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## MENGER'S ANTI-HISTORICAL METHOD VERSUS THE NEOCLASSICAL ANTI-HISTORICAL METHOD

**Abstract.** Due to the famous *methodenstreit* it is often well argued that Menger's approach to social sciences can be seen as anti-historical, as according to him pure empirical studies are insufficient to establish a firm economic theory. By suggesting that some theorems have to precede historical studies, Menger may be seen as a representative of the a priori tradition in scientific method. The modern method in the mainstream of economic thinking is also to a large extent anti-historical and a priori, but because of its lack of realism and extensive reliance on very limiting assumptions. The main strength of the Mengerian anti-historical approach is lesser faith in imaginary constructs, implying a higher degree of realism in theorizing.

*Keywords:* methodenstreit, a priori, Austrian, methodology

*It is concrete acts, destinies, institutions of definite nations and states, it is concrete cultural developments and conditions whose investigation constitutes the task of history and statistics, whereas the theoretical social sciences have the task of elaborating the empirical forms of social phenomena and the laws of their succession, of their coexistence, etc.*

Carl Menger

### Historicism issue over economic laws

*Methodenstreit:* a methodological clash between prominent 19<sup>th</sup> century thinkers of the Historical and the Austrian School, with Menger standing against Schmoller. As much as it is methodological it seems to be mythological too. One side passionately argued for preserving economic theorizing and the other side wanted to crush it in favor of pure historical research (see e.g. Huerta de Soto 1998). Is it really the case that the poles were so extreme, or it is merely a modern interpretation of the debate, which

is path dependent on the emotions and ideologies of the debate's participants? Joseph Schumpeter – perhaps the theorist most distant from any kind of “teaming” in the history of economic thought – offers the following summary of the *methodenstreit*:

Since there cannot be any serious question either about the basic importance of historical research in a science that deals with a historical process or about the necessity of developing a set of analytic tools by which to handle the material, the controversy, like all such controversies, might well seem to us to have been wholly pointless (...) neither party really did question its opponent's position *outright* (...)

The first thing to be observed about all controversies between scientific parties is the large amount of mutual misunderstanding that enters into them (...)

Secondly, this situation is made worse by the fact that methodological clashes often are clashes of temperaments and of intellectual bents (...)

Third, we must never forget that genuine schools are sociological realities – living beings. They have their structures – relations between leaders and followers – their flags, their battle cries, their moods, their all-too-human interests. Their antagonisms come within the general sociology of group antagonisms and of party warfare (Schumpeter 1954, pp. 814–815)

Schumpeter further continues that various terms used in the debate over method – such as empirical, realistic, modern, exact, speculative, futile – are beginning to live a life of their own. They do not necessarily have to denote the word to anything specific, but may stem from personal interest, quarrels, vanities and fights. That of course in turn is a natural feature of human beings, who are not like impassionate computers, passively processing received information. Ironically, in that respect: the best proof for the non-existence of homo oeconomicus are economists themselves.

In understanding the content of the topic one should not, however, be too much occupied with peculiar relations. Putting psychologizing and personal issues aside, the content of the methodological question remains: what is the status of economic science and which methods are used to study it? For Schumpeter the question is not properly framed as it depends on the content of what is being studied. Even though in many hypothetical examples the demarcation line is easy: “price” (a theoretical term) is different than “price for apples in New York in 1993” (a historical term), in reality puzzle solving in economics may involve various sophisticated configurations in which historical factors play important roles in theoretical narration (as I plan to show with some examples below). Inferring from Schumpeter's comments, the conclusion would be quite radical: there is no such thing

as *methodenstreit*. There is only *methodensuche*; a search for proper method at the right time. In-depth study of a particular puzzle is always to some extent a form of *case study*.

A quite fitting example for that can be Mises's argument about the impossibility of socialism (Mises 1990). By the modern Austrians the argument is treated as a strong theoretical argument (rightly so). At the heart of it, however, lies a specific institutional concept: private property. The notion can and is analyzed by theoretical studies, but at the same time it is more context-based than very broad economic categories such as opportunity, cost, value, time preference, capital, and so forth. All those latter concepts are found in the highest forms of economic theorizing about economic action. Private property is a theoretical concept having a lot of cultural, legal, and historical flavor. What does it actually mean to have something privately owned? How does this system differ, say, from "several property", or from "syndicalism", "corporate capitalism" etc.? (see the recent debate Denis 2015; Bylund and Manish 2017).

By making a theoretical argument about the impossibility of socialism, Mises suddenly found himself tangled in institutional considerations about legal systems and their ideological basis. That itself does not mean that he went outside of theoretical considerations. Nevertheless he went directly into empirical waters as compared to his neoclassical colleagues, who wanted to do economic science much like Wieser and, for example, search for a universal value theory that would be applicable under all institutional arrangements (Bostaph 2003; Wieser 1971), including socialism. Lange's reaction may seem like a sarcastic comment, when he stated that Mises, despite being a defender of theorizing, makes an institutional-historical type of argument (Lange 1939, p. 55)<sup>1</sup>. In reality the comment is not that surprising, because Mises's theory follows from a different type of apriorism having more to do with empiricism than the theoretical isolation characteristic of neoclassical thinking.

### **Theory is always needed and the data is always needed too**

Any form of scientific reasoning requires conceptualization in a theoretical framework. In other words, science is virtually always theoretical. Even when confronted with a broad range of empirical data, that data has to be properly arranged and interpreted. To be precise, empirical systems are actually built on information, not just data. The latter is passive acquisition of observations, the former is based on interpretation and inquiry. There is

a famous anecdote about Ludwig Wittgenstein asking someone in a seminar: “Why do people say it’s natural to think that the Sun goes around the Earth?”. The response was “Well, because it looks that way”. Yet a question follows: “And how would it look like if the Earth was moving around the Sun?”. Obviously it would look the same way, therefore on a purely sensualistic ground either conclusion can be reached. Fortunately though, empiricism is not sensualist, as it needs rational appraisal and well thought out reasoning.

Theoretization of data is related in methodology to Duhem-Quine’s thesis, which points to the fact that all observations require some form of background assumptions (see the illuminating discussion in Harding 1976). The thesis itself is related to physics, where measuring equipment for testing a particular hypothesis may be skewed, imperfect, or simply improper. An example could be given by the Michelson-Morley experiment, which was supposed to discover absolute time and space. The experiment, however, eventually ended with the conclusion that instead of absolute time and space, only the speed of light appears to be absolute. One of many potential conclusions from the observations, one should add. From such an inquiry, varieties of other inferences are possible: from saying that there is something wrong with the experiment, through something happening to the equipment, to a radical conclusion that Newtonian physics is flawed (on how the experiment reshaped physics, see: Shankland 1964).

Even though Duhem-Quine’s thesis seems to be crafted for physical considerations, it very strongly applies to the social sciences, where measurement is even more ambiguous. Take the case of the most fundamental variable in economic science: the overall “condition” of the individual. Amartya Sen by extensively studying wealth and development perfectly captured the assessment problem: “You could be well off, without being well. You could be well, without being able to lead the life you wanted. You could have got the life you wanted, without being happy. You could be happy, without having much freedom. You could have a good deal of freedom, without achieving much” (Sen 1985, p. 1). And such problems are only a beginning. No data stands on its own, therefore it requires a theoretical framework to interpret it. The accuracy of such measurements can also be questioned (Morgenstern 1964; Bagus 2011). How much more true that becomes while dealing with individuals, whose well-being is subjective and not entirely based on material factors.

Therefore, no escape from theorizing is possible in science. Theorizing actually constitutes science as it is, since science is based on some form of nomothetic reasoning.

### **Different kinds of apriorism**

If science is always theorizing, then any clashes in science are always collisions of different theories. In the light of the above, one is inclined to conclude that “apriorism” may have many faces. As the term indicates, a priori statements are being made *prior* to some empirical research. Nevertheless those statements themselves may already contain empirical elements in them. Therefore there are varying degrees of apriorism. Starting from the extreme side: there is apriorism based only on deduction and by the power of the already assumed. Such a version of economic theory would be like Euclidean geometry: arbitrarily chosen assumptions and their logical consequences. Apriorism does not have to be that extreme. If one allows for the assumption to be based on a broad range of observations, then economic apriorism is still using deduction, but is not anti-empirical. Rather, it is precisely the opposite (Smith 1996).

Nothing demonstrates the divergence between dissimilar apriorisms better than the difference between the Mises-Menger “anti-historical” method and Knight’s “anti-historical” method (see Knight 1940). Both camps are nominally a priori and often treated as being part of the same tradition against historicism, but the reality is that the Austrian camp would be closer to historicism than they are to Knight’s neoclassical equivocations. The Mengerian Austrian theory is constructed in a logical a priori manner, but it already comprehends many of the empirical notions about reality. The examples are: leisure as a consumption good, diversity of individuals, cognitive peculiarity of human beings, variety of natural resources, dynamism of preferences, heterogeneity of capital, differences in institutional arrangements (property rights versus regulatory intervention).<sup>2</sup> In opposition to that, neoclassical apriorism starts off in a theoretical vacuum without inductive references about human reality.

On the very general level, both the Austrian School and the neoclassical school can properly be labeled as a priori and deductive. They start off, however, with a completely different set of assumptions. In the case of neoclassicism the assumptions may be chosen at random. It is possible to isolate a particular thing from any surroundings and push the abstraction into the extreme. Only later can one make some empirical sense out of them by confronting the predictions of the model with individual experiences as Friedman’s famous position asserts (Friedman 1953; see also Mäki 1994). No other theory in economics is more extreme in its apriorism than the neoclassical approach. No empiricism in the beginning and middle. Empiricism is injected only at the very end of a thought process, when individual forecasts

are confronted with individual *facts* in order to assess the predictive power of theoretical frameworks. Yet before that there is a blank check for offering any kind of model. In some sense, then, Friedman's idea was a license for formal revolution to introduce any unrealistic *a priori* model (Wade Hands 2009, pp. 150–151).

By contrast the Mengerian School uses apriorism in a much more mild and loose way: broad empirical considerations are always at hand along each step of the way in reasoning, and the limitations of theoretical components may be contested at all times. At the same time unrealistic tools (such as equilibrium concepts) are conceptually used, but with careful selection of their worth – always with their deficiencies in mind. Hence, Menger called his method “realistic-empirical” (Menger 1985, p. 66).

In other words, it can well be argued that the Austrian School is both *a priori* and empirical in its descriptions. And much more empirical than its neoclassical colleagues are.

### **“Testing” in economics**

Just as much apriorism has many faces in economics, so is the case with empirical confrontation of a particular theory with sets of data. Perhaps one could mention here Popper's explanation about the falsificationist method. In social sciences it seems that Popper's way is treated as a simplistic form of empiricism, where extremely theoretical systems are being subjected to selective testing by reference to small real world predictions. In reality that is not the case. First, Popper noted that his method is a form of conventionalism: there is no such thing as data against theory, because of Duhem-Quine's. A particular theory is always confronted with another theory (see for example Popper 1959, pp. 79–82; see also Lakatos 1978, pp. 93–101).

Second, Popper suggested a different approach to social sciences than to natural sciences. One can even be restrained in calling this a particular “method”, since Popper simply gave general Socratic advice to social scientists: they should construct their theoretical systems in such a way to make it contestable by critical discussion in any relevant aspect (Popper 1994, pp. 166–172; Boland 1979; Boland 1994, pp. 157–167). Such a tactic is radically opposed to Milton Friedman's views from his famous methodological essay. Friedman makes an argument that unrealistic theory can safely be wrong as long as it conforms to some set of selected predictions. Popper's view is much broader, however: any theory can and should be contested

along the way. Therefore, critical rationalism is not supposed to defend some sets of assumptions, but to contest all of them without exceptions.

Popper's second point brings us close to the question of how empirical notions can harmonize theoretical investigations. Here, let me just briefly mention a few noncontroversial (I hope) examples.

The first and foremost one would be embedded in the so called escape from the Malthusian Trap. Before 1800 one can generally agree with the notion that per capita production could not significantly go beyond the means of subsistence, so in the long run living standards were basically the same over the centuries (Clark 2007, p. 2).<sup>3</sup> The great economic question was: is it a necessary state of affairs, or can growth harmoniously and exponentially happen to virtually all regions on Earth as the market economy may lead to universal and peaceful flourishing among the nations? The answer to such a question can only be "empirical" by understanding the boundaries of the law of decreasing returns. The escape from the Malthusian Trap happened for a variety of reasons, but it was manifested in the commercialization of technological advancement embodied in mercantile products. Hence the possibility to escape the Malthusian Trap crucially depends on productive powers hidden in nature and human capacities to discover and manage them: things we identify by observation.<sup>4</sup>

The second example would relate to the usefulness of economic models for forecasting. We know there is no universal economic model which would predict the behavior of a stock market, or any specific market for that matter. Despite having important and impressive developments of probabilistic modeling, no such venue appears to be possible now. How do we *know* this? Not really through logical reasoning, but from reasonable recognition of the empirical reality around us. Since virtually all human choices are permeated with pure Knightian uncertainty, such predictions fail (on Knightian uncertainty see Hoppe 2007). It is still possible to construct a priori sophisticated models using measurable probabilities. Yet the simple reason we reject them actually stems from their incapability to deliver: they are falsified by experience. If they actually delivered many times again and again, it could be a good reason to think over our skepticism.

A third example would relate to the complexity of production functions, or otherwise put: world heterogeneity. Factors of production are not expressed in natural units and their quantities are not easily related through weights and parameters (as assumed in the infamous Cobb-Douglas function; Georgescu-Roegen 1988, pp. 299–300). How relevant is that empirical notion? It goes well beyond the task of modeling production functions, since it is at the heart of the economic calculation argument about socialism

(Mises 1990). Economic calculation in monetary terms would not be needed, if all processes of production were either completely specific, or completely unspecific. In the case of the former, full specificity would mean that no choice is even possible (since every goal has just one route). In case of the latter, calculation in monetary units would be pointless, as it could be substituted by some form of natural measurement related to homogenous usage (for example kilograms) (Mises, 1966, pp. 206–207).

This is another good example of how empirical notion fits into economic theory and allows it to reach significant real-world conclusions by the power of reasoning.

### **Conclusion: Mengerian anti-Historicism is both a priori and empirical**

The above discussion leads us to the final question of how the Mengerian approach to economics places itself in the development of economic science. Menger has been a member of the economic triumvirate, alongside Jevons and Walras. Jevons can be at times seen as a hybrid of the other two who were poles apart. Both Walras and Menger were pure theorists, using a nomothetic apparatus to state and develop economic laws. Yet they differed greatly in how those laws were formulated. They both believed that economic theories are stated *prior* to specific historical research. At the same time they differed with respect to the emphasis on empirical notions surrounding human beings. Whereas for Walras, the model could assume very unrealistic elements, Menger has been much more careful. His followers have developed a quite unique line of thinking which seems to be at odds with both the “historical” method and “theoretical” method. The approach, however, seems to be a quite sensible path; or to paraphrase Meyer’s alternative: for Menger and Mengerians, truth in economic science is at the expense of quantitative precision.

### **N O T E S**

<sup>1</sup> Just as Mises was called by Lange an institutionalist, so was the case with Menger classified in such a way by Streissler (1973, pp. 172–3) and Jaffé (1976, p. 520).

<sup>2</sup> This point about Mengerian apriorism is perhaps most clearly discussed in Smith (1994, pp. 316–318).

<sup>3</sup> This statement could, however, be questioned. Indeed, there was some very mild growth over the centuries, but it was very limited compared to what happened after 1800.

<sup>4</sup> Of course it does not follow that technology itself can lead to the escape.



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