The Mengers *versus* Mises on Matters Methodological

Scott Scheall

Preliminary draft; do not cite or quote. Some citations and bibliography missing, to be completed

Introduction

The present paper is the latest addition to an ongoing research project concerning the methodological beliefs and practices of members of the Austrian School of economics. The methodology of the Austrian School is frequently associated, if not identified, with the extreme apriorism defended by Ludwig von Mises.[1] This move is often made to serve the purposes of

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[1] Murray Rothbard (1976) describes apriorism as "*The* Methodology of Austrian Economists" (emphasis added). Robert Nozick (1976, 361) explicitly associates apriorism with "[o]ne branch of Austrian theorists (Mises, Rothbard)," but the title of his paper implicitly identifies it with "Austrian Methodology." Mark Blaug (1980, 91-92) associates Misesian apriorism with "Modern Austrians…a small group of latter-day Austrian economists…numbering among its adherents such names as Murray Rothbard, Israel Kirzner, and Ludwig Lachmann[.]" Terence Hutchison (1981, 214) also seems to associate Misesian apriorism with "modern Austrians," with the exception of "Hayek II" and possibly Kirzner (against Hutchison's posit of a Misesian "Hayek I," see Scheall [2015], and against Hutchison's posit of a Popperian "Hayek II," see Caldwell [1992]). Bruce Caldwell (1982) identifies Misesian apriorism with "Austrian methodology." David Gordon (1994) argues that "The Philosophical Origins of Austrian Economics" culminate in an Aristotelian version of Misesian apriorism. Hans-Herman Hoppe (1995) identifies Misesian apriorism with "*The* Austrian Method" (emphasis added). Peter Leeson and Peter Boettke (2006, 247-248) argue that "to the Austrian economists who trained with Mises during his New York University period (1944-1969), like Murray Rothbard,

those many critics (and some defenders) who wish to paint the entire Austrian School as infected (or endowed) with extremism down to its roots. Any attribution of Misesian apriorism to the wider Austrian School, whether it comes from their intellectual opponents or their defenders, should be understood against this background and recognized, and probably decried, as the dialectical gambit it usually is. Such claims are often leveled at Austrians by their adversaries, who consider the accusation of apriorism to be an insult. If Austrians are apriorists, then, these intellectual opponents believe, the scientific status of Austrian economics is undermined, and Austrians are revealed to be little better than dogmatic religionists about economic and related (i.e., political) phenomena. Unsurprisingly, this is a popular argumentative tactic of those who wish to challenge the liberal politics typically associated with Austrian economics.

What is more surprising, given the latter tactic, is to find a few Austrians, whatever their actual methodological practice, explicitly defending something like Misesian apriorism (see, e.g.,

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adherence to methodological *apriorism* is *the* distinguishing characteristic of the Austrian school, and alternative methodological positions are interpreted as undermining Mises' strong claim about the nature of economic reasoning" (emphasis in the original). More recently, Gabriel Zanotti and Nicolás Cachanosky (2015) identify "Austrian epistemology" with Fritz Machlup's (1955) failed attempt (see Scheall [2017b]) to align Mises' epistemology with a moderate kind of proto-Lakatosianism. The subtitle of Alexander Linsbichler's *Was Ludwig von Mises a Conventionalist? A New Analysis of the Epistemology of the Austrian School of Economics* implicitly identifies Mises' epistemology (whatever it was) with that of the entire Austrian School.

Rothbard [1957], Hoppe [1995], and Maclean [1997]). How can a methodological position that their opponents consider a cudgel with which to beat them be embraced by some Austrians as a shield against said beating? A short and unsatisfactory, if basically correct, answer to this question is that some of these Austrians are simply confused about what methodological apriorism does and does not allow them to accomplish—they think it does work for them that it manifestly does not do. For instance, I agree with Alexander Linsbichler (2017) that Mises probably adopted apriorism as a way around the problem of induction. But, if Mises adopted apriorism to protect the laws of economic science from the problem of induction, he grasped a poisoned chalice. Apriorism does not solve the problem of induction as much as wish it away. Apriorism is not a way to establish the veracity of economic laws on the basis of the evidence but a rejection of the value of empirical evidence, which thereby leaves the source of our knowledge of economic laws utterly opaque. Apriorism denies sensory experience as a source of our knowledge of economic laws and replaces it with a vague promise that this knowledge is already there, somehow, in our heads. If you ask, "How did it get there?", the apriorist can only invoke some combination of mysticism and magic: either some higher power put it there or the human mind possesses some mysterious *sui generis* faculty for the discovery of truths about the world of experience that does not require (or, apparently, benefit from) access to that world.

However, the frequent association of Austrian method with Mises' apriorism neglects or obfuscates several important facts about the methodological history of the Austrian School that complicate this history (and make it far more interesting). In particular, there is the fact that many of Mises' most prominent followers explicitly rejected his apriorism (CITATIONS); more

relevant to my main concern in the present paper, there is the fact that the Austrian School had flourished for well over half a century before Mises made his methodological pronouncements.

The questions motivating this research project are simply stated: To what extent have Austrian School economists accepted the methodological apriorism explicated and defended by Mises? Was Mises' apriorism unique to him or has it been followed by some, most, or all Austrian economists? If the correct answer to the latter question is "some" or "most," then which Austrian economists have followed Mises' apriorism? The most influential and internationally recognized Austrian economists, or comparatively less effectual and less heralded Austrian economists?

My approach to these research questions has, in effect, been a process of elimination. I have argued that Misesian apriorism cannot be attributed, at least, to F. A. Hayek, the most famous of all Austrian economists, not excepting Mises (CITATIONS).[2] I have also argued that, though a distinctive apriorism only partially related to the Misesian version can be attributed to Friedrich von Wieser, his fellow second-generation Austrian (and brother-in-law) Eugen Böhm-Bawerk rejected methodological apriorism out of hand (CITATION). In the present paper, I turn my research questions to the first generation, to the founder, of Austrian economics, Carl Menger. Would Menger have recognized Mises' apriorism as the method he described and

[2] The comparative fame and respect that Hayek, relative to Mises, is broadly accorded outside the Austrian School is the reverse of their respective standings within the School, especially in its modern American variant, where Mises is treated as something of a demi-god and Hayek as decidedly mortal.

defended in his own methodological writings, in the infamous *Methodenstreit* ("dispute about methods") with economists of the German Historical School? Or can Menger, too, be eliminated from the dwindling ranks of Austrian adherents of Misesian apriorism? The first purpose of the present paper is to argue that Menger would have also rejected Mises' methodological apriorism, like Hayek, Böhm-Bawerk, and (in his own idiosyncratic way) Wieser later did.

Another question that motivates the project is also simply stated: To the extent that Austrian School economists have *not* followed Mises' apriorism, what method(s) have they adopted? The second purpose of the paper is to establish the broad consistency of Menger's methodology with the *tolerant* methodological attitude that his son, the influential mathematician, logician, and philosopher of science, Karl Menger, developed and defended in the disputes over the foundations of mathematics of the 1920s and 1930s, and in his contemporaneous engagements with the Vienna Circle of Logical Positivism. The younger Menger, who edited the second edition of the *Grundsätze der Volkswirtschaftslehre*, his father's famous theoretical treatise, was intimately familiar with and influenced by his father's ideas. He was also active in the community of economists in Vienna during the 1920s and 1930s, and directly engaged Mises over methodological matters on a few occasions prior to emigrating to the United States in 1937. Karl Menger's methodology can thus serve in a limited way as something of an imperfect proxy for his father's.

In the concluding section, I consider the relationship between the Mengers' tolerance, Mises' less tolerant methodological attitude, and the classical liberalism defended by most all Austrian School economists. I suggest that any Austrian economists inclined to defend both

tolerant classical liberalism and relatively intolerant Misesian apriorism are stuck on the horns of a dilemma.

The Distinctiveness and Extremeness of Mises' Apriorism in the History of Economic Thought

For the purposes of economics (and for science, more generally), whether a particular proposition qualifies as *a priori* is primarily a question of whether it can be exempted from empirical testing. A methodological apriorist exempts one or more of the propositions of economic theory from empirical testing. On this definition, there have been many apriorists in the history of economics. Indeed, on this definition, probably most economists who have considered the matter have been apriorists. Relatively few economists have insisted on the need to empirically test *every* proposition of economic theory (e.g., Hutchison, *Basic Postulates*). Especially prominent names like Nassau Senior, John Stuart Mill, J. E. Cairnes, John Neville Keynes, and Lionel Robbins, have offered arguments for versions of methodological apriorism.

In my essay, "What is Extreme about Mises' Extreme Apriorism?" I survey the arguments offered by famous apriorists in the history of economic thought to uncover what makes Mises' apriorism unique and, as the title of the essay suggests, "extreme," as compared to other versions of apriorism. Mises has been repeatedly accused by various economists and methodologists of defending a uniquely extreme version of apriorism (Blaug, Samuelson, Caldwell, others). However, several Austrians have defended Mises' apriorism as relatively moderate (Machlup, Boettke, Zanotti and Cachanosky, others). In the aforementioned paper, I argue that critics and defenders of Mises' apriorism have frequently argued at cross-purposes.

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The grounds upon which Mises' apriorism has typically been criticized as extreme are distinct from the reasons given by Mises' defenders for the moderateness of his apriorism. Mises' apriorism *is* extreme in some respects, but it is *also* moderate in others.[3]

I argue in "What is Extreme about Mises' Extreme Apriorism?" that there are (at least) three criteria for distinguishing and grading the extremeness of different versions of apriorism.

First, there is the question of the *extent* of an economist's apriorism. How many propositions does the economist propose to exempt from testing? Along this dimension, naturally, the more extensive the set of propositions an economist proposes to exempt from testing, the more extreme their apriorism. Mises insisted on the *a priori* nature of only one proposition, his so-called "action axiom," according to which *human action is purposeful behavior* (Mises [1949] 1998, p. 11). This is the only *a priori* element of praxeology, Mises' science of human action, "the best-developed part of" which is economics (Mises [1949] 1998, p. 237).

[3] I lay the blame for this confusion primarily at the feet of Mises' Austrian defenders, who have tended to ignore the substance of his critics' objections. Critics of Mises' apriorism have offered relatively clear objections that have not been adequately addressed by his defenders, who have tended to declare victory on grounds irrelevant to these objections (see "Mises' Extreme Apriorism" for details).

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> [T]he end of science is to know reality. It is not mental gymnastics or a logical pastime. Therefore praxeology restricts its inquiries to the study of acting under those conditions and presuppositions which are given in reality (Mises [1949] 1998, p. 65).

Praxeology / economics combines the *a priori* action axiom with empirical assumptions and deduces economic "reality" from these premises. As compared to other apriorists in the history of economics, say, Nassau Senior, whose apriorism consisted of "a very few" – more exactly, four – "general propositions" (see Blaug, 1980, p. 59; quoted in Bowley, 1949, pp. 46–48), Mises' apriorism was not very extensive and, thus, not very extreme along the extent dimension.

There is, however, beside the extent of an economist's apriorism, the *epistemological story* that they tell about the propositions they exempt from testing. Such stories typically consist of at least two sub-stories.

First, there are the economist's *reasons* for exempting the relevant propositions. This part of the story typically involves an account of the *nature* or, what might be the same thing, of the *sources* of the relevant propositions such that it is safe and reasonable to set them aside from empirical testing. Apriorists have offered radically different reasons for exempting particular propositions from testing against experience. Some propositions may be exempted from testing as a matter of convention or for some other reason concerning simple methodological choice. For example, there are propositions that are only tentative hypotheses, exempted from testing for the moment in order for inquiry to proceed and to be tested later, and presumably, refined, in order to further extend the relevant inquiry; and there are propositions that are assumed purely for the sake of analysis, in order to discover what can be deduced from them either alone or in

conjunction with other propositions, whose scientific purpose is not to explain empirical reality. Some propositions may be exempted from testing because they state well-accepted facts of everyday experience that no one seriously questions in the current state of knowledge.

Mises offered an epistemological account of the allegedly *a priori* nature of the action axiom that was far more extreme than these other accounts. Testing the action axiom is superfluous, according to Mises, because *pure reason* is sufficient to know its truth. We know the action axiom is true just by thinking about it. More exactly, we know the axiom is true merely in virtue of being human. We do not need experience to know, neither can experience falsify our knowledge, that human action is purposeful behavior. "In all its branches this science [of human action, i.e. praxeology] is *a priori*, not empirical."

> Like logic and mathematics, it is not derived from experience; it is prior to experience. It is, as it were, the logic of action and deed ... in the last analysis, logic and the universally valid science of human action are one and the same ... [W]hat we know about our action under given conditions is derived not from experience, but from reason. What we know about the fundamental categories of 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. Nor could experience ever lead anyone to the knowledge of these things if he did not comprehend them from within himself. ([1933] 2003, pp. 13–14)

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The notion that pure, empirically unadulterated, reason is the source of the assumptions of economic theory is what I call the "Reason without Experience" thesis and no other apriorist in the history of economic thought has defended a position more extreme with regard to it than Mises' did. According to Mises, the action axiom is not a convention or a hypothesis, tentative or otherwise; it is not a mere assumption for the sake of analysis or simply a well-accepted fact of everyday experience. It is an *axiom* in the fullest sense of the word, delivered by reason to all thinking persons.

The second part of the epistemological story that apriorist economists typically tell about the propositions they propose to exempt from testing concerns the degree of *certainty* or *epistemic reliability* that is imparted to these propositions in virtue of their nature / sources. An apriorist who exempts some proposition from testing for conventional or other momentary methodological reasons, or because it states an uncontroversial fact or assumption about everyday life, is probably not inclined to attribute any special certainty or reliability to the proposition. Indeed, the proposition may be, and may be known to be, false, yet might be retained for sensible methodological reasons, e.g., to facilitate the relevant analysis.

On the other hand, however, many economists have defended the "Greater Certainty" thesis, i.e., the claim that social scientists are in a privileged epistemic position compared to natural scientists, because social scientists are (in some sense, never clearly defined) "closer" to the explanans of their scientific explanations than are natural scientists. Some elements of the explanans of social-scientific explanations are products of our own thinking, whereas natural scientists must discover their explanans through sensory observation, experimentation, etc. Those who accept the Greater Certainty thesis insist that relative proximity to the source of

knowledge (somehow, never clearly explained) ensures the greater certainty of propositions derived from the relatively nearby source.

The first thing to note about the Greater Certainty thesis is that it is a non-sequitur. Those who defend the thesis tend to argue from the premise that rational reflection is an easier and more convenient method than observation, experimentation, etc. to the conclusion that, for this reason, knowledge delivered via introspection is more secure than empirical knowledge. However, it does not follow from the fact (if it is a fact) that knowledge of the action axiom is "within" us, as Mises ([1933] 2003, p. 137) put it, that this knowledge is any more certain than knowledge acquired from without. It may be relatively easy and convenient to introspect, but this does not *per se* make knowledge acquired through introspection more epistemically secure. As I have noted previously, this is a bit like thinking that pyrite must be more valuable because it is easier to find than gold. Defenders of the Greater Certainty thesis need, but have not provided, an argument that pure intuition, introspection, etc., is at least relatively less error-prone than observation, if not infallible.

More important for our purposes than the groundlessness of the thesis is that, as with the Reason without Experience thesis, Mises defended a more extreme version of the Greater Certainty thesis than any other apriorist economist. Indeed, those apriorists who exempt various propositions from testing for conventional or other methodological reasons, or who treat such propositions as tentative hypotheses or uncontroversial facts of everyday experience, tend either to remain agnostic or to positively deny that social scientists occupy a special epistemic position as compared to natural scientists. On the other hand, Mises argued that knowledge of the action

axiom was maximally secure, "apodictically certain." Theorems deduced (validly) from the action axiom

> are perfectly certain and incontestable, like the correct mathematical theorems. They refer, moreover with the full rigidity of their apodictic certainty and incontestability to the reality of action as it appears in life and history. Praxeology conveys exact and precise knowledge of real things. ([1949] 1998, pp. 39–40)

In sum, the uniqueness and extremeness of Mises' methodology consists not in the great extent of what he took to be *a priori*, but in his epistemological justification of the allegedly *a priori* elements of economic theory ("What is Extreme…"). According to Mises, only the action axiom need not be subjected to empirical testing. The extent of Mises' apriorism is not unique or extreme among economic theorists. However, Mises' epistemological justification for exempting the action axiom, his defense of both the Reason without Experience and Greater Certainty theses, distinguishes his apriorism from other versions in the history of economics, and places it at the extreme endpoint of the aprioristic methodological spectrum.

Carl Menger on the Reason without Experience and Greater Certainty Theses

If we can determine Carl Menger's attitude toward these propositions, we can get some grasp on how he would have appraised Mises' unique and extreme methodological apriorism. It is important to again emphasize, as I have already suggested, that both theses have long lineages in the history of economics. Indeed, the Reason without Experience thesis expresses an attitude

about the possibility of deriving knowledge, however secure or certain, from reason alone that is of general epistemological significance and by no means uniquely historically related to the social sciences. If we can uncover Menger's fundamental epistemological orientation regarding the capacity or lack thereof of pure reason to deliver knowledge of propositions concerning the world of experience, propositions like Mises' action axiom, we can infer his attitude toward the Reason without Experience thesis. On the other hand, like several other economists, Menger explicitly addressed the Greater Certainty thesis, so his attitude toward it can be directly decided. In any case, the fact that Menger antedated Mises and never knew the latter's extreme apriorism does not seriously hinder us from determining the attitude he likely would have adopted toward it.

Carl Menger versus *the Reason without Experience Thesis*[4]

[4] I do not intend to engage in a comprehensive analysis of Carl Menger's methodological thought or practice. My concerns are narrower in scope. There are a number of unresolved issues in Mengerian historiography that I will not touch upon (and some, frankly, that I would not touch with a ten-foot pole). For example, except inasmuch as it is obliquely relevant to the question about Menger's possible pluralism, I will not venture an opinion concerning the fraught question of the extent to which Menger was an Aristotelian essentialist. Neither will I evaluate either the significance of the *Methodenstreit* for the history of economic thought or the correctness of Menger's methodological arguments. I will also have relatively little to say about the methodological arguments of Menger's adversaries in the *Methodenstreit*, the German Historical

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In his *Reminiscences of the Vienna Circle and the Mathematical Colloquium*, Karl Menger (1994, pp. 34-35) quoted from his father's 1867 notes on a book that he had intended to write, but had never completed. The quotation, according to the younger Menger, is emblematic of a "positivistic undercurrent," "the antimetaphysical spirit of young men," that was widespread in Vienna long before the emergence several decades later of the famously anti-metaphysical Logical Positivists. According to Carl Menger's notes for the never-to-be-completed book (which he had intended to give the catchy title *Critique of Metaphysics and of the so-called Pure Reason from the Empiristic Point of View*),

> All so-called ideas *a priori* and knowledge from pure reason must be presented as empirical statements or as errors, i.e., false experiences or empty compilations of words [*leere Wortzusammenstellungen*].
>
> There is no truth of a metaphysical nature and thus lying beyond the limits of experience. Beyond there are only rational calculi [*Verstandeskalküle*] and fantasies.
>
> There is no metaphysics. There is only a theory of the correct observation and consideration of things that is free of prejudices, be they accidental or created by education.

School. I am sympathetic to the notion, most famously advanced by Joseph Schumpeter (CITATION), that both sides in the battle over methods wrote confusedly, ungenerously, and largely at cross purposes. Whatever may be the case in this regard, it is far from my purpose in the present paper to declare a victor in the *Methodenstreit*.

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> There are neither *aeternae veritates* [eternal truths] as the dogmatists claimed nor forms of perception and thought lying in us by which Kant replaced the *aeternae veritates*.
>
> Kant rejects metaphysics and replaces it by the critique of pure reason. I say, there is no pure reason.
>
> There is no riddle of the world that ought to be solved. There is only incorrect consideration of the world. This objection is directed against the essence of modern philosophy and against the form of empiricism.
>
> Mere materialism has equally pernicious consequences for science as mere idealism. Just as the latter confuses the world, so the former makes it shallow.

At least in 1867 – which is to say, early in his career, "just before developing his interest in economics" (K. Menger 1994, 34) (*Grundsätze der Volkswirtschaftslehre* was published in 1871) and long before he seriously engaged with the methodological problems of the science in the early 1880s – Carl Menger was rather vehemently opposed to the notion that pure reason could deliver knowledge. He would have rejected the Reason without Experience thesis at this early stage of his career.

There are several reasons to think that Menger would have rejected it later in his career, as well. Indeed, in the *Untersuchungen über die Methode der Socialwissenschaften, und der politischen Oekonomie insbesondere* (*Investigations into the Method of the Social Sciences with Special Reference to Economics*) (Menger [1883)] 1985, 37), the opening salvo of the *Methodenstreit*, Menger wrote that theoretical economics had "the task of investigating the

empirical forms and laws (the general nature and general connection) of economic phenomena." In a long footnote appended to this statement, Menger ([1883] 1985, p. 37, fn 4; italics in the original) elaborated that

> the goal of research in the field of theoretical economics can only be the determination of the general nature and the general connection of economic *phenomena*. It is a sign of the slight understanding, which individual representatives of the [German] historical school in particular have for the aims of theoretical research, when they see *analyses of concepts* in investigations into the *nature* of the commodity, into the *nature* of economy, the *nature* of value, of price and similar things.

Regarding a kind of Mises-like methodological apriorism, Menger ([1883] 1985, p. 37, fn. 4; italics added) wrote, "A number of French economists [Menger cited Jean-Baptiste Say and Joseph Garnier] fall into a similar error when, with an erroneous view of the concepts 'theory' and 'system', they understand by these terms *nothing more than theorems obtained deductively from a priori axioms, or systems of these*." Whether or not Menger's opposition to empirically-unhinged metaphysical speculation dampened to any degree between 1867 and 1883, he nevertheless rejected the notion that pure reason was the original source of the premises from which economic-theoretical deductions proceeded.

Menger distinguished between two different "orientations" of theoretical research in economics; that is, two different perspectives from which a problem might be considered and a solution to it attempted. When working within the *realistic-empirical* orientation, the theorist

takes economic phenomena "as these present themselves to us in their 'full empirical reality'; *that is, in the totality and the whole complexity of their nature*" (Menger [1883] 1985, p. 56; italics in the original). The realistic-empirical theoretician takes account of the complexity of economic phenomena, of the countervailing factors that complicate and confound observed regularities, of the *ceteris* that are not always *paribus*.

That Menger did not take pure reason as the source of our knowledge of the "empirical forms" and empirical laws generated in realistic-empirical research is obvious. But, what of the products of the *exact* orientation of theoretical research in economics, the "strict types" and exceptionless "laws of nature" of economic phenomena? Is our knowledge of these found in pure reason or in empirical observation?[5] Menger's answer is "both." The exact orientation

> seeks to ascertain the simplest elements of everything real. […] It strives for the establishment of these elements by way of an *only partially empirical-realistic analysis*, i.e., without considering whether these in reality are present as independent phenomena; indeed, without considering whether they can at all be presented independently in their full purity. […] It arrives at results of theoretical research which, to be sure, *must not be*

[5] Menger acknowledged that there were more than two orientations of theoretical research in economics. The exact and the realistic-empirical orientations were merely the two "most important for our science" ([1883] 1985, p. 55). Indeed, the realistic-empirical orientation breaks down into (at least) two sub-orientations: the *historical* and the *statistical*.

> *tested by full empirical reality* (for the empirical forms here under discussion […] *exist in part only in our ideas*) (Menger [1883] 1985, pp. 60-61).

The simplest element of real economic phenomena is, according to Menger (and, of course, many others), human self-interest. We know this through a combination of self-reflection and observation. We know through introspection the role that self-interest plays in motivating our own actions and through observation, and testimony, the role that it plays in motivating the actions of others. We also know from the same sources that factors other than self-interest motivate our decisions and affect our actions: "along with self-interest, which at most can be recognized as the mainspring of human economy, also public spirit, love of one's fellow men, custom, feeling for justice, and other similar factors determine man's economic actions" (Menger [1883] 1985, p. 84). It is the mark of the exact orientation in theoretical economics, however, that the researcher imaginatively sets aside these other factors (and ignores the possibility of error [Menger (1883) 1985, p. 84]), imagines them to have no bearing, assumes these *ceteris* to be *paribus*, in order to focus exclusively on the effects of self-interest on economic decision-making. The purely self-interested person "exists only in the ideas" of the exact theoretician. The assumption of pure self-interest is made only for the sake of the analysis. It is not an assertion about the real world and it would be absurd to test it against real-world experience. As an empirical assertion, it is false. As an assumption for the sake of analysis, it is indispensable to the exact orientation of theoretical economics.

By the same method of imaginatively isolating a particular causal factor, it would be possible to develop exact theories of the effects on decision-making of, respectively, "public

spirit, love of one's fellow men, custom, feeling for justice" and of each of the other factors that we know through introspection and observation to determine individual action. The fundamental assumption of each of these exact theories, e.g., that human action is determined always and only by either public spirit or altruism, or custom, or concern for justice, would be of the same methodological kind as the assumption that human action is determined entirely by self-interest. They would be empirically false assumptions useful for the purposes of exact research. These exact theories could then be combined with the existing theory of self-interest's effects on human action to produce a more comprehensive theory encompassing more (and, in the extreme case, all) of the factors that motivate human decision-making and action: "Not just any one theory of human phenomena, only the totality of such theories […] will reveal to us in combination with the results of the realistic orientation of theoretical research the deepest theoretical understanding attainable by the human mind of social phenomena in their full empirical reality" (Menger [1883] 1985, p. 63).

Be this as it may, the important point for our purposes is that, although our rational faculties are necessary to the exact orientation, they are not sufficient. Reason is not the exclusive source of our knowledge of the simplest element of economic reality. Menger would have rejected the Reason without Experience thesis.

Carl Menger and the Greater Certainty Thesis

Menger's attitude toward the Greater Certainty thesis is a bit easier to suss out than his perspective on the Reason without Certainty thesis. Like many other economists, Menger accepted the idea that social scientists occupy a more privileged epistemic position with regard to

their explanans than natural scientists stand relative to the premises of their theoretical explanations. There is a footnote in Book Three of the *Untersuchungen* in which Menger argues that

> [t]he ultimate elements to which the exact theoretical interpretation of natural phenomena must be reduced are "atoms" and "forces." Neither is of empirical nature. We cannot imagine "atoms" at all, and natural forces only by a representation, and by these we really understand merely unknown causes of real motions. From this there arise ultimately quite extraordinary difficulties for the exact interpretation of natural phenomena. It is otherwise in the exact social sciences. Here the human *individuals* and their *efforts*, the final elements of our analysis, are of empirical nature, and thus the exact theoretical social sciences have a great advantage over the exact natural sciences. The "limits of knowledge of nature" and the difficulties resulting from this for the theoretical understanding of natural phenomena do not really exist for exact research in the realm of social phenomena (Menger [1883] 1985, p. 142, fn. 51; italics in the original).

In the natural sciences, the empirical elements – the phenomena that we observe – are the effects of the actions and interactions of unobserved, hypothetical, "atoms" and "forces." In the natural sciences, we observe effects and hypothesize about their causes. This situation is reversed in the social sciences. Here, we observe causes – most importantly, human self-interest, but also the other causes of individual action discussed in the previous section, e.g., "public spirit, love of one's fellow men, custom, feeling for justice" – and we theorize about how social and economic

phenomena emerge from the actions and interactions of these directly-observed causal factors. In the social sciences, self-interest and these other causal factors are the empirical elements: we observe them introspectively and learn via testimony from other persons about their own experiences of these causal factors.

If, as Menger believed, it is true that knowledge due to introspection is more certain than knowledge due to sensory observation, then the Greater Certainty thesis follows. It is well to note, however, two respects in which the terms of the epistemological discussion have changed since Menger's time. First, the meaning of empiricism as a theory of knowledge narrowed somewhat, especially, over the first few decades of the twentieth century. It was common in the nineteenth century to include introspection or *self-observation* as a kind of empirical observation. In the twentieth century, the meaning of observation narrowed somewhat to mean *sensory observation*. Second, at the same time, knowledge due to introspection came to be downgraded as rather less reliable than it was thought to be when Menger defended the Greater Certainty thesis. It is now widely agreed that we can be wrong about our mental states. The difference between the respective reliability of introspection *versus* sensory observation is seen to be, at best, one of degree, not of kind. If this is right, then it means that, as noted above, the Greater Certainty thesis is a non-sequitur: nothing about the certainty of the resulting knowledge can be inferred from the relative proximity of introspection to its objects and the comparative distance of sensory observation from its objects.[6]

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[6] Thus, it is to be expected that, for Menger writing in the 1880s, introspection was a form of observation and considered a part of empiricism, while, for Mises writing in the 1930s,

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What is important for determining whether and to what extent Menger would have accepted Mises' apriorism is that, though he accepted the Greater Certainty thesis, Menger's acceptance was rather tepid as compared to Mises'. In particular, Menger did not assert the maximal, absolute, "apodictic" certainty of the *a priori* elements of economic theory, as Mises later did. It should also be noted that Menger relegated his defense of the Greater Certainty thesis to a footnote. Unlike Mises, he was far from making it the crux of his methodology. Indeed, for Mises, the significant role of introspection and its supposedly greater certainty was sufficient to draw a hard-and-fast distinction between the methods of the social sciences and those of the natural sciences. Mises was a methodological *dualist* about the methods of the social and natural sciences. Menger, on the other hand, his acceptance of the Greater Certainty thesis notwithstanding, constantly drew the various sciences together, noted commonalities in their

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introspection was part of his rationalism and not a kind of observation. However, it remains a mystery how Mises, who lived long enough to see the reliability of introspection downgraded to just another kind of fallible knowledge, failed to update his methodology and to moderate his defense of the Greater Certainty thesis accordingly. I have argued elsewhere that Mises occasionally failed to appreciate the significance of various developments in philosophy and science for his methodology. As when he denied the possibility of polylogism after multiple logics had already been discovered (CITATION) or when he offered Euclidean geometry as an example of synthetic *a priori* truth long after Hermann von Helmholtz had conclusively undermined this notion (CITATION), perhaps this is just another instance of Mises insisting on the truth of a proposition that was already passé at the time he put it forward.

methods, and emphasized their parallels. Whatever the role of introspection in the social sciences and whatever its epistemic status, Menger did not think its presence in the social sciences and absence in the natural sciences sufficient to demarcate the methods of the social and natural sciences.[7]

The uniqueness of Mises' apriorism consisted of his extreme attitudes to (what I call) the Reason without Experience and Greater Certainty theses. Carl Menger would have rejected the former out of hand and, though he accepted a moderate version of the latter, would have rejected both Mises' extreme claim about the alleged apodictic certainty of the action axiom and the notion that the part played by introspection in the social sciences sufficed to distinguish their methods from those of the natural sciences. As Hayek, Böhm-Bawerk, and (in his own unique way) Wieser later did, Menger would have also rejected Mises' methodological apriorism.

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[7] Related to this point, see Menger ([1883] 1985, p. 215, fn. 145):

> It is a peculiarity of the exact social sciences that exact research in the realm of the phenomena of human activity starts with the assumption of a definite volitional orientation of the active subjects. This does not, however, establish an essential distinction between exact research in nature and exact social research, for the former starts with presuppositions [e.g., concerning the effects of gravity and other forces] which exhibit a formal analogy to the one under discussion here.

In conclusion of this section, it is worth noting that other authors have perceived Menger's skepticism about an aprioristic methodology of the Misesian sort. According to Terence Hutchison's appraisal (1981, p. 178), "Menger rejects *a priori* axioms and theorems deduced from them." Similarly, Lawrence White (1985, p. *xi*), citing the long footnote from the *Untersuchungen* discussed above, noted that "Menger does not claim, as Mises later would, that pure economic theory is axiomatic, deducing theorems from premises *a priori* to empirical investigation of causal linkages. Indeed, he rather plainly rejects such a vision of economic theory."

Monism, Pluralism, and Austrian Methodology

In the present section, I turn to the question whether Menger and Mises respectively tended more toward *monism* or *pluralism* about economic method, in particular, about method in the sub-field of theoretical economics. Did Menger and / or Mises believe there was only one legitimate way of theorizing about economic phenomena and that attempts to theorize in other ways were necessarily less legitimate, i.e., *less capable of generating theoretical knowledge*? Or were one or both open to the possibility that economic theorizing might proceed in several different ways, depending on the purposes to which it was to be put, and that no single method was necessarily superior for all the purposes of theorizing?

It is important to keep in mind that being a methodological pluralist does not necessarily mean being a methodological relativist or anarchist: acknowledging the possibility of multiple methods need not mean treating these methods as equally valuable or meaningful, or pretending that there are no grounds for choosing rationally between them. Thus, a methodological pluralist

can still defend a single method – as all Austrians surely do with regard to Austrian method, whatever it is, exactly – as more appropriate, either in general or for some particular analytical purpose, while both acknowledging the existence of and even appreciating particular insights due to other methods.

As noted in the last section, Mises was a dualist about the relationship between the methods of the social and natural sciences. The significant role of introspection in discovering the premises of theoretical explanations in the social sciences, together with the inflated epistemic status that he attributed to introspection, was sufficient for Mises to distinguish the methods of the different sciences. Menger was more circumspect about distinguishing the social and natural sciences on such grounds, frequently stressing the significance of their methodological similarities and the relative inconsequence of their differences.

Mises and Menger also held different views about the methods of competing schools of theoretical economics. Menger was a pluralist about economic method, according to Terence Hutchison (1981, p. 198), "essentially a critical, anti-extremist, anti-exclusivist moderate." Mises' attitude on this question is a bit harder to determine conclusively. He never explicitly pronounced on the question. However, given the extremeness of his apriorism – given that he thought the "hard core" of theoretical economics to consist of the immediately knowable and apodictically certain action axiom – it is hard to imagine him countenancing alternative methods, different starting premises, for economic theory. Any method not founded on the apodictically certain action axiom would necessarily be inferior to the praxeological method.

I also discuss in this section the interesting fact that Menger's son, Karl, was a central figure in the development of modern methodological pluralism. I consider the relationship

between the senior Menger's pluralism and the junior Menger's arguments for logical tolerance. I argue that Carl Menger's pluralism is broadly consistent with Karl Menger's tolerance. The section concludes with a discussion of Mises' several exchanges with Karl Menger in interwar Vienna concerning questions of economic method. The evidence of these interactions supports the notion, which I have defended in previous writings, that Mises was occasionally out of his depths in matters logical and philosophical.

How Many Methods for Menger?

Perhaps little else needs be said to establish Menger's *bona fides* as a full-blown methodological pluralist. According to Menger, each theoretical "orientation," every perspective from which a problem might be considered and a solution to it attempted, implies a method appropriate to that orientation.[8] As noted above, Menger distinguished between the "exact" and the "realistic-empirical" orientations in theoretical economics, as the two "most important for our science." Moreover, *apropos* of Hutchison's comment concerning Menger's methodological moderation, if the *Untersuchungen* have a central theme, it is the one driven home repeatedly throughout the text, almost *ad nauseum*, namely, Menger's opposition to "one-sidedness" (*Einseitigkeit*) in matters of economic method.

One-sidedness rears its ugly head in economics when a "special" research orientation, "justified per se," as a branch "of a totality of a theoretical science of national economy" but of

[8] It might be more precise to say that each orientation implies an array of methods more or less appropriate to the orientation.

"more or less secondary importance," is treated as the "exclusive" research orientation to be pursued, and other research orientations are "denied":

> The desire to do away with the unsatisfactory state of [classical] political economy by opening up new paths of research has led in Germany to a series of partly misleading, partly one-sided views of the nature of our science and its problems. […] Everywhere…we meet orientations of research, partly misleading and partly justified per se, but more or less secondary in respect to all of political economy. Yet each one of these is *identified* with research in the field of national economy in general (Preface, 30-31; emphasis added).

The problem is not that the German reformers failed to improve upon the defective state of classical economics and its methodology, neither is it that they lost sight of the main goals of economic research by treating secondary problems as of primary importance. "The core [of the problem] is to be found in the poorly cloaked contempt for a basic negation of all other orientations of research, not infrequently of those which prove to be the most significant in respect to the whole of our science" (Preface, 31).

The central theme of Menger's *Untersuchungen* is opposition to monism in economic theorizing. Menger recognized multiple orientations in theoretical economics and recognized that there could be better or worse ways of pursuing, methods more or less appropriate to, each of these orientations. As we will see, this position is broadly consistent with his son's defense of tolerance as a methodological principle.

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How Many Methods for Mises?

It is rather more difficult to conclusively determine Mises' attitude concerning the monism *versus* pluralism question in the domain of theoretical economics. The answer seems to depend on whether Mises is to be read as an extreme or a moderate apriorist. The moderate interpretation supports the notion that Mises was a methodological pluralist with respect to economic theory. On this interpretation, there may be multiple theoretical approaches, based on different starting premises, different methodological choices or conventions – different (Lakatosian) "hard cores," in other words – and we might choose between them according to pragmatic or some other criteria. On the other hand, the extreme interpretation of Mises' methodological apriorism supports the notion that he was more of a monist about economic method. Every other potential starting assumption is necessarily less certain than the apodictically certain action axiom. Any theory built upon such a relatively insecure starting premise is necessarily less secure than praxeology. The "one" (and only?) economics is based on the action axiom (Mises [1933] 2003, 226).

Without greater clarity about the correct interpretation of Mises' methodological apriorism, we cannot conclusively determine whether Mises was a pluralist or a monist about economic theory. However, it is probably not too speculative to suggest that Mises, if (perhaps) not a full-blown monist, was less of a pluralist than Menger about the methods of theoretical economics.

Karl Menger on Tolerance – Historical Development and Meaning

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In *The Logical Syntax of Language* ([1934] 1937, pp. 51-52; italics in the original), the philosopher Rudolf Carnap, leader of the so-called "liberal" or "left" wing of the Vienna Circle of Logical Positivists, pronounced the famous "Principle of Tolerance in Syntax":

> *It is not our business to set up prohibitions, but to arrive at conventions* […] *In logic, there are no morals*. Everyone is free to build up his own logic, i.e., his own form of language, as he wishes. All that is required of him is that, if he wishes to discuss it, he must state his methods clearly, and give syntactical rules instead of philosophical arguments.

A short footnote followed Carnap's statement of the Principle of Tolerance:

> In the conflict over the logical foundations of mathematics, this attitude was represented with especial emphasis (and apparently before anyone else) by [Karl] Menger [(1930) 1979]. Menger points out that the concept of constructivity, which Intuitionism absolutizes, can be interpreted both in a narrower and in a much wider sense (Carnap [1934] 1937, p. 52; also see Kraft 1953, p. 60; quoted in Menger 1979, p. 11).[9]

[9] Karl Popper ([1935] 1992, p. 33) endorsed the Principle of Tolerance "as a regular tenet of the logic of science" in *Logik der Forschung* (*Logic of Scientific Discovery*). Also see Popper ([1978] 2009, p. 401).

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Menger (1994, p. 141; italics in the original) later described his role in converting Carnap to tolerance from a position of linguistic monism:

> I objected to the recurring references in the Circle to *the* language and repeatedly asked Carnap, [Moritz] Schlick and other members what justified the implied belief in the uniqueness of language. But on this point […] I failed to receive a satisfactory answer. Schlick did not seem to take the question seriously.
>
> In the course of the following years, however, Carnap not only gave up that belief but emphasized the importance of the existence of a multiplicity of languages between which one may choose, while Schlick and [Friedrich] Waismann continued speaking about *the* language.

In an earlier elaboration, Menger ([1934] 1974, p. 111; italics in the original) had elucidated the significance of logical tolerance:

> A mathematical theory, I emphasized, consists of nothing but transformations of precisely stated propositions into other propositions according to precisely stated rules, with freedom in the choice of the rules as well as of the propositions.
>
> In 1930, the strong emphasis on that *twofold* freedom was at variance with all the current writings on philosophy of mathematics – not only with the intuitionists' and Russell's ideas, but also with the views held at that time by Carnap, who was still strongly influenced by Wittgenstein and consistently spoke about *the* language and *the*

> logic in his writings and discussions. In the Circle, not even [University of Vienna mathematics professor and Menger's mentor, Hans] Hahn seemed to welcome my deviating ideas. Only Kurt Gödel (who had entered the Circle about the time when I did and thereafter had been a rather untalkative member) assented[.][10]

Menger treated mathematics as something akin to an art form, "albeit one that had its own rules and often showed itself to be uncannily relevant to science" (Leonard 2020, p. 3). Much as his father had bemoaned the influence of "one-sidedness" in economic methodology for its exclusivity and for the deleterious effects of this exclusivity on the development of methods appropriate to various research orientations, the younger Menger criticized the intolerant attitude in mathematical philosophy for its negative effects on the creativity of mathematicians and for the threat it posed to the future progress of mathematical science. Logical tolerance, on the other hand, opened up "'the boundless ocean of unlimited possibilities'" (Menger 1979, p. 13; quoting

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[10] According to Robert Leonard (2010, po. 127), tolerance (not just of the logical sort) was a "motif" for Karl Menger throughout the interwar period:

> [i]t was the attitudinal thread by which he guided himself through the murky 1930s debates in philosophy and mathematics. It also sustained him as he was drawn to the world of politics, to foundational questions of a different kind, where the 'exclusion of the middle' was taking on a darker connotation (Leonard 2010, p. 127).

Carnap 1934, p. *xv*). Indeed, Menger (1994, P. 176) rejected the Unified Science project associated with some of his Vienna Circle colleagues, Otto Neurath, in particular, precisely for its one-sidedness (Leonard 2020, p. 3): "the idea of a unified science might possibly lead to the exclusion *a priori* of potentially valuable objects or methods of study." Menger was opposed to *a priori* restrictions on most practices, be they scientific, mathematical, logical, linguistic, ethical, or artistic.[11]

Menger rejected the Unified Science project for another reason closely related to logical tolerance and relatable to his father's methodological arguments. It is a fool's errand to provide *a priori* demarcation criteria, be they criteria for distinguishing science from non-science or the natural from the social sciences, or the distinct branches of some given scientific discipline. It is true, Menger (1994, p. 176) argued, that such boundaries must sometimes be drawn for particular practical purposes, "for the organization of studies and research, of schools and libraries, and for

[11] A tolerant attitude toward some field of inquiry implies methodological pluralism in that field, but methodological pluralism need not imply tolerance as Menger (and Carnap) understood it. More carefully, there cannot be intolerant pluralisms, but there can be, as it were, *excessively* tolerant pluralisms. Varieties of cognitive relativism that deny *all* rational grounds for choosing between beliefs, theories, languages, or logics, such as some find in the later work of Paul Feyerabend [CITATION to Against Method] and various sociologists of science [CITATIONS to Bloor, Knowledge and Social Imagery, cf. Laudan Demystifying Underdetermination and Pseudo-Science of Science], and which is manifest in postmodern approaches to science [CITATIONS to e.g., Sokal's targets], are examples of too-tolerant methodological pluralisms.

diverse commercial purposes," but they are "historically conditioned if not altogether arbitrary [and] of hardly any epistemological interest." Given the conditioned nature of such demarcations, reifying them

> in rigid definitions [is] not only useless but likely to slow down normal development and to inhibit progress by limiting *a priori* the objects and / or methods of research […] It is doubtful that this can be achieved except by arbitrary dictates; and it is even more doubtful that anything would be gained if it could be achieved.

For Menger (1938, p. 794), tolerance amounted to a willingness to engage in social relations with other persons, in particular, with persons who accepted norms or values different from those accepted by the tolerant person. An intolerant person, on the other hand, was willing to engage in social relations only with persons who shared their own norms and values. Tolerance and intolerance were thus attitudes of general significance across domains of society, wherever norms or values were operative, not merely in the moral and ethical domains, but also in aesthetics, politics, the humanities, and the sciences. One might be more (Carl Menger) or less (Ludwig von Mises) tolerant with respect to, say, distinct systems of economic-methodological norms and values.

However, a tolerant attitude implied rather more than this for Menger (and for Carnap). Properly understood, tolerance required not only a willingness to engage with those who accepted competing systems of norms and values, but an understanding of these systems sufficient to make informed judgments about them. Tolerance did not necessitate engaging in

social relations with persons who defended confused, incoherent, or nonsensical systems of norms and values; neither did it require engaging in relations with persons who refused to openly and clearly declare the norms and values that they accepted. Tolerance required that norms and values be "frankly proclaimed rather than […] hidden" (Menger [1934] 1974, p. 91). Frankly stating the system of norms and values that one accepted promoted analysis and discussion of its consequences. Tolerance presupposed understanding of and, therefore, considerable clarity about, relevant systems of norms and values. As Carnap ([1934] 1937, p. 52) stated the relevant point, if one "wishes to discuss" their preferred logic or language, "[a]ll that is required of him is that […] he must state his methods clearly, and give syntactical rules instead of philosophical arguments[,]" i.e., he must not try to defend his methods with argument, but show how they are to be used properly so that a discussion might proceed regarding their comparative effectiveness.[12]

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[12] According to the Carnap scholar Richard Creath (Forthcoming):

> Carnap suggested that we think of the philosophical enterprise as one of making proposals for structuring the language of science. These proposals are not theories, not even tentative ones. They do not describe the world but have a different role. There is no fact of the matter about which of these proposals is the correct one because a language is not the sort of thing that is true or false. […] One is then free to adopt whatever language one wants, and in particular scientists are free to choose whatever language they find useful. Philosophers have no right to overrule them. […] Of course, it is permissible to

Clarification allowed for observation of the consequences of acting according to different systems of norms and values. Various systems were to be evaluated according to their respective results. That is, the value of a method – be it a scientific, mathematical, logical, linguistic, ethical, or aesthetic method – cannot be determined *a priori*, but only in application to specific scientific, mathematical, logical, etc., problems. According to Menger ([1932] 1979, p. 44), "[p]recisely what is interesting is to observe the development (that is, the investigation of the consequences) of different systems, and to compare them with one another" (Menger [1932] 1979, p. 44).

The methodological implications of tolerance, according to Menger ([1934] 1974, 35; italics in the original), were a kind of "*logical pragmatism*[,]" the opinion that "the only

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> challenge the logical consistency of an empirical claim or to defend such claims against such a challenge. But such a discussion requires that the parties get clear about what the language of that empirical claim is by explicating the rules that structure that language. Moreover, it is permissible for philosophers to explicate terms in scientific discourse that they find unclear. Such terms can be clarified / precisified / explicated in multiple ways. What is not permissible is for a philosopher to insist that scientists must use some specific clarification. What this reorientation of philosophy does, then, is to give empirical science the primary role rather than treating philosophy as the queen of the sciences and capable of ruling them. […] This reorientation is neatly summarized by Carnap in his Principle of Tolerance.

justification for scientific definitions – or […] for the way words are used in science – lies in their *fertility*; that is, in the possibility of using them in a great number of important propositions, preferably practical propositions."[13] Methodological tolerance emphasizes the observable consequences of following some system of scientific norms and values, and rejects the notion that the value of such a system can be determined in advance of its application to specific problems, especially those problems to which it was meant to be applied. The value of a system of scientific (or humanistic) norms and values is determined by its practical consequences. Regarding mathematical languages, given the creative nature of mathematical activity, the relevant consequences to be evaluated are primarily aesthetic.[14]

Of course, to be consistent, the Principle of Tolerance must be applied to itself. Tolerance is itself a methodological norm / value and, thus, its meta-methodological value is determined by the consequences of its methodological applications.

Logical (methodological) tolerance has cast a long shadow over subsequent philosophy of science. It is implicit in, e.g., Thomas Kuhn's (CITE) methodology of scientific paradigms, Imre Lakatos' (CITE) methodology of scientific research programmes, and Larry Laudan's

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[13] Compare this with some sentences taken from the last paragraph of the ninth and final appendix of Carl Menger's *Untersuchungen*: "As if the worth of a science were dependent on its object, and not rather on the importance, depth, and originality of the results of their investigations!" (Menger [1883] 1985, 237).

[14] Like music, "which certainly does not provide us with knowledge" (Menger [1930] 1979, p. 57), mathematics finds "justification on its overwhelming aesthetic merits."

(CITE) philosophy of scientific progress. Menger's and Carnap's respective arguments for a kind of methodological pluralism were an essential historical precursor of these later pluralistic philosophies of science.

Tolerance and Economic Methodology: Karl Menger on "Austrian Marginalism and Mathematical Economics"

It is among the great ironies of the history of economics that, despite his father's well-known and vehement rejection of the methodological value of mathematics for economic science, Karl Menger was perhaps the chief promoter of the development of mathematical methods in economics (see Leonard 2010, Düppe and Weintraub 2016, Sigmund 2017, pp. 275-278). Regarding the methods of economics, the junior Menger (1973, p. 38) wryly noted, quoting Goethe: "two souls reside within my breast." Menger founded an important discussion group of his own, focused on mathematical topics, in addition to participating in the Vienna Circle. It was in Menger's Mathematical Colloquium that Kurt Gödel developed and elaborated several of the key ideas that eventually led to his pioneering results in the field of metalogic. The Colloquium also provided opportunities to Viennese mathematicians, many of whom were Jewish, at a time when anti-Semitism was on the rise (Leonard 2010, p. 151).[15] Several of the first applications of

[15] Karl Menger's mother, Hermine ("Mina") Andermann, was born Jewish, but converted to Catholicism in 1893 (Scheall and Schumacher). After Menger emigrated to the U.S. in March 1938, administrators at the University of Vienna prompted an investigation by the *Sippenant*

advanced mathematics to economics were made in the Mathematical Colloquium, with Menger's enthusiastic support.

In 1973's "Austrian Marginalism and Mathematical Economics," Menger applied the tolerant methodological attitude to the relationship between the mathematical method and the verbal-deductive methods of Austrian School economists. According to Menger (1973, p. 38), mathematical economists and their non-mathematical Austrian colleagues agree "in most of their fundamental economic views." Their methodological differences, however, move members of these rival schools to treat each other with little "sympathy and mutual understanding." This dogmatic – one might say "one-sided," in Carl Menger's sense – insistence of members of each group on the absolute preeminence of their preferred methods was misplaced, Karl Menger argued. In fact, relative to particular criteria, both methodological approaches have their merits and demerits, and neither is absolutely or objectively superior to the other.

The most meaningful difference between the two schools concerns the form in which they present their ideas and the formal tools they use to transform relevant propositions into others. Naturally, mathematical economists use the language(s) of mathematics to present their ideas; they state their claims in mathematical "formulae which they transform and combine mathematically." Austrian economists, on the other hand, reject both mathematical languages

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("kinship bureau") of his racial background, an investigation that ended with Menger declared a *Mischling* or "half-breed" (Sigmund 2017, p. 356).

and mathematical tools, and "formulate their assertions in sentences of the common language and connect them by logical reasoning" (Menger 1973, p. 38).

Menger (1973, p. 39-40) analyzed the relative value of the two methods in terms of their respective formulations of the principle of marginal utility and the law of demand.

According to their defenders, mathematical formulations of the principle of marginal utility "express *more* than the simple words of the Austrians and, furthermore […] describe the situation *more precisely*. But neither of these claims seems to be justified" (Menger 1973, p. 40). Indeed, mathematical formulations assume "an additional, if tacit, hypothesis […] that is clearly not anchored in economical facts," namely, that the utility function admits a second derivative (Menger 1973, p. 40). The verbal formulation of the Austrians, on the other hand, "is *more general* since it is valid even if there are places where the function does not admit a second derivative and its graph has no curvature, whereas at such places the mathematical formulation fails to assert anything" (Menger 1973, p. 40). Implausible assumptions about the continuity and differentiability of functions must be made, if one is to apply mathematical analysis to economic problems. "This point deserves being stressed since mathematical economists consider it as one of the advantages of their method that it brings tacit assumptions of verbal formulations to the surface. The Principle of Marginal Utility is a case—and […] not the only one—illustrating the opposite situation" (Menger 1973, p. 40-41).

Neither do mathematical formulations necessarily express economic principles more precisely than verbal statements. Mathematical and verbal expressions of the law of demand are "of the same mathematical precision," according to Menger (1973, p. 41). With regard to

generality and precision, mathematical methods may be relatively, but are not absolutely, superior to verbal-deductive methods.

Austrians are committed to an ordinal notion of utility, a commitment that requires "they even eschew arithmetical operations such as dividing or multiplying a utility by a number" (Menger 1937, pp. 41-42). Thus, the Austrians' verbal definition of marginal utility is necessarily less precise than a mathematical one. The principle of marginal utility could be stated verbally, according to Menger (1973, pp. 42-44), but only by limiting the concept of marginal utility to goods consumable in equal increments. The Austrians verbal definition of the principle of marginal utility could be made more precise, in other words, but only by sacrificing its greater generality. Tradeoffs must be made in economic methodology, as in economic life.

It is not the case, moreover, that mathematical methods possess an absolute advantage over verbal-deductive methods with regard to the formulation of economic problems mathematically expressed in systems of equations. "These problems also lend themselves to perfect verbal *formulations*. But he who looks for specific *solutions* is practically forced to switch to […] mathematics" (Menger 1973, pp. 46-47; italics in the original). This fact, however, is irrelevant to Austrians. It is not realistic – and Austrians emphasize the need for a realistic treatment of economic phenomena – to expect to discover exactly uniform prices in the real world. Austrian economists "are satisfied, more realistically, with ascertaining an interval within which any price is advantageous to both bargaining parties […] the exact outcome [depending] on the skill and the knowledge of the parties" (Menger 1973, p. 57). Mathematical solutions of such systems of equations might be interesting and productive for some scientific purposes, but not for those of the Austrians.

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According to Menger (1973, pp. 52-55), other methodological disagreements between the mathematical and Austrian schools are less significant than they might seem, given their vehemence. Austrian economists prefer causal explanations, while mathematical economists, "consciously or unconsciously, following the methodological programme of [Ernst] Mach" (Menger 1973, p. 54), study the functional relationships between economic factors. "But even physical science offers certain causal relationships which Mach himself might have slightly restated but certainly never ridiculed […] What makes mathematical economists expect that all of economics should be on a higher level than those scientific studies?"

However, Austrian economists "move on dangerous ground, surrounded by swamps of pseudo-problems," when they try to define economic concepts in terms of the "essence" of economic phenomena. Naturally, given his preference for (methodo)logical tolerance, Menger (1973, p. 55) argued that concepts can always be defined in various ways and that the only grounds for choosing between them concern their relative "fruitfulness": "one must derive from, or by means of, the definition an extensive satisfactory theory, preferably one that is applicable, i.e., connects the subject with other domains of research."

Dogmatism concerning the absolute dominance of either the mathematical or verbal method over the other was inappropriate. Mutual tolerance, in the pertinent sense of willingness to engage in scientifically-relevant social relations, was necessary. To encourage a dialogue between members of the two schools, they should elucidate their respective conceptual definitions and methods. They should avoid the *a priori* assumption that the rival method was without value for economic science and explicitly acknowledge the merely relative value of their

own preferred method. Neither method is univocally superior in all analytical contexts and for all applications to whatever problems economists might want to investigate.

Methodological tolerance would also encourage members of the two schools to work together on occasions where the potential for mutual benefit might exist. The costs and benefits that might accrue to members of both schools from collaboration and from integration of their respective methods cannot be discovered *a priori*, but only after such collaboration has been attempted—and attempting to collaborate *requires* methodological tolerance. Only tolerance allows the potential of methodological integration to be investigated. The mutual intolerance of each school for the other ensures only collective ignorance of the potential costs and benefits of methodological rapprochement.

> [E]ach of the three founders of marginalism, Jevons, [Carl] Menger, and Walras, apart from their common ideas, made valuable contributions of his own to the development of economic theory; and this tradition has been continued by their schools. This is a fact which, rather than playing off the schools against one another, we should welcome and utilize (Menger 1973, p. 60).

Hints of (Proto) Tolerance in Carl Menger's Untersuchungen *and* Irrthumer Des Historismus

The *Methodenstreit,* as Karen Horn and Stefan Kolev have recently noted, quite perceptively, "seems to be after all a dispute on the meaning of concepts for demarcating what economics and its sub-domains should be about" (Horn and Kolev 2020 [Foreward to *Errors of Historicism* translation], 447). In this regard, the Mengers, father and son, were probably closer to each other

than Carl's infamous rejection and Karl's defense of mathematical methods in economics would seem to imply. In particular, there are suggestions of a tolerant attitude in the senior Menger's methodological work, particularly in some of the appendices at the end of the *Untersuchungen*, as well as in *Die Irrthumer des Historismus in der Deutschen Nationalokonomie*, Menger's second contribution to the *Methodenstreit*, written in reply to Gustav Schmoller's caustic review of the *Untersuchungen*. I do not mean to suggest that Carl Menger's manifest methodological pluralism was identical to his son's methodological tolerance, only that there are commonalities between the two. Indeed, despite the unfortunate fact that the textual and archival evidence required to prove the point does not exist, it is probably reasonable to assume, particularly given what we know about the son's devotion to his father and the significance of the latter's thinking for the former's intellectual development (Scheall and Schumacher), that Carl Menger's engagement in the *Methodenstreit* had some effect on Karl Menger's development of logical tolerance, even if his primary motivation for this came from reckoning with the mathematical philosophy of Intutionism and the debates he engaged in the Vienna Circle concerning logic and language.

In the second appendix to the *Untersuchungen*, Menger argues that the weaknesses of most attempts to define *political economy* (and, one presumes, any other science, discipline, or sub-discipline) primarily consist of a lack of adequate clarity concerning either 1) the exact designation of the science, 2) the object(s) that the science investigates, and 3) the formal perspective from which the science investigates its objects.

Menger elucidates this point further in the fourth appendix to the book, where he notes that "a correct terminology […] prevents countless confusions in the investigation and reception

of scientific knowledge." However, the problem of adequately designating the sub-branches of economic science "according to their character offers formal difficulties not easy to overcome" (Menger [1883] 1985, 207). Indeed, this problem emerges even with respect to the question whether it is more proper to designate political economy as part of the political sciences or as part of the social sciences: "If […] the concept of *society* is viewed as a contrast to that of the *state*," and political economy is designated a social science, then "the point is overlooked that economic policy and the science of finance [i.e., two branches of the science] are political sciences in the truest sense. This designation […] thus seems at least to be unsuitable for presentations of our science which include the latter elements" (Menger [1883] 1985, 207). However, if political economy is counted among the political or *state* sciences, it is ambiguous whether theoretical economics is to be included.

Menger ([1883] 1985, 207) himself preferred "*economics*" to "political economy" to designate his chosen field:

> If the concept "*national economy*" is correctly understood […] as an "organism" of economies from which with advancing culture a political activity […] aimed at benefiting it seems less inseparable, then [economics] actually appears to be a not wholly unsuitable designation for the group of sciences in question here. Also the division of "economics" into a theoretical and a practical portion and of the latter into an "economic policy" and a "science of finance" flows rather easily from these reflections.

There will likely remain, however, a group of economists who prefer the "political economy" designation. Provided that this is done to retain the "international currency of the term," it is "done for a reason which in questions of terminology is of great significance, usually even of decisive significance." However, in a vein of methodological tolerance, Menger ([1883] 1985, 208) bemoans the possibility that the older term "political economy" is retained "just because of its indefinite nature, which expediently cloaks the vagueness of the concept which it designates."

Here we find Menger advancing a particular proposal for a definition of his chosen science and defending it on grounds of the greater clarity it imparts to the problem as compared to rival definitions. At the same time, Menger acknowledges the considerable practical value of retaining a terminology that has achieved "international currency," provided that it really is the international currency of the terminology and not the possibility of "expediently" obfuscating one's meaning in indefinite and vague phrases that motivates its retention. This is, of course, very much in keeping with the notion of logical tolerance later explicated and defended by his son. To reiterate, tradeoffs must be made in economic methodology, as in economic life.

Difficulties are multiplied if the problem is taken to be not the terminology appropriate to political economy "in the present-day conception of it," but the terminology appropriate to political economy *qua* general science of human economy. "A terminology adequate for the nature of the latter can only be the result of full insight into the nature of the various tasks which research in the realm of economic phenomena has to fulfill" (Menger [1883] 1985, 208). Given that this "full insight" has yet to be achieved, if it ever will be achieved, we should expect the terminology appropriate to political economy "in the present-day conception of it" to change as

our insight into the proper tasks of economic research develops over time.[16] "[T]he human sciences […] will need a long development before the various aims of scientific research in the realm of human phenomena are completely clarified and thereby the basis gained for a complete classification and terminology of social science in general and of the economic sciences in particular" (Menger [1883] 1985, 208). We should not expect a final and definitive terminology for economic science to emerge in the state of knowledge as either Menger found it in his day or as we find it in ours.

Menger's recognition that the exact and the realistic-empirical orientations "by no means" exhaust the realm of theoretical economics is also on display in Appendix IV of the *Untersuchungen*. Menger recognizes the possibility that theoretical economics itself might evolve into multiple branches, each consisting of several orientations.

> It is clear that the development of our science can, rather, again and again bring to light new orientations of theoretical striving for knowledge. At present, with the slight development of the social sciences, the results of all orientations of theoretical research in the field of national economy are grouped together suitably into one science, "theoretical economics," into a discipline which, incidentally, just for this reason necessarily lacks strict formal unity in the knowledge it presents and thus lacks a systematic approach also. However, in our estimation there is no obstacle to its gradually splitting into various

[16] Classifications between sciences and disciplines are "historically evolved" (Menger [1884] 2020, 477).

> branches with progressive development, as this has already happened in the field of research in natural science[.] […] Each one of these branches will exhibit a certain, at least relative, independence.

For our purposes, the significant upshot of the fourth appendix of the *Untersuchungen* concerns the always disputable nature of economic terminology – always disputable, that is, till the day comes, surely a long way off, if it ever arrives, when we have achieved "full insight into the nature of the various tasks which research in the realm of economic phenomena has to fulfill" – and what one might think of as a concomitant need for tolerance in the sense of a willingness to engage with members of rival schools of thought who accept competing systems of methodological norms and values, provided that these members of rival schools clarify their norms and values sufficiently to observe the consequences of accepting them. Indeed, considering the *Methodenstreit* in the light of Karl Menger's emphasis on tolerance, it is not necessary to strain one's eyes to see Carl Menger criticizing his historicist opponents for their lack of clarity (and, in Menger's opinion, their scientifically unjustifiable motives for unclarity) and for their equally unjustifiable one-sidedness about method, criticisms interpretable as pleas for greater tolerance, in his son's sense.

This impression is strengthened by Menger's second contribution to the *Methodenstreit.* Written in reply to Schmoller's cutting review of the *Untersuchungen*, *Die Irrthumer des Historismus in der Deutschen Nationalokonomie* (*The Errors of Historicism in German Economics*) provided Menger the opportunity to reiterate and further clarify his criticisms of the one-sided historicist methodology of Schmoller's German Historical School. Indeed, the essay

begins with an attack on the Historical School's "vagueness…regarding the goals and methods of research in the field of political economy" (Menger [1884] 2020, 461).

Menger argues that this vagueness is a consequence of the Historicists' illegitimate importation into economics of methods ill-fitted to the objects of theoretical research. The methodological approach of the Historical School was never "the result of an immersion into the problems of our own science, it did not arise from the scientific needs of scholars who were immersed in the problems of *their* discipline. It implied from its inception a transfer of historical knowledge into our theoretical-practical discipline" (Menger [1884] 2020, 461). Against what he takes to be Schmoller's criticism, Menger emphasizes that he is far from denying the significance of history, statistics, or other fields of inquiry for the purposes of theoretical economics. What he denies is that these fields constitute a potential basis of a reformed method of theoretical research, particularly given the lack of clarity and the one-sidedness with which such reform is proposed. The historicism of German economics constitutes a "scientifically most boggy field […] which has been defended with extreme measures of intolerance and impropriety. […] The one-sidedness of our historical economists may only be countered in a decisive manner through complete clarity over the goals and paths of inquiry in exact economics" (Menger [1884] 2020, 463).

Menger further clarifies both the concerns that motivated the *Untersuchungen* and his criticisms of German historicism in the third and fourth "letters" of the epistolary *Irrthumer*. In the former book, Menger ([1884] 2020, 470) had set out to answer the question, "What is the difference between historical and theoretical social sciences?" Recent work in Germany had "lacked any rigorous distinction between historiography and statistics on the one hand and theory

on the other" (Menger [1884] 2020, 470). The historical school, according to Menger, had run all of these together, as if there were no distinctions to be made and, thus, "put into question the independent significance of theoretical economics" Menger's ([1884] 2020, 471) first goal in the *Untersuchungen* was to defend theoretical economics against the "one-sidedness with which this group has devoted its mental capacity partly only to historical and statistical studies…while at the same time they most regrettably have neglected the theory of our discipline"

However, according to Menger ([1884] 2020. 471), the German historicists had also failed to distinguish between theoretical and practical economics, the latter which "has to research and describe the principles of expedient intervention" in economic phenomena (Menger [1884] 2020, 473) and the principles of "proper regulation of the state budget" (Menger [1884] 2020, 486). Worse yet, this "misunderstanding was even characterized as epoch-making progress for our discipline" (Menger [1884] 2020, 471). Menger's second goal in the *Untersuchungen* was to clarify the relationship between the theoretical and practical orientations of political economy.

Finally, to the extent that the economists of the German historical school had considered theoretical economics at all, they had denied the legitimacy of the "exact" orientation. The German historicists had embraced "a gross one-sidedness in their conception of theoretical economics, by admitting legitimacy not to *all* directions of theoretical research but only to some that are in close connection to historical-statistical studies" (Menger [1884] 2020, 471). Menger's ([1884] 2020, 474) third ambition in the *Untersuchungen* was to "prove the legitimacy of [the exact orientation] of theoretical inquiry in economics, which has been zealously disputed by the historical school"

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In short, according to Menger,

> the historical school…seemed to have lost the conception of political economy and its parts, the understanding of the relationships between its parts and auxiliary disciplines, but most importantly the overview of the different legitimate directions of research in theoretical economics[.] [T]he historical school of German economists lost its insight into the system of tasks whose solution is incumbent upon research in economics"

Thus, the overarching objective that unified the various goals of the *Untersuchungen* was clarification of the appropriate classification of the disciplines and sub-disciplines of political economy, as Menger found it in the given state of scientific knowledge.

In keeping with methodological tolerance, Menger criticized Schmoller and the other German historicists for a lack of clarity in their methodological arguments that obscured the precise nature of their method. Schmoller, in particular, according to Menger ([1884] 2020, 475), buried under "incomprehensible language" the "self-evident truth" that progress in economic theory benefits economic historians, while progress in economic history benefits theorists. Moreover, Schmoller seemed to infer from the undisputed fact that all sciences are related to each other that there were no fixed boundaries between sciences and disciplines, "and that especially the historical sciences of the economy and theoretical economics may be confused with one another" (Menger [1884] 2020, 485). Menger responded that, though the sciences were not *unbridgeably* divided, they were nevertheless divided. Consistent with methodological tolerance, Menger ([1884] 2020, 476) argued that the sciences, as well as distinct disciplines

within individual sciences, were divided *according to their respective goals of inquiry* and that different scientific goals implied different methods:[17]

> The physiologist pursues scientific goals other than those of the anatomist, even though he engages with the results of anatomy for his purposes; the physicist pursues goals other than the mathematician, even though he uses the results of mathematics for his own purposes; and the goal which the theorist in the field of the economy sets for himself is quite different from the one that a historian in the field of the economy sets for himself, even when he carries out historical studies for his puposes […] Here, with regard to the tasks and goals of research, there exist strict boundaries between the above-mentioned disciplines, which must not be blurred unless one opens the doors for confusion and the shallowest dilettantism.

Menger ([1884] 2020, 477) argued that Schmoller, on the other hand, had adopted the methodologically intolerant practice of attempting to "blur the boundaries between these fields of knowledge as far as possible." Schmoller's methodological perspective was ultimately "beyond any serious criticism," not because it could withstand all criticism, but because it could not even be rigorously analyzed and discussed. Historicist methodology exemplified the most serious offense against methodological tolerance: it is "incomprehensible" (Menger [1884] 2020, 499).

[17] Also see Menger ([1884] 2020, 491): the sciences differ "by the tasks they have to solve," tasks that were determined by the unique goals of the respective sciences.

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Schmoller's lack of clarity meant that "his thoughts about the paths of inquiry in the field of our science are protected against any attack" (Menger [1884] 2020, 499).

Also in keeping with methodological tolerance, Menger ([1884] 2020, 491) argued that different sciences, disciplines, and analytical orientations were to be evaluated according to how well they performed their respective tasks and, thus, by how far they realized their respective goals:

> Every science can, however, in a certain sense be…perfected, but not as Schmoller imagines, by assigning to it tasks that are incumbent upon other sciences and that contradict its nature, but by solving the tasks peculiar to every science in as perfect a manner as the state of human knowledge permits.

When Karl Menger Met Ludwig von Mises

The junior Menger completed his dissertation at the University of Vienna under Hans Hahn in 1924. Menger then moved to Amsterdam to work, first, as assistant to the famous Intuitionist mathematician, L. E. J. Brouwer, then as a *docent* (i.e., lecturer) and, finally, starting in May 1926, as Assistant Professor of mathematics at the University of Amsterdam. After falling out with Brouwer over both Intuitionism and a number of priority controversies, in 1927, Menger took the opportunity (offered to him by Hahn) to take over Kurt Reidemeister's recently vacated chair in geometry at the University of Vienna. For the next decade, until his last-minute departure from Austria, soon to be annexed by the Nazis, Karl Menger was an active participant

in Vienna's economics community, in addition to teaching at the University, running the Mathematical Colloquium, and participating in the Vienna Circle.

Interestingly for our purposes, it was during these years that Ludwig von Mises developed his extreme apriorism. Mises' *Grundproblem der Nationalökonomie* (*Epistemological Problems of Economics*) first appeared in 1933.

In a perfect world, we would possess detailed evidence of the discussions that took place during these years in the meetings of both Mises' own discussion circle (the famous "*Miseskreis*") and the *Nationalökonomische Gesellschaft* (Austrian Economic Association). At a minimum, we would know the topics discussed at each meeting and the discussants who participated. Even better, we might possess detailed minutes of each meeting, indicating who said what, who argued with whom, and why. Alas, if it were not obvious for many other reasons, the world we occupy is very far from perfect, for little of this evidence exists today (see Klausinger 2019 for an analysis of the little evidence available). Anyone interested in Karl Menger's reactions to Mises' extreme apriorism is left, therefore, with little recourse but to the relevant texts and the memories of participants.

In his *Reminscences*, Menger briefly described the Viennese economics scene as he found it upon his return from Amsterdam and the rivalries that divided the community. Hans Mayer occupied Carl Menger's chair at the University of Vienna. According to the younger Menger (1994, 11), Mayer "spent most of his time and energy on more or less futile attempts to counteract" the influence of Othmar Spann's nationalistic and holistic (i.e., very un-Mengerian) approach to economics. Mayer, who never published much, also engaged in a series of petty disputes with Mises over their respective influence over students at the University. Perhaps due

to the combined effect of his religion (Jewish), his politics (ultra-liberal), and his personality (difficult), Mises was never given the chair at the University of Vienna that he probably deserved, given the import of his extensive publication record. Nevertheless, many students found more inspiration in Mises' lectures than in Mayer's unproductive and uncollegial hectoring.[18] Menger (1994, 11) credited Mises for his "stimulating lectures" and Mises' circle for its "deep insight into the theories of money, banking, commerce an international trade," while criticizing Mises for failing to distinguish economic theory from "his ideal of complete *laissez-faire*," a criticism still occasionally leveled against Austrian economists.

During these years, Menger made a number of contributions to the canon of the Austrian School, albeit "married to an unprecedented stress on the need for rigorous logico-deductive mathematical reasoning" (Punzo 1994, *xxi*), an approach best exemplified by his book, *Moral, Wille und Weltgestaltung: Grundlegung zur Logik der Sitten* (*Morality, Decision and Social Organization: Toward a Logic of Ethics*), first published in 1934 (Menger [1934] 1974). Menger ([1934] 1979) also adopted the Austrians' methodological individualism and subjectivism in his attempt to solve the famous "St. Petersburg Paradox." For our purposes, however, Menger's most significant contribution to Austrian economics during this period was an explicit, if nevertheless oblique, methodological critique of Mises' apriorism.

[18] Mayer would later gain eternal infamy in Austrian economic circles for his quick expulsion of Jewish members from the *Nationalökonomische Gesellschaft* (Austrian Economic Association) in the same week as the Nazi *Anschluss*.

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> This paper ["Remarks on the Law of Diminishing Returns: A Study in Meta-Economics"] goes back to a conversation about the methodology of economics in which L. Mises claimed that certain propositions of economics can be *proved*. As an example, he mentioned the law of diminishing returns and referred me to the literature for the proofs. After I presented my findings to Viennese Economic Society they were published in [1936 as "Bemerkungen zu den Ertragsgesetzen"]. In 1936, Mises wrote that he learned a great deal from the paper.[19]

Among the many criticisms of Mises' apriorism, Menger's is unique in that it mostly ignores the epistemological story that Mises tells about the allegedly purely rational and apodictically certain nature of the action axiom. Menger's attack is not founded on rejection of the Reason without Experience and Greater Certainty theses, in other words. However, Menger ([1936] 1979; italics added) does not neglect these issues entirely:

> [B]y saying that a proposition follows from certain others, we do not mean that it is valid [in the sense of empirically confirmed]. Even if a scientific proposition can be correctly derived from generally accepted propositions only experience can show whether or not the derived proposition is valid. *If experience should demonstrate that it is not (as well may be the case despite all deductions), then the necessary conclusion is that some of the*

[19] It should be noted that, whatever he might have learned, Mises never modified his methodology in light of Menger's (or, to my knowledge, anyone else's) criticisms.

> *generally accepted propositions cannot be upheld without modification*. Such situations occur frequently in physics: Conclusions are derived from general propositions; *yet no physicist would on this ground neglect to test these conclusions empirically. If they are not empirically confirmed, then the general propositions must be altered*.[20] Much of the progress of physics is due to this procedure.
>
> [W]e emphasize that we attach little importance to the rather subtle logical relationships among the various laws about returns. We believe the crucial issue for economics to be whether or not these laws are empirically confirmable while we regard it as a secondary issue whether or not they follow from certain other propositions. It is, rather, some outstanding economists who have raised this issue by claiming to prove the law of diminishing returns on land logically, and thereby to make empirical tests superfluous.[21]

Rather than the epistemology of the action axiom – the main "general proposition" from which Misesian economic analysis proceeds – Menger criticizes the other side, as it were, of the

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[20] Compare with the passage quoted above from Carl Menger's abandoned *Critique of Metaphysics and of the so-called Pure Reason from the Empiristic Point of View*

[21] Menger cites Bohm-Bawerk here, though the prefatory discussion to the paper as published in Menger's *Selected Papers*, quoted in the text above, indicates that Mises' contention that some laws of economics could be logically proved without reference to experience was the original inspiration for the paper.

Misesian approach, i.e., the claim that, whatever the epistemological status of the action axiom, significant economic truths can be inferred from it, together with other (more empirical) general propositions, using traditional verbal-deductive methods: "All that we say here is that logical relationships, equivalences, deductions, proofs, etc. must be handled correctly" (Menger [1936] 1979, 280). Handling logical relationships "correctly," Menger argues, means employing the tools of modern mathematical logic rather than the verbal-syllogistic logic of Aristotle. When Menger applies these methods to the "proofs" of the law of diminishing returns in the literature, he shows that they fail to meet the more rigorous standards of proof associated with mathematical logic (which, of course, does not mean that the law of diminishing returns is invalid in the sense of empirically disconfirmed).

> The proposition to be proved is often stated without sufficient precision. Propositions are assumed, explicitly and implicitly, to have the same meaning though in fact they are by no means equivalent […] Frequently there is a lack of precision in regard to the logical quantifiers; that is, as to whether a proposition is meant to apply to *all* or only *certain* levels of outlay. Yet it is only after such details have been settled that an assertion becomes precise enough to be subject to deductive treatment […] The proofs are not preceded by a full statement of the assumptions, though without a complete list it is impossible to determine whether a proof is correct, or even whether a proposition is capable of proof. Nor are the conclusions always precisely stated […] Finally, some proofs contain crude mistakes, due to the incorrect formulation of the negations of somewhat more complicated statements (Menger [1936] 1979, 279).

In general, Menger's paper is a plea to economists inclined to the use of verbal deduction to avoid "vague arguments [and] a lighthearted use of such words as 'therefore', 'it follows' or 'since'" (Menger [1936] 1979, 279). In particular, it is a plea to Austrian economists to embrace methodological tolerance – to state their assumptions clearly, to formulate their propositions carefully, to draw their inferences validly, and to state their conclusions precisely – so that the consequences of their theoretical investigations can be analyzed, discussed, and compared with the results of other analyses.

Concluding Remarks: Methodological Tolerance, Intolerance, and Political Liberalism

I have argued for three theses in the present paper. First, I have argued that, whatever might be true with regard to other Austrian School economists, the founder of the School, Carl Menger, cannot be counted among the School's adherents to Ludwig von Mises' extreme methodological apriorism. If this is right, then the common association, if not identification, of Austrian method with Mises' apriorism is further undermined, given arguments that I have offered elsewhere for the same conclusion with regard to other prominent Austrians, F. A. Hayek, especially.

I have also argued that Carl Menger was clearly a pluralist about the methods of theoretical economics and that Mises was rather less of a pluralist, at least, if not altogether a monist, about the legitimate method(s) of economic theorizing. I have tried to show how this pluralism manifested in Menger's methodological writings, primarily as opposition to what he perceived as the one-sidedness of his main dialectical adversaries, prominent members of the younger German Historical School, in particular, Gustav Schmoller.

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I have tried to establish parallels between Carl Menger's methodological pluralism and Karl Menger's methodological tolerance; and, further, that, given these parallels, the junior Menger's methodology is something of an imperfect proxy for his father's. I have shown how methodological tolerance manifested in Karl Menger's writings on economic methodology, both in his analysis of the relationship between the Austrians' non-mathematical, verbal-deductive, method and the mathematical methods of other schools of economic thought, and in his much earlier plea, directed primarily at Mises, to take the rigorous standards of modern mathematical logic more seriously in his defense of the verbal-deductive approach. I have shown that Karl Menger rejected Mises' apriorism for several reasons, some concerning what Menger saw as Mises' unsupportable extremism regarding the Reason without Experience and Greater Certainty theses, and others concerning Mises' lack of rigor in his use of deductive logic. The implied suggestion is that Carl Menger, had he lived long enough to hear Mises advance his extreme apriorism in the meetings of the *Miseskreis* and *Nationalökonomische Gesellschaft*, might have criticized Mises on related grounds.

In this concluding section, I would like to suggest that the Mengers' tolerant methodological attitude is more consistent than Mises' extreme apriorism with the political liberalism typically defended by Austrian School economists.[22] Mises' methodology seems to

[22] Although the need for some degree of brevity means that I cannot pursue this issue in the present context, I believe there to be significant parallels between the Mengers' methodological tolerance and F. A. Hayek's methodology of sciences of complex phenomena, a view that I have described elsewhere as "methodological liberalism" (CITATION to "Kinds"). I am inclined to

imply that there is only one way to do theoretical economics and that all other methods are of lesser legitimacy, if not plainly illegitimate. Inasmuch as liberalism requires recognizing that there are multiple ways to lead one's life, multiple ways to pursue one's goals, whatever they might be, whatever the domain; and, moreover, inasmuch as liberalism requires toleration of competing methods – which is to say, inasmuch as liberalism recognizes the epistemic value of a *plurality of methods* (CITATIONS to Mill and others) – Mises' apriorism is surprisingly *illiberal*. Apparently, on Mises' way of thinking, liberalism – toleration of competing methods – is required in all walks of life, *except with regard to theoretical economics*. In this domain, evidently, only Misesian apriorism will do. Thus, any Austrians inclined to defend both tolerant classical liberalism and intolerant Misesian apriorism are stuck on the horns of a dilemma.

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think that integrating the Mengerian and Hayekian perspectives would lead to a more accurate image of Austrians' actual practices, especially their methodological diversity, but this is an argument for another place and time.

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