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Dispute on Method or Dispute on Institutional Context? Foreword to the Translation of Carl Menger's “Errors of Historicism”

Karen Horn¹ and Stefan Kolev²

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As one of the archetypal disputes in the social sciences, the Methodenstreit (dispute on method) in German economics which unfolded during the 1880s has received substantial attention from multiple angles over the past 140 years. For decades it has constituted a fixture in German-language political economy. One reason for this, beyond the unusually vitriolic tone of the dispute, are the fascinating particular issues of methodology, morphology, and epistemology addressed in its course. Also, many scholars say it shows a dismal disposition to wage more or less abstract, ultimately fruitless methodological wars, instead of addressing pressing real-world problems in a pragmatic manner.³ It is sometimes said to reflect an obsession in the social sciences specifically German (and Austrian, for that matter). Indeed, for good or bad, there is a rich history of such disputes, making *Streite* a trademark of the German social sciences.

1. University of Erfurt, 99089 Erfurt, Germany.

2. University of Applied Sciences Zwickau, 08056 Zwickau, Germany.

3. See, for example, Schumpeter 2006/1954, 782–783; Köster 2011, 121–141; Glaeser 2014, 135–152.

German disputes all over and a canonical Austrian text

A series of disputes extends (at least) from the 1880s Methodenstreit to the Werturteilsstreit (dispute on value judgments in academic inquiry) during the 1900s–1910s, to the Positivismusstreit (dispute on the nature of the social sciences as well as on the status of values) between the Frankfurt School and the critical rationalists during the 1960s, and to what was termed the ‘most recent Methodenstreit’ in 2009–2010 on the legacy of ordoliberalism in German economics departments. To this list may be added the meta-*Streit* over whether these disputes have contributed to the depth and the problem-solving capacity of the social sciences or whether they have instead congested the scholarly discourse with free-floating abstractions and thereby prevented more important applications of scholarly energy. Not very tender in his judgments, Joseph Schumpeter, in his *History of Economic Analysis*, deplors the “large amount of mutual misunderstanding” and considers the whole stream of literature that ran from this dispute as “a history of wasted energies, which could have been put to better use” (2006/1954, 782–783).

The main opponents in the Methodenstreit of the 1880s were Carl Menger (1840–1921) and Gustav Schmoller (1838–1917). Menger, one of the founding fathers of the Austrian School, is known for his contributions to the subjective theory of value and marginalism (e.g., Caldwell 1990; Hagemann et al. 2010). He began his career in journalism (Yagi 1992, 93–98), but then deepened his studies of political economy leading to the publication of his *Grundsätze der Volkswirtschaftslehre (Principles of Economics)* in 1871, which afforded him a habilitation at the University of Vienna. He sought a unified price theory based on a single principle, arriving there by combining an empirical behavioral theory of subjective evaluation and methodological individualism, considering social facts, processes, and institutions as unintended results of otherwise purposeful individual action. Taken alone, none of these elements were altogether new, and to some Menger thus “appeared a little old-fashioned and outdated” (Streissler 1990, 44).

In 1873, Menger was named professor at the age of 33, and in 1879 he became a full professor and held the chair of economic theory at the University of Vienna (Streissler 1988, 197). Starting in 1876, for several years he also served as a tutor of Crown Prince Rudolf (Streissler 1994). Menger’s lectures to the Prince drew especially from Adam Smith’s *Wealth of Nations*, and he expressed his affinities with Smith’s legacy in an essay commemorating the centenary of Smith’s death (Menger 2016/1891).

Menger was an erudite man, versatile in a multitude of fields, but nevertheless a shy, rather fearful person (Bonn 1953, 62–63). He probably never meant to engage in a vitriolic controversy or to distance himself from his colleagues in German political economy when he published his work, as he considered himself neither a rebel nor a revolutionary. He identified his approach as a “product of recent development in *German* political economy,” emphasizing that “the reform of the most important principles of our science here attempted is therefore built upon a foundation aided by previous work that was produced almost entirely by the industry of *German* scholars” (Menger 1976/1871, 49, his italics).

While his *Principles* received friendly but not overwhelming reviews (Streissler 1990, 39 n.23), it seemed to Menger that the importance of the new combination in his approach still escaped some of his readers, and so in 1883 he published a book which can be seen as a sequel, *Untersuchungen über die Methode der Socialwissenschaften und der Politischen Oekonomie insbesondere (Investigations into the Method of the Social Sciences with Special Reference to Economics)*, focusing even more than before on methodology. Menger strongly insisted on distinguishing the historical and statistical study of economic forms both from theoretical economics and from practical fields such as economic policy and public finance. He thought that the historical study of economic structures, while useful, was only complementary to theory and could not seek to replace it (Menger 1985/1883, 41–43).

Gustav Schmoller, the influential leader of the Younger German Historical School and a proponent of an ‘inductive’ approach to economic thinking, then penned an unfavorable and in some parts personally insulting review of Menger’s *Investigations* (Schmoller 1883).⁴ Menger had disingenuously claimed that he wrote as a friend to the Historical School, but his books were “very obviously intended to vindicate the rights of theoretical analysis and to put the Schmoller school in its place—and a very secondary place it was!” (Schumpeter 2006/1954, 782). At least to some extent, Schumpeter says, “the quarrel was about precedence and relative importance” (ibid.). Simon Cook and Keith Tribe sum up the substance of the dispute as follows: “Menger argued, on the one hand, that theory was not susceptible to inductive elaboration—something which Schmoller in truth never even attempted—while Schmoller asserted, on the other, that it was, but never sought to demonstrate how” (Cook and Tribe 2016, 303).

At the time of the dispute, Schmoller had just moved to Berlin to teach at the famous Friedrich-Wilhelms-Universität (today Humboldt-Universität), after having held chairs at Halle and Strasbourg (Balabkins 1988, 18–44). He exerted much of his academic influence through the Verein für Socialpolitik, the

4. On the Younger German Historical School see, for example, Rieter 2002; Peukert 2001. For a criticism of such labeling, see Backhaus and Hansen 2000.

main—and still existing—association of German-language economists, which he had co-founded in 1872 and chaired from 1890 to 1917 (Grimmer-Solem 2003, 171–202), as well as through editing the *Jahrbuch für Gesetzgebung, Verwaltung und Volkswirtschaft im Deutschen Reich*, an important journal (a quarterly, its name notwithstanding) on law, administration, and political economy which Schmoller edited from 1877 to 1917 and which in 1913 was renamed *Schmollers Jahrbuch* (McAdam, Kolev, and Dekker 2020). Schmoller, whose “petty bourgeois appearance could not always hide his passionate temperament and by no means harmless cleverness,” was so powerful that most academic appointments in political economy were based on his recommendations (Bonn 1953, 51, 53). Even in Austria-Hungary the Historical School had a firm grip on professorial positions (Blumenthal 2007, 66–75). Schmoller cooperated effectively with Friedrich Althoff, the near-almighty administrator of the Prussian academic system whose ‘Althoff system’ shaped the young Empire’s academic system (at least) from 1882 to 1907 (Backhaus 1993).

In response to Schmoller’s criticism, which upset him deeply, Menger prepared his second major contribution to the Methodenstreit, composed in the style of a collection of letters to a friend under the title *Die Irrthümer des Historismus in der deutschen Nationalökonomie* (*The Errors of Historicism in German Economics*, hitherto abbreviated as *The Errors of Historicism*). He submitted it to the press already in January of the following year (Menger 1884). This pamphlet, considered by some “a piece of masterly academic polemics” (Campagnolo 2016, 290), “fairly steamed with wrath and of course elicited rebuttal” (Schumpeter 2006/1954, 782). Schmoller himself, though, refrained from engaging in the dispute a second time in substance. In a short announcement in his *Jahrbuch*, however, he reprinted a letter he had sent to Menger, explaining that it was against his principles to engage in such personally offensive “literary armed conflicts” and that, instead of “destroying such a beautifully designed booklet,” he had sent it unread back (Schmoller 1884, 677; Bostaph 1994, 460).

This first battle did not decide the war of the Methodenstreit that had just started. In the decades to follow, the next generations kept discussing the issues which erupted in the early 1880s (Pribram 1983, 222–224), and the Werturteilsstreit during the 1900s–1910s is often called ‘the younger Methodenstreit’ due to the continuities in its agenda to ‘the older Methodenstreit’ of the 1880s (Backhaus and Hansen 2000, 312–313).

Beyond the German-language disputes, *The Errors of Historicism* has constantly been quoted in the burgeoning literature about the Austrian School since the ‘Austrian revival’ during the 1970s. But given today’s rather limited command of German in academia, it is one of those works that many have heard about but almost none have read. In light of the explosion of historiographical efforts related

to the evolution of the Austrian School, it is quite surprising that the text—under 100 pages in the German original—has not previously been fully translated into English. Only a notable English-language “digest” which “paraphrased and abbreviated” the book’s main arguments was produced by Albion Small (1924, 587–597).

In the process of creating and editing this first translation of *The Errors of Historicism*—an effort that is part of a larger project of the NOUS Network for Constitutional Economics and Social Philosophy—we have collected a multitude of observations, both inspiring and frustrating. In this foreword to the translation, we embed those observations within our reflections on the nature and value of the Methodenstreit.

Is *The Errors of Historicism* more than an underdog’s polemic?

The understanding that the Methodenstreit was unproductive may have taken root precisely because of the polemical character of *The Errors of Historicism*. One may therefore ask whether it was the very publication of *The Errors of Historicism* that derailed the dispute, so that a potentially useful controversy was led into a dead end. A look at Schmoller’s review of the *Investigations*, i.e., at the piece to which Menger reacted in *The Errors of Historicism*, is necessary at this point. While Menger’s *Investigations* was translated in 1963 on the 80th anniversary of its publication and have thus been accessible to English-speaking scholars for a long time, Schmoller’s review unfortunately remains untranslated to this day. The NOUS Network for Constitutional Economics and Social Philosophy is currently translating it for a forthcoming publication in the *Journal of Contextual Economics—Schmollers Jahrbuch*, which is about to reach the 150th anniversary of its founding.

In his review, Schmoller (1883) jointly discusses and contrasts Menger’s *Investigations* and Wilhelm Dilthey’s first important theoretical work, *Introduction to the Human Sciences* (Dilthey 1883). The text begins with Schmoller’s outright admission that in the case of Menger’s book he “cannot quite contain the polemic” since he feels that Menger’s “attacks are in part strikes at me personally” (Schmoller 1883, 975).⁵ Focusing his review on Book One of the *Investigations*, Schmoller portrays the book as an attack on “the scholarly division of labor” between the theoretical direction in economics and the collection of “descriptive material” (ibid., 978). After criticizing Menger’s “unworldly ivory tower naivete,” Schmoller utilizes again his notion of the scholarly division of labor to demonstrate how

5. We are responsible for the translations of quotations from Schmoller in this foreword.

earlier generations in German political economy, embodied by Karl Heinrich Rau and Friedrich von Hermann, were unable to found their theories on sound theoretical foundations because of an unwillingness to put away “the blinders of division of labor in specialized sciences” and to work, in today’s terms, along interdisciplinary lines with professional psychologists (ibid., 979). He reads Menger as pushing economics towards disembedding the study of the economy from its institutional context—from “state and society, custom and law” (ibid., 980). This turns out to be a crucial issue which we repeatedly sensed in the translation and editing process of *The Errors of Historicism*. The dispute on method seems to be after all a dispute on the meaning of concepts for demarcating what economics and its sub-domains should be about.

Schmoller concedes that the institutional context does not matter “if theoretical economics is above all confined to the theory of value and price formation, of income distribution, and of monetary issue”—but in his own conceptual demarcation of economics, such a notion of economics “does not capture the general essence of the economy” (Schmoller 1883, 980). As one of the “empiricists digging in the dust,” Schmoller sees himself disparaged by Menger’s alleged willingness to elevate his “set of secured deductive conclusions” to a status against which “empirical reality can be put aside with contempt as something irrelevant” (ibid.). Schmoller shares the aim of “a theory of the general essence and general relationships of the economy,” but he is skeptical about the isolation methods tied to Menger’s abstraction techniques (ibid., 981).

Schmoller (1883, 981) calls for a broad foundation of the social sciences, with a special emphasis on “what is prerequired” before economic theory can be adequately applied to its proper domain. Crucially for him, the complex inter-relatedness of these institutions calls for a “collectivist” capturing of that inter-relatedness in the institutional context. His perception of the difference existing between him and Menger is probably best captured in the following juxtaposition:

This [the necessity of a collectivist view of the interrelated institutions] is inaccessible to him [Menger] because he exclusively starts from the singular observation of the single economy,⁷ always only thinks of exchange, value, money, etc., but not of the economic⁸ organs and institutions which constitute the bone skeleton of the economic body. (Schmoller 1883, 983)

Schmoller’s accusation concerning the institutional disembeddedness in Menger’s approach culminates in reprimanding him for presupposing the institu-

6. *Volkswirtschaft*

7. *Einzelwirtschaft*

8. *volkswirtschaftlich*

tional snapshot of Western Europe as the context for his theory, thus sharing, “with the very old dogmatic economics,” “the great methodical error to assume the essence of his time to be the general essence of the economy⁹” (Schmoller 1883, 983). Regarding Menger’s view of institutions, he is highly skeptical about Menger’s one-sided insistence on what later generations termed ‘methodological individualism’ and ‘unintended consequences’ of human action in the establishment and evolution of institutions—an inclination which Schmoller attributes to Menger’s alleged “Manchesterian antipathy against any conscious activity of the collective organs of society” (ibid., 986). The word *Manchesterism* for Schmoller and others at the time functioned much like the word *neoliberalism* does today, as an ill-defined pejorative used to cut down ideological adversaries.

Beyond economic theory, the debate between Menger and Schmoller also concerns the two other traditional sub-domains within German economics. While the terms vary, the sub-domains as captured by the three main courses during Max Weber’s tenure starting in 1897 at Heidelberg, one of the top universities in the German-language area, are demarcated as follows: ‘Allgemeine oder Theoretische Nationalökonomie’ (general or theoretical economics), ‘Praktische Nationalökonomie’ (practical economics, focusing on various domains of economic policy) and ‘Finanzwissenschaft’ (public finance) (Heilmann 2017, vii). Menger and Schmoller specifically disagreed on the extent to which economic policy and public finance can and have been, in Schmoller’s view, “elevated to the rank of theoretical sciences” (Schmoller 1883, 981), as opposed to being “only” arts. Arts, in Schmoller’s understanding, can only give practical advice on the basis of ideologies such as “free trade” or “state socialism” (ibid., 982). Schmoller insists that the distinction between of theoretical and practical economics is fundamental for Menger, while he himself considers that it can only be a gradual one. As far as Menger is concerned:

A complete system of the actual economic sciences will [...] comprise the following: 1. The historical sciences of economics: economic statistics and economic history. The former has to investigate concrete economic phenomena from the point of view of statics and within definite spatial limits, while the latter has to study them from an evolutionary standpoint and to combine them into a unitary, organic structure. 2. The morphology of economic phenomena, whose function consists in the classification of economic facts in accordance with their genera, species, and subspecies, as well as the demonstration of their generic form, i.e., the description of the common structures of different groups of homogeneous phenomena. 3. Economic theory, which has the task of investigating and establishing the

9. *Volkswirtschaft*

laws of economic phenomena, i.e., the regularities in their coexistence and succession, as well as their intrinsic causation. I have already called attention to the appropriateness of combining the morphography of basic economic phenomena with economic theory. 4. Practical or applied economics, which teaches us the principles and procedures by which generally determined economic aims may be most effectively realized in different circumstances and in the light of existing scientific knowledge. (Menger 1960/1889, 14)

Critical reviews are seldom a delightful read for an author. But ideally, with a fair degree of intellectual openness, they can prompt an author to think twice, to reconsider and improve. That is what academic discourse is all about, and this is essentially how intellectual progress can come about (Horn 2009; Vanberg 2010). However, for this competitive process to be productive, a certain degree of politeness in discourse is a prerequisite: Scholars are human beings, after all. In that sense, Menger’s drastic reaction to Schmoller’s critical review may have been triggered not so much by the analytical differences and methodological accusations but rather by Schmoller’s concluding comments, which depict Menger as a person who is “a sharp dialectician, a logical mind, not an ordinary scholar,” but who “lacks as much a general philosophical and historical education as the natural width of horizon” (Schmoller 1883, 987). But does that imply, in turn, that *The Errors of Historicism* is nothing more than the polemical pamphlet of an underdog in Vienna wounded by the harsh, insulting review that his work has received from a star? While there is something to that, we do not think it is the full story. *The Errors of Historicism* deserves to be considered as one element of a chain of arguments that ultimately had a long-lasting impact upon the evolution of the discipline.

During the process of translating and editing, whose details we will describe in the next section, our group made a few observations about Menger’s content, style, and possible audiences which we would now like to briefly share. Our central impression is that after Schmoller’s review, the dispute was no longer primarily about *methods* in the sense of the textbook story about ‘induction versus deduction’ or something like that. Schmoller later acknowledged succinctly that these two approaches are “like the two legs of a walking man” (1898, 222). Rather, our impression is that what was at stake is the following: (1) *the demarcation lines within the sub-domains of economics* and (2) *the separability of the results of economic theory from the institutional context*. In other words, Schmoller accused Menger of an exaggerated emphasis on what has recently been called “isolating economics” focusing on the market process, while he saw himself as the true representative of a “contextual economics” focusing on the institutional framework around the market process (Goldschmidt, Grimmer-Solem, and Zweynert 2016).

The struggle seems to have been about defining the meaning of concepts and about demarcating theoretical economics, economic policy, public finance,

et cetera. In this reading, the dispute is very much—as explicitly noted above by Schmoller in the less polemical earlier parts of his review—about the *division of labor* within economics and across the interdisciplinary borders to neighboring social sciences like law, history, or geography. Two phrases are particularly recurrent in slight variations throughout Menger’s *Errors of Historicism*: a question whether, in Schmoller’s words, practical economics “can completely strip off the garment of an art,” and a question about “political economy and its parts, the understanding of the relationships between its parts and to their auxiliary disciplines.” Later, Schmoller would write solomonically that “the demarcation of the subject matter and the systematization of each science depend on its inner condition and the practical purposes of its efficacy and teaching” (Schmoller 1949/1893, 15).

But to whom were these attempts at demarcation directed? From the names featured both in Schmoller’s review and in Menger’s *Errors of Historicism*, it is crystal clear that the struggle is not all-encompassing but concerns specifically the soul, and control, of *German* economics. It is still very much a national affair. English or French names hardly ever arise at this stage of the dispute. The notion of Austrian economics is non-existent in the texts, let alone that of an Austrian School—Menger very obviously craves to be considered a full member of the *German* tradition, nothing else. The aforementioned preface to his *Principles*, amounting to an almost servile bow, demonstrates this almost painfully: “Let this work be regarded as a friendly greeting from a collaborator in Austria, and as a faint echo of the scientific suggestions so abundantly lavished on us Austrians by Germany through the many outstanding scholars she has sent us and through her excellent publications” (Menger 1976/1871, 49). In his endeavor to attach himself closely to German economics, Menger neglects many Austrian-born authors or German-born economists teaching in Austria (Streissler 1990, 35). After attacking Schmoller, Menger’s wrath would turn against F. J. Neumann, Lujo Brentano, and Friedrich Kleinwächter, all of whom also took “the opportunity to present their methodological views, which in part are in direct opposition to my own” (Menger 1960/1889, 23).

As the Methodenstreit unfolded, however, rival endeavors at school-building came to clash. The Historical School had emerged decades earlier—at the very moment of the Methodenstreit it was already in the mode of transformation from the dominance of the Older Historical School earlier headed by Wilhelm Roscher, Bruno Hildebrand, and Karl Knies to the Younger Historical School that came to be dominated by Schmoller. Menger’s group, however, was only about to form in the early 1880s (Wasserman 2019, 37–48). In 1884, Schmoller was 46 years old, while Menger was 44—the ideal age of mid-career scholars to start building their networks of students. This is a background that may help the reader understand what may be the most frustrating part of the dispute as we experienced

it during the process of translating and editing: not so much the heat of the polemical parts, but the fact that the two opponents seem to be consistently talking past each other. Perhaps they never really tried to persuade each other but were talking to a third party: to those young scholars entering the scene of German economics who were in need of inspiration and orientation. Ex post, the vitriolic fireworks of the Methodenstreit might have served precisely as such a signaling device.

This conjecture of course runs the risk of attributing too much strategic stringency and purposiveness to what was going on in a rather chaotic manner. But at least by hindsight, the hypothesis of such undercurrents may seem corroborated by the interlocutors whom Menger mentions at the end of the preface to *The Errors of Historicism*. These are two Austrians and two Germans, three of them Menger’s juniors: the Austrians Eugen von Böhm-Bawerk (1851–1914), who had just received a professorship at the University of Innsbruck and whose capital theory and critique of Marxian economics would famously carry the Austrian School into the future, and Emil Sax (1845–1927), a professor of political economy at Charles University in Prague; as well as the Germans Wilhelm Lexis (1837–1914), who at the time of Menger’s writing was teaching *Staatswissenschaften*, or sciences of the state, at the University of Breslau (now Wrocław), and Heinrich Dietzel (1857–1935), a professor of political economy at the University of Dorpat (now Tartu) and later at the University of Bonn.

As far as school-building is concerned, over time Menger’s signals proved more successful than Schmoller’s. In the course of the subsequent Werturteilsstreit during the 1900s–1910s, Schmoller ended up losing the brightest minds of the next generation, most notably Max Weber and Werner Sombart, and this at several meetings of the Verein für Socialpolitik, Schmoller’s institutional stronghold: of all places, a crucial clash of the Werturteilsstreit took place at the Verein’s 1909 meeting in Vienna. In contrast, Menger bonded even more closely with his associates, most notably Böhm-Bawerk and Friedrich von Wieser. They became not simply faithful followers, but original contributors to his research program—sometimes also clashing with Menger, famously on issues of capital theory (Garrison 1990)—and pillars of what was seen as Menger’s school. They published explicitly under the brand of “Austrian” theories of value or capital in the very early volumes of English-language journals, and they engaged in post-Methodenstreit skirmishes with German economists (Caldwell 2004, 83–97; Kolev 2018).

Seen in this light, on the one hand, the Methodenstreit exerted both an immediate and also a long-lasting effect on the institutionalization of the school-like structures in German economics, which prevailed still in the interwar period (Eucken 2018/1938; Eucken 1940), and *The Errors of Historicism* played an important role in that. On the other hand, however, the pendulum did not fail

to swing back. Schmoller's preoccupations with exact empirical socio-economic research and also with ethics are widely shared today, and his work can even be seen an important building block for the ordoliberal program and the theory of the "social market economy," aiming at a harmonious combination of free markets and social policy (Schefold 1996; Peukert 2000; Goldschmidt and Wohlgemuth 2008; Goldschmidt 2013; Zweynert 2013). Even though the aspirations of the Historical School did not bring a breakthrough, today's empirical turn in economics amid the availability of big data and new econometric methods in "the age of the applied economist" (Rodrik 2015; Backhouse and Cherrier 2017) or the recent "state capacity" debate for the order of economy and society (Cowen 2020; Hammond 2020) are not too distant from its agenda. A parsimonious evaluation should therefore be in order: Both Menger and Schmoller have made an impact, both have contributed important insights to the discipline, and it is up to the modern researcher to heed—and balance—their calls.

'Crowd-translating' and its challenges: the project of the NOUS Network

Translating longer texts certainly does not feature among the activities which today's economics profession holds in particularly high regard. And yet when we co-founded the NOUS Network for Constitutional Economics and Social Philosophy in 2015, we stipulated that a crucial mission of NOUS would be to make the insights of German-language political economy accessible to an international audience. This of course includes the promotion of original research, and currently we are conducting projects on the Colloque Walter Lippmann for the *Journal of Contextual Economics—Schmollers Jahrbuch* and on the 'thinking-in-orders' tradition for *History of Political Economy*. But given the rather limited command of German in academia, especially among younger scholars, we see making classic German-language texts accessible to be an important task of NOUS. Again, we are currently translating Schmoller's review of the *Investigations* and his disparaging letter to Menger of 1884, soon to be published in the *Journal of Contextual Economics—Schmollers Jahrbuch*. In that same journal, another set of eight translations is currently being finalized, very much from the age which we discuss here, including articles by Schmoller, Georg Simmel, Ferdinand Tönnies, Walter Eucken, and a long article by Schumpeter directly relevant here, "Gustav von Schmoller and the Problems of Today" (Schumpeter 2018/1926).

When Dan Klein, a member of NOUS, approached us regarding a translation of Menger's *Errors of Historicism* on behalf of *Econ Journal Watch*, this was

a perfect match. We were happy to be able to build upon a translation of the early sections of *The Errors of Historicism* conducted by Diana W. Thomas, another member of NOUS. And for what followed, the NOUS Young Affiliates ([link](#)) turned out to be a truly invaluable resource. Currently 80 in number, we incorporate young scholars into the existing greater academic network through a structured admission procedure open to students on the bachelor’s, master’s or doctoral level. And while ‘crowd-funding’ has become a common practice today, we conjectured that an experiment in ‘crowd-translating’ could also be worthwhile, and so approached our Young Affiliates. Ten of them enthusiastically volunteered to translate the Letters making up *The Errors of Historicism*, and painstakingly they produced excellent translations, notwithstanding Menger’s extra-long sentences, 19th-century lingual peculiarities, specifically Austrian expressions, and not easily tracked citation habits. To enable the reader to directly see particular subtleties in Menger’s language like the co-existence of synonymous terms like ‘Wirtschaftsgeschichte’ and ‘Geschichte der Volkswirtschaft,’ we provide the German originals in footnotes. Three of the Young Affiliates, Adam De Gree, Tessa Bohling De Gree (both from the U.S.), and Tim Tewes (from Germany), all master’s students of Philosophy, Politics, and Economics at CEVRO Institute in Prague, formed a first editorial council and worked through the translations as submitted by the Affiliates. From that fantastic manuscript produced in Prague, Karen Horn and Stefan Kolev finalized the work. Given the substantial efforts of the ten Young Affiliates, it would not be enough to merely list their names, and we gratefully accepted the possibility offered by *Econ Journal Watch* to produce a gallery of pictures of these promising aspiring minds in the domain of Philosophy, Politics, and Economics. Thank you all for helping make available an important text in the history of economics for the broader international academic community.

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About the Authors



Karen Horn is honorary professor at the University of Erfurt, Germany, where she teaches the history of economic ideas and economic journalism. She serves as chief editor of *Perspektiven der Wirtschaftspolitik*, a scientific journal of the Verein für Socialpolitik, the traditional association of economists in German-speaking countries, and as associate editor of the *Review of Austrian Economics*. She is co-founder, co-president and coordinator of the international interdisciplinary academic

Network for Constitutional Economics and Social Philosophy (NOUS). Karen has published monographs on the history of economic thought, ordoliberalism, the social market economy, the work of Friedrich A. Hayek, and an oral-history collection of in-depth interviews with ten Nobel laureates in economics. Currently, her interests concentrate on the intellectual shifts and rifts during the interwar

period in Germany and in France. A former journalist, she regularly writes columns, analyses and comments in the press, in newspapers, magazines and online services. Her email address is karen.horn@uni-erfurt.de.



Stefan Kolev is professor of political economy at the University of Applied Sciences Zwickau, Germany, and the deputy director of the Wilhelm Röpke Institute in Erfurt. He serves as co-editor of the *Journal of Contextual Economics—Schmollers Jahrbuch* and of the *ORDO Jahrbuch für die Ordnung von Wirtschaft und Gesellschaft*. He is among the co-founders of Network for Constitutional Economics and Social Philosophy (NOUS) and is in charge of the NOUS young scholars seminar series.

His research focuses on the link between the history of liberalism and the history of German-language political economy. Stefan has published on the connections among Friedrich A. Hayek, the “Old Chicago” School, and the ordoliberals, as well as on the revitalization of ordoliberal thought today and tomorrow. Currently, his interests focus on the German Historical School, on early German-language economic sociology, especially Max Weber, as well as on the connection of the Bloomington School to ordoliberalism. His email address is stefan.kolev@fh-zwickau.de.

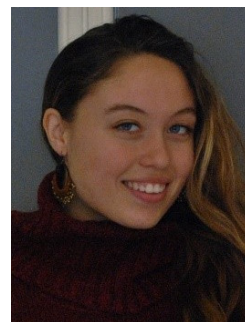
**NOUS Young Affiliates who
participated in translating and editing
Carl Menger’s “The Errors of
Historicism in German Economics”**



Alexander Albrecht
Political Economy
King’s College London



Adam De Gree
Philosophy, Politics, Economics
CEVRO Institute Prague



Tessa Bohling De Gree
Philosophy, Politics, Economics
CEVRO Institute Prague



Justus Enninga
Political Economy
King’s College London



Lachezar Grudev
Economics
University of Freiburg & Duke University

**NOUS Young Affiliates who
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Carl Menger's "The Errors of
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Dr. Christian E. W. Kremser
Economics and Philosophy
Goethe University Frankfurt



Dr. Daniel Nientiedt
Economics
Walter Eucken Institut



Konstantin Peveling
Political Economy
King's College London



Sebastian Reil
Economics and China Studies
Peking University



Tim Leto Tewes
Philosophy, Politics, Economics
CEVRO Institute Prague

DIE
IRRTHÜMER DES HISTORISMUS
IN DER
DEUTSCHEN NATIONALÖKONOMIE.

VON

DR. CARL MENGER,
O. Ö. PROFESSOR DER STAATSWISSENSCHAFTEN AN DER WIENER UNIVERSITÄT.

WIEN, 1884.
ALFRED HÖLDER,
K. K. HOF- UND UNIVERSITÄTS-BUCHHÄNDLER,
ROTHENTHURMSTRASSE 15.

The Errors of Historicism in German Economics

Carl Menger
edited by Karen Horn and Stefan Kolev
*translated by the Network for Constitutional Economics
and Social Philosophy (NOUS)*

Preface. [III]

The vagueness of the “historical school of German economists”¹ regarding the goals and methods of research in the field of political economy, an ailment that was already obvious at the very founding of this school, has not been remedied even by the passage of almost five decades of development.

From the very beginning, the “historical school” was not the result of an immersion into the problems of our own science; unlike historical jurisprudence, it did not arise from the scientific² 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. External circumstances created it; [IV] it was not scholars in our discipline, but historians who originally founded it. The historical method was carried into our discipline from the outside.

The historical school has never been able to free itself from these defects of its origin. The external connection of solid *historical* knowledge with a careful but aimless eclecticism in *our* discipline constitutes its starting point and at the same time the peak of its development. Several very serious attempts were made along the lines described above, intended to bring history and political economy into a closer, more organic connection, but the elevation of our discipline from its backward condition promised by the historical economists has not been achieved; indeed, it seems today to be almost further away than it was when Hermann and Rau were teaching.

That the above-mentioned reform attempts, which were in part made with unusual ability, did not reach their goal was not a coincidence; they had to fail due

1. [„historische Schule deutscher Volkswirthe“]

2. [scientific]

to the error of recognizing history as the point of departure, and its connection with political economy³ as the pivotal point of the intended reforms. The erroneous hypothesis that the connection of historical knowledge with political [V] economy as such implies a reform of the latter, i.e., the false dogma of historicism in the field of our discipline, was incapable of being the basis of a promising modification of the latter from the beginning.

The reform of a discipline can only arise from within, from the depths of its own spheres of ideas;⁴ it can only be the work of scholars immersed in the very own problems of their discipline. Political economy will be elevated from its current absorbed obscurity⁵ neither by historians, mathematicians, or physiologists nor by those who are blindly following their tracks. Reform of political economy can only originate from ourselves, from us professional peers⁶ who are at the service of this discipline.

What other disciplines and their representatives can offer and accomplish for us is a progressive immersion into their own problems, the perfecting of the results of their own research. To the extent that they are relevant for the development of our own discipline, we want to use these results carefully and with appreciation, the results of historical research as well as those of statistics, psychology, logic, and of the technical sciences. In the future, however, we will have to resolutely fend off the reformist meddling of other disciplines, the intrusion of viewpoints and methods alien to political economy [VI] into the latter, if German economics is not to expect new disappointments after another half-century.

The next and most important task to be solved in the field of political economy in Germany seems to be clearly delineated by the current condition of the discipline. Historians have set foot on the ground of our discipline like foreign conquerors to force upon us their language and customs—*their* terminology and *their* methods—and to intolerantly fight every research trend which does not correspond with *their* own idiosyncrasies. This state of affairs has to be stopped. The problems and paths of inquiry⁷ that derive from the nature of our discipline must be brought back to honor; this discipline must be liberated from its historicizing tendency⁸ and the biases of historicism. Once political economy in Germany has

3. [Menger employs a number of terms for “economics,” most commonly *Nationalökonomie* and *politische Ökonomie*. We consistently translate *Nationalökonomie* as “economics” (also in the title of the book) and *politische Ökonomie* as “political economy.” When Menger employs other terms like *Volkswirtschaftslehre* or *Wirtschaftswissenschaften*, we have added a special reference. Appendix IV in Menger’s *Investigations* is particularly illuminating in that regard.]

4. [*Ideenkreise*]

5. [*Versunkenheit*]

6. [*Fachgenossen*]

7. [*Erkenntniswege*]

8. [*historisierende Tendenz*]

rediscovered itself, its notion,⁹ and its methods, and as long as it further maintains for itself the spirit of universality—a spirit which makes use of the results of other research and those of other fields of knowledge for its own purposes, especially those of history and statistics—then we must not fear the further development of the discipline.

The following methodological letters are dedicated to the above purpose. [VII] They are intended to place under the light of criticism a scientifically most boggy field of historicism in German economics, which has been defended with extreme measures of intolerance and impropriety, i.e., historicism's most recent outgrowth, to provide an appropriate answer for unqualifiable¹⁰ attacks which have not been provoked by anything, at least not anything in such a manner.

In this short essay, primarily intended as a defense, I have avoided the obvious temptation to treat the actual method of exact research in the field of theoretical economics. In the *Investigations into the Methods of the Social Sciences* I have sought to offer the proof for the justification—heavily contested by the historical school—of the above-mentioned direction of theoretical inquiry¹¹ in the field of economics, reserving a detailed depiction of the respective paths of inquiry for a separate essay.¹² The preliminary observations about this have nevertheless become the object of lively discussion among the evaluators of my methodological investigations—a pleasant sign of interest in the above-mentioned important branch of theoretical research in the field of German economics, despite the dominance of the historical school. I will now seek to speed up the fulfilment of my promise [VIII] because 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. In this pursuit, I will also find the opportunity to consider the competent remarks by E. v. Böhm, Emil Sax, W. Lexis, H. Dietzel, and others which have been made to oppose specific parts of my argument.

Vienna, January 1884.

The Author.

9. [Begriff]

10. [unqualificierbar]

11. [Erkenntnisstreben]

12. *Investigations*, p. 62 [43]. [The numbers inside the brackets correspond to the pagination in the German original, the numbers ahead of the brackets correspond to the English translation *Investigations into the Method of the Social Sciences with Special Reference to Economics*, NYU Press, 1985. If Menger uses direct quotations from the *Investigations*, we reproduce their formulation in the English translation.]

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[On the final page of the book (p. 88), Menger lists ten errata. We have incorporated them into the text without comment.]	

First Letter.

[Introduction: External motives for these letters. About the utility which could be drawn for the scientific discussion even from shallow criticisms by evaluators not sufficiently versed in the subject matter] [1]

You write, my friend, that the best response to the equally inconsiderate and challenging critique which my *Investigations into the Methods*¹³ has received from the editor of the Berlin *Jahrbuch für Gesetzgebung*¹⁴ is to overlook it with silence, which is the most effective answer to attacks of this kind.

Whoever read my book, even if cursorily, you suggest, would gather on their own to what extent the attacks by Schmoller are grounded in knowledge of the subject and impartiality, and would be able to base their judgment upon that. But his critique would receive the appropriate appreciation even by those who do not know my *Investigations*, as it originates from a man whose scientific erudition has long been acknowledged appropriately in academic circles, despite his constant reminders of the historical and philosophical [2] studies to which he has devoted himself, of the lectures on methods for which “he was just arming himself,” and more of the same. You say that reviews of the kind which Schmoller has handed over to the public for years without sufficient learning, filled with invective and obviously without taking any responsibility, are harmless for those knowledgeable readers which we first seek to reach with our scientific publications, and any response to them is beneath the dignity of a serious scholar.

Allow me, my friend, to nevertheless be of a somewhat different opinion in the above matter. I do not want to debate whether such reviews are harmful or harmless for the authors of the reviewed contribution. Consider them harmless for the author or even as amusing incidents of academic life. From that it does not seem to follow, however, that one should leave them completely unacknowledged. What may be harmless for the author of a piece, may still be harmful for the subject he is advancing; and even if that were not the case, why should we disdain to use for a matter so close to our hearts what does not harm it? Critiques by knowledgeable authors are useful to us in the sense that they correct and instruct us, and thereby

13. C. Menger, *Investigations into the Methods of the Social Sciences with Special Reference to Economics*. Leipzig: Duncker & Humblot, 1883.

14. G. Schmoller, “Zur Methodologie der Staats- und Sozialwissenschaften.” In: *Jahrbuch für Gesetzgebung, Verwaltung und Volkswirtschaft im Deutschen Reich*. Leipzig: Duncker & Humblot, 1883, N. F. Vol. VII, Nr. III, pp. 239–258.

deepen the scientific discussion. Why should critiques of the kind that Schmoller publishes merely be harmless and not also provide utility, if not of course of a completely different kind?

[3] Every scholarly piece has a certain intellectual level below which the author descends only reluctantly. Not all formulas are explained in mathematical writings, the knowledge of positive law is required in legal writings, and generally much ability and knowledge are presupposed in scientific writings. Here lie, painfully recognized by every reasonable author, the limits for the understanding and the dissemination of his ideas. Shallow attacks directed at us by ignorant critics do, however, offer us the welcome opportunity to push back those limits, and that all the more effectively the closer the evaluator is to the less knowledgeable readers and the more ruthlessly he believes himself to be acting against us.

In such reviews objections are raised against the results of our research, objections which the author also had in mind but which he refrained from responding to, due to the erroneousness of the objections being obvious to the knowledgeable expert. When such objections are raised by a critic with strong emphasis, as is usually the case, then we are in a position to engage with them without violating the respect we owe the readers of scientific writings. Objections and attacks of the kind described above thus offer the opportunity to raise our ideas to the level of general intelligibility, which is not otherwise typical in scholarly writings and superfluous for the actual scholarly audience, yet which is not without utility with respect to a certain portion of the readership of scientific writings.

[4] But critiques of the kind I am talking about here do us another, incomparably greater service. Objections are raised in them that are so far away from the spheres of thoughts¹⁵ of serious scholars that no one who has some learning in the subject, and least likely the author himself, would ever come to think of them. These are, however, objections that usually arise from a strange harmony of thoughts in the minds of all superficial readers of scholarly writings not sufficiently acquainted with the materials treated.

It is through critiques of this kind that we thankfully arrive at knowledge of the greatest misunderstandings that our work is subjected to in certain circles of readers and, in this way, obtain the opportunity to counter them effectively. Critiques of the category I am speaking of here play the role of a certain figure in Italian comedy, who seems to inhibit the plot through its partially misleading, partially ill-natured interjections, but actually promotes it in an equally effective and exhilarating manner.

Indeed, it is not common that a well-known author of manifold laudable merits assists an author who is anxious about the distribution of his ideas by taking

15. [*Gedankenkreise*]

on a role of such secondary nature in the scientific discussion; it is nothing short of a stroke of luck when our opponent is a scholar who, because of the external means of power he unifies in his hands and because of the manner in which he uses them, is praised by the small and fearful, and wisely left unmentioned by the stronger. [5] Thus, our interest in promoting our scientific endeavors is joined with our interest in cleansing the literature of the influence of a shallow kind of reviewerdom¹⁶ not qualified for the high tasks of scientific critique.

And I should let this opportunity pass by completely unused, which is offered to me by the editor of the Berlin *Jahrbuch* in such an unintended manner, an opportunity to remove a series of misunderstandings and errors about the foundational problems of our discipline, and perhaps even to repair some other “historically evolved”¹⁷ obstacles to an appropriate scientific discussion in the field of economics in Germany?

Second Letter.

[*Continuation: About disfigurements in the field of scientific criticism and how to counter them*] [6]

With kind concern, you alert me to the fact that a dispute with Schmoller would not merely have a scientific side, but also a completely different one. According to you, there is no other scholar in Germany, or anywhere else, who would be as ruthless in his choice of means when it comes to fighting an opponent. You say that I should be prepared for every possible and impossible disfigurement of my words, and that Schmoller is a master of a very personal and vulgar style of writing, of which I have just received downright frightening samples—as an aside, this may be the only mastery one can attribute to this man regarding his German.

You are right, my friend, when you consider a scientific discussion with Schmoller as not merely scientific;¹⁸ seeing that this man is more than known for his pronounced tendency to misinterpret the opinions of others, and as a representative of impropriety in the field of scientific polemic.¹⁹ [7] Truly, not

16. [*Recensententhum*]

17. [*„historisch geworden“*]

18. [*scientific*]

19. In his reviews of my writing, Schmoller does not limit himself to swearwords like “unworldly ivory tower naïveté” [*„weltflüchtige stubengelehrte Nâivität“*], “scholastic thought exercises,” “blinders of scientific effort,” [7n.] “abstract schemes,” “mental tuberculosis,” and more of the same, but intimates to me, apparently in order to strengthen the force of his arguments, that I would be “*immediately thrown out*” of every group of exact scientists due to my methodological views (*Jahrbuch*, p. 243). The specific part of his critique, which provides proof that Schmoller earned his spurs in his scientific career in tradeschools

without some hesitation do I take steps towards fighting off this side of his attacks against me. [8] There are situations, however, in which it would be a betrayal of one's own cause to remain silent. I would be more than happy to leave this unedifying business I have to complete here to someone else, if it were easy to find such a person, given the kind of critique Schmoller is formulating in the field of our discipline. It is exactly what you list as a reason for me to remain silent in the face of Schmoller's attacks that must be an additional motive for me to raise my voice against him.

"One should grant anyone's unearned praise"—says Lessing—"...but when such a precariously famous man will not be [9] content with his unearned honor,²⁰ when the will-o'-the-wisp,²¹ which has been allowed to rise to the prominence of a meteor, now wants to scorch and burn and spread noxious fumes all around, who can refrain from indignation? And what scholar, whose circumstances allow for it, is not obliged to attest to his indignation?"

Well, my circumstances allow me to counter Schmoller's misunderstandings, disfigurations, and improprieties in the field of economic critique.

I just beg you, my friend, to not see this as proof of even the slightest sort

[*Handwerkervereine*] (e.g., Schmoller: *Zur Geschichte der deutschen Kleingewerbe*, p. VI), not without merit for his style of writing, reads as follows: "The chemist can dare to abstract from the physical characteristics of a chemical object, but *he would be immediately thrown out of the laboratory* if he studied the atmosphere and said, according to Menger's principle of isolation: 'I am only considering nitrogen, because it is dominant.'" Those who merely know the elements of logic know that the method of isolation presupposes the isolation of only those moments of a phenomenon that are *accidental*, and those who have read my book know that nowhere in it do I further the nonsensical opinion that the method of isolation assumes the isolation of *essential* moments of a phenomenon. Schmoller's comment is therefore not just improper and almost crude, but in addition completely inappropriate. I dare to make this remark even at the risk that Schmoller could, in a careless moment of forgetting that he is currently a member of one of the most illustrious scholarly associations, roll up his sleeves and get ready to present his dreadful arguments "immediately."

By the way, I take it for granted that it is well known how Schmoller practices the disfiguration of the opinions of others and extremely inappropriate expressions not just against me, but quite habitually. Prof. Treitschke felt compelled, almost ten years ago, [8n.] in an open letter to Schmoller („*Der Sozialismus und seine Gönner*. Berlin 1875, pp. 102ff.) to point to the fact with numerous references that Schmoller's polemic is "abundantly graced with personal attacks" which compelled him (Treitschke), against his inclinations and custom, to send some personal remarks ahead of his rejoinders." And these remarks culminate in the fact "*that Schmoller hurls at almost all of his opponents remarks which do not further communication.*" Treitschke makes the following remarks with respect to Schmoller's love of truth: "Like you, I would have to fill ten pages if I wanted to prove how you are exaggerating my claims here, how you are turning them on their head there, how you are portraying conditional claims as unconditional, how you are angrily holding up my own thoughts against me, as if I had opposed them, and how through such dialectic tricks you create an image of my work *in which I do not recognize a trace of my true opinion.*"

The fame of being the pinnacle of Schmoller's abusive style of writing falls, however, most probably to his critique of my *Investigations*.

20. Lessing uses a different term here. G. E. Lessing. *Briefe, antiquarischen Inhalts* [*Letters of Antiquarian Content*], Letter 56. Berlin, 1768–1769: Friedrich Nikolai.

21. Lessing uses a different term here.

of heroism, because on the one hand I am of the opinion, that my “unworldly ivory-tower naïveté” can stand up to a pushiness²² as worldly and uneducated as it can be in the field of science, and I believe on the other hand that I have several other reasons not to fear my opponent too much. Men like Schmoller only have the ability to rise to the surface as a result of the downright desolate state of a science. Only when the leaders of a discipline are not quite sure of their ground, when serious doubts regarding their own fundamental views worry them, and when they require the leniency of inferior minds in more than one respect, are those lower minds capable of organizing the kind of half-obnoxious, half-ridiculous terrorism against the representatives of other opinions, terrorism which is currently being exercised in some of the journals in our discipline. However, I do not demand [10] leniency from these men; I have neglected nothing to avoid even the appearance of asking for Schmoller’s leniency. What reason could I have then to fear him?

Perhaps that he could prove that I committed errors? I wish this were a threat, a most genuine threat; how thankful would I be for any instruction, if such could be found in the work of writers of his kind, writers who, as I have shown, commit errors page by page, which—but I do not want to adopt the same tone as my opponent.

Or should I recoil from the fact that Schmoller may disfigure and misinterpret my opinions? I admit that such an experience does not please an author. *An erit, qui velle recuset os populi meruisse?*²³ How easy is it to take a portion of the loyal reward of honest work from an author through such “reporting”? How easy? Indeed! However, this only happens when we cede the field to the heroes of such endeavors and do not assert our right to objective reporting.

What is the editor of a scientific journal that we should quietly stand by as he disfigures the results of our scientific investigations? What else is the editor of a scientific journal if not a man who is in service to the truth and to the scientific requirements of the readership of his journal, a man who has assured of honest and unencumbered reporting and who acts in violation of his duty when he, instead of keeping his promises, disfigures the truth? [11] And against someone like this should there not be any means of defense? No defense against the abuse of scientific outlets, the existence of which the scholarly world, and only it, makes possible through its intellectual and material support?

The means is just as simple as it is effective. It is important to expose the disfigurements of the results of our scientific investigations, rather than to silently tolerate them. If we expose the disfigurements in many cases, our readership will,

22. [*Streberthum*]

23. [Translation: Lives there, who would not at applause rejoice, and merit, if he could, the public voice? (The quotation is from the *Satires* of Persius.)]

instead of trusting the reporting of certain reviewers blindly, get in the habit of reassuring itself of the soundness of particularly conspicuous assertions of the reviewers. In doing so, the spell of those men who disfigure the opinions of others instead of offering objective reviews is broken with one strike. If everyone does their duty in the aforementioned manner, we will soon not have to fear the likes of Schmoller. Instead, they will soon find themselves forced either to put down the critical pen or to be particularly conscientious in their future reporting. Once mistrust of such critics has awakened, they cannot help but be particularly conscientious in their reporting out of mere self-interest. What more cruel punishment can you think of for men such as these but to force them to be objective in their criticism?

Third Letter.

[About the different directions of inquiry in the field of economics] [12]

Both the historian and statistician as well as the social theorist deal with societal phenomena; how is their scientific activity distinct? What is the difference between historical and theoretical social sciences? This question, in itself important for the philosophy of science,²⁴ had taken on particular importance for me. In addition to some other errors, which I will address in more detail below, opinions had appeared in the more recent economics literature in Germany which lacked any rigorous distinction between historiography and statistics on the one hand and theory on the other. A school of economists had emerged, which had earned recognized merits in the fields of history and statistics of the economy acknowledged by no one more readily than I, but which also often confused the above-mentioned disciplines with theoretical economics, and as a result of treating it like a historical discipline almost put into question the independent significance of theoretical economics.²⁵

[13] I had made it my task to counter this one-sidedness, which had become detrimental for the development of the theory of the economy.²⁶ Not that I misjudged or ever underestimated the utility and significance of historical and statistical research in the field of the economy as such, or as auxiliary disciplines for theoretical economics; to the contrary, I have acknowledged the importance of this

24. [*Wissenschaftslehre*]

25. See in this regard H. Dietzel: *Ueber das Verhältniss der Volkswirtschaftslehre zur Socialwirthschaftslehre*. Berlin 1882, pp. 4ff., pp. 7ff.

26. [*Theorie der Volkswirtschaft*]

kind of inquiry in the field of economics unmistakably without any restraint. What I have criticized about the aspirations of this large group of German professional peers who, known under the collective name of the historical school of German economists,²⁷ have taken on such a prominent place in the more recent economics literature in Germany, was the one-sidedness with which this group has devoted its mental capacity partly only to historical and statistical studies, i.e., to auxiliary disciplines of political economy, while at the same time they most regrettably have neglected the theory of our discipline, which is in such urgent need of reform, and partly they even seem to treat theoretical research in the field of the economy with misguided contempt, as if only historical research in economics were justified.

The historical school of German economists gave cause for several concerns in another related matter as well. Some of its distinguished representatives lacked any more serious separation of the theoretical and practical disciplines of the economy. [14] The boundaries of the above-mentioned fundamentally different directions of research were misunderstood, and these misunderstanding was even characterized as epoch-making progress for our discipline, not only in most of the recent doctrinal systems,²⁸ i.e., in the practice of depiction, but also in foundational methodological discussions.

In a third regard I also saw an error in the methodological principles of the historical school. Even those followers of the school who do not outright deny the independent significance of theoretical economics, who acknowledge the legitimacy of a science of the “laws” of the economy in addition to historical-statistical studies and socio-political research, even those followers of the historical school of German economists did not seem to be fully free from 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 (like philosophy of economic history²⁹ etc.),³⁰ while unjustifiably disregarding all the others, among them some of the most fundamental significance.³¹

[15] To me, the historical school of German economists seemed to have lost the conception of political economy and its parts, the understanding of the

27. [*historische Schule deutscher Nationalökonomien*]

28. [*Lehrgebäude*]

29. [*Wirtschaftsgeschichte*]

30. See on this point the appropriate remarks of H. Dietzel, *ibid.*, pp. 31ff.

31. “Both directions (the historical and the organic) but in particular the historical, gained ground quickly in Germany and today they dominate the German science almost completely. The manner in which they execute their domination, one can no longer deny, is not very tolerant. Every direction of inquiry that diverges from the dominant tradition is misjudged or ignored as ‘abstract,’ ‘ahistorical,’ or ‘atomistic.’” E. v. Böhm-Bawerk in *Zeitschrift für das Privat- und öffentliche Recht der Gegenwart*. Vienna [1884], Vol. XI., p. 209. [Menger erroneously puts the publication year as 1883.]

relationships between its parts and auxiliary disciplines but most importantly the overview of the different legitimate directions of research in theoretical economics—in short, the historical school of German economists lost its insight into the system of tasks whose solution is incumbent upon research in economics. One group of its representatives occupied themselves exclusively with the history and the statistics of the economy, in other words disciplines auxiliary to political economy, while they fancied themselves to be working, directly or indirectly, on the expansion of the discipline. Another group was busy with the solution of practical, especially socio-political problems, while believing themselves to be reconfiguring economic theory. Finally, yet another group was exhausting its mental capacity in the pursuit of certain particular directions of theoretical research which stand in the closest connection to historical-statistical studies, while rejecting all other directions of theoretical inquiry in the field of economics as misunderstanding the true goals of economic research.

To fight these aberrations of a notable portion of German economists seemed to me all the more important, since the same underlying misjudgment regarding important goals of political economy [16] was perniciously affecting the development of our discipline, which was in need of reform especially in its theoretical part. I believed to observe that as a result of the errors described above, i.e., since the founding of the historical school, theoretical research in economics in Germany was generally underappreciated, and in some parts no longer practiced at all, to the great detriment of our discipline.

The path I had to take to fight the one-sidedness and errors of the historical school did not raise doubts for me. The error of the above-mentioned German economists lies in their views of the nature of political economy and its parts, of the relationships between the parts as well as their relationship to certain auxiliary sciences of political economy, and finally in their one-sided doctrines regarding the nature of theoretical inquiry in the field of economics. However difficult and comprehensive the investigation turned out to be, the nature of the above-mentioned disciplines and their position amid the sciences had to be generally clarified before I could refute the errors of the historical school that were so detrimental for the development of political economy.

It would mean to repeat a significant portion of my deliberations on this issue in a way which would extend beyond the scope of this piece, if I wanted to treat the above-mentioned fundamental questions, which are so important for research in the social sciences in general and political economy in particular, in great detail here anew. [17] What I intend to do here is to counter the attacks on my *Investigations* by some well-known representatives of the historical school of German economists. Only the most recent results of my research, and only to the extent that they have become part of scientific discussion, shall be here summarized in a few words.

It is the acts, the fates, and the institutions of certain states and peoples that the *historian* and the *statistician* have to research and describe, the former from the perspective of development, the latter from the perspective of state;³² the *theorist* in the field of state and social phenomena, on the other hand, has the task to make us aware—not of concrete phenomena and concrete development—but of “manifestations” and “laws” of the respective human phenomena; the researcher in the field of *practical* state and social sciences, however, is supposed to teach us the “principles” of expedient intervention in state and social conditions, i.e., the principles by which certain goals, for example the maintenance of the economy and the administration of the government budget, can be realized in the most expedient way possible.

Along these lines, I said that the *historian* and the *statistician* have to research and describe the concrete phenomena of human life and their concrete relationships in space and time (the former from the perspective of development, the latter from the perspective of state!), whilst the *theorist* has to research and describe the manifestations of human life and the laws of the latter phenomena (the types and typical relationships between human phenomena). [18] The one working in *practical* state and social sciences, however, has to research and describe the principles of expedient intervention in state and social phenomena.

I did not stop with this classification and its application to economics. The central errors of the historical school of German economists concern their views regarding the nature of theoretical economics, their one-sided inclination towards *particular* directions of theoretical research that are closely connected to historical studies. While I initially set myself the task to describe the outlines of the complete system of problems which the human mind has to solve in the social sciences in general and political economy in particular, the narrower task then came up to determine the system of legitimate directions of *theoretical* research in the field of the economy. Along these lines, I elaborated that there are two main directions of theoretical research. Both have the purpose to determine the manifestations and laws of economic phenomena. The first (*the empirical*) is supposed to determine the manifestations of the real phenomena of the economy “in their *complete* empirical reality” as well as the observable regularities in the sequence and the coexistence (the “empirical laws”) of economic phenomena, whereas it is incumbent upon the other (the *exact* direction of theoretical research) to trace the real phenomena of the economy back to their most simple and strictly typical elements in a manner *analogous*, although by no means identical, to the exact natural sciences. [19] In addition, the purpose of this latter direction of theoretical research is, on the basis of the method of isolation, to outline the exact laws, by which the more

32. [Zuständlichkeit]

complicated phenomena of the economy³³ develop from the above-mentioned elements, in order to provide the theoretical understanding of social phenomena not in “*their full empirical reality*” but of their *economic aspects*.

I have taken great care to prove the legitimacy of this latter direction of theoretical inquiry in economics, which has been zealously disputed by the historical school of German economists.

Now I do know that I can only offer my readers a highly incomplete picture of my investigations by summarizing the results of only a portion of them. After all, the main value of scientific results lies in their genetic development and methodological justification. However, even the schematic form in which I am recounting them here will be sufficient, I believe, to allow my readers to estimate the value of the attacks which my *Investigations into the Method of Research* [sic] have experienced from a portion of German economic criticism.

Fourth Letter.
[That political economy and the
history of the economy are sciences
which have to be strictly distinguished] [20]

The contrast between the historical and the theoretical social sciences, as I have characterized it in my last letter and as I have further elaborated in my *Investigations into the Method of the Social Sciences*,³⁴ is not contested by Schmoller but acknowledged in his manner. He admits,³⁵ that the separation of the directions of inquiry³⁶ from which I start is—legitimate? No! This phrase is obviously missing from Schmoller’s peculiar reviewer argot—“of a certain legitimacy.” “*But this contrast should not be understood as an unbridgeable divide.*” [21] “The science of the particular”—Schmoller would “prefer to say” *descriptive science*³⁷—“provides the groundworks for the general theory; these groundworks are the more complete, the more the phenomena are described in all their essential characteristics, changes,

33. [*complicirtere Erscheinungen der Volkswirtschaft*]

34. *Investigations*, pp. 35ff. [3 ff.] and pp. 207ff. [252 ff.].

35. *Jahrbuch*, p. 241.

36. [*Erkenntnisrichtungen*]

37. I, for one, do not “prefer to say” it this way. Botany, zoology, petrography, etc. are certainly not disciplines of science of the *particular* but they are *descriptive*. It is not without reason that I have left behind the old terminology, which counts the above disciplines as “*historia naturalis*,” as “historical sciences” in a way much different than the modern understanding of the term, and have described the historical sciences as “sciences of the particular” in today’s sense. Schmoller misunderstands me here, as in many other places, when supposedly correcting me.

causes, and consequences. The complete description presupposes, however, a complete classification of phenomena, a complete concept formation, a correct classification of each according to the observed types, and a full overview over the possible causes. So, every complete description is a contribution to the assessment *of the general nature of the respective science.*"

"Of the general nature of the respective science!" What is that supposed to mean? What is "the general nature of a science"? Maybe Schmoller means the recognition of the general (the manifestations!) in any field of research? But I do not want to bother him too much with such questions. Meanwhile, what is the aim of the above-mentioned explanation with its strange terminology?

If Schmoller aimed to say in the explanations above that historical studies for theorists, and likewise the knowledge of economic theory for historians, are relevant, and thus that all progress in the field of historiography benefits theory and likewise [22] that all progress of the latter [viz., theory] benefits history, he is right, absolutely right, and the only question that could arise is, why does Schmoller like to cover such a self-evident truth in such incomprehensible language? Schmoller will not be burdening his readers to take his aforementioned sentence, even if it was put in still stranger language, for a new truth that yet has to be proven, or make them believe that it is unknown to *me*?

I have indicated (in my *Investigations*) that theoretical research in the field of the economy finds a very valuable empirical foundation in the history of the economy,³⁸ and have emphasized³⁹ that a more highly developed theory of economic phenomena is unthinkable without the study of the history of the economy. Also for the practical sciences of the economy (economic policy and public finance) I have stressed the meaning of historical studies in an unmistakable manner.⁴⁰ I have explicitly labeled the historical sciences of the economy as auxiliary sciences of political economy and likewise I have labeled the latter as an auxiliary science of the former.⁴¹

So what is Schmoller's intention with the above remarks in this lecturing tone?

What is his intention with this in a critique of my book? [23] Probably he only wanted to tell his readers of his opinion that these trivialities, which he recites in a half-incomprehensible language, are unknown to me? He wants to lecture me about things of which I, for the sake of the humor which lies in certain pretensions of historical economists, have proven⁴² that they have been repeated, and are still

38. [*Geschichte der Volkswirtschaft*]

39. *Investigations*, p. 117 [123].

40. *ibid.*, p. 163 [187].

41. *ibid.*, p. 45 [18].

42. *ibid.*, p. 163 [187].

repeated, again and again by authors of “practical philosophy” since Plato and Aristotle!

Yet even if the statements above were original, if the patina of two millennia did not lie on them, what do they have to do with the question of the *boundaries* between history and theory in the field of the economy? That the history of the economy, and not only this but countless other disciplines as well, can be labeled as auxiliary sciences of theoretical economics, and that their every progress benefits theoretical economics, yes, that even all sciences are somewhat related to one another—who would deny this, who has ever denied this? Only a very unknowledgeable evaluator would be capable of drawing the conclusion that *there are no fixed boundaries at all between the individual disciplines*, and that especially the historical sciences of the economy and theoretical economics may be confused with one another. Only against that, against those mistakes which our historical economists have fallen for in this respect, have I taken my stand.⁴³

[24] No unbridgeable divide separates history from theory of the economy;⁴⁴ as little separates them as anatomy from physiology, mathematics from physics and chemistry; between theoretical economics and history of the economy, and yes, between sciences in general, there is self-evidently no such unbridgeable divide as between transcendental philosophy and a Great Dane;⁴⁵ yet, in every case *a very specific boundary* exists, insofar as it is possible for one to exist, between the sciences. 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 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 purposes. “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 of” (historical!) “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.”⁴⁶

Here, with regard to the tasks and goals of research, there exist strict boundaries between the above-mentioned disciplines exist, [25] which must not be blurred unless one opens the doors for confusion and the shallowest diletantism. What I reproach the historical school of German economists with is not that they

43. *ibid.*, pp. 41 [11ff.].

44. [*Theorie der Volkswirtschaft*]

45. [*dänische Dogge*]

46. *Investigations*, pp. 42ff. [12ff.].

practice the history of the economy as an auxiliary science for political economy, but that a part of its followers have lost sight of political economy itself while conducting their historical studies.

Fifth Letter.

[Why Schmoller strives to blur these boundaries] [26]

You are asking me, my friend, why Schmoller does not wholeheartedly admit the obvious statement that the historical sciences of the economy (the history and the statistics of the latter) are merely in an auxiliary relationship to political economy, why he instead attempts to blur the boundaries between these fields of knowledge as far as possible? The explanation for this, or to use Schmoller's noble language, the explanation for his aversion "against the *blindens* of the scientific division of labor" is pretty obvious. No reasonable person denies the importance of historical studies for research in the field of political economy. Also, nobody denies the utility that history of the economy as such has for the understanding of economic phenomena. This, however, is not enough for the editor of the Berlin *Jahrbuch*. He wants to continue to conduct his historical-statistical miniature painting,⁴⁷ and yet not give up the pretension of being considered a representative of political economy and in particular of the theory of the economy. Hence his aversion to the "*blindens* of the scientific division of labor," but in truth against any appropriate determination of the boundaries between the history and the theory of the economy. [27] Hence also his tenaciously held opinion that the history of the economy is the *descriptive part* of political economy,⁴⁸ although it is not at all a part of political economy, but an auxiliary discipline of the latter. To cross this difficult-to-bridge divide, he postulates the theory of the not-at-all-unbridgeable divide between writing history and doing theory in the field of the economy. "The opposition of the above sciences must not be understood as an unbridgeable divide." With this, the question of the boundaries between historical and theoretical sciences is settled! Settled entirely in Schmoller's sense!

Bienheureux les Ecrivains—I want to proclaim with Balzac at this point—*qui se contentent si facilement*.⁴⁹ For Schmoller to be able to proceed undisturbed in his historical-statistical micrography,⁵⁰ historically evolved and generally recognized scientific classifications are to be overturned; for him to continue to dedicate

47. [*Kleinmalerei*]

48. *Jahrbuch*, p. 241.

49. [Translation: Lucky are the writers who can be so easily satisfied.]

50. [*Mikrographie*]

himself undisturbed to his Strasbourg historical walks, and still to be considered a representative of political economy, all scientific categories are to be turned on their head! No mistake! That would be worthwhile! And therefore, once again: Someone who utilizes findings of historical research for the purpose of conducting research in the field of political economy is certainly a political economist, but someone who inquires into the history of the economy itself is in this function a historian of the economy. [28] As an aside, one is obviously a scientific historian⁵¹ only insofar as one is familiar with the sources and the methods of historical research. That is how it is, and hopefully it will stay that way, even if it thus became clear that Schmoller fundamentally misjudged the task of political economy.

Sixth Letter. [29]

[The overestimation of historical studies in the field of political economy. Its causes and their disadvantages]

Even if Schmoller had admitted without hesitation the fundamental difference between the historical sciences of the economy on the one hand and political economy on the other, and in particular the difference between the *historical* sciences of the economy and *theoretical* economics,⁵² instead of obscuring an obvious truth by all kinds of evasions, the difference would still have become apparent between my views and his on the relationship of history to political economy.

There can be no reasonable doubt among fairly knowledgeable evaluators that history and statistics of the economy are merely auxiliary disciplines of political economy in general, and its theoretical part in particular, so that history and statistics of the economy have to be strictly distinguished from them; nor can there be any doubt that the historical sciences of the economy are important not only in and of themselves, but also in the above sense, i.e., as *auxiliary sciences* of political economy. [30] There is no auxiliary science whose harnessing is not of some importance for the purposes of research in the discipline to which it stands in the relationship considered here. This can already be found in its acknowledgement as an *auxiliary science* of the respective discipline. Just as no one can deny the character of the historical sciences of the economy as auxiliary sciences of political economy, no one can deny their importance for research in the field of the latter.

51. [*wissenschaftlicher Historiker*]

52. [*Volkswirtschaftslehre*]

A fundamentally different question, however, is the degree to which different directions of research into a specific field of the phenomenal world⁵³ are warranted. No reasonable person would doubt that in this regard there is the possibility of underestimation, but also of exaggeration.

Now I am well aware that among all the tasks of scientific discussion none is more difficult than to determine the correct limits of scientific endeavors. By its own idea, all science is infinite; every exaggeration of a direction, no matter how one-sided this exaggeration might be, has its utility and is therefore, from a certain point of view, justified. Nobody thinks of claiming *that even the most one-sided commitment of the representatives of our science to historical studies has no indirect utility whatsoever for research in the field of political economy*. [31] As I said before, all this is not questioned by anyone who has even some degree of experience in scientific research.

Equally uncontested, however, is the fact that the number of scholars in every nation and every period is a limited one, and that the infinity of scientific tasks is not met with an equally unlimited ability to solve them. Thus every exaggeration of particular directions of research, though justified, is synonymous with an equally one-sided neglect of others; in this sense, the almost exclusive commitment of many German economists to historical studies ought to be regarded in all circumstances as a pernicious one-sidedness, even if the “history of the economy” were indeed a *part* of “political economy.” Under the above assumption, this commitment should be designated as a one-sidedness, a pernicious one-sidedness at that, because it is necessarily accompanied by a similarly one-sided neglect of theoretical research in our discipline, while it is precisely theoretical economics that, because of its backward condition, needs to be urgently reformed.

The history of the economy, however, is not a *part*, but an *auxiliary discipline* of political economy—a useful, an indispensable auxiliary discipline, but only an auxiliary discipline. [32] Thus the almost exclusive commitment of erudite German economists⁵⁴ to the treatment of the same is such an obvious one-sidedness that it is inconceivable how an opposition of opinions even emerged here.

Do you believe that after what was said here there can be any reasonable doubt about my position on the above question? Certainly not for someone who cares about the truth.

My friend, as we fight the one-sidedness of the historical school of German economists, let our rivals still complain about our not recognizing their merits in the field of history and our ignorance of the importance of the latter for our discipline. Henceforth, no sensible and unprejudiced evaluator can be doubtful

53. [*Erscheinungswelt*]

54. [*gelehrte deutsche Volkswirthe*]

about the fact that by making such assertions, Schmoller merely attempts to evade the actual subject of discussion.

What I fight is the aforementioned one-sidedness of the historical school; what I advocate is *the reinstatement of all legitimate directions of human inquiry in the field of the economy*. It is not me who wears the “blindness of the scientific division of labor.”⁵⁵

“Someone who lets Menger’s account work on them⁵⁶ in an unprejudiced manner, and in particular not as a representative of a one-sided direction, will derive from his account *the full appreciation of the mutual dependence of all directions of research as a result of the predisposition of our mind*.”⁵⁷

[33] By contrast, someone who follows Schmoller’s literary activity in an equally unprejudiced manner will, from the half-dozen writings that he has thus far published about the development of the conditions in Strasbourg’s trades,⁵⁸ surely have gained no less than the impression of universality.

Seventh Letter.

[About the opinion that economic history has to be fully researched before one can proceed with reforming political economy] [34]

What I fought against in my *Investigations* was not the repeatedly emphasized indirect utility of historical studies for research and teaching in the field of political economy, but the confusion of theory and historiography, the one-sided devotion of a substantial part of the German representatives of our discipline to the treatment of an auxiliary science.

What is the reason for this one-sidedness? Why, you ask me, has the above-mentioned error emerged, which has become so pernicious for the development of political economy in general and for its theoretical part in particular?

I do not want to speak exclusively about Schmoller and his like-minded professional peers. I have already spoken about the specific causes of the historicism of these writers. However, the conditions hinted at here are only coincidental after all; such a widespread phenomenon as historicism in the field of the German economics can only be the result of much more universal causes. [35] The one-sided overestimation of historical studies on the part of some of

55. *Investigations*, pp. 28ff. [XVIII ff.].

56. [*auf sich wirken lässt*]

57. E. Sax, *Das Wesen und die Aufgaben der Nationalökonomie*. Vienna, 1884. p. 32.

58. [*Entwicklung der Strassburger Gewerbeverhältnisse*]

our German economists is indeed rooted in a series of errors about the nature of political economy and the relationship of historical studies to this discipline; a series of false fundamental assumptions which prevail among our historical economists and which, however, at first glance are able to give one-sided historicism in our science the appearance of justification.

Here I would like to commemorate the widespread opinion among German economists that the path to a reform of political economy, or at least *the next step towards it, is the exploration of economic history*.

“When science temporarily proceeds in a predominantly descriptive fashion,” Schmoller writes⁵⁹ “this by no means implies a neglect of theory, but attention to the necessary substructure ... That such work temporarily prevents part of scientific energy from further work on theory is the nature of the scientific division of labor.”

I hope I have already emphasized in a more than sufficient manner that history and statistics are important auxiliary sciences of political economy and, in this sense, a “substructure” of the latter. However, from the importance of history and statistics as auxiliary sciences of political economy, even if this importance were exaggerated in the most one-sided way, the consequences drawn by Schmoller do not follow at all. [36] If the historical sciences of the economy are important, even indispensable, auxiliary sciences for theoretical economics, it can only be reasonably concluded that research in the field of the latter should collect the results of historical research and statistics, and use them for its purposes. It would follow from this that the political economists would have to collect and use the historical and statistical material researched by historians and statisticians most diligently and carefully for *their* purposes—for determining the “laws” of economic phenomena, etc.

Never have the historians of all peoples paid more attention to cultural history and to cultural statistics in general, and to the history and statistics of the economic side of the life of peoples in particular, than they do in our time; never before has the volume of historical-statistical material to be dealt with by the theorists in the field of the economy been greater; never have the circumstances been so favorable, even for those branches of economic theory which are predominantly based on the results of history and statistics, as it is in the present. Indeed, social philosophers today lack historical-statistical material for theoretical research in the field of the economy less than ever before—even those who cultivate the above-mentioned branches of economic theory.

[37] That is not at all the motivation for the historical economists of Schmoller’s strict observance. Not the utilization of the results of historical

59. *Jahrbuch*, pp. 241 ff.

research for political economy, but historical research itself, especially historical-statistical miniature painting in the field of the economy, is what traps the spirits of the above-mentioned group of scholars who do not want to renounce their claim to be political economists. They do not want to let go of their historical micrography—there would be nothing to object to that; they want to be considered to be working in the field of political economy, not in the field of auxiliary sciences—one could even be silent about that; but they want once and for all—or at least for uncounted generations—exclusive, or the almost exclusive, control of economic history in the field of political economy—and every prudent person has to repudiate that!

In order to cloak the completely untenable point of view mentioned above in the semblance of a certain justification, the history and statistics of the economy must be stamped as the descriptive “*part*” of political economy, whereas in reality they are no *parts* at all, but only sciences auxiliary to political economy; for the same purpose, the idea must be maintained that only this “descriptive part” of economics must be worked out, at least at first or predominantly.

“In the future,” says Schmoller, “a new epoch will come for economics, but only through the utilization of all the historical-descriptive and statistical material that is now being created;” [38] in the meantime “it is no neglect of theory, but the necessary substructure for it, when in our science one proceeds in a predominantly descriptive manner.”⁶⁰

A. Wagner and H. Dietzel rightly protest “against this bill with a somewhat long expiration date,”^{61,62} and E. Sax just as correctly observes that “it is quite a distorted thought to deny our time the vocation of obtaining a satisfactory theory of the economy until an incalculable number of studies in the field of economic history have been completed.”⁶³ However, it seems to me that Sax is still far too optimistic if he wants to calculate the required time period in *generations*. If economic history were to be completed in the spirit of Schmoller’s historical micrography—just think of the meat prices of Elberfeld! of Pforzheim! of Mühlheim! of Hildesheim! of Germersheim! of Zwickau! etc.—before one can resume work on theoretical economics, only eons would suffice. Just like astronomers had to introduce the concept of light years into their science in order to calculate their enormous distances, we economists would also have to begin to calculate at least in ages of solar systems in order to get even an approximate concept of the time periods that would be necessary to gain a complete historical-

60. *Jahrbuch*, pp. 241 ff.

61. [„gegen diesen Wechsel mit etwas langer Verfallszeit“]

62. Hildebrand’s *Jahrbücher [für Nationalökonomie und Statistik]*, edited by J. Conrad, 1884, N. F., VIII. p. 109.

63. E. Sax, p. 3.

statistical foundation for theoretical research in Schmoller's sense.

[39] It should also be taken into account that the historical material to be researched constantly renews itself as a result of the fact that economic history does not stand still, and even multiplies in a certain sense as a square function⁶⁴ with regard to the upswings of the economic side in the life of a people, while the genuine Schmollerian historiography would at most be able to follow it in arithmetical progression, and thus Schmoller's adventurous thought only seems even more adventurous.

But even if we disregard the special type of Schmoller's historiography in the field of the economy, there still remains so much naïveté in the above thought that it will be difficult to take it seriously. Schmoller's thought is as unqualified as that of a historian or statistician who would advise his professional peers to leave historical and statistical studies for periods on end, and to meanwhile dilettantishly dabble exclusively, or at least predominantly, in the field of social philosophy—namely for the reason that theoretical social sciences are important auxiliary sciences of historical research, but the results of the former are still deficient! According to Schmoller, the current backward state of the sciences of the economy should actually be an invitation for historians and statisticians to deal with theory, and for theorists with history and statistics! Of course! [40] Just do not work in your own field of research! This is much too common, and by the way much too arduous and difficult, while dilettantishly dabbling in foreign fields is just as noble as easy. Nothing in the world is more convenient than this “ut aliquid fecisse videatur”⁶⁵ in the field of a science.

And even to that nobody would object if Schmoller did not appear with the strange pretension that his historical micrography is, whether at all or at first, the principally justified direction of research not in the field of historical sciences of the economy—one could even argue about that—but in the field of political economy!

Schmoller apparently has—I do not know why—no idea how much needs to be done in the field of political economy, even on the basis of our current auxiliary tools, and how much more important it is for *our* science that the material researched by historians and statisticians is used for the purposes of theory and practical sciences of the economy, rather than that new material is unearthed (by the representatives of our science!) about any peculiarities of the economy, for example about Strasbourg meat prices or certain clothier guilds.

By the way, does Schmoller seriously believe that a theorist who takes his task seriously will turn to the results of *his* research for historical or statistical instructions? I will not speak here about whether Schmoller's scientific tempera-

64. [*in quadratischem Verhältnisse*]

65. [Translation: “Showing that something has been done.”]

ment is particularly conducive to unbiased historical research and objective depiction of history. [41] In fact, I would like to describe his historical works as quite respectable. Schmoller, however, will probably not indulge in the belief that these have the guarantees of reliability that the theorist demands of historical and statistical works—guarantees of reliability that only professional historians and statisticians in the field are able to offer. Schmoller's historical and statistical works are in any case very worthy achievements;⁶⁶ however, our praise of the author could be much more unrestricted if these works came from a chamber of commerce secretary, from an editor of a trade newspaper, or from the history club of some Prussian provincial town. Historical and statistical works of such provenance are used by the theorists from the outset with the precaution that corresponds to the guarantees of their reliability and the expertise of their authors. However, that a professor of *political economy* almost exclusively brings to light almost exclusively such valuable work in fields whose methods he does not master completely, is at any rate an unusual phenomenon; it would be close to ridiculous, however, if Schmoller, for the sake of his above-mentioned works, seriously considered himself a historian.

Truly, Schmoller's example is not so tempting that any representative of political economy could be induced to leave his own field of scientific research to devote himself to dilettantism in the field of historiography!

Eighth Letter.
**[About the opinion that economic history is the
exclusive empirical foundation of research
in the field of political economy] [42]**

I believe that I would not exhaust the objections of the historical school against my position in the question of the relationship of political economy to the historical sciences of the economy if I did not consider a peculiar form of historicism in our science, which has contributed, to no lesser extent than the form characterized in my previous letter, to the overestimation of historical studies and to the one-sided devotion of German economists to the latter: I mean the widely held view among German economists *that history is the exclusive empirical foundation, both of theoretical economics⁶⁷ and of the practical sciences of the economy.* To clarify this erroneous view, both in terms of theoretical economics and the practical sciences

66. [*sehr wackere Leistungen*]

67. [*Volkswirtschaftslehre*]

of the economy, seems all the more important to me since the doctrine in question on the issues of methodology of our science is of crucial importance for the whole position of the historical economists.

[43] The supporters of the above-mentioned opinion seem to me first and foremost to overlook—to start with *historicism in the field of theoretical economics*—that in addition to history, common life experience (knowledge of motives, of goals, of circumstances determining success, and of the successes of individual economic activity) is a necessary basis of theoretical economics. The complicated phenomena of the economy are predominantly the result of the contact of individual economic endeavors,^{68,69} so that the understanding of these and their interrelationships is a necessary condition for understanding the complicated phenomena of the economy. However, the history of the economy does not offer us the knowledge of individual economic processes⁷⁰ [44] or even less of their psychological motivation; indeed, for reasons which I have considered in detail elsewhere, it cannot grant us such knowledge at all.⁷¹ Only those who completely misunderstand the nature of historiography can describe history as the exclusive empirical foundation of theoretical economics.

Even less can history be described as the exclusive empirical foundation of the *practical sciences of the economy*; rather, it is self-evident that even a thorough knowledge of the past of the peoples would not per se allow us to identify the principles of expedient intervention in the economy, and of expedient action in it. The economic life of the peoples incessantly brings to light new tasks of economic policy and the administration of public finance,⁷² which cannot be solved exclusively on the basis of studies of the past but only on the basis of recognizing the respective exigencies of political life that go far beyond mere historical and statistical knowledge, the changing view of the tasks of state action, the state of

68. [*des Contactes individualwirtschaftlicher Bestrebungen*]

69. *Investigations*, pp. 193ff. [232ff.].

70. Theoretical economics has not only to study the general nature and the general connection of those phenomena of the human economy, such as market prices, prices of bills and stocks, currency, banknotes, business crises, etc., which are phenomena of the “*economy*”—which is the result of the contact of individual economies connecting towards a higher unity, by the transport of goods or by the state activity directed towards the care of this organism of individual economies (*Investigations*, pp. 194ff. [pp. 233ff.])—but also the *nature of the singular phenomena of the human economy and their connection to the phenomena of the “economy”* in the above understanding of the word. Economics must also, for example, explain to us the nature of “individual needs,” the nature of “goods,” and even the nature of economic phenomena such as “use value,” which are quite subjective, and real only in the *individual*. How could it draw the knowledge of the nature of these phenomena and their connection to the phenomena of the “economy” exclusively from history? The opinion that history is the exclusive empirical foundation of the social sciences is a striking one-sidedness (*Investigations*, pp. 116ff. [121 ff.]).

71. *ibid.*, p. 117 [122].

72. [*Aufgaben der Volkswirtschaftspflege und der Finanzverwaltung*]

the art in technical sciences, etc. [45] The historian who is “a prophet facing backwards”⁷³ cannot be the sole authority in the field of practical economics. Historicism in the above sense is also a striking one-sidedness with regard to economic policy and public finance.⁷⁴ If theoretical and practical economics are to some extent supposed to be in line with life’s requirements upon science, then historicism is completely untenable and can only be explained by the errors of our historical economists regarding the nature and tasks of political economy.

Whoever, like the historical economists, regards *theoretical economics* as a “science of the parallelisms of economic history,”⁷⁵ and who sees in practical sciences of the economy only a systematic representation of the economic goals pursued by the main civilized peoples⁷⁶ of the past, of the measures taken in the past to achieve those goals and resulting successes, that person may indeed find his satisfaction in historical studies. Whoever, in contrast, recognizes in these endeavors of our historical economists only particular, albeit highly appreciable branches of research in the field of political economy, whoever recognizes theoretical economics as the science of the manifestations and laws of economic phenomena, and whoever recognizes the practical sciences of the economy as the sciences of the principles [46] of expedient economic policy and of proper regulation of the state budget, that person will be able to describe history and statistics of the economy as important auxiliary sciences, but never as the exclusive empirical foundation of research in the field of political economy.

By devoting themselves almost exclusively to historical studies, our historical economists, especially our neo-historical economists,⁷⁷ thus not only fall into the one-sidedness of putting an auxiliary science in the place of that science which they are primarily responsible for. Instead of studying the “laws of the economy” and the “principles of expedient action in the field of the economy,” they collect empirical material for identifying the above-mentioned scientific truths. Yet their one-sidedness is much greater. They deal only with one of the numerous auxiliary sciences of political economy and, moreover, with one that is able to offer us only a part of the empirical material necessary for identifying the truths of political economy, while they fancy to be working in the field of political economy itself.

The above view is comparable to that of the carter who wanted to be considered the architect because he had brought some loads of stones and sand to the building.

73. [The quotation is from Friedrich Schlegel, *Athenaeum-Fragmente*, Berlin, 1798, Fragment 80.]

74. [*Volkswirtschaftspolitik und Finanzwissenschaft*]

75. [Discussion of “parallelisms of economic history” is found in Roscher (1849, 180–182).]

76. [*hauptsächliche Culturvölker*]

77. [*neuhistorische Volkswirthe*]

Ninth Letter.

[That historical-statistical material which is ordered according to certain scientific categories must not be confused with political economy] [47]

Do not believe, my friend, that the opinion that history is the exclusive empirical foundation of political economy is the last trump card that historicism has played in our science. In the same way as any one-sidedness needs to be pursued up to its most extreme inexorability,⁷⁸ to play itself out as it were, to be generally recognized as such in the end, so too historicism in the field of political economy has not stopped at the above view. A part of our historical economists gave up the idea of theoretical and practical sciences of the economy altogether, and recognized historical depictions as the only legitimate task of research in the field of the economy. Yet even those who hold onto the idea of theoretical and practical sciences in the above field of phenomena more or less inexorably were able to take historicism in political economy one step further beyond the previously characterized position.

[48] Whoever regards the results of historical research as the exclusive empirical foundation of theoretical economics and of the practical sciences of the economy disregards the importance of all other empirical foundations, and moreover disregards the rational foundations of theoretical and practical direction of inquiry in the field of the economy. He will find in the “laws of economic phenomena” mere “developmental laws,” “parallelisms of economic history;” and in theoretical economics not a science of the “laws of economic phenomena,” but a science of these “parallelisms of economic history;” he will be guided by the one-sided view described above to regard the practical sciences of the economy not as depictions of the principles for expedient action in the field of the economy which is suitable within a given situation, but instead solely as depictions of experiences borrowed from economic history about the goals, the measures and successes of economic policy and public finance, etc.

However one-sided his position may be as a result of the views described above, he will nonetheless deny neither the existence of “laws” of the phenomena nor the existence of “principles for expedient action” in the field of the economy in general. History and statistics will also for him just be the empirical *foundation* on which—one-sidedly as they may be perceived—the truths of theoretical

78. [*bis in ibre äusserste Consequenz*]

economics and of the practical sciences of the economy still need to be researched. [49] The theoretical and practical insights will be, also according to this view, different from the historical-statistical material on whose basis they are obtained.

Even this view of the nature of our science, however, seems to not have satisfied the one-sided historicism of a number of German economists. These economists rather formulate as a postulate of research the principle that in political economy, too—in both its theoretical as well as its practical parts—“history has to speak for itself.” *Historical-statistical material ordered according to certain categories is supposed to take the place both of laws of economic phenomena and of principles for the expedient advancement of the economy, or of the expedient setup of the national budget.* If Schmoller demands that “economics should proceed essentially in a descriptive manner and offer the student a concrete individual picture, but ordered by concepts, types, and relationships, etc., and specialized to the point of pursuing the unique in the phenomena and causes,”⁷⁹ he is merely documenting himself as a representative of a view which is barely compatible with political economy: As a representative of a view who wants to replace theory and the practical sciences of the economy “essentially” with [50] historical-statistical material which has been ordered according to certain categories—this is no doubt the lowest position on which a social science can be based.

What, in this respect, was the path of development within the historical school of German economics?^{80,81}

Theory! — Theory garnished with historical-statistical notes and pierced by historical digressions! — Sheer notes and historical digressions with a claim to be a theory!

Further “progress” in this direction is, however, hardly possible.

Tenth Letter. [About Schmoller’s opinion of the tasks of practical economics] [51]

My remarks on the *relationship of theoretical economics to the practical sciences of the economy* were also unable to find Schmoller’s agreement. I had characterized theoretical economics as the science which has to study and describe the general nature (the manifestations!) and the general connection (the regularities in

79. *Jahrbuch*, p. 246.

80. [*historische Schule der deutschen Nationalökonomie*]

81. [Menger’s common caption is “*historische Schule deutscher Nationalökonomien*,” so “of German economists” and not “of German economics.”]

coexistence and sequence—the laws!) of economic phenomena;⁸² in contrast, economic policy and public finance I had characterized as the sciences of the principles, the maxims, according to which—depending on the various circumstances—the economy can be advanced most expediently or the public budget be set up most expediently.⁸³ For the sake of a more precise explanation, I compare the relationship between the first and the latter two sciences [52] to the one between anatomy and physiology on the one hand and surgery and therapy on the other.⁸⁴ Theoretical economics is in a similar way the theoretical foundation of the practical sciences of the economy, as are anatomy and physiology the theoretical foundations of those sciences which teach us the principles and procedures for expedient intervention in the human organism.

I believed that I had expressed myself to those for whom scientific works are written in a sufficiently intelligible manner. Even though it was obsolete, I added to the above-mentioned explanations the remark that the practical sciences of the economy themselves are fit for practical application, and that the above-mentioned sciences as well as the practice of politicians in charge of economic policy and of administrators of public finance are therefore not to be confused. There is the same difference between the latter as, for example, between surgery and therapy (which are also practical sciences!), and the practice of scientifically educated physicians, or as between chemical and mechanical technologies and the activity of practical chemists and mechanics.⁸⁵

Let us now hear what Schmoller has to object to these remarks.

He literally writes: “These disciplines (economic policy and public finance), as they are usually presented, and treated and mistreated in older textbooks, certainly want to be practical instructions at the same time; [53] the older, partially still used books were nothing but recipe collections in social policy, administrative law, and public finance. But it is a progress of more recent times that they moved beyond this; Roscher’s second and third volumes especially, as well as Stein’s and Wagner’s public finance, represent the most successful attempts *to elevate* these disciplines (economic policy and public finance!) *to the rank of theoretical sciences.*”⁸⁶

Schmoller hence considers it to be a deficiency of economic policy and public finance, a *mistreatment* of these sciences, if they, as he purports to be the case in older textbooks, “want to be practical instructions *at the same time*”? What else, with all due respect, should a practical science⁸⁷ “want to be,” after all, but a

82. [*volkswirtschaftliche Phänomene*]

83. *Investigations*, p. 39 [9] and pp. 203ff. [245ff.].

84. *ibid.*, p. 204 [246].

85. *ibid.*, pp. 203ff. [245 ff.].

86. *Jahrbuch*, p. 245.

87. In many passages, I designate the practical sciences as *so-called* arts and I believe also here to have

practical instruction in the above sense? There is no practical science which is *per se* something other than a practical instruction in the above meaning of the word, and the practical sciences of the economy are obviously no exception. They should teach us, not only “*at the same time*,” but *altogether*, the principles for expedient action in the field of the economy, depending on the various circumstances. [54] So, what is supposed to constitute the mistreatment of the practical economic sciences in the “older books”? Only in the eyes of a scholar in whose mind a complete confusion prevails about the nature of political economy and its parts, may the endeavor to solve the natural and proximate tasks of the practical sciences of the economy appear as a mistreatment of these sciences.

Indeed! As follows from his remarks, Schmoller can only think of the practical sciences in the predominant meaning of the word “mainly” as *recipe collections*; “the older, partially still used books are,” deems Schmoller, “nothing but recipe collections in social policy, administrative law, and public finance.”

So a science which teaches us the *principles*, the *maxims* for expedient action depending on the various circumstances, is a practical instruction in the sense of a recipe collection? Schmoller can only think of a science for expedient *principles* for the care of the economy, depending on the differences in local and temporal circumstances, or of the *principles* for the expedient setup of the national budget, as an economic recipe collection? Surgery and therapy are practical sciences, hence recipe collections? Technology is a recipe collection?

And the older, partially still used books about economic policy and public finance, except Roscher, Wagner and Stein, were nothing [55] but recipe collections in social policy, administrative law, and public finance?

How profound must the insight of an author be into the nature of the practical sciences of the economy, and his knowledge of the respective literature, for such a view to become possible!

Eleventh Letter.

[About Schmoller’s idea to elevate the practical sciences of the economy to theoretical ones] [56]

To ensure that economic policy and public finance can avoid being recipe collections in the future as well, Schmoller demands “that these disciplines *be elevated to the rank of theoretical sciences*,” which means in his sense to transform them into theoretical sciences; indeed “Roscher’s second and third volumes, Stein’s and

expressed myself intelligently enough for anyone who wants to understand me.

Wagner's public finance already are successful attempts to elevate these disciplines to the rank of theoretical sciences."⁸⁸

Above all, I would think that all sciences, whether theoretical or practical, possess the same rank, the latter no lower than the former. Surgery and therapy, mechanical and chemical technology, economic policy and public finance pose different—though certainly not lower—demands to the scholars' efforts and their genius; only in Schmoller's fantasy are they inferior to the "rank" of their corresponding theoretical sciences. A ranking⁸⁹ of the sciences in the sense of Schmoller [57] does not exist at all; the practical sciences do not need any "elevation" to theories.

The sciences do not differ—which Schmoller seems to overlook—by their "rank," but by the *tasks* they have to solve. The theoretical sciences have to study and to describe the general nature (the manifestations!) and the regularities in the coexistence and sequence of phenomena (the laws!); the practical sciences, on the other hand, have to study and to describe the principles for expedient action, for expedient intervention into the manifestations. In this, in the diversity of the tasks, lies the difference between the theoretical and practical sciences, and the elevation of the latter to the former is a thought approximately of the same depth as if the "elevation" of the foundation to the façade, or of a column's capital to its pedestal, were presented as an epoch-making revolution in the field of architecture. As pointless as it would be to "elevate" surgery and therapy to anatomy and physiology, chemical and mechanical technology to chemistry and mechanics, as little can one reasonably speak of an elevation of the *practical sciences* of the economy to a *theory* of economic phenomena.

Every science can, however, in a certain sense be elevated, i.e., 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 [58] in as perfect a manner as the state of human knowledge permits. Of course, this also applies to the practical sciences. These also are capable of unlimited perfection, but certainly not in the way planned by Schmoller, through transformation into *theoretical* sciences. In order for the practical sciences not to be recipe collections, one must base them, as I have explained in detail,⁹⁰ on the theoretical sciences: Surgery and therapy on anatomy and physiology, and yet not only on these, but also, as I have shown, on physics, mechanics, chemistry, etc.; mechanical and chemical technology on mechanics and chemistry, and yet not

88. [This quotation on Roscher's, Stein's, and Wagner's works deviates slightly from the similar quotation in the Tenth Letter.]

89. [*Tschin*]

90. *Investigations*, p. 211 [257].

only on these, but also on physics, mathematics, etc. Finally, the *practical sciences of the economy* (economic policy and public finance!) first and foremost on theoretical economics—however, not only on this, but on all those theoretical sciences whose knowledge is necessary to identify the principles for expedient action in the field of the economy.

Practical sciences of this kind have their “rank” in the circle of sciences by themselves—they do not need any “elevation” other than the one just described, least of all Schmoller’s proposed elevation into theoretical sciences.

Schmoller belongs to those scholars who [59] have an insurmountable aversion to the treatment of all problems arising from the nature of individual sciences. No science is good enough for him in this respect. He wants to elevate the *theory* of the economy to a *historical* science, and *practical* economics to a *theoretical* science. If he were a professional historian, he would attempt to elevate *history* to a “natural science;” if he were a therapist, he would attempt to elevate his discipline into a “physiology;” if he were practicing botany, he would undoubtedly seek to “elevate” it to a “zoology of the plant world.”

He is the prototype of the “problematic nature” in the field of science.

Twelfth Letter. [How Schmoller imagines this elevation] [60]

After what has been said, you, my friend, will certainly be no less curious to find out how Schmoller actually imagines the elevation of the *practical* sciences of the economy to the rank of *theoretical* ones. Let him teach us about this himself. He literally writes the following:

*Practical economics can completely strip off the garment of an art*⁹¹ if it presents the special development of the German, and possibly of the German and the French-English economy of the last centuries in the field of the agricultural, industrial and trade policy, and its causes and consequences in detail. It then confines itself to essentially proceeding descriptively, but is perhaps a just as good or better educational aid for future civil servants than if it merely wanted to be an art, *i.e., than if it gives free-trade or state-socialist advice.*⁹²

Note, my friend, the delicious logic [61] that lies in the final sentence of these remarks. But this only in passing. Let us examine the modalities under which Schmoller, a modern Apollo, does not want to “completely strip off” the skin of

91. [das Gewand der Kunstlehre vollständig abstreifen]

92. *Jahrbuch*, pp. 245 ff.

practical economics, his Marsyas, but “the garment of an art.”

That the depiction of the special development of the German, and not only “possibly” but *certainly* also the “English-French,” besides probably also the Italian, the Spanish, the Portuguese, the Dutch, the American economy, etc., and namely “depiction of them in their causes and consequences in detail,” including all “fields” and periods (not only those mentioned by Schmoller!), in short and plain German: “*that an economic history of the civilized peoples⁹³ adequate for its tasks*” is an expedient educational tool “for the statesman and, of course, also for the future civil servant” was probably known already before Schmoller. The usefulness of the history of the economic policies of individual states and their financial systems, as well as the usefulness of financial statistics, are so beyond all doubt, so beyond all discussion for the scholar of practical economics,⁹⁴ that Schmoller should finally spare us with that sort of thing. History and statistics are useful for scholars in the field of political economy—useful for theorists, useful for practitioners, useful for students, useful for future civil servants, useful for everyone. How many times have we heard this?

*Factum est jam tritum sermone proverbium!*⁹⁵

[62] How should the above truth relate to the question of the “elevation” of the practical sciences of the economy to a theory of the latter?

Or is it possible that Schmoller even thinks about this elevation in a way that—? No! It is impossible. And yet, you, my friend, refer me to his own words. Should Schmoller even be of the opinion that a *practical* science could be “elevated” to a *theoretical* one by putting a *historical* one in its place, assigning it the tasks of historical research in the respective field of knowledge? A *mammal* should be elevated to a *reptile* by putting a *bird* in its place?

No! My friend, I do not believe that even Schmoller is capable of such thought evolutions,⁹⁶ specifically at the very moment when he “prepares himself, after a long interruption, to hold his lecture on methodology of the sciences of the state.”⁹⁷ Again, no! Such absurdity is impossible, especially in such a solemn moment for the sciences of the state! Let us read again before we trust our eyes.

Practical economics can completely strip off the garment of an art if it presents the *special* development of the German, and possibly of the German and the French-English, political economy of the last centuries in the fields of

93. [*Wirtschafts-Geschichte der Culturvölker*]

94. [*praktische Wirtschaftswissenschaften*]

95. [Translation: ‘That old proverb is employed in conversation! (The quotation is from Cicero’s *De officiis* I,10.)]

96. [*Denkevolutionen*]

97. *Jahrbuch*, p. 239.

agricultural, industrial, and trade policy, and its causes and consequences in *detail*. It then confines itself to *essentially* proceeding *descriptively*, [63] but is perhaps a just as good or better educational aid for future civil servants than if it merely wanted to be an art, i.e., than if it gives free-trade or state-socialist advice.

If in the field of any other *practical* science—let us take the example of surgery or therapy—a writer would conceive the idea of not basing these disciplines on physiology and anatomy (the corresponding theoretical sciences!), but of elevating them to these latter disciplines, i.e., in Schmoller's sense transforming them into theoretical natural sciences, then all of that person's professional peers would begin to shake their knowledgeable heads with concern. But if the same author wanted to elevate surgery or therapy to the status of physiology or anatomy in such a way that he wanted to replace them with a historical science such as ethnography or anthropohistory,⁹⁸ a most active sympathetic concern⁹⁹ among his medical colleagues would certainly turn to him immediately. And yet, fundamentally, he just would have failed to choose the right terrain for the publication of his discoveries. Had he pronounced the same thought in the field of political economy, he would not only be able to describe it as the result of his tireless historical and philosophical studies, but perhaps even find believing souls who would be willing to accept thoughts like that for epoch-making truths.

Thirteenth Letter. [64]

[Another opinion of Schmoller about this same topic]

You reply to me that Schmoller is not only of the opinion, mentioned in the previous letter, about the way in which practical economics¹⁰⁰ should completely strip off the garment of an art and be elevated to a theoretical science, but also of another opinion and that it therefore would be unfair not to mention it at this point. You are right and, in order not to tantalize anyone's curiosity, I would like to express Schmoller's second view here immediately. Shortly after the passage quoted in my previous letter, he writes:

It (practical economics which has completely stripped off the garment of an art)¹⁰¹ then gives (!?) the student a concrete individual picture, but *one that is*

98. [*Anthropohistorie*]

99. [*allgemeinste werktätige Teilnahme*]

100. [*Wirtschaftswissenschaften*]

101. [The expressions interjected with parentheses into the quotations, e.g., (!?), are additional, comment-

ordered according to the concepts, types, relationships which result from the general theory of economics and that is specialized to the point of pursuing what is unique in the phenomena and causes, which are either completely absent or recede in the pale overall picture of general economics. [65] And the same is true of public finance.¹⁰²

Schmoller might ask himself whether he suddenly changes his mind in this sentence. In order to elevate the *practical* sciences to the level of *theoretical* ones, he still wants to “strip” the former of all the *principles* for purposeful intervention in the phenomena of the economy, i.e., that which makes them practical sciences. He adheres to this idea. However, he—if I have understood Schmoller correctly—no longer wants to replace the practical sciences completely stripped of their “garment of an art” with economic history per se, but rather with historical-statistical depictions of the individual fields of the economy, *ordered according to the categories of “general” economics*.

I will apply the juridical principle “*Lex posterior derogat priori*”¹⁰³ to Schmoller’s remarks and assume that, whatever else seems to emerge from his remarks, he is not of his first but of his second opinion. And now you ask me what I have to say about the above-mentioned way in which Schmoller intends to “elevate” the practical sciences of the economy to theoretical ones?

Even my opponent, who so strongly dislikes the practical sciences of the economy to the point of completely negating them as independent sciences, will probably no longer burden me with dealing seriously with the above-mentioned view. [66] Certain thoughts are refuted as soon as their meaning is made clear, as soon as they are stripped of the phraseology in which their author wraps them. If someone wants to begin the elevation of the practical sciences of the economy to theoretical ones by first “stripping off” all the principles for purposeful action in the field of political economy, i.e., everything that makes the above-mentioned disciplines what they are: There is as little to argue with such a person as with a surgeon who wants to regenerate an organism by the amputation of all its organs. If “we strip the practical sciences of the economy of all the principles for purposeful action in the field of political economy,” then as much remains of them as of an economic history from which we strip it of all “depictions of historical developments,” or of a theoretical political economy from which we strip it of all “laws of economic phenomena”—i.e., the well-known knife without blade and handle.

But let us assume that Schmoller wrote down the sentence quoted above

like interventions by Menger.]

102. *Jahrbuch*, p. 241.

103. [Translation: “A later law repeals an earlier.”]

conscious of the consequences resulting from it. Let us also assume that the nirvana in the field of practical sciences indeed represents his ideal, or the first stage in his quest to elevate the practical sciences of the economy to theoretical ones: So the question arises immediately how he wants to solve his problem by starting from this negative basis?

[67] Certain historical representations more closely specified by Schmoller ought to be classified—Schmoller believes—“according to the concepts, types, relationships which result from the general *theory* of economics” (!!!), respectively to be added to the corresponding doctrines of “*general*” economics.

However, someone could be so presumptuous to ask how a *theoretical* science can become a *practical* one by adding *historical* depictions of any kind to it?

*Quidquid non est simpliciter tale, illud non est cum addito tale.*¹⁰⁴

In the addition of historical depictions to a *theoretical* science—so you interject—one can, at most, even in the lowest sense, recognize a historical treatment of this theoretical science; but it is not foreseeable how by doing so *practical* sciences are to emerge, which have completely stripped off the garment of an art and which have been elevated to theoretical sciences.

How little you can follow the soaring thoughts¹⁰⁵ of Schmoller! Just listen to what he continues to write:

It (practical economics which has been elevated to a theoretical science!) then gives the student a concrete individual picture, but one that is ordered according to the concepts, types, relationships which result from the general theory of economics and that is specialized to the point of pursuing what is unique in the phenomena and causes, which are either completely absent or recede in the pale overall picture of general economics.

[68] Are you still not understanding?

You object that a theoretical science and therefore also a science of the economy has neither a concrete nor an abstract *picture* to teach us, but the *laws* of phenomena; offering us a concrete picture of phenomena, in contrast, is the task of the historical sciences. Notwithstanding that: How would that pale image of phenomena, which Schmoller calls *general* economics, be able to become a “*practical* science of the economy” by adding to it historical depictions of any kind, and even more so one that is “elevated” to a theoretical science?!

You repeat, my friend, this unpleasant question. Yes, you doubt that there is a second science in Germany in which such argumentation could be seriously put forward, and put forward by the editor of a scientific journal. You think that

104. [Translation: Whatever is not like this in a simple way also isn't like this if you add something.]

105. [*Gedankenflug*]

these are monstrosities which show a deep decay of abstract thinking in the field of political economy. Where, you cry out, did even the simplest, the most fundamental concepts of philosophy of science¹⁰⁶ end up in the new historical school¹⁰⁷ of German economists, if such things are possible?¹⁰⁸

[69] “*Rarus...ferme sensus communis in illa fortuna.*”¹⁰⁹

That is what *you* think.

But now let us listen to what Schmoller himself thinks about this: “For someone who has this point of view,” he exclaims triumphantly, “the methodological differences in the treatment of theoretical and practical economics are only gradual, not fundamental as for Menger. Who thinks and teaches like this cannot regard it the worst scientific crime to have mixed the method of theoretical and practical economics.”^{110,111}

[70] Joking aside, Schmoller is right: For someone who has this point of view, someone who thinks and teaches like this, *everything* seems indeed already to be one. There is no longer an unbridgeable gap between history and statistics of the economy on the one hand and theoretical economics on the other; economic policy and public finance have been completely stripped of the garment of an art; by adding *historical* depictions to *theoretical* economics, the *practical* sciences of the economy have been elevated to a *theoretical* science, figuring as more special parts of the more general and therefore pale “*image*” that theoretical economics henceforth presents to us, etc., etc.

Anyone who does not recognize in this a conception of the essence of political economy that is as deep as it is philosophical, its parts and the relationship

106. [*Wissenschaftslehre*]

107. [*neuhistorische Schule*]

108. It should also be noted here that individual passages in Schmoller’s critique (p. 245) show that he thinks of the practical sciences of the economy, which have been elevated to theoretical sciences, [69n.] as *more special* parts of a *general* theoretical economics. In doing so, Schmoller overlooks the fact that *practical* sciences can never stand in this relationship to *theoretical* ones, but rather that both theoretical and practical sciences have general *and* special parts. As little as chemical technology can be described as a special or “more detailed” part of chemistry, or therapy a special part of physiology, can the practical sciences of the economy be described as special parts of a general economics. In reality, economic policy has as much a general *and* a special part as theoretical economics. The same applies to public finance (*Investigations*, p. 204 [247]).

109. [Translation: “Generally common sense is rare in that rank.” (The quotation is from Juvenal’s *Satires*, VIII.73.)]

110. [The quotation is from *Jahrbuch*, p. 246. In Menger’s original the closing quotation mark is omitted.]

111. “The dispute whether (with regard to political economy) we are dealing with a ‘*science*’ or an ‘*art*’ has been settled. Both are present: Pure theory as foundational science and art as applied science; although it was only recently that one had to recall the methodological consequences entailed by the differences between the two parts of the entire field of knowledge.” (E. Sax, *Das Wesen und die Aufgabe der Nationalökonomie*, 1884, pp. 21 ff.). But if Sax thinks that economists can hardly disagree on this issue, [70n.] then the above-mentioned passage in Schmoller’s remarks should prove the opposite.

of these latter to each other and to their auxiliary sciences, is an immodest and philosophically not sufficiently educated person who cannot do anything better for his scientific¹¹² education than to sit at the feet of the editor of the Berlin *Jahrbuch* to see and to hear how this profound methodologist¹¹³ “thinks and teaches.”

Fourteenth Letter. **[About the characteristics of Schmoller’s fighting style] [71]**

You say that I almost seem to feel satisfaction with my opponent’s deficient learning in the questions of methods treated by him and with the confusion of his concepts; meanwhile, all this, in connection with the external influence of this man in the field of our science, provokes the most serious considerations about the present state of German economics.

I know, my friend, that it is a great sin to laugh at the ridiculous; however, it is so difficult not to indulge in a tone of derision towards a bleak and haughty opponent. However, what other tone do the remarks of such a man deserve who, without the slightest solid learning in the questions of scientific methodology, behaves as a full-fledged judge on the value or worthlessness of the results of methodological investigations? Is there in the field of science a phenomenon less suitable for serious consideration than a bloated non-science¹¹⁴ which makes severe judgments over the results of careful scientific research?

Can one discuss seriously the most difficult questions of epistemology with a [72] man in whose spirit every quest for reform of theoretical economics, even the cultivation of the latter, appears as Manchesterism? Can one discuss, without indulging in a mocking tone,¹¹⁵ the above questions with a scholar whose entire fairly original learning in the realm of theoretical economics consists in a primeval slime¹¹⁶ of historical-statistical material, with a scholar who constantly confuses the simplest concepts of science? Should such a quarrel render me satisfaction? If Schmoller’s objections to the results of my methodological investigations were not valuable to me for reasons which I have already discussed in my first letter, how gladly I would refrain from the discussion with him that I do not desire at all and confine myself to correcting the most conspicuous disfigurements of my views in his

112. [*scientifisch*]

113. [*dieser tiefsinnige Methodiker*]

114. [*Unwissenschaftlichkeit*]

115. [*in einen heiteren Ton verfallen*]

116. [*Urschleim*]

Jahrbuch, in a similar way as I have done to a like-minded professional peer of his elsewhere.

Also, do not imagine that a discussion with an opponent like Schmoller would be more effortless than with a scholar who is well-versed in the questions being addressed. When interacting with a well-versed scholar, is it not easy to instruct such a scholar, or to let him instruct you? Is it not easy—relatively—to discover individual inaccuracies within the consequent thought system¹¹⁷ of a knowledgeable scholar, i.e., individual inconsistencies and errors, and by exposing and correcting them to contribute to the advancement of science? Moreover, how gratifying is it to be able to express to an author the gratitude we owe him for correcting our own views and for the instruction we have drawn from his writings? [73] The most difficult and unpleasant issue in the field of science is always the critical contact with one-sided representatives of practical party aspirations, with men who transfer their one-sidedness and bad habits of party fight to scientific discussion; how much more unpleasant when such opponents even appear with the claim to be scientifically superior!

In a specialized library set up by a knowledgeable hand, however rich it may be, the eye of the expert is easily able to discern individual gaps. In an arbitrarily assembled library, the eye searches in vain for a resting point and finally turns away because it does not actually provoke serious judgment. The same is true for the judgment of an author's knowledge. The strength of Schmoller's methodological standpoint lies in the fact that it is incomprehensible and beyond any serious criticism. Here you want to fault me for not being misled by the historical-philosophical studies which he constantly tells us about, nor by his lectures on the methodology of the sciences of the state for which he currently "equips" himself, and for not taking the methodologist Schmoller more seriously than he deserves to be?

What would you say, for example, if I were to shed more light on Schmoller's thoughts about the actual methodological problems of our science?

[74] His thoughts about the inductive and deductive methods in the field of our science?

The results of his profound investigations into the nature and certainty of these paths of inquiry in general and in political economy in particular?

From your dismay I notice how little you take the methodologist Schmoller seriously. But do not worry, you do not have to fear the worst. Whoever gropes in the dark about the goals of research in the field of economics as completely as does the editor of the Berlin *Jahrbuch*, his thoughts about the paths of inquiry in the field of our science are protected against any attack.

117. [*consequentes Gedankengefüge*]

I would like to evoke only a few remarks by Schmoller that refer to the epistemological problems I have already dealt with because they are extremely characteristic of the way in which he expresses criticism and of his fighting style.

I described that the task of the historical sciences is to examine and present the individual nature and connection of human phenomena (their individual relationships in space and time!).

Here arose for me naturally the interesting question, which had already been raised many times by the scholars of methodology of the historical sciences: in what way were the historical sciences able to solve their task in the face of the innumerable individual phenomena of human life?

Regarding the above-mentioned question, the majority of authors believe that the historian [75] has to present the more important human phenomena and set aside the less important ones,¹¹⁸ and in doing so let himself be guided by his own fine sense, since there is lack of an actual principle for the choice of the “historical” phenomena in contrast to those phenomena whose depiction is not a matter for the historian.

I had come to believe that I had found a solution to the above interesting question in such a way that the historian should not only investigate a part of the human phenomena, since doing so would contradict the principle of the universality of the sciences. Rather, the historian should present the totality of the human phenomena, but all this from a collective point of view. I said:

The historical sciences can respond to their task in universal fashion only under the presupposition of collective consideration of human phenomena and the historical economic sciences only under the presupposition of *collective* consideration of economic phenomena. This evidently must be the case if we consider the vast number of individual phenomena of human life or economy and the exigencies of the technique of scientific presentation. The historical economic sciences are just because of their universal-scientific task [76] necessarily *presentations of human economy from the point of view of a collective outlook*.¹¹⁹

And in a note about the above remarks I say:

118. Already Pliny the Younger (*Letters*, 5.8.9–10) writes, not without any relationship to our question: *Habet quidem oratio et historia multa communia, sed plura diversa in his ipsis, quae communia videntur. Narrat sane ipsa, narrat haec, sed aliter. Huic pleraque humilia et sordida et ex medio petita, illi omnia recondita, splendida, excelsa conveniunt. Hanc saepius ossa, musculi, nervi, illam tori quidem et quasi iubae decent!* [Translation: Oratory and history have many things in common, but they also differ greatly in the points that seem common to both. There is narrative in both, but of a different type; the humblest, meanest and most common-place subjects suit the one; the other requires research, splendor, and dignity. In the one you may describe the bones, muscles, and nerves of the body, in the other brawny parts and flowing manes.]

119. *Investigations*, pp. 209ff. [253ff.].

Here also the basis for solving a problem frequently concerning historical research is to be sought: which phenomena of human life is it the task of the historical sciences to lift out of the vastness and to present? These disciplines really have the task of presenting the individual phenomena of human life from the point of view of collective considerations, and of presenting the individual phenomenon only insofar as it is per se significant for the *collective image of human life*. Only in this way can they satisfy their particular task universally.

What is called the *artistic problem of writing history* is also sufficiently explained in the above conception of the nature of history and its relationship to the individual phenomena of human life: “The real art of the historian (also of the statistician!) consists chiefly in the ability to make us aware of the immense number of individual phenomena of human life from the point of view of a collective outlook. It consists in the ability to offer us a collective picture of the development and of the condition of human phenomena in their totality.”¹²⁰

[77] This theory that I have put forward seems to have pleased my critic to some extent; he is so far away from fighting it that he rather accepts it without reservation.¹²¹ However, in what way characteristic of this man’s fighting style?

“Menger”—writes the same—“does not see that all of the more important economic phenomena are so extensive in space and time that they are only accessible by a collectivist view, as history and statistics are able to. That is closed to him.” *For this I was lacking the organ!*

There you have Schmoller! The full Schmoller!

That a critic opposes, in a tone of angry superiority, an author with the thoughts he himself clearly expressed—Lessing somewhere says: “sprinkles the author with his own fat”—is a shabbiness which is not quite unusual for a certain category of reviewers; but that a critic denies someone the knowledge of his own theory, *even denies him the organ for understanding it*, this is a phenomenon which is unparalleled even in the present desolate state of a part of scientific criticism in the field of political economy in Germany.

Fifteenth Letter. [Continuation] [78]

You interject that a procedure like the one I outlined in my previous letter might almost be unbelievable, since one cannot assume that a scholar who to some

120. *Investigations*, p. 210 [255]. See also pp. 92f. [86] and 117ff. [122ff].

121. *Jahrbuch*, p. 247.

extent values his academic reputation would use such extravagant means in order to strike such a tone of superiority towards a scholarly opponent, i.e., for a small and—considering the possibility of correction, only passing—thrill of vanity. How little you know Schmoller! As if he had not been fighting every academic opponent in exactly the same manner for more than a decade now! Read, my friend, the citations from my work, as well as those from his review, follow his other critical activities and you will stop being surprised.

And still, one could ignore these and other things if Schmoller's attacks did not present a much more disquieting aspect.

[79] I leave aside that Schmoller, whenever he makes my academic positions the subject of his review, makes me often say the opposite of what I actually said, that he confronts me in a lecturing tone¹²² me with things which I claim myself, that he reprimands me for things which he praises others for, as well as all other artistic resources of Schmollerian critique. Obvious as the misunderstandings and misinterpretations of my views may be which I encounter in Schmoller's critique, and plausible as the question how justified the misleading and thoughtless scribblings in an academic discussion may at best be, I do not want to draw any conclusion about the love of truth of the editor of the Berlin *Jahrbuch*. Partiality in preconceived opinions, superficiality of reading, inadequate learning in the discussed material, the bad manners which regularly result from doing critique as a craft,¹²³ and a character apparently predestined and educated for the lower forms of party fight rather than for academic discussion: With men like Schmoller, all these circumstances combined allow even the most obvious misinterpretations of someone else's opinions as mere errors to excuse.

The case is different when we deal with untruthful claims where every misinterpretation is excluded from the outset by the nature of the matter, untruthful claims whose only purpose is to prevent the rightful appreciation of an author and the results of his research by his professional peers. [80] Untruthful claims of this kind are intrigues¹²⁴ and no rebuke can be too harsh, no ridicule too bitter when these claims are to be denounced.

Schmoller accuses me by claiming that I “complain about W. Roscher and B. Hildebrand's historical work,”¹²⁵ he tries to invoke in his readers the impression that I “dismiss” Knies with just a few words,¹²⁶ he calls me a follower of Manchesterism,¹²⁷ he imputes to me sympathies for the mysticism of Savigny's

122. [*in belehrendem Tone*]

123. [*handwerksmässig betriebene Kritik*]

124. [*Cabalen*]

125. *Jahrbuch*, p. 242.

126. *ibid.*, p. 250.

127. *ibid.*, p. 241, p. 251.

Volksgeist,¹²⁸ and so forth.

All these accusations are utterly unfounded, insinuations for which there is not the slightest foundation in my work.

I am supposed to have complained about Hildebrand's and Roscher's historical work? The truth is that nowhere did I speak a word about Hildebrand's historical work, while I explicitly acknowledged Roscher's "outstanding merits and his advancement of the historical understanding of a number of important economic phenomena" (*Investigations*, p. 189 [225]).

I am supposed to have "dismissed" Knies, according to some words cited by Schmoller? The truth is that I call Knies *the most outstanding methodologist of the historical school of German economists* (*Investigations*, p. 191 [228]). I do not criticize his work in just [81] a few words, as cited by Schmoller. Instead, I dedicate to it multiple pages of my work and conclude that Knies completes the sphere of ideas¹²⁹ of the historical school regarding the methodology of political economy. I conclude that what since his time has been brought to light by the investigations into the methodological problems of historical economics, this author [Knies] had at least already hinted at.¹³⁰

Now you see, my friend, what is the matter with Schmoller's claim that I have "dismissed" Knies through the words cited by him. This allegation is just as untruthful as all the other allegations made by Schmoller.

Can it be true that these too are only simple errors? Be well aware, my friend, of the tendency resulting especially from these highly personal allegations against me!

But I almost forgot to mention that I do not just call Knies the most outstanding methodologist of the historical school of German economists, and the new ones only as his epigones; among those latter, and truthfully in second place, I also named Schmoller: *Him*, in second place, the editor of "*his*" *Jahrbuch*! Reckless me, I did not only refuse him the accustomed glorification, but I nothing short of violated the considerations which I owed to Schmoller's privileged position—an action which obviously triggered certain sensitivities. "If he is itching, he shall scratch," so I thought in an adequate appreciation of [82] this economic Rhadamanthus. But Schmoller does not want to appear sensitive, which is why he scratches Roscher, scratches Knies, and even scratches the dead Hildebrandt—because it is actually his own wounded vanity which is itching.

128. *ibid.*, p. 250.

129. [*Ideenkreis*]

130. *Investigations*, p. 192 [230].

Spare me, my friend, the necessity to defend myself against Schmoller's accusation that I am a follower of the Manchester party¹³¹ or [83] of the "mysticism of Savigny's spirit of the people."¹³² Both accusations are completely pulled out of thin air. If anything reconciles with Schmoller's influence on our science, spiteful in so many respects, it is the fact that he fights with unmistakable devotion side-by-side with venerable men against social evils and for the fate of the poor and weak, a fight in which, as different as the direction of my research may be, my sympathies are completely on this side. I want to dedicate my limited capabilities to inquiring into the laws that govern the economic life of men; however, nothing would be further away from the direction of my research than serving capitalism's interest. None of Schmoller's charges is more untruthful, no accusation more frivolous than the one that I am a follower of the Manchester party; unless the simple pursuit of establishing the economic laws, or the reference to the necessity to seriously bear in mind the previous achievements of civilization when all economic reforms are considered, is sufficient to justify the above accusation—an idea, however, which would only originate from a truly dissolute mind.¹³³

Further, considering Schmoller's accusation of me being a follower of the "mysticism of Savigny's spirit of the people," [84] I have not only not argued *for* it, I have explicitly argued *against* it. I literally write (p. 177 [208] of my *Investigations*):

Against these efforts of the Smithian school there was revealed to our science a vast realm of fruitful activity in the sense of the orientation of Burke-Savigny—not in the sense of simply maintaining what had organically developed as unassailable, as if it were the higher wisdom in human affairs as opposed to the intended

131. Being a follower of the so-called Manchester School is definitely not dishonorable; it merely means holding on to a set of scientific convictions the most important one of which is that the free play of individual interests as the most beneficial to further the economic common good. Social philosophers who are far superior to Schmoller intellectually and led by the most noble love of truth have maintained the above principle and the resulting maxims for economic policy. As I said, being called a follower of the Manchester School does not imply *per se* the slightest accusation.

It is different when it comes out of the mouth of such a one-sided partisan of the so-called socio-political variety like Schmoller. Manchesterism in *his* mouth is the stigma with which he denounces all dissenting minds, an abusive word he throws towards his opponents—whenever he lacks arguments.

Rightfully, H. Dietzel (Hildebrand's *Jahrbücher*, 1884. N.F. VIII. p. 110) therefore protests the fact that the stigma of Manchesterism is thrown at those who deal with the exact analysis of economic phenomena.

In my opinion, Manchesterism has roughly as much to do with question of the justification of an exact theory of the economy as a gunpowder conspiracy has to do with the legitimacy of theoretical chemistry.

132. [„Mysticismus des Savigny'schen Volksgeistes“]

133. At several points in my *Investigations* I argue against the so-called "ethical" direction of political economy, while I strictly separate it from the "socio-political" variety of economic research (p. 189, fn. 123 [226, fn. 123]).

ordering of social conditions. The aim of the efforts under discussion here had to be, on the contrary, the full understanding of existing social institutions in general and of organically created institutions in particular, *the retention of what had proved its worth against the one-sidedly rationalistic mania for innovation in the field of economy.* The object was to prevent the dissolution of the organically developed economy by means of a partially superficial pragmatism, a pragmatism that contrary to the intention of its representatives inexorably leads to socialism.

I believe that I have not defended the aspirations of “Manchesterism” and “mysticism” in the field of the economy but fought against them in adequate manner on the basis of my scientific point of view; and yet, Schmoller throws the accusation of mysticism and Manchesterism at me—this popular socio-political Hep! Hep! which the editor of the Berlin *Jahrbuch* cries out every time and at any place no matter how inappropriate, [85] whenever the arguments desert him.

I believe, my friend, we now have clarity about Schmoller’s impartiality in scientific critique. His propensity to misunderstand is clearly not the most regrettable side of his critical influence in the field of our science.

Sixteenth Letter. [Closing remarks] [86]

“*We are done with the book!*”—these are the triumphant words, expressing the most noble gratification, with which Schmoller closes his critique of my work, a critique which might have no equal at least among the academic literature when expertise and objectivity of judgment are considered.

The future, and I hope not a too distant one, will judge whether Schmoller was “done with” my methodological inquiries or whether I “was done with” the methodologist Schmoller. It almost appears as if the development of the methodological dispute so far which I reignited with my investigations shows that the editor of the Berlin *Jahrbuch* put on the *toga picta* and the *toga palmata*¹³⁴ prematurely, yes, that he who performed as the roaring lion of the historical school did a disservice to it.

However this may be, one thing is certain to me already. May the methodologist Schmoller in the future pace the sands of the Spree as lionishly as he wishes, [87] shake his mane, lift his paw, and yawn epistemologically, only children and fools will still take his methodological gestures seriously. But through the wide

134. [In ancient Rome, this formal attire, the “*toga picta*” (painted toga) and the “*toga palmata*” (toga ornamented with palm twig motifs), was worn by generals on the occasion of their triumphs.]

cracks in his scholarly mask some inquisitive minds¹³⁵ will peek, unfortunately also some gapers,¹³⁶ and contemplate the true appearance¹³⁷ of this epistemologist with amusement and gratification.

But my small effort will be rewarded through the awareness of having done a service, in more than just one regard, in the field of German economics.

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135. [*Wissbegierige*]

136. [*Neugierige*]

137. [*Gestalt*]

About the Author



Carl Menger (1840–1921) was an influential thinker and professor at the University of Vienna. His *Principles of Economics* of 1871 was a breakthrough in economic theory, notably for its marginalist approach and its subjectivist approach to value. His individual-centered conception of the economy was soon perceived as revolutionary and has ever since attracted generations of scholars to expand on this conception, with his immediate and closest associates being Eugen von Böhm-Bawerk (1851–1914) and Friedrich von Wieser (1851–1926). Following the publication of his *Investigations into the Method of the Social Sciences with Special Reference to Economics* in 1883, Menger engaged in what later would become known as the *Methodenstreit*, a controversy which, among others, addressed the relative roles of theory and history in economics. His principal opponent was the head of the Younger Historical School, Gustav Schmoller (1838–1917). In the decades after the *Methodenstreit*, Menger remained influential, both as teacher and as a public figure, but did not publish any new treatises. After the birth of his son Karl in 1902, Menger increasingly withdrew from public life, and Wieser succeeded him at the University of Vienna as the professor of economic theory. After his death, his partner Hermine Andermann sold his library to Hitotsubashi University in Japan. His archives are preserved at Duke University. Between 1933 and 1936, Friedrich A. Hayek edited the four-volume *Collected Works of Carl Menger*. Menger is widely credited as one of the founders of the Austrian School of economics.

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