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## **Austrian and Mainstream Economics on Mathematics – a Comment on Pieniżek (2018): Reply to Machaj (2019)**

### **Introduction**

Machaj's (2019) interesting comment on my original article (Pieniżek, 2018) makes several points. The major one is that in my discussion of the usefulness of the Austrian School of Economics' (henceforth: ASE) tool of study, which is *praxeology*, I have drawn a faulty conclusion about its non-usefulness. Next, he discusses the status of mathematics as a language in general and in particular its applicability to economics. Finally, he tries to give an example of an ill-suited application of mathematics to economics by considering a neoclassical production function in the context of economic growth.

Before I address the content of Machaj's remarks, I think it would add to clarity if we consider the terminology he employs. He mentions at the beginning of his comment that he prefers to use the term *neoclassical economics* over *mainstream economics* (henceforth: ME) and seems to equate the two in the remainder of the text. He does not, however, provide any reasons for this nor does he argue with the distinction of *mainstream economics vis-à-vis neoclassical economics* that I employed in my original article. Why does he disregard, then, the typical convention that, say, an authoritative figure in the field, Acemoglu (2009) followed in his exposition of economic growth, i.e. the very topic that Machaj uses as an example of economics in his comment? Acemoglu divided the models he analyzed into the neoclassical group and the rest, despite the fact that both of the groups are undoubtedly mainstream, which suggests that the two terms are not equivalent. Although I did not elaborate deeply on the issue in my original article, I was sticking to the spirit of Colander (2000), who argued for the terminasia of *neoclassical economics*, i.e. for the economist-assisted killing of a term whose use is inconsistent and whose content is difficult to determine, typically being so ambiguous that it renders the term almost meaningless. It should not surprise us, as since 1900 up to the present, it has meant to describe very different ways of doing economics since the 1840s up to the present; however, the discipline has experienced such an enormous change since the 1840s (now even greater than at the time when Colander wrote his paper) that it could not possibly mean the same thing as

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even a century ago, rendering the term a very imprecise and hence misleading category. Moreover, even McCloskey (2010, pp. 8, 414) that Machaj mentions twice in his comment, classified *Mengerian* (i.e. the ASE's) economics as *neoclassical*<sup>1</sup>. Given that Machaj himself juxtaposes the *neoclassical* with the ASE's economics, it creates an even greater classificational mess. For all of these reasons, I prefer to terminate (*nomen est omen*) using the term neoclassical economics in the current context and, following my original article, stick to the *mainstream economics* category when denoting the economics that is contemporarily being practiced in the top journals and at the top universities. Having in mind this terminological remark, I am ready to address Machaj's major objections to my original article.

### 1. The case of faulty generalization?

I am happy to read that Machaj expresses agreement with my critique of the ASE's arguments against the use of mathematics by ME, but still in the opening section he writes that having in mind my goal of considering the ASE as an alternative to ME, my study of the ASE's methodology with which I open my analysis "in itself is already disappointing, since methodology of economics is not really economics<sup>2</sup> just as much as Austrian School's methodology is not Austrian School's economics". This accusation is surprising, as it actually amounts precisely to the novel (or at least not widespread) observation concerning the comparison of the ASE's and ME's methodologies that I emphatically made by writing on p. 216 that typically "considerable attention in methodological disputes is paid to methodological declarations instead of to actual practices of doing economics". While not being itself the main part of my analysis, my brief juxtaposition of the respective methodologies served the purpose of being the starting point of my proper comparison of the actual practices of doing economics as well as allowing me to highlight the contrast between a typical discussion of the differences between the two schools boiling down to their methodological declarations and my discussion stressing that the characteristic by which, as I argued, the two schools crucially differ, is rarely mentioned as a methodological difference but rather manifests itself mainly in practice, i.e. the ASE being almost entirely verbal while ME being largely mathematized. Machaj's main objection, however, is that my main conclusion that the ASE is generally not useful as an alternative paradigm to ME, constitutes a faulty generalization. I cannot agree with that, but I concede that my argument might have been not persuasive enough, as although I tried to elaborate on the specific blocks of my main concluding argument as meticulously as possible, given the constraints of my lengthy article, I probably did not elucidate its structure and the connecting joints clearly enough. Let me then briefly reconstruct it, this time hopefully in a more lucid manner.

First, I tried to establish the fact that, contrary to mere declarations, methodologically the two schools don't differ much from each other, especially when it comes to the ASE's methodological postulates and principles purportedly not fulfilled by the practice of ME. This observation constitutes my first assumption; for clarity's sake let me call it [A1]. Second, there exists however one big difference in the way the two schools actually do their economics, which manifests itself mainly in practice and does not stem from any single fundamental methodological principle, i.e. the ASE is almost entirely verbal while ME

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<sup>1</sup> McCloskey herself accuses the term *neoclassical economics* of perpetuating anachronism (pp. 414, 456).

<sup>2</sup> It is not clear what he has in mind writing about "economics" without any adjective, but we can only suppose that he means ME, to which I would obviously applaud, but given his declared defense of the ASE, I would just treat equating "economics" with ME on his part as a Freudian slip.

is largely mathematized. This is my second assumption [A2]. Third, given that I refuted the ASE's arguments against the use of mathematics in economics and I claimed that the numerous arguments for the benefits of the mathematization of economics hold true, this gives a lot of advantages to this breed of economics which not only employs words but also freely uses suitable and reasonably applied mathematics when compared to the breed of economics that restricts itself mainly to the natural languages. This is my third assumption [A3]. Now, if the only major difference in the actual doing of economics between the ASE and ME is in their use of mathematics [A1, A2], while it does not come with any substantial costs, it confers multiple advantages listed by the benefits-of-mathematization arguments [A3] on the latter over the former [conclusion (intermediate)]. This, in turn, implies that ME (at least in its research potential, if not in the actual theories it has already developed) is superior to the ASE in terms of both their theoretical and empirical scientific capabilities [conclusion (final)].

Although the above reasoning (the two implications) does not amount to any definitive proof, because its joints and blocks, on which I elaborated at length in the original article, involve imprecise phrases (like 'mainly', 'largely', 'major', "suitably", 'reasonably') and are by themselves not apodictic truths, it hopefully amounts to a sound argument culminating my original article. Realizing that so far it was largely abstract and therefore perhaps not persuasive enough, I supplemented it with a discussion of the more concrete and palpable example of research in the field of business cycles done by the two schools. I carefully compared the methods typically employed by them to arrive at the conclusion that even though ME makes simplifying assumptions, compared to the ASE it actually specifies much more pertinent characteristics of the phenomena to be inspected and describes them in a more detailed manner; hence, it provides a much more realistic, insightful and reliable analysis. In contrast, the Austrian business cycle theory (ABCT), specifying its assumptions in much less detail and trying to analyze complex issues with verbal language alone, suffers from dubious connections between its assumptions and conclusions. It is actually so ambiguous that it is even incapable of formulating itself clearly and coherently enough to be reliably tested empirically or meaningfully compared with any modern ME theory in terms of their abilities to generate realistic fluctuations or to fit the historical data, which could help in adjudicating on their relative empirical relevance. Given that the ABCT is one of the ASE's flagship theories<sup>3</sup>, other ASE research subjects, studied in a similar fashion as the ABCT, result mostly in much less detailed and refined ASE theories. Therefore, my critique of the ABCT *vis-à-vis* ME theories, without loss of generality can via the argument of similarity (along similar lines or even *a fortiori*) be extended to other ASE theories. This supports the above conclusion that the whole of the ASE's edifice is inferior to that of ME in terms of their respective scientific capabilities (if not in terms of the quality of their respective existing theories).

## 2. On the language of economics and suitability of mathematics

Machaj argues in the second section of his comment that "mathematics is not a language, but the means of expressing and communicating thinking". This is, however, the rough sense in which I was using the term *language* in the context of economics in my original

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<sup>3</sup> Perhaps, I was even too generous to the ASE in my original comparison just as I am now, but e.g. according to Lucas's (1988, p. 5) definition of a theory, i.e. "an explicit dynamic system, something that can be put on a computer and run", the ABCT in its current form does not fulfill this criterion and, hence, does not even deserve to be called a theory.

article. Next, he claims that particular symbols of any language “are merely tools of expressing the lines of thought”, disregarding the possible influence that mere language itself exerts on thinking (a weak version of the Sapir–Whorf Hypothesis) and, especially, the extent to which language, particularly a written one, facilitates thinking and expressing it both for the purposes of an author herself and in order to communicate thoughts to a wider audience. Economics can be a perfect example of both of these roles of a language if math is to be considered a one.

In the third section, Machaj tries to dilute the problem of the importance of language in economic theorizing, and hence denies the existence of any kind of ranking of methods that can be established at least for the specific tasks or fields in economics. Suggesting that we don’t really even precisely know what mathematics is and that the ASE’s economic theorizing fulfills some criteria of being called mathematical, he argues that “ultimately what matters is *suitability* of a particular theory” rather than its language.

First, referring to the fact that mathematics itself does not have a clear-cut definition, Machaj suggests that the ASE can also be characterized as mathematical. This might be due to his understanding of the term *mathematical* as referring to the nomothetic system of thinking, a definition that I am very reluctant to accept because this stretching of the meaning of *mathematical* to basically any deductive, logically coherent system of thinking would mean that basically any heterodox economics school of thought’s reasoning is virtually mathematical economics, which renders the term so wide and general that it becomes almost meaningless. Instead, we can reach a compromise and agree that there are degrees of mathematization measured by, say, the sophistication of mathematical tools used in an economic study, or the variety of theorems employed, or, simply, the number of equations and inequalities used to express a theory. But then, the ASE with its occasional use of the simplest math would be very low on the continuum or the scale compared to ME which uses increasingly mathematically sophisticated tools, rendering any description of the ASE as mathematical almost devoid of any meaning or offering a misleading characterization. If this is true, then Machaj might want to consider other, probably more useful distinctions, reflecting a common understanding of mathematics in the field, like algebraic vs. non-algebraic economics or quantitative vs. qualitative economics. The ASE’s criticism of mathematics in economics would then be easily translated into a criticism of algebraization and quantification of economics. Basically, almost symmetrically, the arguments for the benefits of mathematization in economics would then be easily translated into arguments for the benefits of algebraization or quantification. We have, however, no reason to think that, apart from changing the terminology, this would contribute anything of value to the debate over the usefulness of the competing paradigms.

Second, by stressing the *suitability* of a theory and ignoring the method used for its development and expression, he relativizes the role of mathematics in economics, in practice employing a variation of the “futility of mathematics argument” and disregarding the vast arguments for mathematization of economics I listed in my original article. While we can agree that in principle it is the suitability of a theory that should matter, a couple of issues emerge. While I hope we may roughly agree about what are the characteristics of a suitable (good) theory that Machaj does not list, the attributes of which I mentioned in my original article, being: realistic assumptions, scope of applicability, the degree to which it illuminates our perception of the mechanisms under study, empirical accuracy of predictions, etc., if we are to test it rigorously against empirical observations and, especially, assess its relative empirical suitability with some competing theories, then we often have to make use of some of the tools that the ASE generally eschews, like econometrics. Also, if we are to study the conditions under which the emergence of such

suitable theories is more likely, we also have to ask ourselves the question of what methods facilitate developing such suitable theories, especially those that, apart from being empirically accurate, are likely to display internal consistency and are expressed in a form that helps them to be clearly communicated to the research community, which in turn can build on it, contributing to scientific progress even further. Careful analysis of the benefits-of-mathematization arguments, which I presented in my original article, suggests that it is ME's rather than ASE's method that assists better in attaining these goals.

### 3. Example considered

In the fourth section, Machaj mostly considers the example of a production function in the context of an economic growth theory. He does not provide any reason why he prefers to discuss the example for his abstract arguments using a field different from that with which I started this discussion, i.e. business cycle theory. This forces us to change the context, vocabulary and some already established arguments from my original article, but let it be so. However, before embarking on a further discussion of the content of his example, let me, again, briefly remark on one issue related to the terminology and relative merits of the two paradigms considered in this debate.

Machaj interchangeably uses the terms *neoclassical production function* and *Cobb–Douglas production function* and, in particular, he claims (on p. 258) that the former function is the latter function. While the latter is conventionally defined precisely as a product of power terms, the former, given the problem with the whole term *neoclassical economics* that I already discussed, is only seldom defined in terms of math, which may create confusion (note the clarity-benefits-of-mathematization argument). But when the term is expressed in math, like in the already mentioned Acemoglu (2009, p. 34), the neoclassical production technology function is defined as one with diminishing positive returns to inputs, constant returns to scale and satisfying the Inada conditions. If these are the characteristics of the neoclassical production function, then the Cobb–Douglas function (see Acemoglu 2009, p. 36) is only a special case of the former and Machaj's statement is simply wrong by assuming the reverse inclusion. I can only bet that if he had written out the formal definitions of the two classes of functions, he would have easily avoided the mistake – again, a case in point of the clarity-benefits-of-mathematization-in-economic-theorizing argument.

Machaj (p. 258) starts his discussion of the example intended to illustrate his claim about the primacy of *suitability* over the way the theory was derived or the form that it possesses, with a claim that “in case of the Austrian versions production functions are not written in forms of equations, because of two primary reasons. One, all the variables and their influences are not known. Second, so is the case with coefficients, possible tradeoffs, and issues of complementarity versus substitutability.” The claim suffers from several problems, including inconsistencies, that should be discussed.

First, the same arguments could with equal validity be used against literally any equation in economics (except perhaps for identities), which would simply mean that ME theories are basically almost by definition less suitable than those of the ASE, which would largely render any discussion of suitability of a particular economic theory redundant. Second, is it really true that the Austrians don't express production functions in terms of equations? What Rothbard (2009 [1962], p. 37) did in his exposition of a production theory in Table 1, in which he put the amounts of inputs together with outputs, was to essentially express a production function in an equation (i.e. a statement) without using much algebra. As

Machaj (p. 256) soberly stresses, “particular symbols, be it letters, drawings, diagrams, or numbers are merely tools expressing the lines of thought. They are not the real *content* in itself.” Therefore, the same content as that present in Rothbard’s exposition can be expressed, with  $f$  being the production function, with a little bit more of algebra as below:

$$f(X, Y) = \begin{cases} 0 & \text{if } X = 0 \text{ and } Y = 3, \\ 4 & \text{if } X = 1 \text{ and } Y = 3, \\ 10 & \text{if } X = 2 \text{ and } Y = 3, \\ 18 & \text{if } X = 3 \text{ and } Y = 3, \\ 30 & \text{if } X = 4 \text{ and } Y = 3, \\ 40 & \text{if } X = 5 \text{ and } Y = 3, \\ 45 & \text{if } X = 6 \text{ and } Y = 3, \\ 49 & \text{if } X = 7 \text{ and } Y = 3, \end{cases}$$

Although the form of the production function is different now, the content has not changed at all; however, it is probably more explicit and shows clearly that Austrians (at least in their exposition of production theory) do occasionally use production functions in the form of equations. Third, perhaps largely due to this excuse, the Austrians even verbally don’t specify the exact properties of the relations between inputs and outputs (likewise in case of virtually any other relation) both in their theorizing and in their more applied research, the problem that I mentioned in my original article while discussing Long’s (2006) distinction between precise and nonprecise abstractions. Note that the fact that production functions are not explicitly specified with algebra does not mean that all the problems Machaj mentioned with production functions in general (not only in the form of algebraic equations) disappear in the process of theorizing! The fact that something is not even verbally specified or discussed does not eliminate or solve the problems and constraints it imposes, but only amounts to sweeping the problem under the carpet. This is the reason why the connections between the assumptions and conclusions of the ASE’s theories are so vague, the problem I mentioned in my original article when discussing the example of the ABCT. Without an explicitly defined relationship between inputs and outputs, the ABCT suffers from a structural gap, as in the exposition of, say, Huerta de Soto (2009). To be more specific, if the production function(s) is (are) not even verbally described in detail, how do we know about, say, how strong production (either aggregate or at every stage of its structure) responds to monetary policy shocks or what are the distributional consequences imposed by business cycle fluctuations? In contrast, one is hard-pressed to find any modern ME theory involving a production economy that does not explicitly specify the production function(s) involved. Not only quantitative, like in the case of the canonical New Keynesian model of the business cycle (see, e.g., Galí (2015), chapter 3), but also non-obvious analytical qualitative results often hinge on specific functional forms and their parameters, as, e.g., in the so-called canonical model of the wage structure (see Acemoglu and Autor (2010), chapter 12.3). Therefore, in theorizing it is of critical importance to specify all the necessary assumptions explicitly, which is enormously facilitated, if not forced altogether, by the mathematization of economics – yet another argument for its benefits that I mentioned in my original article. Probably, the natural solution to this particular problem with ASE theory is to either confine it to pure-exchange-economies theories or else to paraphrase Wittgenstein (to whom Machaj referred while discussing language), by concluding that “whereof one cannot speak [precisely enough], thereof one

must be silent.” Fourth and finally, the consequence of the fact that the expositions of the production theory by the ASE are simplistic and obscure, as in Rothbard (2009 [1962]), which involve some inputs and outputs, is that production relationships barely manage to be called functions. Also because of this, the extent to which the production process is analyzed by the school is much narrower than in ME, even though the Austrians stress the importance of the so-called structure of production. I would advise them that even if the *true* (or *exact*) production functions are too complex to be written out and meaningfully analyzed when applied to specific historical problems, at least some gains in understanding could be reaped if the Austrians for some narrowly-defined theoretical tasks would assume that all the factors and relevant circumstances like institutions, culture, technology, human capital, etc. are being held fixed and would analyze the properties of the production processes by the means of simple functions, with standard inputs being capital and labor. But they don't do that and continue to talk about factors of production and production process (e.g. in the context of the ABCT) without any coherent framework connecting the two, rendering its conclusions precarious if not dangerous altogether.

Later, Machaj, attempting to provide evidence for the claim that “math in itself is not really an issue in economic theorizing”, writes that “Austrian analysis is definitely more promising in explaining economic growth than a typical neoclassical modeling is”. The latter claim is very strong and I cannot agree with it. Moreover, it is very vague. In particular, I am not sure how to measure “promisingness” apart from evaluating the capacities of the respective methodologies, being exactly what I was doing at length in my original article (and what apparently was ignored by Machaj), nor do I know what exactly he had in mind when writing about “typical neoclassical modeling”. However, if we for a moment were to skip the respective research potentials of the two approaches, and focus on their already existing research on the topic of economic growth, we see a vast literature on the part of ME (see Acemoglu 2009) providing numerous insights into the specific mechanisms and quantitative studies that explain a lot of the phenomena involved, though not necessarily every particular historical event, which task is reserved for historians rather than economists<sup>4</sup>, while very little on the part of the ASE can be observed on that front. Perhaps Machaj has in mind some successes I am not aware of. The only source that he cites to back up his claim is McCloskey (2010) that allegedly “can be a perfect demonstration of how much Austrian economics can add to our understanding of long-run economic growth.” First, I would not agree that her work can be characterized as Austrian. I am not aware of her using the ASE's praxeology to any extent. Also, she admitted to being inspired by the ideas of economists like Schumpeter, Hayek, Lachmann and Kirzner, commonly associated with the ASE, but not of those from the line of Menger – Böhm-Bawerk – Mises – Rothbard, with which for the purpose of such discussions I carefully defined the ASE in my original article. Instead, when referring to her own eclectic methods of work, she summarized them in McCloskey (2017) without mentioning the ASE: “we happy little band of humanist-economists keep the Samuelsonian, positivist, Chicago-School instruments shipshape, but look too at the rest of the logic and evidence.”

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<sup>4</sup> Another reason why I would have preferred to stick to the example of business cycle theories is that they are more economic in the sense that both approaches deal with the phenomenon without much help from other sciences, therefore they are not as much contaminated by other sciences' involvement. Instead, when considering long-term economic growth, any economist must work heavily with the scattered historical evidence, the quality of her work often being determined more by the quality of her historical knowledge and skills rather than the particular economics she employs. Moreover, long-term growth is often hypothesized to hinge on certain, say, geographical factors (see, e.g., Acemoglu (2009) section 4.3.2) transcending, comparable across paradigms, purely economic explanations centering on human actions, culture and institutions.

Second, although McCloskey's account of modern long-term economic growth is largely praised, it is not necessarily undeniably true.

Specifically, Machaj suggests that neoclassical modeling cannot explain long-run economic growth because the Cobb-Douglas production function and the Euler theorem are not suitable for that purpose. However, a production function, either simple or complex, is only one of many elements of some complete description of an economy that could potentially attempt to explain both growth and income distribution phenomena, like a version of a Ramsey-Cass-Koopmans model as in Acemoglu (2009, Chapter 8). In particular, the very plain Cobb-Douglas production function is employed by Galor (2011, Chapter 5) in his Unified Growth Theory that accounts for endogenous transition across various historical growth regimes, which provides us with at least some rigorous account of the empirical long-term growth patterns, a thing that neither praxeology nor McCloskey's story were able to deliver. That being stated, instead of strictly competitive, one can view all these three theories as being at least to a certain extent complementary in explaining different sets of facts or describing only certain aspects of the phenomena under investigation.

Finally, I am obviously willing to agree with Machaj that suitability is a crucial issue in economic theorizing, but typically the question of its form and the sophistication of the tools involved, contrary to what he intends to argue for, unsurprisingly turns out to be of no less bearing, and shows that in practice math really is the issue in economic theorizing.

## Conclusion

Just as I characterized most of the ASE critiques of mathematization of economics as non-essential, Machaj in his fifth and concluding section uses the same description to characterize my critique of the usefulness of the ASE. Perhaps it really is non-essential. Never claiming it to be of an opposite character, I agree with the statement in the sense that it was not my intention to show that every ASE claim is wrong or that every possible ASE study fails to render any illuminating insight. What I did, instead, was to provide arguments that ME, with its superior methods palette, is much better than praxeology, the verbal method of the ASE, both at producing internally consistent theoretical insights in economics as well as at performing empirically meaningful quantitative analysis. In principle, it is possible that praxeology can develop theories that will be much more coherent and detailed in their analysis, much wider in their scope of application, providing more insights into phenomena under inspection, more accurate empirically, etc. – in sum, displaying higher scientific merit than ME theories, but in practice it is not observed, which is, as I claim, due to the very reason of the existence of the crucial difference in the ways the two breeds of economics are being practiced: the (non-) use of math. As a result, the ASE is not nearly as useful as ME in the task of doing economics, hence in its current form it cannot be considered a serious alternative to the modern ME paradigm.

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