the following: an Austrian and a non-Aus-trian each look at historical data of price fluctuations in countries that fractional-reserve banked stretching back to time immemo-rial. The Austrian may point to price fluctuations and emphasize the *primacy* of fractional-reserve banking's effects in each case and that imperfect goods-market arbitrage (also a true cause, per prax-eology) was often only a de *minimis* contributory force, if anything. Conversely, the non-Austrian may accentuate imperfect arbitrage and not even mention fractional-reserve banking, being ignorant of its existence or considering it of inconsequence (see e.g., Rogoff, Froot, & Kim 2001). And Mises himself contends there is no way to sort out which story is the better one. One can legitimately ques-tion (and, per Mises, such scrutiny supposedly cannot be objec-tively dismissed) the *primacy of relevance*. [An] Austrian [h] ypothesis, although praxeologically true, may have only tertiary or quaternary empirical relevance, and perhaps not even be wor-thy of mention, being only a comparatively weak force behind real historical business cycles, rather than the «root cause» (Hülsmann 2000).»

Examining relevance enables a deeper grasp of «truth» than Austrians regard, because it has tested whether a theorem has *pri-macy of effect* in the real world. It is only by an additional test of rel-evance, not praxeology alone, that one can assess correctly whether «the Bank of Amsterdam caused an Amsterdam commodity price boom- bust cycle in the 1780s by steeply lowering its fractional-re-serve» is true. Said in a folksy way, praxeology provides the eco-nomic streetlights that illuminate dark paths at night, but only by

also contemplating relevance may one learn whether there were precious coins under one or more particular streetlights.

One other thing to consider, left unresolved here, is that Austri-ans believe that all praxeologically derived theorems, however far removed in the chain of logico-deduction from the «Ultimate Given» of human action, are always and everywhere operative (Mises 1998, 36). But others caution that such a claim may exceed its metaphysical warrant. Nobelist Ronald «Coase believe[d] that long chains of deductive reasoning have a tendency to reduce the linkage between theory and reality, and that the import of deduc-tive analysis lies in the construction of short connecting chains of reasoning between inductive insights» (Medema 2012, 226). At present, it seems unclear whether the matter is objectively answer-able. Others should think about what consequences apodictic the-orems' «deductive distance» from human action has for achieving correct practical economic theory.

2. Relevance is often best described quantitatively. Causal relevance can be expressed qualitatively or quantitatively. One may say, «drinking vodka last evening caused my headache» (qualitative relevance). But quantitative analysis provides greater precision and accuracy about the relevance of the purported cause (vodka) to the effect (headache). Determining the amount of vodka consumed and the time elapsed between drinking and start of the headache, among other factors, can tighten confidence that the vodka was, in fact, a primarily relevant cause of the headache. In contrast, if the drinker took only the tiniest sip of vodka, then the possibility that an unrelated migraine started after drinking may become a much more relevant inquiry.

3. Relevance: Subjective, Objective…or both? Mises (1998) only slightly acknowledged the potential of relevance to move eco-nomics beyond apodictic praxeology towards practical epistemic knowledge (*Verstehen*) by integrating theory and reality. However, he dismissed relevance as economics' analog to physics, appar-ently because he believed it impossible to get universal scientific consensus about how to judge relevance:

> «[T]here necessarily enters into [economic] understanding [*Verste-hen*] an element of subjectivity. … Two historians … may fully agree in establishing that the factors *a*, *b*, and *c* worked together in producing the effect *P*; nonetheless they can widely disagree with regard to the relevance of the respective contributions of *a*, *b*, and *c* to the final outcome. … [T]hese are not judgments of value, they do not express preferences of the historian. They are judgments of relevance. … [A]s far as historians disagree with regard to judg-ments of relevance it is impossible to find a solution which [sic] all sane men must accept» (Mises 1998, 57-58).

Mises trod on too thin a metaphysical ice. First, if the criterion of relevance is that all sane economists must accept a judgment about relevance (and the resulting economic theory) for the theory to be true, then Mises fell into the same «consensus science» trap that often plagues the physical sciences. Among many, Crichton (2003) forcefully denounces the substitution of consensus for sci-ence:

> «I want to … talk about this notion of consensus, and the rise of what has been called consensus science. I regard consensus sci-ence as an extremely pernicious development that ought to be stopped cold in its tracks. Historically, the claim of consensus has been the first refuge of scoundrels; it is a way to avoid debate by claiming that the matter is already settled. Whenever you hear the consensus of scientists agrees on something or other, reach for your wallet, because you're being had. Let's be clear: the work of science has nothing whatever to do with consensus. Consensus is the business of politics. Science, on the contrary, requires only one investigator who happens to be right, which means that he or she has results that are verifiable by reference to the real world. … There is no such thing as consensus science. If it's consensus, it isn't science. If it's science, it isn't consensus. Period».

But what if by Mises' oblique hat tip to the futility of judging relevance, he merely meant that there is no consistency among researchers as to the appropriate *method* by which to test relevance in the real world, particularly because unlike the physical sciences, history is non-repeating? That is, that relevance is entirely *subjec-tive* because the selection of a methodology for judging relevance

is arbitrary. If relevance is entirely a subjective inquiry, the chasm between praxeology and practical economic theory is not scientif-ically spanned, or so this logic goes.

Of course, there are frequent methodological debates in eco-nomics. Indeed, Guzelian & Mulligan (2015) suffered multiple rejections under peer review because numerous reviewers felt that our use of fractal Hurst exponents to calculate Amsterdam com-modity price volatility in place of traditional ANOVA analysis was «unfamiliar», or, to the epistemologically more daring reviewers, «wrong». But it does not automatically follow from the facts that there are methodological debates and camps of economists that there are not objective, right answers to relevance. Guzelian (2016) sets out a compelling argument that relevance can attain a qua-si-objective status. Also, one should not overlook the good work of causality theorist Mario Bunge. Testing relevance is part science, part art. «The best grasp of reality is not obtained by respecting fact and avoiding fiction but by vexing fact and controlling fiction» (Bunge 1979, 129).

4. Specific relevance/General relevance; Historical/Future Relevance.

The relevance of an apodictic theorem (e.g. low-fractional-re-serve banking causes economic cycles) is always, in the first instance, for a *specific* historical case with *specific* historical data. We chose the Bank of Amsterdam and Amsterdam commodity prices in the 1780s. Conceivably, we might have instead picked the Bank of Venice in 1590, the Hamburg Reichsbank in the 1870s, et cetera.

If enough singular cases of relevance exist, one might start to infer that a praxeological theorem has *general* relevance in the real world. For instance, Hazlitt (1965) states the praxeological proposi-tion that budget deficits can indirectly cause inflation. Bernholz (2015) identified 29 worldwide cases of hyperinflation since Roman times, and in sifting through the data, found that at least 25 of those times were preceded by substantial government budget defi-cits. Can we say that because the apodictic proposition held in 25 of all 29 specific hyperinflations, it is valid *general* economic the-ory? Or would it have to have occurred unswervingly all 29 times?

Would 21 times be sufficient? Making the call that an economic proposition is generally relevant is a somewhat arbitrary practice.

Testing relevance is usually an historical process. It relies on past data to make an assessment about whether an economic phe-nomenon has occurred. Sometimes relevance can be predictive and future-oriented. But we will not know whether an economic theory, if generally relevant historically, will hold in the next future circumstance until it occurs, although we may *perhaps* justifiably presume it will. (Foster 1983).

CONCLUSION

To our critics: Guzelian & Mulligan (2015; 2018) appropriately «tested economic theory». We did not test praxeological theorems. We tested one theorem's specific, historical relevance using the most correct empirical method available and thereby produced economic theory. Hopefully this note by a lawyer who knows more about relevance than economics does not further confuse econo-mists about what they are doing.

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