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## **Austrian and Mainstream Economics: How Do They Differ?**

### **Introduction**

In the account of Kay (2011, 2012), in the years following the 2008 crash, the reputation of economists and their science suffered and as a result was diminished due to the global economic crisis<sup>1</sup>. Economics was accused of being unable to predict the crisis and policy makers felt misguided by formal economic models, which Kay criticized heavily. During the prolonged recessions and stagnations both in the U.S. and in various European countries, policy makers were not given clear, consistent advice by economists. There was no consensus on what to do regarding the conduct of monetary and fiscal policy or how to reform broader economic institutions like the labor market (see Krugman 2009, Boaz 2009, Cochrane 2011 or Levine 2009). In the words of Krugman (2012, p. 108) “at the decisive moment, when what we really needed was clarity, economists presented a cacophony of views, undermining rather than reinforcing the case for action”. Baily (2009) suggests that there was no agreement either as to how far economic theory influenced the practice of policy making and, therefore, how much it can be blamed for the crisis. Nevertheless, this multifaceted lack of consensus seemed almost identical to that of the 1930s, which might give the impression that there has been no major progress in (macro) economics since then. As a matter of fact, Skidelsky (2010 [2009], p. 11) sees “economics as a fundamentally regressive discipline, its regressive nature disguised by increasingly sophisticated mathematics and statistics”. In that situation, one might be tempted to look more closely at what heterodox schools of economic thought, particularly those that do not use *sophisticated mathematics and statistics*, might offer to solve the problem, especially given the fact that according to “The Economist” (2011), “they have thrived on the back of massive disillusion with mainstream

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<sup>1</sup> Their reputation have not recovered yet since it constantly receives blows due to poor macroeconomic predictions like those concerning the effects of Brexit, as Lanchester (2017) reports.

economics”. Moreover, some mainstream economists themselves partly lost faith in aspects of their profession, such as heavy mathematization or wildly unrealistic assumptions<sup>2</sup>, and consider alternatives to mainstream economics in e.g. old Keynesianism as Krugman (2012) did or in heterodox alternatives like the Austrian school of economics, as Leijonhufvud (2009) did. In particular, economists of the Austrian school of economics (hereafter: ASE), which is an old and established school of economic thought within heterodox economics, employ none of the *sophisticated mathematics and statistics*. Moreover, they tend to put forward the most unusual interpretation of what has happened and the most radical economic policy prescriptions, like the introduction of free competition in the use of currencies or the complete abolition of central banks (see e.g. Cochran 2015).

The purpose of this paper is to attempt to partly answer the question of how useful the theory of the ASE can be in addressing economic problems, like the problem of understanding what happened before the crisis and the problem of policy making, by examining the ASE’s methodology with special emphasis on one aspect of it: its attitude towards the general use of mathematics in economic science. Although the present text deals exclusively with ASE economists’ contentions on that matter, they might be shared by other economists as well, hence, the conclusions regarding the usefulness of the ASE’s economics can be applied to other approaches to economics to the extent that they share the ASE’s attitude towards the use of mathematics and formalism in economics<sup>3</sup>. Despite the fact that in 2018 the use of mathematics in mainstream economics (henceforth: ME)<sup>4</sup> seems to be natural, it still seems to be worthwhile to review it from time to time and consider some alternatives.

First, the history of the ASE will be sketched briefly and the term ASE will be defined. Next, its methodology will be critically reviewed. Second, the issue of the ASE’s attitude towards mathematics in economics will be carefully examined. Finally, conclusions drawn from the following analysis will be presented and illustrated with the example of business cycle theory.

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<sup>2</sup> Even distinguished mainstream economist P. Romer (2016) shares Skidelsky’s view when analyzing the current state of macroeconomics saying: “I have observed more than three decades of intellectual regress”; however he is criticized by Orrell (2016) for not looking outside mainstream economics for readily available heterodox alternatives.

<sup>3</sup> While the ASE seems to postulate methodology which is unique, original, comprehensive and at the same time highly distinctive from that of mainstream economics, other heterodox schools, e.g. post-keynesianism, are not so fixated on the methodological differences between them and mainstream economics, but rather stress the importance of certain, neglected in their opinion, features and mechanisms of economics.

<sup>4</sup> The term mainstream economics denotes here the type of economics which is currently practised in top scientific journals and taught in top graduate programs. For example, Weintraub (1993) wrote over two decades ago that “what is mainstream economics today is neoclassical economics”. However, he defined this metatheory too narrowly for modern mainstream economics, because instead of focusing on methods being used by economists, he listed fundamental assumptions of neoclassical economics (and, therefore, of mainstream economics), which he said are beyond any discussion, among others: rationality of individuals and profit maximization of firms, all of whom act on the basis of full information. Today, making deviations from these assumptions is, nevertheless, common, and, therefore, Weintraub’s classification seems to be slightly dated.

## 1. The Austrian school of economics' methodology

### 1.1. What is the Austrian school of economics?

Like every rich school of thought with a long tradition, the ASE is heterogeneous. The same is true of its methodology. As Caldwell (1984) puts it, “the Austrian approach to methodology has never been monolithic” and “there exists no single Austrian position on methodology.” In this paper, in order to narrow its focus, only one strand of the school will be dealt with, namely, that which currently draws the most heavily from the works of the line consisting of Menger – von Böhm-Bawerk – von Mises – Rothbard. This tradition can be traced back to the works of the late-Scholastic School of Salamanca (see Hülsmann (2007)), which, through Condillac, inspired the founder of the school Carl Menger who, in the so-called *Methodenstreit*, debated the German Historical School on methodology. The ASE's distinct methodology was, however, most explicitly and fully articulated in the late writings of Mises, who called the more general discipline of investigation into human action a praxeology. This approach to economics was most forcefully continued by Rothbard<sup>5</sup> and is currently employed mainly by scholars who write for “The Quarterly Journal of Austrian Economics”, which is published by the Mises Institute of Auburn, Alabama. This group of adherents of the ASE's methodology, which is currently dominant within economists who claim any relation to or are said to be associated with the ASE, seems to be the most comprehensive and coherent in its stance and to present the most clear-cut alternative to ME. Therefore, in this article when the phrase *the ASE* is used, for the sake of simplicity, it denotes only this group and not necessarily other economists sometimes associated with the ASE, like famous F. von Wieser, J.A. Schumpeter or Nobel Memorial Prize in Economic Sciences laureate F.A. von Hayek (on the discussion on whether they can be classified as ASE economists see e.g. Hülsmann 2007).

### 1.2 The school's methodology critically examined

The most distinctive feature of the ASE's methodology<sup>6</sup> seems to be its aprioristic approach to economics. For example, Wiśniewski (2014) starts his presentation of modern ASE methodology by examining the divide between apriorism and empiricism. Although, as Smith (1990) points out, there are differences in the precise sources of aprioristic knowledge that the ASE's economists claim to possess (and, therefore, there are various types of apriorisms they employ), they tend to derive

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<sup>5</sup> Moreover, Rothbard (2011 [1976]) notes, for example, that the “praxeological method (...) was the basic method (...) of a considerable segment of the older classical school, in particular of J.B. Say and Nassau W. Senior.”

<sup>6</sup> For a review of the methodological stances of various ASE economists see White 2003 (1977).

it via introspection, as purportedly every man knows the essence of human action or the means-goals framework of human purposeful behavior, i.e. the axiom of action upon which an economist can elaborate necessary truths about the action as such. In itself it is, however, so general that rather empty, hence only coupled with some subsidiary postulates (like the fact that action takes time or that resources are heterogeneous), which are rather empirical and said to be self-evident (as Rothbard wrote, “they are so generally true as to be self-evident”), it can serve as a basis for deducing the ASE’s economic system with all of its universal laws and theorems. As this basis is claimed to be true, via the use of deduction „all these elaborated laws are absolutely true. They are only applicable in concrete cases, however, where the particular limiting conditions apply” (Rothbard, 1957). Determining if particular conditions of a theory apply empirically is the task of a historian or an applied economist. “There is consequently no need for empirical «testing», either of the premises or the conclusions and the deduced theorems could not be tested even if it were desirable” (Rothbard 1957). What Rothbard had in mind is rather that even the signs of the basic relationships between economic variables are not to be tested (as these are *a priori* true) and not, speaking in the language of ME, particular functional forms of these relationships or their parameters, as these are the resultants of complex interactions specific for particular historical circumstances and, therefore, not known *a priori*. As a result, these functional forms and parameters are not constant over time and locations and cannot be verified empirically due to what modern econometricians would call an *omitted-variable bias*<sup>7</sup>. This stems from the fact that in economics, contrary to the natural sciences, experiments cannot be conducted since an economist cannot control experimentally for every possibly intervening variable and, instead, is presented only with historical data on variables, which are necessarily too few in number and far from ideal statistical quality. As Mises (1998, 1949, p. 55) put it: “There are, in the field of economics, no constant relations, and consequently no measurement is possible. If a statistician determines that a rise of 10 per cent in the supply of potatoes in Atlantis at a definite time was followed by a fall of 8 per cent in the price, he does not establish anything about what happened or may happen with a change in the supply of potatoes in another country or at another time. He has not “measured” the “elasticity of demand” of potatoes. He has established a unique and individual historical fact”. As a result, statistical analysis, as Wiśniewski (2014) wrote, “is unsuitable to making economic predictions with any degree of quantitative precision.”

Moreover, the ASE’s economists reject the usefulness of probability theory in the study of human (inter)actions as they draw a distinction between class and case probabilities. The first one – quantifiable – is related to classes of phenomena which can be experimentally reproduced so that the probability of the occurrence

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<sup>7</sup> Keynes shared a similar objection to econometrics (see Skidelsky 2010 (2009, p. 150)), however it should be noted at this point that this problem is at least to some extent alleviated by the use of structural modeling or regime switching and time-varying parameters in reduced modeling in econometrics.

of certain events can be calculated using frequencies. The second – unquantifiable – relates to historical social events, which are all unique and unrepeatable in their complexities, so that economics can only explain them by employing an *a priori* theory and cannot in any way genuinely aggregate them to create a class in order to compute the probability of the occurrence of any events (for details consult e.g. Mises 1998 (1949), chapter VI, and Murphy & Gabriel 2008, pp. 45–50). All these statements form a more comprehensive view of the ASE called methodological dualism (see e.g. Wiśniewski 2014), according to which social scientists, given that they can – via introspection – use the fact of human goal-seeking, should employ a different methodology than that used by natural scientists, who do not have any *a priori* ultimate knowledge about the very basic aspects of the physical world to start from in theorizing. The ASE economists, therefore, stress that their economics is *a priori* and deductive as opposed to ME which is *a posteriori* and inductive. However, there is a plethora of problems with these features of the ASE’s methodology, which are here only flagged and not investigated in depth.

First, the contentious basis of methodological dualism is that *homo sapiens*, who is part of nature, is somehow qualitatively different from the rest of the natural world and therefore social sciences need a specific apparatus to study human (inter)actions. Consequently, as every science deals with specific subject matter and encounters specific problems, it would mean that every science needs a completely different apparatus, a view one might call methodological infinitism, as potentially an infinite number of disciplines can each demand a specific methodology. This point is made not necessarily to endorse methodological monism, but instead to find really good reasons for adopting the yet unsubstantiated claim of methodological dualism, which rests on shaky grounds, as the mere fact that a researcher can have access to some very basic *a priori* knowledge of the means-end categories does not in itself establish the validity of methodological dualism and preclude using other, possibly *a posteriori*, knowledge in economics.

Second, the ASE’s insistence on the distinction between class and case probabilities might stem from an idealized view of the conduct of research in natural sciences. Indeed, it is not clear where is the demarcation line between genuine experiment, in which the researcher does not always have the ability to list and measure every possible intervening factor in the phenomenon under investigation, and empirical study conducted via the use of fragmented historical observation only, yet no one should claim that studies done using only perfect experiments can have any scientific value. As a matter of fact, many studies in natural sciences are not conducted in circumstances of idealized experiment, free of any of uncontrolled for intervening factors, yet they still add to our knowledge. Furthermore, and related to this, as Caplan (1999) notes, every event is at least in some respect unique, hence ASE economists, if consistent enough, should reject the notion that probability can ever be empirically quantified. In sum, the ASE’s case against the empirical verification of theories seems not to be strong.

Third – a point that Nozick (1977) made – even if the ASE’s *a priori* theory of human action is true, it may be the case that it is far worse scientifically (e.g. in

terms of the width of its scope or the ability to explain and predict crucial phenomena) than some other *a posteriori* theories (this author himself considered, just as an example, the Skinnerian theory of operant conditioning). Especially if that is the case, the applicability of the ASE's theory (just like every other theory) to real-world phenomena ought to be determined through as rigorous procedures as possible instead of only using the vague and precarious tool of understanding (*verstehen*). These can show that, again, although the ASE's theory can be perfectly true, at the same time its applicability can be so narrow that its scientific value can be very little. In particular, it is not clear where is the precise distinction between an action (purposeful behavior) and a behavior which is not an action and, therefore, cannot be explained by the ASE's theorems. In addition, this raises the question of whether animals or robots, at least sometimes, act too, and, therefore, what is the scope of the ASE's theory.

Fourth, as Caldwell (1984) argues, if there are other *a priori* systems of economic theory (he mentions, just as an example, the “Classical-Marxian” system of Hollis and Nell), which are all logically consistent, there is no universal tool to adjudicate among them, since they all claim their axioms and conclusions are untestable. Still, one can think of, for example, a theory's scope or the ability to explain the mechanisms of economic phenomena as an adjudicating criterion (or some synthetic measure of criteria); however, one cannot see any consensus on that among the adherents of competing alternatives. As a result, as there is no real proof (or evidence) of the uniqueness of the validity of the ASE's theory, there is a problem of, what we could call, the methodological uniqueness of the ASE as potentially an infinite number of *a priori* economic systems are possible, among which the process of discrimination should be conducted, but the rules of this process are not obvious.

Finally, and in what may seem to be in contrast to what was written before, the ASE's methodology may in fact not be dramatically different from ME's methodology in terms of the deductive/inductive and *a priori/a posteriori* distinctions. In the end, as already stated, the ASE's theorems are always based not only on the action axiom, but also on some other empirical subsidiary postulates and applied to only certain empirical phenomena when the limiting conditions are satisfied; therefore, the ASE's system is not purely aprioristic. With regard to ME, as Hausman (1989), who focuses on the actual practice of ME's economists rather than on their mere methodological declarations, argues, despite various assurances of mainstream economists who try to follow the trends of the contemporary philosophy of science, ME is still mostly a deductive and not strictly an entirely empirical science<sup>8</sup>. Therefore, differences between the ASE and ME on the deductive/inductive and *a priori/a posteriori* continua seem to be usually overstated.

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<sup>8</sup> See, for example, Hoover (2012) on the critical problem of empirically tracing causes in (macro)economics. In light of such problems of causal inference in econometrics, ASE's aprioristic methodology can be viewed as attractive, but so can ME's purely deductive theory. Recently, Romer (2016) even criticized the process of empirical verification of ME macromodels for excessive reliance on deduction.

There are also other features of the ASE's methodology that are often stressed, but they, too, do not seem to be nowadays especially distinctive compared to the alternative of ME. Among them, first of all, there is, connected with the action axiom, the principle of methodological individualism<sup>9</sup>, which states that only individuals and not groups can act, hence in the end only actions of individuals can be analyzed by economics (see Mises 1998, pp. 41–44 and Murphy & Gabriel 2008, pp. 12–13)<sup>10</sup>. Indeed, this seems to be obvious knowledge and it is really hard to find any theory in modern ME that in the general sense assumes otherwise. The second feature of the ASE is the principle of methodological subjectivism that says that the phenomena of objective economic reality are determined by actions based on the subjective valuations, reasoning and expectations of individual actors<sup>11</sup>. Stated so generally, it is, again, difficult to see how this principle can be violated by modern ME theories.

Yet another allegedly methodologically distinctive feature of the ASE is its so-called causal-realist approach to economics. Its first part means that the ASE's economists declare that they employ the concept of causality in their analyses so that they can “trace and work out the causal relations between various economic phenomena according to a clear logical progression” (Wiśniewski 2014). In contrast, the ASE's economists (see Rothbard 1956) accuse ME of rejecting causality in economic analysis and espousing the so-called *mutual determination* approach appropriate in physics, in which objects of analysis are not motivated by anything, and which is contrasted with economics in which objects of analysis have purposes which can be understood thanks to introspection. It is, however, not clear how the possession of this *a priori* knowledge in itself renders improper the approach used by physics. One can, moreover, draw inspiration from Hausman and greet such declarations on both sides with skepticism, focusing instead on the practice of economics. Although one can elaborate deeply on the issue of causality in science in general and in economics in particular, ME's economists can in general agree with the ASE that the driving force of economic phenomena is human action, or more broadly: human behavior, and, therefore, theorizing should start with it. If they speak of *mutual determination*, they might think of the fact that this driving force is also itself simultaneously influenced by external reality, which in turn is at least partly shaped by this very behavior, hence the *mutual determination*.

The second part of the phrase “causal-realist” means that “Austrians aim at specifying the empirical assumptions on which their deductions are built as accurately and consistently with reality as possible” (Wiśniewski 2014). It is, however, highly

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<sup>9</sup> E.g. Boettke (2008) lists this principle as the first major methodological proposition of the ASE and Gorazda (2014, p. 220) even treats it as a distinctive feature of the school.

<sup>10</sup> The related principle, methodological singularism (see Mises 1998, pp. 44–46, and Murphy & Gabriel 2008, p. 13), which states that economics should analyze only particular actions and not vague actions in general, is even more outdated in the context of the comparison of the ASE's methodology with that of modern ME.

<sup>11</sup> Boettke (2008) lists this as the first proposition of ASE's microeconomics and Huerta de Soto (1998) also stresses its importance in several contexts.

debatable whether the ASE really obeys this declaration, because its theories are very simplistic in the sense that they do not specify various important features of the phenomena they try to explain (specific example in the last section); hence they abstract a lot and in this regard they are necessarily unrealistic. Moreover, even Rothbard (1957) himself admitted that he had been making use of the unrealistic equilibrium concept of the evenly rotating economy (as, he claimed, an *auxiliary construct*) and the simplifying, and therefore untrue, assumption of profit maximization by firms, which he deemed a necessary *convenience of analysis*. Nonetheless, the ASE's economists claim, following Long (2006), that their abstract theories are realistic in the sense that certain features of the real world which are not necessarily causes of economic phenomena are absent from specification in these theories and they do not specify the absence of certain crucial features as ME purportedly does. However, this distinction (between *nonprecise* and *precise* abstractions) seems to be in the context of economics a false dichotomy as both these approaches to abstraction are in practice, i.e. in theorizing and not in declarations, basically equivalent, as, for example, there is no difference between the artificial (theoretical) economy without the specification of the financial sector and the same economy when it is specified that there is no financial sector. Moreover, realism is valued by ME, but its economists also value other features of their theories and recognize that in practice there is a trade-off between realism and these other features of a theory, like the extent of its applicability, its tractability, ability to give answers to certain questions, and its amenability to empirical testing given available data, as ME is faithful to the realism of not only the assumptions of theories but also of their conclusions and does not claim to know *a priori* which characteristics are necessarily crucial and which are only peripheral, so it tests hypotheses regarding the significance of various features that are typically not analyzed by the ASE, but can potentially turn out to be important. In contrast, Rothbard did not give even the slightest empirical evidence for his assumption of profit maximization by firms.

To summarize, given all of the above remarks about characteristics of the ASE's methodology, one can get the impression that there is much less disagreement between the ASE's and ME's methodologies than various authors suggest; some even claim, like Mayer (1998), that “one should not exaggerate the incompatibility of paradigms”.

## **2. The Austrian case against mathematical economics**

There is, however, one last major difference between these approaches to economics, which might actually be more serious than is thought, as considerable attention in methodological disputes is paid to methodological declarations instead of actual practices of doing economics and to theory verification instead of methods of theory formulation. This stark difference seems to lie in the language of the theory formulation of the two approaches to economics; while modern ME tends

to describe economic phenomena, prove theorems, settle disputes, communicate ideas within academia, teach students, etc. using mathematical models, the ASE uses almost solely verbal language for all these purposes. This difference might also be part of the reason why currently there is so little dialogue and exchange of ideas between competing approaches to economics. Since this difference does not directly stem from any single deep, underlying methodological assumption, but, if anything, is rather the resultant of various assumptions and manifests itself mainly in practice, it might be the reason why it gets so little attention from both exponents of the ASE's methodology and its critics, particularly given the fact that after WWII the school was not (and is still not) popular, so that critiques of its methodology in general, and decent, well-informed, and high-quality ones in particular, were unfortunately, with some exceptions, quite scarce.

Some common criticisms, or rather misperceptions, of the ASE's methodology are enunciated and countered by, for example, Rothbard (1951a, 1951b) and Caldwell (1984), who also, in addition to what was stated earlier, questions the reliability of the “verbal chain of logic” in the ASE's theorizing<sup>12</sup>, however he does not propose any solution to this problem in the form of formal tools, which one might consider a natural alternative. Forty years ago, Nozick (1977), in addition to what was recalled, scrutinized ASE's methodological individualism and some problems related to preferences, choice and actions; however, he not only did not connect these problems with the issue of the relatively unclear and imprecise “literary” character of the ASE, but he did not even touch this issue in any other context despite the fact that he showed himself to be fairly well-acquainted with contemporary ME's (micro)economic literature. Cowen and Fink (1985) devastated the equilibrium concept of the evenly rotating economy – the ASE's tool for much of its economic theorizing. Caplan (1999), apart from examining the role of probability in economics, criticized the ASE's approach to, for example, indifference, continuity, especially in the case of utility functions, and topics in welfare economics. None of these critics, however, specifically addressed the problem of the language used by the ASE. At the same time, the ASE mainly reiterated the arguments of the founding figures of Mises and Rothbard, but they have not been thoroughly revised since. Only recently, as an internal voice of the ASE, Hudik (2015) listed well-known costs and benefits of mathematization in economics and suggested that the ASE should be as mathematized as possible since “it should be stressed that mathematization by no means is in conflict with the Austrian methodology”. However, he did not address at length the reasons why ASE's economists object to the use of mathematics in economics as such, which is the task of the next subsections, since a careful examination may show that after all they were right about their rejection of mathematics. In particular, attention is focused here on the arguments of Mises and Rothbard, as they still serve as the most influential figures in the modern ASE.

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<sup>12</sup> He gives the example of obtaining through the ASE the results of the beneficence and stability of markets. Regarding similar doubts, see White (2003 (1977), p. 23).

## 2.1. The founding fathers

The founder of the school, Carl Menger, according to Hayek (in his introduction to Menger's (2004 [1950]) *magnum opus*), “has nowhere commented on the value of mathematics as a tool of economic analysis”. Nonetheless, as stated in Jaffé, and cited by Hülsmann (2007, p. 107), Menger's correspondence with Walras reveals that the former was not ignorant of mathematics but “declared his objection in principle to the use of mathematics as a method of advancing economic knowledge”<sup>13</sup>, which is dictated by his declared (in another letter to Walras, quoted by Hülsmann (2007, p. 106)) pursuit of the explanation and establishment of the laws of complex phenomena by analyzing the simplest underlying factors. In Hülsmann's interpretation (2007, p. 107) “only in this manner was it possible accurately to describe the essence of economic phenomena, and not just the contingent quantitative relationships in which they might stand with other phenomena at certain times and places”.

However, it is hard to see nowadays how only the verbal language, in contrast to its formal counterpart, can explain complex economic phenomena by using the simplest elements, like agents' preferences, knowledge, expectations, endowments, features of market institutions, and technology. Only by reference to highly aggregated *ad hoc* macroeconomic models can one show that this supposed advantage of verbal analysis holds true, but certainly not in case of fully specified modern macromodels with microfoundations. In addition, one has to remember that mathematics provides the researcher with the tools to also analyze phenomena qualitatively without specifying precise (explicit) functional forms of relationships or their parameterization *at certain times and places*. Moreover, it is hard to grasp what *the essence of economic phenomena* really means and how it can help answer any meaningful economic questions. Likewise, Samuelson (1952) implicitly criticized Menger's accusation that mathematical economics is unable “to get at the essence of a phenomenon” by writing that “I wish I thought it were true that the language of mathematics had some special faculty of drawing attention away from pseudo problems of qualitative essence” and calling this qualitative-only, verbal methodology *sterile*. In response, Machlup (see Gruchy and Machlup 1952), who was a former Mises disciple, countered that “I for my part continue to be interested and concerned with the problem of the essence of value even if it takes other languages than mathematics to talk about it”; but today with our mathematical tools, one is hard-pressed to know what exactly these problems are and if they are of any importance and not fictitious. He also submitted “that the basic human attitudes that underlie economic conduct cannot be described and analyzed exclusively in mathematical language”, but it is again not clear what he meant, in particular, if he meant that preferences, beliefs or expectations cannot be expressed and analyzed by mathematics alone or if he had something else in mind.

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<sup>13</sup> According to Mensik (2015), though, Menger generally viewed economic reality in a way “more than inviting for mathematical treatment”.

Böhm-Bawerk also abstained from the use of mathematics in economics. As Schulak and Unterköfler (2011, p. 37) noted: “Böhm-Bawerk did indeed demonstrate an almost unconcerned, pragmatic-eclectic attitude when it came to methodological questions. Characteristic of this attitude was his rejection of the use of mathematics in economics. This was not for fundamental epistemological reasons, as was the case with Menger, but because he, along with most of his faculty colleagues, utterly lacked the necessary mathematical skills”<sup>14</sup>. As is evident, the ASE’s literary character and rejection of the use of mathematical methods in economics started already in the nineteenth century and continued ever since.

## 2.2. Ludwig von Mises

In the twentieth century, the towering figure of the ASE was Mises, whose magnum opus *Human Action* (1998 [1949]) not only provided the methodological foundations of the ASE, but also its corpus of economic theory intermingled with its applications as well as the author’s loose thoughts on socio-politico-economic problems. This treatise also contains Mises’s views on the use of mathematics in economics, which are scattered throughout the book. They appear, not always explicitly, in various contexts and repeat themselves often, so it is not always easy to interpret and appraise them *en bloc* appropriately. Instead, we can examine those of Mises’s statements which seem to be the most serious, on a theme-by-theme basis. What is interesting, most of Mises’s views on the use of mathematics in economic theory appear not in chapters on methodology in Part One of the book *Human Action*, but in Part Four “Catalactics or Economics of the Market Society”, Chapter XVI “Prices” and in Part Five “Social Cooperation Without a Market”, Chapter XXVI “The Impossibility of Economic Calculation Under Socialism”. This alone might suggest that he himself did not think of the use of mathematics as a major methodological difference between his method and that of other economists at the time. Indeed, Moorhouse (1993) argued that while the Misesian praxeology differs in form and style from mathematical economics, the tenets of the former (like deductivism, methodological individualism, and subjectivism) are in general consistent with the latter. But, let us give a voice to Mises himself.

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<sup>14</sup> As Hülsmann (2007, p. 944) notes, all Austrian economists before the Second World War, contrary to much of the rest of the world’s mathematicians-turned-economists at the time, had to obtain a degree in law before switching to economics. Although, according to Hülsmann, “Böhm-Bawerk had in his youth a great interest in theoretical physics”, when Stigler pointed out that this ASE economist was not trained in mathematics, Rothbard (2009) replied: “for which we may all offer a silent prayer of thanks.” It’s hard not to think that his bizarre comment promotes ignorance, especially given the fact that he himself majored in mathematics and economics from Columbia University (see Gordon 2007). A similarly absurd statement was uttered by Skidelsky (2010 [2009], p. 11), who confessed that his ignorance of mathematics and statistics “saved me from important errors of thinking”.

On pp. 99–100 he wrote that like mathematics and logic, the praxeological system is aprioristic, deductive and itself atemporal, however it differs from them in that “it implies the categories both of time and causality”. According to Murphy and Gabriel (2008), what Mises had in mind writing this and that “[i]n the frame of the praxeological system any reference to functional correspondence is misleading and at best metaphorical” was ME’s mathematical models, which use the notion of simultaneous determination and are unsuitable to deal with causality. It is hard to say which particular model could Mises have had in mind using this “causality argument”, but in general he is wrong (or at least Murphy and Gabriel are in their exegesis), since even in simple *ad hoc* static, deterministic models of the market, there is a clear distinction between exogenous and endogenous variables, with the former playing the role of causes and the latter of effects resulting from some form of a *tâtonnement* process. More fundamentally, preferences, endowments and technology entering into the model via the parameters of supply and demand functions can be said to be the ultimate causes in the analysis. In simple models of timeless consumer behavior, the obvious ultimate cause of the act of choice are preferences coupled with some form of the thought process (in the limiting case: rationality) employed by the acting individual in order to maximize satisfaction. The same certainly holds true for any more sophisticated modern dynamic stochastic models in which shocks, that are occurring ceaselessly, are a clearly identified driving force of change in human action. In short, mathematical methods in economics do not allow dealing only with mutual determination and do not preclude using causal analysis; in contrast, they facilitate it even more clearly than the verbal method does.

Next, on p. 251 Mises for the first time explicitly addressed the method of mathematical economists<sup>15</sup> “who disregard dealing with the actions which, under the imaginary and unrealizable assumption that no further new data will emerge, are supposed to bring about the evenly rotating economy.” In particular, he attributed this process of change to the entrepreneurial activity of individuals who spot previously unnoticed profit opportunities and who react to a constant influx of new information. Mises accused mathematical economists of focusing solely on the equilibrium states instead of the underlying market process (whose explanation is *the task of economic theory*), which moves the whole economy towards new equilibria, which are, however, never reached due to constant changes in the data upon which individuals make their decisions. Later on, he several times raised essentially the same argument, which we may call “a preoccupation with states instead of processes”, modifying its form only slightly in new contexts, like

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<sup>15</sup> On p. 347 Mises distinguished, and consecutively criticized, three separate currents of mathematical economics. The first one is what we today call econometrics, the subject that was already dealt with in the previous section. The second must be the rather obsolete current which dealt with the relationships between monetary economy market prices and costs, both in terms of other goods or in terms of utility, instead of money prices. However, Mises gave neither specific publications nor even names representative of this current, so it is pretty difficult to determine what exactly he had in his mind. Only the third current, which is what we nowadays call mathematical economics, is dealt with in this section.

the analysis of monopoly pricing or the critique of arguments for central planning, on pp. 330, 351–353, 374–375, 697–698, and 706–707. Moorhouse (1993) claimed that by doing so Mises offered a criticism for which mathematical economists did not yet (over two decades ago) have a satisfactory answer. However, he himself ignored the fact that models that try to accomplish this particular task are quite old (see e.g. Chiang 1984 (1967), pp. 475–479, 529–534, 561–569)<sup>16</sup>. Although they are not completely satisfactory (just as no theory or model is), as, depending on the economic context, there is no one universal way of moving towards market equilibrium and one should not expect to see an all-encompassing model, they at least aid in the clarification of the problem and provide insightful explanations of how this can be achieved (if at all) or what are the conditions for that in certain circumstances. He, however, later contradicted his previous statement when he conceded that “major contributions have been made during the last two decades to our understanding of market and non-market processes by economists, employing mathematical methods.” Since then, the literature on this has only grown, providing via mathematics further new insights into the understanding of the market process, especially on the very micro level. Nonetheless, Mises’s own account is simplistic too, as it basically does not specify any quantitative market decisions based on preferences, information sets, expectations, endowments, and technology, but instead confines itself to the trivial notion that the driving force of ceaseless change in the market is the entrepreneurial struggle to gain profit. In many respects, even these already mentioned simple old dynamic models of markets provide much more insight into the market process since they at least provide the exact paths of prices and quantities and can examine market stability – the very feature that Caldwell (1984) contends is difficult to analyze through verbal logic and thus unclear in the ASE.

Another kind of argument of Mises against mathematical economics that can be inferred from his book is that mathematical models in economics “are unreal, self-contradictory and imaginary expedients of thought and nothing else” (p. 257). This argument, which might be called “realism of equilibrium analysis”, is basically somehow repeated in other words on pp. 251, 347, 349, 353, 375, 697, and 707. However, even at the time Mises was writing *Human Action*, models of mathematical economics should still be defended as valuable. As Moorhouse (1993) wrote in the context of a static equilibrium model that Mises criticized, “[t]o argue that because it does not describe an actual economy, it is useless or misleading is to misunderstand the role of theory in the explanation of complex economic phenomena.” Indeed, although simple static models of the market are not meant to be completely realistic nor to explain everything that economics deals with (given the very definition of a *model*), and in particular they assume away the problem of the market process which is described by other explicit models, they can still

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<sup>16</sup> It is not clear, when Mises on p. 353 contended that “[t]he problems of process analysis, i.e., the only economic problems that matter, defy any mathematical approach”, whether he was ignorant of the mathematics of dynamic analysis, specifically applied to economics, or whether he just meant something arcane by the phrase *process analysis*.

be useful scientific tools in a variety of ways and especially more valuable than Mises's verbal equilibrium concept of the evenly rotating economy, for they can quantitatively determine equilibrium prices and quantities or be used for policy experiments (e.g. the introduction of a minimum wage or maximum rents) and highly insightful sensitivity analysis of results of changes in the underlying causes (*comparative statics*). As Mayer (1998) wrote, “[w]hat is important (...) is that we use a theory and its abstractions to deal with a particular problem or question. A valid abstraction when addressing one question can be an invalid one when addressing another”. Mises, however, seems to have ignored that and demanded from a model<sup>17</sup> that it describe every aspect of a problem, provide an answer to every possible economic question and yet be tractable. Nevertheless, on p. 330 he conceded that there is one virtue of geometrical or algebraic exposition of the model of the market: a pedagogical one. One can only suspect that Mises could have thought that the same holds true for any other model. If so, then, following Hudik (2015), one can ask “why restrict this benefit [of formalization] only to students? Should not economists always communicate with their colleagues in the clearest possible way, especially when presenting new ideas?”

Yet another kind of argument that Mises used against mathematical economics – which might be called an “invalid analogy argument” – is that its practitioners are wrongly inspired by the science of physics and in particular “[t]heir ideal is to construct an economic theory according to the pattern of mechanics. They again and again resort to analogies with classical mechanics which in their opinion is the unique and absolute model of scientific inquiry” (pp. 351–354). It does not matter for the present text if these metaphors are of any value, but rather if the mathematical methods are adopted in economics in a valid way. However, Mises can be interpreted (just as Murphy and Gabriel (2008) did) to have gone further when he suggested that while in physics the researcher knows “nothing about the ultimate forces actuating” observable changes, in economics the ultimate basis of any analysis is the knowledge that humans act purposively and that this distinction somehow makes the use of mathematics in physics valid and in economics invalid<sup>18</sup>, since while in physics there is a *mutual interdependence of data*, in economics we only face a one-way causation, which brings us back to the “causality argument” (considered in this section) and to the issue of mutual

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<sup>17</sup> Moreover, although they do not use the specific term regarding their own work, Austrians also *model* in a general sense, since they try to describe and explain abstract phenomena, though using different tools than ME.

<sup>18</sup> Some, as Samuelson (1952), might even argue that basically “if we enumerate one by one the alleged differences between the social sciences and other sciences, we find no differences in kind”. He even insisted that “[t]here are no separate methodological problems that face the social scientist different in kind from those that face any other scientist”. Although it is obvious that every science faces its own specific problems (not necessarily any deep methodological ones), at least in case of the application of mathematics to economics it is hard to argue alone on the basis of the fact that natural sciences and economics differ, that one should not use in economics the mathematical techniques that are also applied to natural sciences, as Higgs (2011) suggests while critiquing Samuelson on the language of economics and defending methodological dualism.

determination (analyzed in the previous section). Although intentionality is itself a rather philosophical problem and one can discuss its presence and meaning, also in the case of economics, it should be noted that at least for the purposes of economics, it does not rule out the use of mathematics. As a matter of fact, it can be accounted for in economic modeling, for example, in the specification of preferences and maximization behavior of individuals, or, more generally, manifested in some other micro rules of behavior. Moreover, Mises claimed that equations can be used in mechanics because it deals with constant relations between various elements, however, “[n]o such constant relations exist between economic elements”. To suggest so regarding the deep general level of the operation of the worldly mechanisms seems equivalent to making an ontological claim that there are basically no rules governing our world and all events are only completely random. If, however, Mises thought about mechanisms on the more aggregated level of human interactions, then it is the case that, as Moorhouse (1993) pointed out, “mathematical formulations need not be based on constants and typically those employed by mathematical economists are not”, as in their theoretical work they rarely deal with explicit functions and “[i]n spite of the qualitative nature of the formulation, quite interesting theorems can be derived.”

The last major argument of Mises (p. 347) against the mathematical method is “that it must be rejected not only on account of its barrenness. It is an entirely vicious method, starting from fake assumptions and leading to fallacious inferences. Its syllogisms are not only sterile; they divert the mind from the study of the real problems and distort the relations between the various phenomena.” The author spoke in a similar vein on pp. 251, 351–352, and 374–375, accusing mathematical economics of being a “vain play, futile pastime, useless piece of mental gymnastics” that “does not contribute anything to the elucidation of the market process”. It is not entirely obvious if Mises, when writing about mathematical economics, had in mind only particular topics in the context of which he made the above epithets or mathematical methods applied to economics in general. If the first is true, then some usefulness, or even advantage over the verbal-only account, of every model he criticized can be shown, as was done with the models of the market (in this section). If, however, the second is the case, it is still surprising when contrasted with Mises’s own claim on p. 119 that “the external world to which acting man must adjust his conduct is a world of quantitative determinateness. In this world there exist quantitative relations between cause and effect.” Having this in mind, to counter this “futility of mathematics argument” of Mises, one can only reiterate well-known benefits of the use of mathematics in economics, which were partly already stated in this section in the context of specific models, such as much easier solution of quantitative problems, wide applicability and substantial flexibility of applied techniques, the ability to easily account for and better elucidate the analysis of complex mechanisms with non-trivial results, as in the case of the stability of markets, and to provide a more thorough understanding of phenomena examined, and, finally, a more efficient – more intelligible, easier and briefer – form of communication, both among academics and between teachers and students.

There are also other arguments for mathematization not yet stated in this section. For example, although Samuelson (1952) insisted on the equivalence in principle of verbal and formal languages, he, already in 1951<sup>19</sup>, wrote that “[t]he convenience of mathematical symbolism for handling certain deductive inferences is, I think, indisputable”. In a similar manner to Mises, he recognized that “the problems of economic theory (...) are by their nature quantitative questions.” Therefore, best suited to these problems is mathematics, for, although it does not prevent the making of *mistakes* or *irrelevant hypotheses*, mistakes in formal logic, in comparison with its verbal counterpart, are extremely rare. Moreover, formalism forces a theoretician to state the, otherwise tacit or even neglected, assumptions explicitly so that anyone can readily check them and the internal consistency of the argument. Thanks to this, conclusions are more reliable and precise not only in the numerical sense, but also in terms of taking account of every aspect of the assumptions, especially when several mechanisms with opposite outcomes operate simultaneously and one wants to know which of them outweigh the others. One can even argue that in mathematical economics conclusions are to a large degree automatically functions of assumptions. In addition, Hudik (2015) claims that often “unless one is forced to express ideas formally, one is perhaps not even aware that the language is ambiguous” and gives as an example the very case of the model of the market, before the arrival of which theoreticians encountered huge difficulties in conceptualizing even the basic categories of supply and demand. Moreover, he points out that mathematics can easily demonstrate possible inconsistencies in existing theories, which, for example, was famously done by Samuelson with the Marxian theory of wages and interest and by Hudik himself with some of the claims of the ASE’s economists.

Formalism also economizes on the labor of the theoretician since, as Chiang (1984, p. 4) noted, there are many already elaborated mathematical theorems which can be readily applied to any aspect of reality, including economics, and which do not have to be arduously reinvented to help solve economic problems, like Euler’s theorem on homogeneous functions applied to the theory of distribution. The formal way of theorizing in economics is also more productive in the sense of the enlargement of our knowledge for the profession as a whole in other ways too. As Moorhouse (1993) claimed, “the step-by-step mathematical derivation of a theory suggests both a host of additional new questions and a framework for analyzing them. (...) The point is that a simple mathematical model naturally generates additional models of increasing complexity”. Likewise, Mayer (1998) noted that “some formalist models can be justified, not as the end product of economic analysis, but as an intermediate product”, a “caricature” with limited purpose, upon which only future researchers can elaborate and construct more sophisticated and realistic models of reality. To conclude, although it is possible to arrive at certain results without the explicit use of mathematics and communicate them clearly, in practice it is at least very difficult, if not entirely impossible. One can list countless examples of that. For instance, Moor-

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<sup>19</sup> In Mirowski’s (1991) account, mathematics made its way into economics slowly and painfully in the first half of the twentieth century.

house (1993) recounted the distinction between substitution and income effects of a price change, which “has clarified a number of economic issues, and is a distinction unlikely to emerge from the application of even a rigorous chain of verbal logic”. One can also give a more applied example of the economics of exhaustible resources (see e.g. Chiang (1992), pp. 148–157) to show how the conclusions critically hinge on assumptions that turn out to be crucial only after rigorous formal treatment of the subject, which it is hard to think of in terms of verbal logic alone.

Mises also criticized mathematical economists for other things, like too much aggregation<sup>20</sup> in the context of the equation of exchange (p. 396) or the use of the notion of indifference in decision theory (p. 351) – the problem discussed, for example, by Nozick (1977)<sup>21</sup> and Caplan (1999) – but these are critiques of the adoption of various assumptions independent of the use of mathematics rather than of formal or mathematical economics as such. Moreover, he attacked game theory (pp. 116–117) as a tool, for dealing only with zero-sum games, which do not, in his account, describe market economies in which both parties of the exchange can benefit. However, as Murphy and Gabriel (2008, pp. 49–50) note, the field has progressed since and currently also includes positive-sum games, in which all players can benefit from cooperation. Buchanan (2001) even claimed that “[i]t is only as and when game theory extends its reach to analyse positive sum interactions that its potential emerges.”

In sum, apart from the “futility of mathematics” argument, Mises rejected specific inspirations, assumptions, models and tools rather than the mathematical method itself for being in principle unable to deal with economics. Much of this critique might have stemmed from the author’s ignorance of particular mathematical tools (like dynamic analysis) and/or from mathematical economics being still in its infancy phase at the time his critique was being written, hence one can only speculate that such a wise scholar as Mises would not have used the same arguments against mathematical economics, or would even have endorsed it altogether, had he been writing his treatise about half a century later.

### 2.3. Friedrich von Hayek

Although the Austrian economist F.A von Hayek was, both by received education and in his research, more a disciple of von Wieser than of Böhm-Bawerk or Mises,

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<sup>20</sup> Gorazda (2014, p. 220) even suggests that the ASE’s skepticism towards the use of mathematical economics and econometrics stems from its principle of methodological individualism.

<sup>21</sup> Nozick didn’t touch the issue of the language of the ASE and at least some of his criticisms can be the result of the relatively imprecise language of the ASE, but even his own criticisms would have been at some points more clear had he used graphical or algebraic exposition of them, e.g. in the context of considering the possible (dis)utility of labor and leisure or the amounts of time devoted to them (and to consumption or, possibly, shopping), where there are a lot of variables and, hence, it is quite difficult to account for all of them without any possible omissions. The same is true of any verbal debate on complex issues, like the Austrian-American debate on capital theory (see Hudik (2015)) or the Hayek-Keynes debate on business cycles (see Wapshott 2011).

he is often said to have been a member of the ASE<sup>22</sup>, therefore it might be interesting to know his opinion on mathematical economics. According to Hülsmann (2007, pp. 1083–1084), Hayek’s approach to the problem of central planning was distinctive from that of Mises due to his “acceptance of mathematical general equilibrium analysis as the most advanced expression of modern economic science.” In Hayek’s opinion, this approach could account for “multifarious interdependencies and solve the intricacies of the imputation [of value] problem” and the “Walrasian School of mathematical economics had already successfully tackled problems of a similar nature” (Hülsmann (2007, pp. 413)). Moreover, when acquainting Lionel Robbins’s circle with the views of Austrian economists, Hayek “championed the notion that general equilibrium theory was the state of the art and that all verbal economists, including Mises, worked within the very same framework” (Hülsmann (2007, pp. 541)). These opinions might be further evidence of the fact that in practice, apart from the methodological declarations and the language of analysis, the Misesian approach to economics (praxeology) is not as different from that of mathematical general equilibrium as one might initially think. To conclude, let us *in extenso* quote Hayek (already in 1974) who, appreciating the success of mathematical economics, expressed a desire “to avoid giving the impression that I generally reject the mathematical method in economics. I regard it in fact as the great advantage of the mathematical technique that it allows us to describe, by means of algebraic equations, the general character of a pattern even where we are ignorant of the numerical values which will determine its particular manifestation. We could scarcely have achieved that comprehensive picture of the mutual interdependencies of the different events in a market without this algebraic technique.”

#### 2.4. Murray N. Rothbard

Although Rothbard’s arguments against the use of mathematics in economics are not identical to Mises’s, they are quite similar. Notwithstanding the fact that he might be regarded as a prolific author, a closer look at his economic writings reveals that he had been basically repeating the same arguments against mathematical economics throughout his career. His criticisms can be tentatively divided into two categories: those regarding various assumptions mathematical economists typically make in their work, which is primarily a non-essential critique, and those that refer to more fundamental issues regarding the application of mathematics to economics.

The first argument that belongs to the family of non-essential critiques is against the use of the notion of indifference in economics (see Rothbard’s 2009

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<sup>22</sup> This is largely due to the fact that he was, together with Mises, the co-author of the business cycle theory, later espoused by Austrian (in the sense of the term defined in this paper) economists. On this theory see e.g. Huerta de Soto 2006 (1998).

(1962), p. 307). This argument cannot, however, be used as a critique of mathematical economics, since it is rather an argument against some notion in the theory of choice independent of the use of mathematics – the issue dealt with, for example, by Nozick (1977) and Caplan (1999). Rothbard (2009 [1962], pp. 130, 305–307; 2009) also criticized mathematical economists’ use of the assumption of continuity of variables in their analyses. However, even if it is true that not all individuals see and act on infinitesimal magnitudes, continuity can often be an extremely useful approximation of only minor unrealism. As a matter of fact, as Caplan (1999) points out, Rothbard’s own works are filled with continuity, as in the case of intersecting supply and demand lines, which are used throughout his magnum opus *Man, Economy, and State*. Moreover, as Moorhouse (1993) noted, all the major theorems of neoclassical economics can be derived without the assumption of continuity. Another kind of argument of Rothbard (2009 [1962], pp. 324, 325, 842, 845) against the use of mathematics is the one, already discussed in this and previous sections, about the constancy of economic relationships. In particular, in the context of the methods used by Samuelson, Rothbard (1956) accuses this prominent economist of assuming in his work that individuals have constant preferences over time, while there is supposedly no reason for making any such assumption. Even if, in general, these preferences are not constant, it does not preclude the use of mathematics, as a researcher can assume and model explicitly time-varying or endogenous preferences.

The first argument against mathematical economics which is more essential is against the use of static equilibrium analysis and its futility (see Rothbard 2009 (1962), pp. 323, 325), which is rather similar to that of Mises and can be addressed in the same way as done earlier in this section. It can, however, be classified as rather fundamental because Rothbard (p. 325) wrote something important about the use of mathematics, namely that “it cannot describe the path by which the economy approaches the final equilibrium position. This task can be performed only by verbal, logical analysis of the causal action of human beings. (...) Since mathematics is least badly accommodated to a static state, mathematical writers have tended to be preoccupied with this state, thus providing a particularly misleading picture of the world of action.” This ignorant argument is especially bizarre given the fact that the author made it 13 years after Mises published *Human Action* and that he was trained mathematically. Moreover, he stated that the concept of equilibrium “is indispensable because it is the goal, though ever-shifting, of action and exchange”, not recognizing that there is no guarantee of a market process converging to its intertemporal equilibrium and especially to a stationary one – the ASE’s state of the *evenly rotating economy*. If the “goal” was to be understood here as a “purpose”, then the contention is equally false, as certainly the *evenly rotating economy* is not the purpose of most of the participants of the economy. Other two arguments of Rothbard are also almost identical to that of Mises. The first is a mix of, already examined in this and previous sections, the “causality argument” and the “invalid analogy argument” against the supposed use of the so-called *mutual determination* approach, which is allegedly valid only

in physics and not in economics, in which the ultimate cause is known: *individual purpose* (see Rothbard (1956; 2009 (1962), pp. 306, 322–327, 785–786; 2009; 2011 (1960))). The second is the “futility of mathematics argument” which Rothbard (1956; 2009 (1962), pp. iv, 75–76, 325, 589; 2011 (1976)) had been using invoking the principle of Occam’s Razor. Rothbard (2009 (1962), p. 835) even wrote that “[m]athematics can at best only translate our previous knowledge into relatively unintelligible form; or, usually, it will mislead the reader.” To counter this argument, one can only reiterate the benefits and advantages of the use of mathematics already discussed in this section.

Rothbard used some non-Misesian arguments too. First, Rothbard (2011 [1976]) quoted Bruno Leoni and Eugenio Frola’s assertion that the informality and imprecision of human language reflects people’s behavior and that mathematical modeling of this behavior might transform humans into *virtual robots*. As Hudik (2015) points out, “it is not at all clear why researchers should use imprecise language just because their researched subjects are imprecise; one can (and, indeed, should) talk precisely even about imprecision.” Also, Rothbard does not explain in what sense modeling human behavior as if it was a behavior of a *virtual robot* can be a problem for economic theory. Another argument that Rothbard (2011 [1976]) used against mathematical economics, following others, like Say and Keynes, is that, unlike the literary economics, it necessarily simplifies complex economic phenomena and, in particular, it assumes independence between various factors involved. In mathematical economics, however, one can easily endogenize various variables and it is rather the ASE that formulates much less complex theories<sup>23</sup>. Rothbard (2009 [1962], p. 589) even wrote that “[t]o analyze means-ends relations logically, as economics does, requires taking all relations into account. Failure to do so (...) is equivalent to abandoning economics.” By this he made the preposterous suggestion that the verbal language of the ASE never abstracts from anything (or at least from anything logically necessary for human action – as though only ASE economists somehow possess the required knowledge about this) and that the ME is not economics at all, as it uses abstract theories (or supposedly abstracts from some relations which are necessary in the ASE’s theory).

Finally, what seems to be Rothbard’s (1956; 2009; 2011 (1976)) original argument, he made a distinction between the different characters of verbal and formal languages and argued that it implies that only the verbal language preserves *meaning*, hence mathematical logic is inappropriate in economics. But let us quote Rothbard (2009 (1962), p. 75) *in extenso*: “it is the great quality of verbal propositions that each one is meaningful. On the other hand, algebraic and logical symbols, as used in logistics, are not in themselves meaningful. Praxeology asserts the action axiom as true, and from this (together with a few empirical axioms—such as the existence of a variety of resources and individuals) are deduced, by the rules of logical inference, all the propositions of economics, each

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<sup>23</sup> In the last section, a specific example of this will be shown as was suggested in the previous section in the context of the discussion of abstractions and realism of economic theories.

one of which is verbal and meaningful. If the logistic array of symbols were used, each proposition would not be meaningful.” Rothbard can be interpreted to have been correct when saying that algebraic and logical symbols are inherently not meaningful, but the same is true of symbols (letters) and their concatenations (words) in any written (and spoken) language. The meaning of these is only given by the users in the process of communication (authors when stating claims and readers when attempting to interpret them). Therefore there is no fundamental difference in the meaningfulness of verbal and formal languages, even if strong equivalence between the two, *a la* Samuelson, does not hold. The ridiculousness of Rothbard’s statement lies in the fact that not only did he make a claim that various expressions of intermediate products in mathematical operations, which are sometimes difficult to interpret, do not have meaning, but a much more serious one that somehow algebraic language at every stage of reasoning (i.e. including assumptions and conclusions) is always devoid of meaning, while verbal language somehow possesses meaning and preserves it at every step of reasoning. He, however, gave neither any proof nor any compelling arguments for that. Given the fact that, as Moorhouse (1993) noted, “each step in a mathematical derivation can be given an economic interpretation”, only *a fortiori* is every conclusive proposition meaningful too, hence Rothbard was plainly wrong. One can even argue that, on the contrary, it is the ASE whose theorems have dubious meanings, as the relations they describe are based on vague assumptions and do not have any explicitly stated, clear, specific form of mathematical relations.

In sum, Rothbard, unlike Mises, criticized not only specific inspirations, assumptions, models, tools and the usefulness of mathematics, but went one step further and using the argument listed here as the last one also rejected the mathematical method itself as a whole for being in principle unable to deal with economics.

## 2.5. Modern economists of the ASE

Nowadays, adherents to the ASE still condemn mathematical modeling in economics harshly (see e.g. Salerno (2009)) and make basically the same arguments against the use of mathematics in economic theory as previous generations of ASE economists (see e.g. Boettke (1997, 2008), Higgs (2011), Wiśniewski (2014)). In general, they ignore arguments for the superiority of formal language over its verbal counterpart. Some seem not to even recognize why mathematics is used in ME theory at all, in particular neglecting it as a method of enquiry. For example, Machaj (2010) when considering the question of why ME economists use mathematical models, answers only that the reason for this is that the task of forecasting is facilitated, disregarding the whole array of arguments regarding the benefits of mathematization, especially for the purpose of theoretical investigation. Others, like Huerta de Soto (1998), still seem to repeat the “preoccupation with states instead of with processes” argument, which was already dated in the

days of Mises and *a fortiori* in those of Rothbard. Moreover, most contemporary Austrians continue with their abstinence from mathematics, yet at least some of them claim their knowledge to be sufficient for them to competently formulate strong convictions about mathematical economics. For example, Murphy (2006, p. 149) wrote that “Rothbard’s 2009 (1962) critiques of Fisher’s equation of exchange (pp. 831–42), and various Keynesian concepts (pp. 859–68) are simply brilliant, and should put to rest the frequent allegation that Austrians are incapable of mathematical reasoning.” Content with the ASE’s adherents’ mathematical knowledge and ability, Murphy appears to be unaware of (or simply ignores) the fact that, although it can be criticized for various reasons, Rothbard’s own critique of the *absurdity* of the Keynesian multiplier is fundamentally flawed due to the very basic (to mathematical economics) mistake of conflating expression that stems from manipulation of untrue identities with a genuine multiplier based on behavioral assumptions (see Callahan 2012). For it is one thing to make a mistake, but it is another to repeat it for decades or call it a *brilliant* critique and claim that Austrians even remotely understand some very basic notions of mathematical economics.

### 3. Concluding remarks

Although in light of the alleged failure of ME, the ASE’s methodology might look like an attractive alternative, its foundations are not as strong as they first appear, since the ASE’s methodology suffers from a lot of problems that are not readily visible when considering only the ASE’s economic theorems. First, its basis in methodological dualism is dubious. Second, its case against empirical verification of hypotheses and theories seems not to be strong. Third, although the ASE’s theory can be perfectly true, at the same time its scientific value (measured in, for example, the scope of applicability) may be low, especially in comparison with other theories, not necessarily *a priori* true. In contrast to what is usually believed about the differences between the ASE and ME, especially on the basis of methodological declarations alone and those about deduction/induction and *a priori/a posteriori* in particular, they may well be overstated and the two approaches may in fact not be so dramatically different in terms of methodology. The ASE’s methodological requirements of postulates like methodological individualism or subjectivism are satisfied by ME. Moreover, the ASE’s standard of a causal-realist approach to economics may be at least equally well met by ME. There is, however, one great difference in both approaches to economics, which seems not to stem directly from their fundamental methodological declarations, but rather manifests itself mainly in practice. While the ASE, for the most part, uses only verbal logic, ME theory is fully mathematized. The fact that the two communicate in virtually two different languages might also be at least part of the reason why at present there is so little dialogue between the two, something that is necessary especially during these post-crisis times of questioning the ME paradigm.

The ASE's rejection of mathematics as a tool of economic theory started with its founder in the 19th century and has continued ever since. Menger objected to its use in principle, asserting that it is unsuitable for explaining and establishing the laws of complex phenomena, although without giving reasons convincing for the modern reader. In the 20th century Mises offered several arguments against the use of mathematics in economics, but they too are either dated or suffer from a lack of understanding of what mathematics in service to economics is capable of. In particular, his "futility of mathematics argument" can be easily countered by listing the numerous benefits of mathematization of investigations in economic theory. Rothbard mostly repeated these arguments, but he too, blind to the process whereby notation of a language gains meaning, grandiosely stated that in contrast to the verbal language of the ASE, mathematical symbols, and therefore the conclusions that result from their manipulations, are meaningless, so mathematics as an engine of economic theorizing should be rejected totally.

In sum, careful examination did not show that the Austrians, after all, were right about their rejection of mathematics, as the reasons they gave for it do not survive scrutiny and fail, sometimes dramatically, therefore the ambitious project of Mises and Rothbard to reconstruct economic theory and, hence, its policy prescriptions cannot be considered a serious alternative to existing ME paradigm<sup>24</sup>.

Despite the fact that ASE economists had and continue to have many interesting ideas, as Buchanan (2001) notes, "[t]heir influence has been limited, however, surely in part due to their eschewal of formal tools of analysis." One can also argue that only now they have at their disposal sophisticated enough tools of e.g. search theory, game theory or dynamical systems to employ in modelling intricacies that they merely used to speak about. As long as mathematics is not used in the ASE, at least as a tool for communication, ME economists accustomed to the language of mathematics cannot be blamed for ignoring vague literary arguments of modern Austrians, who do not refer to any essentially new reasons to reject mathematics and, in general, seem deaf to the arguments for mathematization in economics, which certainly does not facilitate any dialogue between the ASE and ME. Only recently, benefits of the use of mathematics in economics have been noticed by Hudik (2015) who called for the mathematization of the ASE's economic theory, within which there seems to be the possibility and room for the change postulated by Hudik (in fact, there have already been attempts to formalize some of the ASE's theory as documented by Hudik), but if this change is sufficiently consistent, then it will probably lead to the absorption of the ASE into ME. In this situation, the ASE will become in principle not distinctly different from ME, but will rather only emphasize certain aspects, sometimes not stressed enough (in its

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<sup>24</sup> Although modern ME is often accused of being overly mathematized and one may even have the impression that the profession in general is aware of that and is trying to change it, on the contrary, it may be the case, as P. Romer (2015) argues, that mathematics in ME is sometimes done in a sloppy manner and not rigorously enough.

view) by ME<sup>25</sup>, such as problems of causal inference in empirical research, more tests of the stability of parameters through time, and more models of out-of-equilibrium market processes than of equilibrium states and smooth transitions between them. The potential benefits of this for ME seem to be barely visible.

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As a closing illustration of the results of the emphasized difference between the two approaches to economics considered here, one can use the case of business cycle theory, which is so vital to the issue of policy making. While ME employs dynamic, stochastic algebraic models with microfoundations, the ASE uses verbal narrative, occasionally with some help from simple geometrical illustrations of the structure of production or the market for loanable funds or the economy's production possibilities frontier (see Garrison 2001 or Huerta de Soto 2006 (1998)). While ME's models, in accordance with the principles of methodological individualism and subjectivism, scrupulously specify the technologies of the analyzed economy, agents' preferences (also regarding leisure or time preference), knowledge and expectations, equilibrium conditions, price- and wage-setting mechanisms, whether the economy is closed (if not: how big it is), etc., basically none of these is specified in detail even verbally in the Austrian descriptions (see e.g. Żółkiewski (2010) or, on the specific topic of expectations, Mueller (2014)). Therefore, while in ME models, clear behavioral rules or policy functions can, in general, be computed and analyzed, in the Austrian theory they are all unspecified and highly vague. As a result, in the case of ME models, one can easily check an economy's completeness, internal consistency, and stability conditions, simulate it (given some clearly stated initial conditions and numerical values of parameters), and perform policy experiments. Again, generally none of these are conducted using the ASE's description of the economy, as it would be hardly possible, if at all, to do this without explicit behavioral rules, which Austrians dispense with when theorizing, which is the reason why the conclusions of their theories are so loosely connected with their assumptions and, hence, precarious. Also, given the clear statement of various ME models, one can unambiguously estimate and compare them in various aspects or test sharply defined hypotheses. In the case of the ASE, although numerous attempts have been made to empirically test its business cycle theory (see e.g. Mulligan 2002, 2006; Bismans and Mougeot 2009; and Lester and Wolff 2013), as there are no clearly stated algebraic expressions that describe the phenomena considered by this wholly narrative theory, it is hard to put it to an honest, rigorous test, hence attempts to

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<sup>25</sup> This scenario would imply that the relation of the ASE with ME in terms of its attitude towards mathematization of economics would become similar to that of Post-Keynesianism's with ME, which is summed up by Rosser (2003) who wrote that "there is no necessary relation between the use of mathematics and whether or not certain economic discourses are Post-Keynesian."

verify it are of dubious value, which is not due to any errors the researchers could have made, but to the ambiguity of the theory itself.

In sum, while ME's accomplishments continually advance scientific progress, the Austrian business cycle theory seems to be even incapable of expressing itself clearly and coherently, and, therefore, entirely misguided and of little value. As a consequence, it is hard to think of an exercise in which one could perform any meaningful comparison between the ASE's and ME's theories of business cycles in terms of e.g. their ability to generate fluctuations similar with those observed in real economies or to fit the empirical data. Although modest attempts in the right direction have been made to formalize and clarify ASE business cycle theory (see e.g. Fillieule 2005; 2007), the models that try to achieve this are still *ad hoc*, static and deterministic, hence one still has to wait to see a fully specified, formal model of the Austrian business cycle theory to even begin considering it as a serious alternative to, without a doubt full of shortcomings, ME's theories of the business cycle.

Received: 26 October 2016

(revised version: 3 April 2017)

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## SZKOŁA AUSTRIACKA I EKONOMIA GŁÓWNEGO NURTU: CZYM SIĘ RÓŻNIĄ?

### Streszczenie

W następstwie kryzysu finansowego i gospodarczego z 2008 r. ekonomia głównego nurtu była oskarżana o nieumiejętność przewidzenia, uniknięcia i złagodzenia tegoż kryzysu. W tej sytuacji interesujące może być rozpatrzenie, czy alternatywy dla ekonomii głównego nurtu mają jakąś wartość. Celem tego artykułu jest próba odpowiedzi na pytanie, czy teoria austriackiej szkoły ekonomii może być uznana za poważną alternatywę dla ekonomii głów-

nego nurtu. Autor poszukuje odpowiedzi poprzez zbadanie jej metodologii, a w szczególności jej stosunku do wykorzystania metod matematycznych w teorii ekonomii. Wskazane zostały pewne słabości metodologii szkoły austriackiej, które czynią ją mniej atrakcyjną, niż można by pierwotnie sądzić. Następnie dokonano przeglądu argumentów ekonomistów austriackich przeciwko matematyce w ekonomii; dokładne badanie pokazuje, że nie są one wiarygodne. Wszystko to świadczy o tym, że austriacka szkoła ekonomii nie może zostać uznana za poważną alternatywę wobec ekonomii głównego nurtu. Ostatecznie wniosek ten jest zilustrowany porównaniem metod, którymi wyprowadza się i analizuje teorie cyklu koniunkturalnego w dwóch rozważanych tu podejściach do ekonomii.

**Słowa kluczowe:** austriacka szkoła ekonomii, ekonomia głównego nurtu, metodologia ekonomii, ekonomia matematyczna, formalizm w ekonomii

**JEL:** B41, B53.

## AUSTRIAN AND MAINSTREAM ECONOMICS – HOW DO THEY DIFFER?

### Summary

In the aftermath of the 2008 financial and economic crisis, mainstream economics (ME) was accused of being unable to predict, prevent, and alleviate it. In this situation, one might be curious if alternatives to mainstream economics are of any value. The aim of this paper is to answer whether the theory of the Austrian school of economics (ASE) can be considered a serious alternative to ME. This is done by examining its methodology and especially its attitude towards the use of mathematics in economic theory. Some shortcomings of the ASE's methodology are considered that make it a less attractive alternative than might initially be assumed. Next, the arguments of the ASE economists against the use of mathematics in economics are reviewed; careful examination indicates that they are not sound. All of this shows that the ASE cannot be considered a serious alternative to ME. Finally, this conclusion is illustrated with a comparison of methods with which theories of business cycles are arrived at and analyzed in the two approaches.

**Key words:** Austrian school of economics, mainstream economics, economic methodology, mathematical economics, formalism in economics

**JEL:** B41, B53.

## АВСТРИЙСКАЯ ШКОЛА И ЭКОНОМИЧЕСКИЙ МЕЙНСТРИМ: В ЧЕМ ИХ РАЗЛИЧИЕ?

### Резюме

Финансовый и экономический кризис 2008 года дал повод обвинить главные течения (мейнстрим) экономической науки в неумении предвидеть, избежать и облегчить этой кризис. В этой ситуации интересно задать вопрос, имеют ли какую-либо ценность альтернативы экономического мейнстрима. Автор статьи попытался выяснить, может ли теория австрийской школы считаться серьезной альтернативой для главных теорий

экономики. Ответ на этот вопрос ищется посредством исследования методологии австрийской школы и особенно ее отношения к использованию математических методов в теории экономики. Были указаны некоторые слабости методологии этой школы, которые делают ее менее привлекательной, чем можно было бы первоначально ожидать. Затем был проведен обзор аргументов австрийских экономистов против математики в экономике; сделанный анализ показывает, что они не являются убедительными. Все это свидетельствует о том, что австрийская школа экономики не может быть признана в качестве серьезной альтернативы для экономического мейнстрима. Окончательно этот вывод проиллюстрирован через сравнение методов двух рассматриваемых здесь течений экономической науки, с помощью которых выводятся и анализируются теории конъюнктурного цикла.

**Ключевые слова:** австрийская школа экономики, экономический мейнстрим, методология экономики, математическая экономия, формализм в экономике

**JEL:** B41, B53.