

WIESER, HAYEK AND EQUILIBRIUM THEORY*

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1. Introduction – Hayek and Wieser

Friedrich A. Hayek entered the University of Vienna directly upon returning from military service at the end of World War I. In his last year at university, Hayek found a major professor in the person of Friedrich von Wieser, who had himself recently returned from service as the final Minister of Commerce under the monarchy. Hayek's 1926 obituary notice¹ for his "revered teacher" is laudatory, which might be expected, but remarkably, even more than fifty years later his impressions do not seem to have changed much:

*"He was a most impressive teacher, a very distinguished man whom I came to admire very much. I think it's the only instance where, as very young men do, I fell for a particular teacher. He was the great admired figure, sort of a grandfather figure of the two generations between us...he was for a long time my ideal in the field, from whom I got my main general introduction to economics"*².

Wieser's politics also apparently appealed to the youthful Hayek, for Wieser believed that marginal utility analysis provided a scientific justification for a progressive income tax.

If Hayek venerated Wieser as a teacher and (at least initially) was attracted to his politics, it is perhaps harder to trace any direct influence from Wieser on Hayek's own work. After Hayek completed what was to be his first degree in 1921,

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¹ Hayek-1992.

² Hayek-1983a, p. 14.

Wieser convinced him to write a dissertation on imputation, the Austrian approach to marginal productivity theory, which he began in the summer 1922 and which led to his second degree, awarded in the spring of 1923. Hayek later published an article on the subject.³ In reminiscences he claimed to have learned some things while doing the thesis, but he also added that he hoped that no copies of it had survived.⁴

On the other hand, there is ample evidence of possible indirect influences. For example, Wieser argued in *Natural Value* that no matter what the form of economic organization, such things as factor returns must be calculated because “the economic-technical service, that of controlling production, would remain” even under communism.⁵ This argument, together with his emphasis in *Social Economics* on economic “management and value”⁶ anticipate Misesian arguments in the socialist calculation debate, all of which impinged on Hayek’s later contributions to the debate. We might also add that Wieser’s discussion of “social institutions” in *Social Economics*, in particular his emphasis on how “laws and morals” constrain behavior, and on the inadequacy of “the rationalistic utilitarian assumption” for explaining the evolution of institutions,⁷ might well have been remembered by Hayek when he turned to these subjects himself in later years.

What I viewed as a dearth of compelling evidence of any direct influence from Wieser on Hayek once led me to state (at a conference on Hayek in Rimini, Italy) that I thought that the influence of his teacher on Hayek was minimal. Jack Birner was in the audience, and during the discussion period he disagreed, pointing out (quite correctly) yet another area of similarity, namely that Hayek’s methodological approach in *The Pure Theory of Capital* had affinities with that of Wieser in *Social Economics*. Further support of Birner’s point might be provided by Hayek’s praise in his obituary notice of Wieser’s method of “decreasing levels of abstractness” which he heralded as an “exemplary and trendsetting social science methodology”.⁸

The question of Wieser’s influence on Hayek, then, is certainly still a live one, one for which conflicting plausible interpretations are possible. That said, there seems to be little evidence to support Joseph Salerno’s recent claims that Wieser was an advocate of “(verbal) Walrasian general-equilibrium analysis” and that it was his (and, less directly, Joseph Schumpeter’s) influence on him that caused Hayek to become enamored with equilibrium theory.

Salerno presents his thesis in a paper entitled “The Place of *Human Action* in the Development of Modern Economic Thought”,⁹ *Human Action* referring to Ludwig von Mises’ monumental treatise on the science of human action. Salerno’s claim is embedded within a larger historical portrait in which the decline of the

³ Hayek-1984a.

⁴ Hayek-1994, pp. 64-65.

⁵ Wieser-1893, p. 63, pp. 78-81; cf. Wieser-1927, p. 113.

⁶ Wieser-1927, p. xvii.

⁷ *Ibid.*, p. 158; p. 165.

⁸ Hayek-1992, pp. 115-16.

⁹ Salerno-1999.

Mengerian causal-realistic approach to price determination is traced. Salerno identifies four causes of the decline, one that was not reversed in Salerno's opinion until Mises resuscitated the Mengerian tradition with the publication of *Human Action*. The causes of decline include 1) the "wilting of Böhm-Bawerk's vital and creative powers" and "the concurrent flowering and intellectual influence of Joseph A. Schumpeter" 2) "the rapid rise of Marshallian economics in Anglophone countries" 3) "the great stimulus given to research in Continental general-equilibrium analysis at the London School of Economics by the arrival of Friedrich A. Hayek in 1931", and 4) deficiencies in the Mengerian approach itself.¹⁰ In what follows, only selected aspects of Salerno's extended account will be scrutinized.

Salerno documents Schumpeter's rapid rise to fame in the years before the war, and points out (quite rightly) that in his 1908 book *Das Wesen und der Hauptinhalt der Theoretischen Nationalökonomie*, Schumpeter introduced and indeed recommended Walrasian general equilibrium analysis to German speaking economists. Salerno argues that Schumpeter's book had a profound impact on fourth generation Austrians like Hayek, Morgenstern, Machlup and Haberler, basing his claim principally on reminiscences in which these writers noted that the book was viewed at the time of its publication as an important one.¹¹ Doubtless recognizing that one needs something more than this to establish influence, Salerno introduces a role for Hayek's teacher Wieser in constructing his story about how Hayek became attracted to general equilibrium theory. It is this story that I wish to challenge.

2. Wieser and General Equilibrium Theory

Now, it is manifestly true that Hayek in his early work on the trade cycle exhibited an attraction towards what he called "equilibrium theory." In the opening chapter of *Monetary Theory and the Trade Cycle*, Hayek basically asserted that to be scientific, an explanation must employ a theory, and that the theory that economics employs is "equilibrium theory".¹² In the 1930s Hayek even hoped to develop a dynamic equilibrium model of a capital-using monetary economy. But it is also true (as Salerno acknowledges) that Hayek's attitude towards (particularly *static*) equilibrium theory was complex, and that he appears to have changed his mind about it rather dramatically over time. No one to my knowledge has yet

¹⁰ Salerno-1999, p. 38.

¹¹ *Ibid.*, pp. 43-44.

¹² Hayek-[1933] 1966, chapter 1. I disagree though, that this means that Hayek necessarily endorsed "general equilibrium theory." Salerno cites as evidence Hayek's famous footnote, in which Hayek praises the Lausanne school for their having "most perfectly expressed" the general theory of economic interdependence. But Hayek did not use the Walrasian system of equations approach in his own theory. By praising Walrasian theory but not using it, we will see that Hayek was following a tradition going back to Menger that viewed the general equilibrium approach as apt for *representing* certain aspects of economics, but not useful for actually doing economic research.

explained the reasons behind his initial attraction to it, so Salerno's thesis is an original one that merits further examination.

What I contest, and it is a fundamental objection given his thesis, is Salerno's claim that Hayek's teacher Wieser was actually an advocate of general equilibrium theory. Salerno offers various pieces of evidence for the claim. He notes, for example, that both Mises and George Stigler considered Wieser to be a follower of the school of Lausanne.¹³ More substantively, Salerno argues that "natural value," a concept that provided the name for Wieser's second book, is itself a general equilibrium concept. Salerno states that Wieser's purpose in writing the book "was to construct his own peculiar ideal of social welfare on a state of general equilibrium that he called 'natural value,' and to link it through the concept of marginal utility to foundations in human psychology".¹⁴ Next, Salerno points out that, in his review of Schumpeter's *Das Wesen*, Wieser did not include a criticism of Walras's approach among his criticisms of Schumpeter.¹⁵ His final piece of evidence is the assertion that Wieser's later writings also show him to be an advocate of "verbal" general equilibrium theory. The last claim is part of Salerno's larger revisionist historical tapestry:

"The decade leading up to World War I thus represented the watershed decade for the Austrian School in the nation of its birth. A physically debilitated Böhm-Bawerk, although still an influential teacher, was no longer able to undertake original work in pure theory, while Menger had already retired from active teaching and publishing in 1903. In this situation, the publication of Schumpeter's two books sparked a powerful movement to recast the price-theoretic core of Austrian economics along the lines of (verbal) Walrasian general-equilibrium analysis. The publication of Wieser's Theorie der gesellschaftlichen Wirtschaft in 1914 added momentum to this movement. Since Wieser's book, later translated into English as Social Economics (1927), was the first comprehensive treatise on economics produced by the Austrian School, this meant that the field of pure theory was now almost completely dominated by the general-equilibrium wing of the school".¹⁶

Let us take up Salerno's more substantive claims in turn.

It is hard for me to make sense of Salerno's first claim that "natural value" is a general equilibrium concept. *Natural Value* is divided into six books. In Book One Wieser discusses valuation by a single individual of a good when there are a number of units of the good available. He reaches the usual marginalist result that the value an individual places on a good depends on the marginal utility, or the value that the last unit of the good, provides to the person. In Book Two he notes

¹³ *Ibid.*, p. 40.

¹⁴ *Ibid.*, p. 37.

¹⁵ *Ibid.*, p. 40.

¹⁶ *Ibid.*, p. 42.

the analogy between a single individual who is evaluating units of a good, and the exchange value of a good as determined in a market. In the latter case, the exchange value (or market price) of a good depends on the maximum amount that the marginal, or last, buyer is willing to pay for a unit of the good. Just as marginal utility allows an individual subjectively to assess the value of a good, market prices allow individuals in a market to assess the objective exchange value of a good. He notes finally that exchange value depends on *both* the utility that the good provides to people in a market (as is true in the case of an individual valuing a good) *and* on differences in wealth and income, or purchasing power, which may exist among the various market participants.

Wieser defines “natural value” as the value that would exist in a hypothetical communist economy in which prices accurately reflect relative scarcities, all persons economize perfectly (no errors are made), and all differences in purchasing power are eliminated.¹⁷ Wieser immediately points out that even in such a situation there would still exist such things as land rent, because one would need to know what land contributed to the total return, even though under communism the “part it plays, as a source of private income, would fall away”.¹⁸ This of course was meant directly to challenge the widespread socialist belief that only labor gives rise to value. It does not, Wieser hastens to add, mean that a socialist state is impossible:

*“On this account the examination of natural value will be useful, as well as for those who wish to understand the economy of the present, as for those who wish to evolve a new one. Defenders of the existing order of things, equally with those who are fighting to prepare the way for a new and ideal state, may, without prejudice and without going against their principles, unite in this study. Natural value is a neutral phenomenon, the examination of which, whatever may come of it, can prove nothing for and nothing against socialism. If land rent and interest on capital are natural phenomena of value, they will have their place in the socialistic state also...”*¹⁹

The concept of “natural value” thus is intended by Wieser to provide a neutral method for studying factor returns, returns that would exist no matter what the system of property ownership might be. In subsequent parts of the book Wieser examines such things as “natural land rent,” the “natural return to capital,” and “the natural cost value of products.”²⁰ Interestingly, what Wieser considered to be his chief contribution, the “natural value” construct, is not one that is used by economists today. In any case, what is not clear to me is why Salerno thinks that the book is best characterized as a contribution to general equilibrium theory. What does Wieser’s natural value, a methodological construct meant to simplify the

¹⁷ Wieser-1893, pp. 60-61.

¹⁸ *Ibid.*, p. 63.

¹⁹ *Ibid.*

²⁰ Hayek-1992, pp. 115-17 summarizes Wieser’s contributions in the book.

analysis and designed to show that the calculation of factor returns is necessary no matter what the form of property ownership, have to do with general equilibrium theory?

Nor does Salerno's second example establish Wieser's affinity with Walrasian theory. On Salerno's reading, Wieser's review is "highly laudatory",²¹ whereas I tend to read it as a polite rebuff of Schumpeter's thesis that positivist natural science methods have any place in economics. In any case it is true enough, as Salerno states, that Wieser did not directly attack Walras. One is tempted to say: So what? Wieser was after all reviewing Schumpeter's book, not one by Walras. That he failed to say anything about Walras need not be taken as an endorsement.

And indeed, if one looks at places where Wieser in his writings actually *does* say anything about Walras (as for example when he compares his own work to that of other marginalists in the Preface to *Natural Value*), his words though (again) polite are hard to mistake for praise:

*"[The analysis] of Walras, though admirable of its kind, suffers, to my mind, from the preponderance of the mathematical method. The laws which govern amounts of value undoubtedly allow of a mathematical expression; nay, the more complicated of these can be expressed exactly only by means of mathematics; and here certainly mathematics has a great task to fulfill. But in the value theory we have to do with something more than the expression of the laws of amounts. The obscure conception of value is to be made clear; all its manifold forms are to be described; the service of value in economic life is to be analyzed; the connection of value with so many other economic phenomena is to be shown; in short, we have to give a philosophy of value which needs words, not numbers. And, besides all this, the empirical existence of the alleged facts is to be established."*²²

In fact, Wieser here sounds very much like Menger had in his letters to Walras in the 1880s (and, as noted in our first footnote, like Hayek in *Monetary Theory and the Trade Cycle*).²³ The mathematical systems-of-equations approach favored by Walras might be useful for *expressing* certain economic principles, but it was not useful for *understanding* economic phenomena.

What about Wieser's *Social Economics*, completed more than twenty years later, by which time (if Salerno's thesis is correct) a Walrasian influence presumably should have been thoroughly evident? In 462 pages of text, Walras is mentioned

²¹ Salerno-1999, p. 40.

²² Wieser-1893, pp. xxxiii-xxxiv.

²³ In a letter to Walras in 1883, Menger acknowledged that the mathematical method was useful for expositing or demonstrating certain results of economic reasoning, but denied that it was helpful for conducting research because it failed to get at the "essences" of economic phenomena, something only his own "exact" method was capable of doing. Antonelli-1953 reproduces the letters, and Kauder-1957 offers a discussion.

exactly once, on page 17, where his work is listed along with that of 41 other contributors to the modern literature. A passing remark in Earlene Craver's article on the Viennese economists also speaks to the question of whether Wieser's analysis shared much in common with Walras'. Roy Weintraub had raised a question about why Karl Schlesinger had failed to solve a particular problem in general equilibrium theory. Axel Leijonhufvud speculated that it might be "because he had learned in the Mayer seminar to think in Wieser's rather than Walras' terms".²⁴ From this perspective, Wieser's approach was quite different from that taken by Walras.

Wieser, then, appears to have been about as lukewarm as Menger was to the use of mathematics to elucidate economic principles. Salerno's claim, however, is that Wieser is a proponent of "verbal" general equilibrium theory, so perhaps it is not enough to show that Wieser was critical of, and his approach apparently incompatible with, mathematically expressed general equilibrium theory à la Walras. It would have been helpful had Salerno defined "verbal' general equilibrium theory," but he does not. So we will just have to look at Wieser's texts to see what is there. The *précis* above of *Natural Value* suggests that it is not a candidate, but what about *Social Economics*?

Wieser begins *Social Economics* with the study of want satisfaction in "a simple economy." The "simple economy" construct is another one of Wieser's methodological abstractions. It is an economy that is run by a single central authority, a simplifying assumption that allows Wieser to focus attention on the constituents of value in an economy but in the absence of exchange. I suspect that the assumption had a certain strategic value. Previous analysts had gotten at the same idea using the fiction of a Robinson Crusoe economy, but that construct had become a target for critics of marginalism, who derided theories that used the analysis of the actions of an isolated individual to explain social phenomena. Wieser's device of a simple economy neatly avoided the criticism, while enabling him to discuss the relationship between wants and value.

After his preliminary discussion, Wieser next builds up theories of labor, capital and costs. He then moves to a theory of a social economy, examining institutions of exchange, product pricing under different market structures, the emergence and working of various factor markets, and the use of money. In his third major section he looks at public finance, or the economy of the state. In the last he examines the world economy. As I read *Social Economics*, it looks to me like Wieser is doing the same kind of (mostly market-by-market) equilibrium analysis that one might find in an intermediate price theory text. It differs in including money, which in today's textbooks is a macroeconomics topic; in including a considerable amount of institutional detail; and most dramatically in not including any graphs.

Is this general equilibrium theory? Because the Austrian analysis of costs emphasizes that the demand for factors of production depends on the prices of the goods that they will be used to manufacture, *Social Economics* does emphasize the

²⁴ Craver-1986, p. 12.

interdependence of factor and product markets. This led the historian of thought D.P. O'Brien once to describe the Austrians (not just Wieser, but Austrians *generally*) and followers like Lionel Robbins as engaging in general equilibrium analysis.

*"The focus of the analysis was upon equilibrium – general and not partial equilibrium. This led in turn to an approach to cost theory which was in terms of opportunity cost – which could only be analyzed in a general equilibrium framework."*²⁵

In a comment on O'Brien's paper, Mark Blaug criticized O'Brien's claim that the Austrians were doing general equilibrium theory.

*"...this is not general equilibrium theory in the sense of Walras but rather what I choose to call "total equilibrium" analysis. By "total equilibrium" analysis I mean any kind of economics that stresses the interdependencies between different markets and particularly factor markets and product markets. What Walras did was to interpret total equilibrium analysis in a particular way: he conceived the question of the existence of multimarket equilibrium in a capitalist economy as being analogous to the mathematical problem of solving a set of simultaneous equations, and it is for this reason that he became so preoccupied with the counting of equations and unknowns. It is perfectly possible to theorize about total equilibrium without committing oneself to the Walrasian scheme, and this is precisely what the Austrians did."*²⁶

I agree with Blaug's characterization of the Austrian approach. The Austrians in general were aware of the interdependence of markets. As good marginalists it was hard for them not to be, but this does not make them general equilibrium theorists. And even if one insists on using that language, there is no reason to use the criterion to distinguish Wieser from the other Austrians. If Wieser is a general equilibrium theorist, then so are other Austrians, including Mises.

I should add finally that there is one section of *Social Economics* that might be read as directly supporting Salerno's thesis. Section 10 of his treatment of the simple economy is titled "The Unity of the Economy," and in it Wieser looks at the interconnectedness of an economy in terms that certainly approximate a "verbal general equilibrium approach." Thus Wieser talks there about products being "bound together" so that "any change in the conditions affecting a single group of products must influence others thru the common factor of labor; for it follows that, as more or less effort is applied in one direction, less or more of it is available in others".²⁷ He provides an even fuller statement on the next two pages:

²⁵ O'Brien-1990, p. 158.

²⁶ Blaug-1990, pp. 185-86.

²⁷ Wieser-1927, p. 49.

*“Owing to this general unity, every considerable change in supply or demand that occurs within one stem will communicate its effect to all the others as well. ...Indirectly, most or all the other branches will be concerned... A far reaching adjustment will be made. The entire plan of production will require careful revision to reestablish the realization of the greatest possible total utility”.*²⁸

Significantly, Wieser follows this passage with one in which he comments upon the “economic statics” of “mathematical economists,” in whose number must be included Walras.

*“A number of mathematical economists have perceived this unity and have been led to try to exalt economic theory into an economic statics.... This static conception is useful to economic theory: it stresses most forcibly the unity of political economy. However, it threatens to introduce the methods of mathematical physics, which are not suited to the subject matter of economics. There is a further and even more serious risk that by the use of this method heterogeneous, though associated, ideas might creep in”.*²⁹

Wieser follows this with a few paragraphs on why the approach of the “mathematical economists” is less than congenial. Among the criticisms that he mentions is that “No adjustment is ever effected which tends to establish a condition of strict equilibrium, a perfect level”.³⁰ This suggests that, to the extent that static equilibrium theory suggests full and perfect adjustments, it is false. He also admonishes that “Rather than to economic equilibrium, theory should turn its attention to margins of use”.³¹

These sentences suggest to me that Wieser had a clear notion of the interdependence that pervades the economic system, but also that he felt that the general equilibrium type of framework that had been developed by Walras and others, one that stressed *equilibrium* conditions for the system, was less helpful than his own, which focused on the *constant marginal adjustments* made by agents.³² Wieser does not show any interest in answering the questions that are dear to the heart of Walrasian or Paretian general equilibrium theorists. He does not ask about the existence, or determinateness, or uniqueness of equilibrium. Wieser on my reading is best described as a systematic marginalist, one who understood about general economic interdependence but who thought that a

²⁸ *Ibid.*, pp. 50-51.

²⁹ *Ibid.*, p. 51.

³⁰ *Ibid.*

³¹ *Ibid.*, p. 52.

³² For this reason, perhaps a better term for describing the Austrian approach than Blaug’s “total equilibrium analysis” is “total adjustment analysis.” The latter emphasizes that the Austrians were most concerned with elaborating the process of adjustment that would follow a disturbance to a system. Mario Rizzo suggested the phrase at the Austrian Colloquium of New York University, and those assembled there agreed that it captured the Austrian approach better than Blaug’s “total equilibrium analysis.”

focus on multi-market equilibrium was not the best way to explore the nature of the system. I would not describe him as a general equilibrium theorist of any stripe. The phrase simply does not capture the essence of his work.

3. Was There a Schumpeter-Wieser-Hayek Axis?

Why, then, is Salerno so keen to identify Wieser as an advocate of general equilibrium theory, verbal or otherwise? As may perhaps be evident from the extended quotation from his paper that was reproduced earlier, Salerno's assertion about Wieser is embedded in a larger historical account. The way Salerno tells it, Schumpeter and Wieser (and their followers, among whom he includes Hayek) diverted the Austrian School from a path laid out by Menger and Böhm-Bawerk. Ludwig von Mises was finally able to recover this path through the publication of *Human Action*.

Salerno's claim, then, involves a retelling of the history of the Austrian movement. His revisionist account about Hayek's inclusion in a Weiser-Schumpeter dominated movement to promote general equilibrium theory is both controversial and impressively documented, so I'll offer a critique of it here, too. In my opinion, for all of his citations, Salerno offers a highly selective reading of history. He simply ignores a number of well-known but inconvenient facts.

For example, one would think that his account at least suggests that Menger, Böhm-Bawerk and Mises should be considered as part of a unified group, a group whose members should be differentiated from Schumpeter, Wieser, and Hayek. Now this sort of distinction would seem to imply, at a minimum, that greater affinities exist in the works of those who are members of the same group. But there are problems here. Menger and Böhm certainly do not form a seamless front. Menger disagreed with Böhm's capital theory, once apparently calling it "the greatest error ever committed".³³ For his part, Mises thought that, among other faults, both members of his putative group, Menger and Böhm-Bawerk, had been insufficiently subjectivist. He put it this way:

"...above all others, Menger and Böhm-Bawerk are the ones responsible for this misunderstanding of the theory. Neither understood it in all its ramifications, and both in turn were themselves misunderstood. The writings of Menger and Böhm-Bawerk include propositions and concepts carried over from the objective theory of value and therefore utterly incompatible with the subjectivism of the modern school".³⁴

³³ Schumpeter-1954b, p. 847, note 8. Given Salerno's thesis, perhaps the fact that it is Schumpeter who tells the story about Menger's opinion of Böhm's capital theory may be thought to cast doubt on the account. Endres (Endres-1997, p. 146) notes, however, that others have emphasized differences between the two on capital theory, and mentions five of these writers in a footnote.

³⁴ Mises-[1960] 1981, p. 167.

Salerno recognizes the divisions and handles them by claiming that there were “unresolved problems” within the Mengerian tradition that Mises solved in *Human Action*. But if this is the case, should Mises really be considered as part of (as opposed to as having corrected) the Mengerian tradition?

There are other problems with the “two camps” scenario. For example, there are some well-established affinities between the work of Mises and Wieser that Salerno ignores. Wieser’s 1903 inaugural address applied marginal utility analysis to money, an approach that Mises would follow in his book *The Theory of Money and Credit*. In addition, Wieser’s argument that even socialist economies would have to take into account factor returns anticipated Mises’ arguments about rational calculation under socialism. (Is Mises then a Wieserian?) Furthermore, if there was an heir to Menger’s idea that social institutions are the unintended consequences of human action, one can as plausibly argue that it is Hayek rather than Mises. And for his part, Schumpeter is probably best considered as occupying his own unique place quite separate from all the rest. To place these economists into two distinct groups based on their economics is problematical if not impossible.

But perhaps it wasn’t economics that Menger, Böhm-Bawerk and Mises were supposed to have shared in common. In his paper, Salerno tends to identify members of the Mengerian tradition as following a common methodological approach. But there are problems too with this sort of division. Most recent studies of his methodology identify Menger as an Aristotelian realist.³⁵ Mises’ views are much harder to pin down. Lachmann, for example, expressly denied any Aristotelian influence in Mises’ work, and even those who have posited its existence admit that it coexists uneasily with the neo-Kantian elements in Mises’ thought.³⁶ Mises³⁷ himself noted the similarities of his own methodological views to those of his putative opponent Wieser, a point that Terence Hutchison³⁸ picked up on. And for his part, Böhm-Bawerk, who is supposedly in the Menger-Mises camp, disavowed *a priorism*, which was of course a fundamental part of Mises’ position.³⁹

Another of Salerno’s claims intended to support the “two camps” thesis is that there was a split in the Austrian camp in the 1920s, and that “Hayek always considered himself an adherent of the ‘Wieser tradition’ rather than the Böhm-Bawerk (and Mises) tradition”.⁴⁰ The sentence carries a footnote, “Hayek explicitly

³⁵ There is some controversy in the secondary literature about Menger’s debt to Aristotle, but as the reviews of these debates by Cubeddu-1993 and Oakley-1997, Oakley-1999 show, an emerging consensus seems to see an Aristotelian influence.

³⁶ Lachmann-1982, pp. 35-36; Oakley-1997, chapter 7.

³⁷ Mises-[1960] 1981, pp. 21-22.

³⁸ Hutchison-1992, pp. 20-21.

³⁹ Hutchison-1981, p.204, provided this translation of Böhm’s views.

“The best known among the doctrines of the abstract-deductive school is the theory of value based on final utility. How did they arrive at it? By some soaring *a priori* speculation? Not at all. In the first place they simply observed how men practically regard property.”

⁴⁰ Salerno-1999, p. 43.

distinguished between 'the two original branches of the Austrian School,' the Böhm-Bawerkian and the Wieserian, and characterized himself as an adherent of the latter branch."⁴¹

Hayek discussed the question of how much of a split existed among economists in Vienna in the 1920s at length in his Oral History interviews, which Salerno does not cite. Salerno is right that Hayek distinguished between "the Böhm-Bawerk tradition and the Wieser tradition – and Mises was representing the Böhm-Bawerk tradition, and Meyer the Wieser tradition".⁴² His interviewer, Axel Leijonhufvud, asked if the difference in "traditions" was based political or ideological grounds, and Hayek responded that those sorts of differences were not fundamental. His interviewer then asked:

"Leijonhufvud: So what were the differences, then, between the Mayer circle and the Mises circle?"

Hayek: Oh, things like the measurability of utility and such sophisticated points. Wieser and the whole tradition really believed in measurable utility".⁴³

A little later, Leijonhufvud pressed Hayek further:

"Leijonhufvud: But that doesn't explain a split between the two groups.

Hayek: Oh, there wasn't really. You see, Mayer — and also Rosenstein, perhaps — kept away from the Mises circle for political reasons. There were no very good Mayer pupils. I mean, Mayr, who became his successor, while a very well-informed person, was really a great bore".⁴⁴

Now this looks like Hayek was reversing his own previous claim that political differences were not the source of the split. Understandably, a bit later Leijonhufvud pressed him again:

"Leijonhufvud: So my previous question was: Was there an Austrian School? And you said yes, definitely.

Hayek: Theoretically, yes.

Leijonhufvud: In theory.

Hayek: In that sense, the term, the meaning of the term, has changed. At that time, we would use the term 'Austrian School' quite irrespective of the political consequences which grew from it. It was the marginal utility school which to us was the Austrian School".⁴⁵

⁴¹ Hayek-1983b, pp. 17-18; Hayek-1992, pp. 108-09, p. 157.

⁴² Hayek-1983a, pp. 49-50.

⁴³ *Ibid.*, p. 51.

⁴⁴ *Ibid.*, p. 52.

⁴⁵ *Ibid.*, p. 55.

As the exchanges above show, Hayek was not always crystal clear when it came to his reminiscences. In particular, at first he denied that the differences between the two branches were based on politics, and then he seemed to reverse himself.

But there may be a way to make sense of all this. I think that the best way to read the apparent reversal is to interpret the second reference as being to “academic politics” rather than “politics” proper. Read this way, what Hayek was referring to was the *academic rivalry* that existed between Mises and Hans Mayer. Mayer was jealous of Mises, whose seminar (which was not even attached to the University) was attracting all the good students, while he, the occupant of Wieser’s chair, was only able to attract the likes of Franz Mayr. The Austrian school in the 1920s was united in terms of their theory (this is what Hayek meant when he said there *was* an Austrian school, “theoretically”), for they all believed in the subjectivist marginal utility approach. Their divisions had mostly to do with academic politics, and peripherally with such theoretical niceties as the measurability of utility. It was only later that the Austrian school came to be associated with a particular political stance. And the split that existed in the 1920s was between Mayer and others, including Mises, not between a Schumpeter-Wieser-Meyer-Hayek general equilibrium wing and a Misesian wing.

This reading makes even more sense if we consider what a strange bird Hans Mayer was. Most Austrians remembered him less for his economics than for his faults. Hayek spoke disparagingly of his inability to do any serious work after having acceded to the chair, describing him in one interview as “a coffee-house man”⁴⁶ and in another⁴⁷ as “a severe neurasthenic,” the latter being an obsolete psychiatric term used to describe someone with depression due to nervous exhaustion. In addition to his infantile hostility towards perceived competitors like Mises and Othmar Spann, Mayer is also remembered for his despicable performance during the *Anschluss*: in the week following Hitler’s arrival in Vienna, Mayer sent out a letter as the President of the National Economics Association dismissing all of the Jewish members.

Anyway, one of Salerno’s chief claims here is that “Hayek always considered himself an adherent of the ‘Wieser tradition’...”⁴⁸ Whatever the nature of the splits in the 1920s, this seems to me to be an almost willful misreading of the text. If we look at Salerno’s own sources, what Hayek said was this:

*“I came originally from the other of the two original branches of the Austrian School. While Mises had been an inspired pupil of Eugen von Böhm-Bawerk, who died comparatively early and whom I knew only as a friend of my grandfather before I knew what the word economics meant, I was personally a pupil of his contemporary, friend, and brother-in-law, Friedrich von Wieser.”*⁴⁹

⁴⁶ Hayek-1983a, p. 38.

⁴⁷ Craver-1986, p. 9.

⁴⁸ Salerno-1999, p. 43.

⁴⁹ Hayek-1983b, p. 17.

Hayek is simply pointing out that while Mises was Böhm-Bawerk's student, he had himself been a student of Wieser's, Böhm having died before Hayek got to university. A little further on in his account Hayek notes that he soon began working with Mises, and that in the decade that followed "he certainly had more influence on my outlook of economics than any other man".⁵⁰ As is well known, Hayek's economic research, first in monetary theory, later on the economics of socialism, followed Mises' lead during most of the interwar period. When taken together with Hayek's own dismissive remarks about Mayer, it is hard to know on what basis Salerno could reach the conclusion that Hayek "always considered himself an adherent of the 'Wieser tradition'."

So, there was a split of sorts in the Austrian school in the 1920s. It appears to have been between Mises and Mayer, and principally a matter of personalities and of the petty politics of the academy. (It was Henry Kissinger, I think, who once said that academic politics are the most nasty and intense kind, because the stakes are so small.) Hayek himself, characteristically, never had much patience for personality conflicts, and tried to reduce the tensions by reconstituting the Austrian Economic Association in the later 1920s.

"The reason why I then took the initiative of reconstituting [the association] was because I rather regretted the division which had arisen between the Mises and the Mayer circle. There was no forum in which they met at all, and by restarting this no-longer existing society there was at least one occasion where they would sit at the same table and discuss".⁵¹

There were in addition to the personal antagonisms some differences in theoretical emphasis, but for Hayek and others, the label "Austrian school" in the 1920s essentially meant "marginal utility analysis." This brings us to yet another claim by Salerno: that because he was a proponent of a "fusion" of approaches, the "effective dissolution of the School in the 1930s was approvingly cited by Hayek in 1968 as evidence of the School's triumph".⁵² To support this claim, Salerno quotes a statement by Hayek noting that by the 1930s most of the economics profession had reached a general agreement concerning microeconomic theory.

If this be apostasy, one wonders what Salerno would make of the following statement, taken from a speech that Ludwig von Mises delivered on September 30, 1932 at the meeting of the *Verein für Sozialpolitik*:

"We usually speak of the Austrian and the Anglo-American Schools and the School of Lausanne.... [Yet it is a] fact that these schools of thought differ only in their mode of expressing the same fundamental idea and that they are divided more by their terminology and by peculiarities of presentation than by the substance of their teaching".⁵³

⁵⁰ *Ibid.*, p. 18.

⁵¹ Hayek-1983a, p. 45.

⁵² Salerno-1999, p. 57.

⁵³ Mises-[1960] 1981, p. 214.

Or this one, taken from his last book, talking about the microeconomic theory of the 1920s:

“...one no longer distinguished between an Austrian School and other economics. The appellation ‘Austrian School’ became the name given to an important chapter in the history of economic thought; it was no longer the name of a specific sect with doctrines different from those held by other economists”.⁵⁴

Mises’ statement at the meeting of the *Verein* may have been a summary of the state of the discipline at the time, at least as he saw it. On the other hand, Kauder⁵⁵ notes that “of the ten essays which were prepared by the participants of this meeting, only three were consistently in favor of the Austrian value theory,” which suggests that Mises’ claim of disciplinary unanimity may have been more strategic, or an attempt to rally the troops, than descriptive. His later reminiscence, though, seems to support the notion that, at least in Mises’ memory of it, in the interwar period most Austrians no longer viewed themselves as a distinct school. In any case, that Hayek should later repeat Mises’ own claim hardly constitutes evidence of his celebrating the dissolution of the Austrian school. Indeed, in this light it is ironic that in 1932 an article appeared that sought to promote the causal-genetic approach of the Austrian school over the functionalist approach of Walras. The author of this tract designed to show the distinctiveness of the Austrian approach was not (as one might expect from Salerno’s claims) Ludwig von Mises. It was Wieser’s successor, Hans Mayer!

4. Why Did Hayek Praise “Equilibrium Theory”?

A loose end remains to be tied up. In *Monetary Theory and the Trade Cycle* Hayek insisted that any adequate theory of the cycle must be consistent with the results of “static equilibrium theory.” If it wasn’t Wieser or Schumpeter who brought him to these views, why was he so keen to endorse equilibrium theory in his trade cycle writings?⁵⁶

Monetary Theory and the Trade Cycle was first published in 1929 in German, and it is best read as being aimed at a German audience, and in particular, at German cycle theorists. By the 1920s in Germany, the legacy of the historical school was being finally thrown off, so that the notion of a German economic *theorist* was no longer a hopeless oxymoron. There was still considerable resistance there, however, to *monetary* theories of the cycle. Germans associated

⁵⁴ Mises-1969, p. 41.

⁵⁵ Kauder-1965, p. 111.

⁵⁶ I say “trade cycle writings” because Hayek also expressed at least some reservations about the use of static equilibrium theory in his paper on intertemporal equilibrium (Hayek-1984b).

monetary theories with crude quantity theories, and these were taken to be an Anglo-American vice of sorts, one that had been appropriately discredited by earlier generations of critics. Hayek shared this opinion about quantity theories, but he faced a dilemma, since his own theory was a monetary one. His message to his German counterparts might be summarized as reading: Don't throw out the monetary theory baby with the quantity theory bathwater.

Hayek's argument was classical in form: he used his opponents' own stated beliefs to establish the grounds for accepting the Austrian theory of the cycle. His first step was to make common cause with German cycle theorists against the latest developments in American economics. He did this by enlisting the methodological writings of the leader of the Kiel School, a young and brilliant German cycle theorist named Adolf Löwe, against the empiricist claims of the institutionalists.⁵⁷ Next, he added a novel methodological twist to Löwe's position, in a sense turning it on its head, a move that enabled Hayek to argue for the necessity of a *monetary* theory of cycle. Finally, by adding the (very Austrian) requirement that an adequate theory of the cycle must incorporate the idea of *changing relative prices*, he was able to exclude those monetary theories that are expressed solely in terms of aggregates. By a process of elimination, his own preferred theory was the only contender that remained.

Hayek's claims about the limitations of empirical work were taken directly from Adolf Löwe's 1926 article, "How Is Business Cycle Theory Possible at All?"⁵⁸ So too was his claim that any adequate theory of the cycle must be consistent with, and presuppose the validity of, standard equilibrium theory.⁵⁹ Hayek assumed here that standard equilibrium theory was widely accepted within the profession, that the theory provided the axiomatic foundation for economics. If that was true, then:

"We cannot superimpose upon the system of fundamental propositions comprised in the theory of equilibrium, a Trade Cycle theory resting on unrelated logical foundations. All the phenomena observed in

⁵⁷ See Hagemann (Hagemann-1994) for the importance of his debates with Löwe and other members of the Kiel School in the development of Hayek's thought. Löwe's article was recently translated (Löwe-1997), and is preceded by an introduction by the translator, Christian Gehrke. Löwe left Germany for England in 1933, changed his name to Adolph Lowe in 1939, and in 1940 left England for a permanent position as Professor of Economics on the Graduate Faculty at the New School for Social Research in New York. Both Alfred Schütz and Felix Kaufmann were also on the faculty at the New School, and Forstater (Forstater-1997, p. 158) notes that Schütz influenced Lowe in his later methodological writings.

⁵⁸ Löwe had reviewed recent work on the cycle, both theoretical and empirical, in a 1925 paper, and wrote the 1926 article to explore the methodological question of what a *theory* of the business cycle must look like. The first few pages of Löwe's article read much like Hayek's discussion of the institutionalists in his paper on the American economy (Hayek-1984c). Thus, Löwe thanked Mitchell and the institutionalists for deepening our knowledge of typical regularities and patterns in the data, but went on to criticize them for claiming to have thereby made any contribution to theory. His commentary on the proper relationship between empirical and theoretical work followed (Löwe-1997, pp. 246-48).

⁵⁹ Cf. Löwe-1997, 235.

cyclical fluctuations, particularly price formation and its influence on the direction and volume of production, have already been explained by the theory of equilibrium; they can only be integrated as an explanation of the totality of economic events by means of fundamentally similar constructs".⁶⁰

But if Hayek's argument about the necessity of building one's cycle theory on existing foundations found its origins in the work of Löwe, his next step was wholly original. Non-monetary theories of the cycle usually posited the same proximate cause of the cycle, namely, a disproportionality that arises between the production of capital goods and consumer goods during the boom phase. But a question naturally follows: If one accepts the results of equilibrium theory, if one assumes that prices automatically equilibrate markets, or that full equilibrium obtains, how could such a disruption take place? Hayek's answer was clear. No such disruption could take place, *unless* one was willing temporarily to abandon equilibrium theory to generate a cause of a cycle. That move, of course, would violate the methodological tenet that one must utilize equilibrium theory. Even worse, after abandoning equilibrium theory to *generate* the cycle, proponents of non-monetary theories would then have to return to it to trace out the cycle's trajectory, involving themselves in a double contradiction.

"None of them is able to overcome the contradiction between the course of economic events as described by them and the fundamental ideas of the theoretical system which they have to utilize in order to explain the course".⁶¹

How might one get out of this dilemma? It is precisely here that Hayek's answer deviated from that provided by Löwe. Löwe claimed that the problem identified above proved that a static equilibrium system could never be used to explain the business cycle. He concluded that an alternative, *dynamic* theory was therefore required.

"The business cycle problem is not a reproach for, but a reproach against the static system, because in it is an antinomic problem. ... Those who wish to solve the business cycle problem must sacrifice the static system. Those who adhere to the static system must abandon the business cycle problem. ... The transformation of our existing static system into a dynamic one thus appears to be required from open problems of economic theory over the entire field".⁶²

⁶⁰ Hayek-[1933] 1966, pp. 28-29.

⁶¹ Hayek-[1933] 1966, p. 52; cf. p. 87.

⁶² Löwe-1997, pp. 267-68, emphasis in the original.

Hayek on the other hand argued that the static system of equilibrium could be retained in a theoretical explanation of the business cycle. But to do so, one had to “widen the assumptions” of standard theory by introducing money into the system.

*“The obvious, and (to my mind) the only possible way out of this dilemma, is to explain the difference between the course of events described by static theory (which only permits movements towards an equilibrium, and which is deduced by directly contrasting the supply of and the demand for goods) and the actual course of events, by the fact that, with the introduction of money (or strictly speaking with the introduction of indirect exchange), a new determining cause is introduced. Money being a commodity which, unlike all others, is incapable of finally satisfying demand, its introduction does away with the rigid interdependence and self-sufficiency of the ‘closed’ system of equilibrium...”*⁶³

Hayek’s early touting of equilibrium theory, indeed his insistence that any adequate theory of the cycle had to be built up from equilibrium theory’s foundations, was thus part and parcel of his ingenious twist on Löwe’s brief for the necessity of a dynamic theory of the cycle. Hayek used Löwe’s arguments to defend the Austrian monetary theory of the cycle. But to do so he had to argue, as Löwe had, for the necessity of equilibrium theory. This, then, is the origin of Hayek’s early fascination with equilibrium theory, and pretty clearly, Schumpeter and Wieser had nothing to do with it.

⁶³ *Ibid.*, p. 44.

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