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Formalism in Economics: Perspectives from Philosophy of Mathematics
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In the long-running debates over whether economic methodology has become "too formalistic," one recurrent flash point concerns mathematization: is economics too mathematized or wrongly mathematized? In a classic discussion, Alfred Eichner argues that mathematization has encouraged "adherence to a non-scientific methodology": rather than explaining what is observed, economic theories determine the conditions that must be satisfied if a certain goal is to be achieved -- thus turning the "purpose of science ... on its head" (1983, 517). More recently, Stephen Pratten says that the "blinkered insistence upon formalistic modelling has prevented economics from developing in a way that helps us understand the world in which we live"; in particular, excessive use of mathematics in economics can lead us away from practical relevance (Pratten 2004a, 37; see also Blaug 2002). Roy Weintraub says these arguments fail, partly because their proponents fail to address the central issue: what counts as excessive in the use of mathematics (2004, 47)?

In a series of recent publications, co-authors Thomas Boylan and Paschal O'Gorman and Vela Velupillai propose a new perspective, focusing attention on philosophy of mathematics. Adopting formalist methodology, they say, has led economics down the wrong path: classical mathematics is inappropriate for use in economics; intuitionistic foundations and constructive mathematics should be used instead. A crucial implication is that the equilibrium theorems at the core of neo-classical economics would be undermined. I argue first that their line of thought faces several challenges: their argument regarding the role of foundations of mathematics rests on a false dichotomy between formalism and intuitionism; work in philosophy of applied mathematics complicates their claim that infinitary mathematics is inappropriate for finite systems; and their argument conflates formalism in mathematical foundations with formalism in axiomatization. However, I then draw on their analyses to propose a novel frame for questions about mathematization in the economics context -- a frame that centers questions of scientific methodology, philosophy of applied mathematics, and idealization rather foundational debates about formalism versus intuitionism.

1. Philosophy of mathematics and economic methodology: Boylan, O'Gorman, and Velupillai

Recently, co-authors Boylan and O'Gorman (2007; 2018) -- and, in a similar direction, Velupillai (2005, 2012) -- have brought forward a new perspective on mathematization in economics and its relation to formalism. I focus here on the argument in Boylan and O'Gorman's (2018) book, with some references to their other writings and to Velupillai's contributions. Characterizing "formalism" as "the extensive exploitation of mathematics in economic theorising," Boylan and O'Gorman argue that the philosophy of mathematics "is a gale of creative destruction through the programme of the mathematisation of economics" (2018, 7). More precisely, their central claim is that "Brouwerian mathematics is crucially significant for the epistemic critique of the neo-Walrasian programme" because if intuitionistic foundations and constructive mathematics stands up to critical scrutiny, "the very core of the neo-Walrasian programme" -- i.e. the existence proof of general equilibrium -- "is *mathematically undermined* (2018, x; emphasis in the original).

Let me begin by unpacking some of the concepts in this last statement. "Brouwerian mathematics" in this context refers to mathematics as understood through intuitionism about foundations of mathematics. Generally speaking, classical mathematics uses standard first-order logic including the Law of Excluded Middle (LEM). In this framework, not-not-P is equivalent to P, which means "P or not-P" is a law of logic, and one can prove P by assuming not-P and deriving a contradiction; relatedly, one can prove "P or Q" without having a way to prove either P or Q individually. Functions are typically defined in terms of sets, and it's possible to prove theorems -- such as fixed-point theorems -- that assert existence without providing a method for constructing what is claimed to exist. But intuitionists such as the early twentieth-century mathematician Brouwer conceptualize mathematical objects not as fixed objects with determinate properties but rather as constructions of the human mind. It follows that the law of excluded middle, proof by contradiction, and non-constructive methods for existence proofs are not valid. Where we lack a construction for P and for not-P, we cannot claim "P-or-not-P. Since LEM is not a law, we cannot prove P by assuming not-P and deriving a contraction; instead this simply proves not-not-P (Iemhoff 2020; see also Shapiro 2000).

While there are various ways to understand intuitionism, it is generally agreed that adopting intuitionistic foundations leads to a different conceptualization of many fundamental concepts in mathematics, especially in fields like analysis. In resulting forms of "constructive mathematics," infinity is potential rather than actual, central concepts such as "real number" and "function" are reformulated, and standard real analysis is rejected. There is no equivalent to the standard real number line; instead, one works with free choice sequences instead of real numbers, and functions must be constructed according to rules. It follows that there are results diverging dramatically from those in classical mathematics -- e. g., every function is a continuous function -- and fixed-point theorems are not valid unless there is a method for constructing the fixed point.

By "neo-Walrasian programme," Boylan and O'Gorman mean general equilibrium theory (GET) as typically understood in economics, mathematized through standard real analysis and classical mathematics, and developed axiomatically. General Equilibrium Theory (GET) was first proposed by Léon Walras in the late nineteenth century, then developed by Debreu and Arrow in the twentieth. GET attempts to explain the relations of supply, demand, and prices in a whole economy (rather than just a single market), and central to the theory is the conclusion that under certain conditions, markets tend toward equilibria. These equilibria are desirable in a sense indicated by the concept of Pareto optimality: a change is a Pareto improvement when it benefits at least one person and makes no one worse off, and a situation is Pareto efficient when there are no Pareto improvements available. The relationship is encapsulated in the two fundamental theorems of welfare economics: 1) "if agents are rational, self-interested, and well informed, and if they interact only through voluntary exchange in a perfectly competitive market, then a general equilibrium exists, which is Pareto efficient" and 2) conversely, "every Pareto efficient outcome is a general equilibrium of voluntary exchanges among rational and self-interested agents, given the proper initial distribution of resources among the agents." (Hausman 2007, 24).

These two theorems are often taken to be the core of the neoclassical conception of economics. Their usual proofs rely on non-constructive conceptualizations in a number of ways, the most obvious having to do with the framing in terms of classical (not constructive) real analysis and with the use of fixed-point theorems that posit existence, especially of equilibria, in the absence of constructive methods for establishing where those fixed points or equilibria are. Thus, as the conclusions associated with the neo-Walrasian program are typically established using non-constructive mathematics, challenges to non-constructive mathematics lead to challenges for the neo-Walrasian program, and specifically for conclusions about Pareto optimal equilibria.

With respect to Boylan and O'Gorman's reference to "formalism," we will see in detail later that this term has many meanings. In the course of making their argument, Boylan and O'Gorman refer not only to "formalism" in economic methodology but also to "formalism" in philosophy of mathematics. As a view in foundations of mathematics, formalism is the idea associated with David Hilbert and others that (some or all of) mathematics can be understood as a formal system -- that is, a collection of rules for manipulating otherwise meaningless symbols. In Hilbert's version of formalism (sometimes called finitism), basic number theory is a "privileged part of mathematics" whose domain is the finitary numerals understood as signs (such as five strokes on a page for the number five), and abstractions in mathematics are to be justified according to whether they are truth-preserving in their implications for truths about this domain (Zach 2023). Hilbert hoped the introduction of symbols with rules could be shown to result in a system that was consistent, complete, and decidable, but Gödel's theorems famously show that (under certain conditions), for any axiomatization of mathematics there will always be undecidable statements -- that is, statements neither provable nor disprovable from the axioms -- and that furthermore, such an axiomatization cannot prove its own consistency. Other versions of mathematical formalism are more comprehensive, and understand all mathematical objects, including the natural numbers, merely as symbols introduced with definitions and axioms subject to certain rules of manipulation. Formalism as a view in foundations of mathematics is usually interpreted using standard first-order logic and thus is associated with classical mathematics in practice (see Shapiro 2000).

In this paper, I concentrate on Boylan and O'Gorman's "pragmatic" argument: while it is possible to be a "strict" intuitionist who regards classical mathematics as broadly illegitimate, their "pragmatic" view is that when both are mathematically legitimate, classical framings are inappropriate for the economic context and intuitionistic ones would be more suitable. In their analysis, they discuss both equilibrium and rationality; I'll focus here on the former. And while their arguments often proceed through analysis of historical figures, I will use a more explicitly philosophical lens in my interpretation and conclusions. In the remainder of this section, I first give a brief overview of the main points of their texts with quoted passages to convey to the reader how the relevant arguments unfold; I then identify and explain three specific philosophical points from these arguments that I interpret as central to the current project.

Crucial to Boylan and O'Gorman's argument is analysis of the style of formalism in economic methodology they associate with seminal figures such as the twentieth-century economist Gérard Debreu, who was famously influenced by Hilbert and the Bourbaki collective -- the post-war mathematicians in France seeking to bring a style of rigorous axiomatization to mathematics (Weintraub 2002). Boylan and O'Gorman argue that under the influence of mathematical formalists, Debreu adopted a formalist, mathematized, and axiomatized approach to economic theory, an approach that has become influential to for contemporary economics methodology. Notably, Debreu highlights the special challenges of theorizing in the economics context where we lack what he calls a "secure experimental base." A crucial difference between economics and physics is that the latter gains support through "very sophisticated experimentation" (Boylan and O'Gorman 2018, 6); in economics, controlled experiments are often difficult or impossible to carry out. Where sophisticated experimentation is impossible, Boylan and O'Gorman say, the theoretical economist working in the methodology inherited from Debreu "uses various theorems of advanced classical mathematics, and, by recourse to a range of assumptions, gives these theorems economic interpretations (2018, 199)."

It is in light of the challenge of experimental justification, Boylan and O'Gorman argue, that Debreu urges the importance of "rigorous" axiomatization in the "formalist" style. In his 1959 *Theory of Value* Debreu writes that he will be treating the subject "with the standards of rigor of the contemporary formalist school of mathematics" and that this "effort toward rigor substitutes correct reasonings and results for incorrect ones" (1959, viii; see Velupillai 2005, 859). Methodologically, Debreu says that allegiance to rigor "dictates the axiomatic form of the analysis where the theory, in the strict sense, is logically entirely disconnected from its interpretations" (1959, x). In addition to emphasizing rigor, Debreu talks of the importance of imposing a "terse" mathematical language and of an "aesthetic code" with a preference for simplicity and generality (1992, 7). In addition, for the theory to be justified, we require a "perfect fit" between the uninterpreted mathematized theory and the relevant "economic content": roughly speaking, this fit is between the formalism of the theory (disconnected from interpretations) and the abstract economic content we would find if we stripped away local contingencies such as the "legal forms of corporations" (Boylan and O'Gorman 2018, 94-97). This fit is achieved partly through adjusting assumptions when necessary.

Boylan and O'Gorman argue, and Velupillai agrees, that Debreu's methodology and influence have led us down an unfortunate formalist path, especially in its "Hilbertian" approach to axiomatization. In what the authors call a "Euclidean" axiomatization, one starts with the interpreted basic principles of a domain, then uses logic to obtain theorems. Euclidean geometry begins as a body of truths from which a selection is made of axioms that can generate and organize those truths in an illuminating way. While there are subtleties regarding the nature of the axioms and their relationship to reality, this way of proceeding takes a "semantical" approach to existence: symbols are meaningful in context and are taken to have referents outside the theory. This echoes the approach of Frege, where logic and mathematics are seen as more than just formal symbol manipulation. For example, for Frege numbers are objects, and part of properly defining them is to say "*which* objects they are" (Zalta 2025). By contrast, in a formalist -- what the authors call "Hilbertian" axiomatization -- mathematics is a purely syntactical matter -- a "meaningless calculus." When it comes to the operation of an axiomatic system in applied contexts, the formalists (they say) assume that "the applied mathematical sciences are the domain-specific interpretations of sections of the axiomatised, syntactical calculus of pure mathematics and logic" (2018, 112). For Boylan and O'Gorman, it is evident that Debreu and his many contemporary intellectual descendants are using a "Hilbertian," syntactical axiomatiztion; this style, they suggest, is "rooted in" Hilbert's formalist philosophy of mathematics (2018, 165),

While Boylan and O'Gorman's critique points to a wide range of alleged problems with the Debreuvian combination of Hilbertian formalism, syntactical axiomatization, and classical real analysis, I will focus here on three I take to be the most central. First, since the proof that ends in the conclusion about equilibrium relies on non-constructive fixed-point theorems, there is no way to construct what is said to exist, so no way to find this equilibrium. The adoption of classical mathematics and syntactical Hilbertian formalism means we have existence proofs of fixed points that cannot be found.

Second, and highlighting the significance of the non-constructible fixed point, the mathematical existence of an equilibrium is proved within infinitary mathematics, which they say is crucially different from the finite reality of economic phenomena; for this reason, the equilibrium that is claimed to exist in GET cannot be claimed to actually exist. Economic reality is discrete, but the mathematics is "continuous" in the sense that variables take on values along the standard real number line. Therefore there is, Boylan and O'Gorman say, a bad "fit" between

between the mathematics of real analysis and economic reality. This problem is hidden from view by the formalistic style including the use of syntactical, "Hilbertian" axiomatization with uninterpreted concepts. Boylan and O'Gorman describe the reaction of economists Frank Hahn and Nicholas Kaldor to Debreu's way of proceeding. Hahn, they say, argues that Debreu's existence proof establishes only the logical possibility of equilibrium and nothing more: "Debreu's monumental achievement does *not* prove that this logically consistent concept actually or even approximately applies to or describes an actual economy." (Boylan and O'Gorman 2018, 136). For Kaldor, the assumptions of neo-Walrasian theory have become so unrealistic that the theory has become "a less usable tool than it was thought to have been in its early and crude stage (Kaldor 1975; quoted in Boylan and O'Gorman 2018, 143). That is, proving the existence of an equilibrium in infinitary mathematics is not useful given that economic reality is discrete and finite.

Echoing these comments about a bad "fit," Velupillai (2012) argues that prices take on only fractional values and should not be modelled with the classical real numbers. To choose which mathematics is appropriate to our task, he says, we must notice what is being studied. Velupillai notes that the standard approach in GET frames prices as taking on "non-negative" values -- that is, values along the positive real number line (861). But this doesn't make sense, he says, given that actual prices are necessarily integers and fractions (2012, 866-867). Further, individual traders produce, sell and purchase only discrete, integer, units of whatever they are selling.

Intuitionistic foundations and constructive, computable mathematics, they all say, do not face these difficulties. For Boylan and O'Gorman, economies are social phenomena arising from human activity; for that reason, they are not "natural givens" but rather "specific, complex, constructed systems (2108, 179)." Likewise, the intuitionistic view of mathematics sees mathematical objects as abstract, open-ended, and constructed by the human mind. This means intuitionistic mathematics is "tailor-made" for what is being described (2018, 179). Velupillai says that since reality is finite and often discrete, number theory methods are "the obvious and natural place to begin" (2012, 867). One advantage of such an approach is that these techniques impose "simple, realistic, constraints on the domain of analysis -- integer units" (Velupillai 2012, 867).

The third problem identified by Boylan and O'Gorman is that the work of Gödel, Church, and Turing undermines the assumption that classical mathematics can be understood as an axiomatized system that is "consistent, complete, and decidable": there are always "trade-offs" between consistency and completeness, and classical mathematics "contains algorithmically undecidable propositions" (2018, 3). This fact recontextualizes our methodological choices. It has generally been assumed, they say, that the use of classical mathematics in economics is straightforward and unproblematic, because classical mathematics itself forms a complete system to which there are no alternatives. But this is not so. Reflection on foundations of mathematics shows us the disputed nature of the logico-mathematical objects used in economic reasoning.

Because the theory leads to non-constructible fixed points in a mathematical framework that doesn't fit with reality, the "neo-Walrasian" explanation of prices -- the explanation in contemporary proofs of GET -- is rendered "economically vacuous" (2108, 187). Because of undecidability, using classical mathematics is not deploying an epistemologically straightforward system and because of intuitionistic foundations, classical mathematics is not the only game in town. In the pragmatic frame, economists have a genuine choice between "Hilbertian formalism" and the classical mathematics associated with it, on the one hand, and Brouwerian intuitionsism and the constructive mathematics associated with it, on the other. The problems of existence and fit show we should avoid formalism and adopt intuitionism. Velupillai also laments the fact that the "modern masters of mathematical general equilibrium theory" embraced the "formalist school of mathematics" (2012, 859). All three authors say the project of reframing economic concepts with constructive and computable mathematics will close off some possibilities but open up others: applying numerical methods such as Diophantine equations will lead to theorems unlike those in GET.

Summarizing, Velupillai (2012) says "What kind of lessons are we to draw from this particular exercise in exegesis? There is almost no better way to phrase the main lesson to be drawn than in the words of a leading mathematical macroeconomist [Thomas Sargent]: '[A]s economic analysts we are directed by, if not *prisoners* of, the mathematical tools that we possess.' Should we not, if we are 'prisoners' of anything, try to liberate ourselves from that which imprisons us?'" (Sargent 1987, xix; Velupillai 863). In this perspective, then, while economists may have been initially misled by an attraction to formalism, partly through Debreu's association with the Bourbaki group, once we realize this folly, we should shift our focus.

2. Critical analysis

While my arguments ultimately support the conclusion that there are unresolved questions in the application of mathematics to contexts such as equilibrium theory, I show here that the perspective presented in the previous section faces three challenges.

First, Boylan and O'Gorman create a false dichotomy when they set up their analysis by dividing philosophy of mathematics into "two groups" (2018, 2). The first group "takes classical mathematics ... as unproblematical and sets about defending it on logico-philosophical grounds"

(2018, 2). Of this group, they write that "By the second decade of the twentieth century, Hilbert and his formalist school at Göttingen were acknowledged as the principal exponents of this heterogeneous group" (2018, 2). In the associated Hilbertian formalist program, classical mathematics is reconstructed as an axiomatic system which would ideally be shown to be consistent, complete, and decidable. In the second group, we find intuitionist philosophy of math and constructive mathematics, associated with figures such as Poincaré, Brouwer, and Dummett; as we've seen, philosophical approaches in this group support constructive, not classical, mathematics.

Their argument about intuitionism hinges on presenting problems with the approaches in the first group and showing these problems undermine the aptness of classical mathematics in the economics context. But this argument structure wrongly ignores the multiplicity of views in philosophy of mathematics that can be used to support classical mathematics. In the naturalism of Penelope Maddy, we look to mathematical practice to form ideas about how mathematical concepts should be developed. Roughly, in a naturalistic perspective, reality is understood through science itself, and not through prior philosophy, so we should look to what scientists and mathematicians do when trying to understand what is epistemologically apt; given the role mathematics plays in natural science, we have reason to take actual mathematical practices seriously. While these practices may be rationally assessed and critiqued, they stand in no need of extrinsic, philosophical, foundational support (see, e. g., Maddy 1997, 2007). From this starting point, we notice that the use of classical mathematics permeates scientific reasoning. Methods like real analysis that are based on a mathematical continuum are often successfully used to model situations in which the targets are discrete and non-continuous. Even scholars of quantum mechanics, an area once thought to be possibly ripe for alternative logics, now use mathematical structures embedded in classical logic (see e. g. Chiribella and Spekkens 2016). Beyond naturalism, there are also views such as platonism, if-then-ism, structuralism, fictionalism, and so on; all have some variation that aims to support the use of classical, rather than constructive, mathematics. For example, contemporary platonist views conceive of mathematical objects as existing abstractly and independently of our thoughts and practices (see Linnebo 2024) and fictionalism is the view that mathematical statements are not, strictly speaking, true, even though they are useful (see Balaguer 2025; for further reading on a range of views in philosophy of mathematics, see Shapiro 2000). It follows that a rejection of formalism does not go far in supporting an argument in favor of intuitionism. What is needed is a specific connection between the critique and the positive proposal; in the following section I suggest a more direct argument to a conclusion about the applicability of math that does not involve intuitionism.

Second, the view of mathematical "fit" deployed by all three authors differs strikingly from common accounts of applicability in philosophy of science and mathematics. Boylan and O'Gorman suggest that constructive mathematics is a good fit for economics because the reality is constructed and finite so the mathematics should be as well. But an aim of fit is not how applied mathematics typically functions. In a range of contexts, "continuum mathematics" --- mathematics where the variables range along the full real number line, as in standard real analysis -- is applied where reality is discrete and finite. As Maddy points out, this range of contexts includes not only statistical mechanics, but also fluid dynamics and "garden-variety statistics," where "discrete phenomena like household incomes or numbers of correct responses are routinely treated as continuous variables" for reasons of computational practicality (2008, 17). That is, when we use statistics to model populations, or probabilities involving people, the things being measured are all discrete and finite, but our methods often use concepts from calculus and other areas embedded in standard, non-constructive, infinitary classical mathematics. Use of infinitary continuum mathematics where reality is discrete and finite is common.

In philosophy of applied mathematics, the view closest to one of "fit" is the "mapping" account, in which the usefulness of mathematics in science is explained by appeal to the fact that mathematical structures "capture the important structural relations of an empirical set up" (Bueno and Colyvan 2011, 346): math is useful the same way a street map is. But here we need only a structural similarity, not the kind of fit in which details correspond to details. In their overview of the mapping account, Bueno and Colyvan (2011) emphasize that the mappings in question can take various forms and that often what is represented is more complex than the relevant representation. And notably, their "inferential conception" builds on the map metaphor partly to deal with these differences in complexity.

Furthermore, Mark Wilson points out that in applications of mathematics, we may have methods that works well even when we don't understand why: in such cases, he urges that there is no reason to reject the method. Instead, "lengthy periods of semantic agnosticism" may be recommended (1994, 528). Whether a method or mathematical structure is working well in the relevant sense is evaluated not with respect to fit but rather with respect to norms related to empirical adequacy, successful prediction, and experimental support.

With respect to the reasons our methods work well when they do, Maddy emphasizes that while an attitude of semantic agnosticism may often be appropriate, we should not give up on understanding why applications are successful. By contrast to the metaphor of fit, she points out that applied mathematicians often pay careful attention to determining when relevant "idealizations and simplifications" are "beneficial and benign" (2008, 14). For example, in fluid dynamics, using the relevant differential equations requires assuming that quantities such as temperature can be defined at a point; while this is not literally correct due to molecular structure, the idealization works, partly because there is a "plateau" between regions too small for definition and too large for uniformity. For Maddy, one aim of the mathematician is to help developed a well-stocked toolbox for applications, because we may not know in advance what kind of mathematical theory might become useful for a given purpose. Then in scientific practice, application of mathematics is understood not in terms of isomorphism and similarity, but rather in terms of when a mathematical tool can be usefully applied.

These perspectives on mathematics in application suggest that insisting on finitary methods where reality is finite would be mistaken, because this ignores the possibility of seeing a given mathematical structure as applicable due to useful and "benign" idealizations. Perhaps Boylan and O'Gorman center "fit" in their analysis because of Debreu's claims about how a formalism should fit "economic content"; in that case, a more direct argument would be needed explaining the significance of the relevant methodology and its implications for applicability. In the following section, I say more about the formalist methodology and its implications for evaluating applications and idealizations.

When mathematical applicability is framed in terms of useful idealizations, the question of intuitionistic foundations is rendered irrelevant, and the matter becomes one of which mathematical tools are apt for the context. In fact, Velupillai's (2012) reference to Arthur Lesk's (2000) views about the use of mathematics in biology supports an interpretation in which the question of application is not about abstract fit but rather about usefulness. Lesk says that mathematics is "ineffective" in molecular biology as a contrast to Wigner's claim of the "effectiveness" of mathematics in science -- a point Vellupillai interprets as supporting his conclusions about the superiority of computable methods in economics. But notably, Lesk does not discuss foundational issues or fit. He discusses the limited usefulness in molecular biology of the kind of concepts used in physics. But for this frame of "usefulness" the question is not one of matching mathematics and reality. And notably, examples from biology have been used to showcase successful mathematical explanations (Baker 2005) and the effectiveness of mathematics in the special sciences (Colyvan 2014). Overall, it may well be that Diophantine equations and computational mathematics have usefulness in economics that surpasses that of analysis, but if so, the argument for that would be based on standards of success for science such as fruitfulness not on considerations of fit.

Third, Boylan and O'Gorman's argument elides distinctions among topics in 1) foundations of math, 2) axiomatization in mathematical practice and 3) axiomatization of a science. The first concerns what mathematical objects are. The second concerns mathematical practice. The third concerns a project in an empirical domain.

With respect to 1) foundations versus 2) mathematical axiomatization, in the context of discussing Hilbert's formalism and its effect on economics, Weintraub (2002) usefully distinguishes between two different projects both of which are sometimes called Hilbertian "formalism." The first is a project in foundations of mathematics, in which mathematics is reconstructed as a formal system that would hopefully preserve arithmetical finite truths and be complete and consistent. Weintraub aptly calls this Hilbert's Finitistic Program for the Foundations of Mathematics (FPFA). The second is a way of developing and interpreting mathematical theories and thus a project in mathematics. In an overview of Hilbert's approach in the mathematics context, Michael Hallett (2024) says that here a "mature presentation" of a theory requires axiomatization, which in turn has both programmatic elements and proof elements. Among the programmatic elements, we find the idea that the axioms tell us everything there is to know: "although a domain is asserted to 'exist,' all that is known about the objects in the domain is what is given to us by the axioms and what can be derived from these through 'finite proof'" (Hallett, 2024) Among the "proof elements," we find the importance of determining which assumptions are strictly necessary for which conclusions (see also Mitsch, 2022). Contrasted to FPFA, Weintraub calls this second part of Hilbert's project the "axiomatic approach" (AA).

In the axiomatic approach to mathematics (AA), to axiomatize a theory is a way of making it rigorous: to show exactly what the assumptions and definitions are, to make the logic of it clear, and to determine which assumptions are strictly needed to prove which conclusions. Drawing on historical research, Weintraub argues that AA indicated a shift from the late nineteenth century, when mathematical "rigor" was understood in terms of mathematics having a closer connection to physical reality to the twentieth, where rigor became associated with abstraction and axiomatization. The shift is reflected not only in Hilbert's desiderata already mentioned, but also in the approach characteristic of Bourbaki. Jean-Pierre Marquis argues that for Bourbaki, the proper methodology involves identifying "the appropriate abstract structures involved in a given context," then considering what it is about those structures that are relevant to the given problem," then solving the problems "by using all and only the abstract properties needed" (2012, 227). Furthermore, he notes, the aesthetic of the presentation style of the work is "extremely dry, severe, austere, unified and terse" (2012, 211). This suggests a view in which not only is axiomatization a useful project reflecting the maturity of a mathematical theory but also the aesthetics of axiomatization infuses mathematical practice more generally. Weintraub laments that both FPFA and AA are sometimes called "Hilbert's formalist program," not only because are they different, but also because AA, but not FPFA, survived what historian of mathematics Morris Kline calls the "loss of certainty" associated with Gödel's incompleteness theorems (Weintraub 2002, Chapter 3).

As we've seen, a central complaint of Boylan and O'Gorman is about "syntactic" axiomatization: using uninterpreted abstract formalizations, which are then approached through using classical mathematics to prove theorems. Debreu's approach, they say, is thus "rooted in" Hilbert's formalist philosophy of mathematics. But this complaint concerns AA not FPFA because it is about axiomatization and does not have an obvious connection to formalism in foundations of mathematics. Its challenges are therefore no support for intuitionism. Fitting with this, the methodology of Bourbaki, known to have influenced Debreu, does not engage the ultimate nature of mathematical objects, nor Hilbert's views about that nature nor his finitism. Notably, Frege himself -- whose "semantic" views of axiomatization they endorse -- endorsed standard first order logic and classical mathematics. In an earlier paper, Boylan and O'Gorman say that "formalism, finitism and axiomatics are inextricably linked in the Hilbert programme" (2007, 430). They may be linked in some sense but given that the axiomatic approach can be understood and used without finitism, and without a link to formalism in foundations of mathematics, they are not inextricably linked philosophically. And philosophical links would be necessary to show mathematical foundations and intuitionism are relevant to the practice of economic methodology.

Not only is FPFA (1) distinct from AA (2), but furthermore axiomatization in the mathematical context (2) is different from axiomatization in the context of science (3) because the latter, unlike the former, involves the relationship of a theory to a set of non-mathematical facts. In properly axiomatizing a mathematical theory, Hilbert insists that when a domain of objects is identified, nothing is known about those objects except what follows from the axioms (Hallett 2024). By contrast, the characterization of axiomatization of quantum mechanics attributed to Hilbert (and his colleagues Nordheim and von Neumann) is that it includes 1) physical axioms 2) analytic machinery and 3) physical interpretation; notably, the basis for the "physical axioms" is "empirical" (Oddan 2024; see Mitsch 2022).

While there may be similarities between axiomatizing a mathematical theory and axiomatizing an empirical theory, there are important differences. There is no obvious analogue in the mathematics context to being constrained by the facts of the "physical theory." Further, the important aspect of "economic interpretability" in Boylan and O'Gorman's own analysis has no analogue in the axiomatization of a mathematical theory. In fact, Boylan and O'Gorman's own critique hinges partly on observations about the infelicities of mistaking economics for being simply mathematics -- this supports my claim that mathematical theory and scientific theory are distinct contexts for axiomatization.

With respect to the relationship between the two, in a discussion of Bourbaki's influence on economic method, Weintraub says that "It seems clear that Debreu intended his *Theory of Value* to serve as the direct analogue of Bourbaki's *Theory of Sets,* right down to the title." The format of both books "mirror each other," and "many aspects of the Bourbakist program find direct correspondences in the details of Debreu's version of mathematical economics" (2002, Chapter 4). Boylan and O'Gorman quote Debreu's approval of "the terse language" that mathematics imposes on economics (2018, 80); this echoes the aesthetic of the Bourbaki group. These expressions all point to the relationship between axiomatizing mathematics and axiomatizing economics being best understood as an analogy or similarity, not identity. This point complicates Boylan and O'Gorman's use of Frege to illustrate "Euclidean" axiomatization. It is correct to say that in philosophy of mathematics, Frege emphasized that terms must be interpreted and not merely formal and that existence cannot be inferred from consistency while Hilbert proceeded syntactically. But this concerns the axiomatization of mathematics, and is therefore of limited relevance to the challenges of axiomatizing a science.

If the idea is that Debreuvian axiomatization wrongly adopts an axiomatization methodology from mathematics and fails to pay sufficient attention to economic facts and interpretability, so that it is the target of their critique who is conflating axiomatization of mathematics with that of empirical science, then this criticism would need a different argument from the one they are providing, and would not seem connected to intuitionism. This framing would connect with the existing literature on whether economic methodology lacks sufficient attention to empirical matters. But then it would be unclear what the argument has to do specifically with mathematics. Even very simple models like the Sakoda-Schelling checkerboard models of segregation are seen as raising questions of justification in the absence of direct empirical support: if a theory analyzes structural relationships in the abstract, distinct from any interpretation, how can we use it to make inferences about the world (Sugden 2000)? But the mathematics of the checkerboard model is simple and not the crux of the matter. Critiques of axiomatization that focus only on the lack of empirical support thus do not engage questions specifically about mathematics.

Boylan and O'Gorman's criticism is thus best understood as one about how economics should be axiomatized as a science, which should be distinguished from both FPFA and from the matter of how mathematics should be axiomatized (AA). Their framing the issue in terms of foundations of mathematics is a red herring: no simple morals about the axiomatization of economics follow from a rejection of mathematical formalism. What is needed is a way to frame a critique of formalist axiomatization that bears specifically to our choices of what mathematics to use in a given context. Is there a way to frame a critique of formalistic axiomatization that carries implications specifically for mathematical applicability? I point toward such an argument below.

In this section, I have argued that intuitionism and foundations of mathematics are not central to the conclusions of Boylan and O'Gorman's critique: conclusions about intuitionism rest on a false dichotomy and unsupported claims about "fit" in applied math; the complaints about formalism are best understood as focused on axiomatization of economics not mathematics. In a striking passage, Weintraub writes: "Mixing the connection between mathematics and economics with the idea of formalism in economics is explosive for those who try to reconstruct the history of economics in the twentieth century. It is easier to reject 'Formalism' than it is to come to terms with the Axiomatic Approach. I suspect this is the reason why the related topic of the proper role of mathematics in the social sciences is so very controversial. What is at stake is, to put it starkly, the concept of scientific truth -- economic truth -- itself" (2002, 99). So let us now grapple with the axiomatic approach.

3. A novel frame for questions about mathematization

Is there a way Boylan and O'Gorman's critique of methodology can be used to frame a more precise and relevant question regarding mathematization in economics? Here I draw the relevant connections, which have to do not with foundations of mathematics but rather with perspectives from philosophy of applied mathematics.

First, recall the relevant characterization of the formalist economic methodology: where we lack a "secure experimental base," the theoretical economist working in the style of Debreu "uses various theorems of advanced classical mathematics, and, by recourse to a range of assumptions, gives these theorems economic interpretations" (Boylan and O'Gorman 2018, 199). The axiomatization is "syntactic" in the sense that it is carried out separately from any interpretation. As Weintraub explains, citing Ingrao and Isreal (1990), the method is to create a "self-sufficient formal structure." In this approach, the conclusion may not have empirical support through experiment, and the theory is not intended to showcase how actual equilibria arise in the actual world. Instead, as Boylan and O'Gorman say, "the possibility of general equilibrium is rigorously established (2018, 199)."

This is a distinctive method: a formalism is created with the goal of concluding that a certain state of affairs is a possible one. To get a better sense of how we might evaluate the application of a given set of mathematical concepts in this kind of methodological context, it is useful to consider recent work on formal theoretical models in economics (e. g., Ylikoski and Aydinonat 2014; Verreault-Julien 2017). This work foregrounds both the lack of direct empirical support and the fact that the conclusions concern possibility in the use of some models.

There is no consensus regarding how models should be evaluated in the absence of direct empirical support, but one common thread in various proposals is that they are useful for providing how-possibly explanations (HPEs) and for helping to answer questions about counterfactual circumstances. In a series of papers, the economist Robert Sugden proposes that these models should be judged for credibility: they describe a way the world "could be" (2000, 2013). Credibility is justified by relevant similarities between the real world and the model world. Picking up on the modality of "could be," some argue that modality is central so that such models provide "how possibly" explanations (HPEs) rather than "how actually explanations (HAEs) (Grüne-Yanoff and Verreault-Julien 2021). In this perspective, we may not learn whether there is actually a general equilibrium in reality, but we learn when and how an equilibrium would be possible and when and how it would not.

With respect to the Arrow-Debreu model, Verreault-Julien (2017) points out that equilibrium models in economics "pose an interesting challenge" to standard accounts of modelling in philosophy of science because their "main contribution is a purely mathematical result, viz. two theorems of existence" (298). The Arrow-Debreu model is "paradigmatic" in being "highly mathematical" and having only an "indirect" connection to "real-world phenomena" (300). The associated "axiomatic method" used by Arrow and Debreu consists in "building a mathematical system from basic propositions or postulates, namely the axioms" (300). The axioms are then "used to deductively derive the theorems ..." From these passages, we see that though he is talking about a "model" rather than a "theory," Verreault-Julien's interpretation of the relevant axiomatic method has the same basic components as that described by Boylan and O'Gorman.

Verreault-Juilien says the Arrow-Debreu model should be regarded partly as a consistency proof -- it shows that because there is a coherent mathematization with a possible equilibrium, the assumptions of the model are consistent -- something non-obvious before the proof was introduced. The idea of Smith's "invisible hand" having been put forward it is important to know "not only whether it *is* true but also whether it *could be* true (Verreault-Juilien 2017, 306)." The Arrow-Debreu model "ruled out that it is mathematically impossible for the equilibrium to exist" (309).

Relatedly, the Arrow-Debreu model is said to be useful for determining the counterfactual conditions under which the relevant equilibria would or would not arise, and thus for determining what would need to change to make our reality closer to the model. Boldyrev and Ushakov (2016) argue that the model offers "constructive mechanisms" -- elements regarded as making the model true and thus informing us how to "transform economic reality" so that it would become "closer to the model." That is, we can use the model to gauge what we would need to change to move closer to satisfying the model's assumptions. In a recent "non-technical" presentation of macroeconomics, Kartik Athreya (2013) says that the Arrow-Debreu model provides us with "a clear benchmark against which to measure the dysfunction of the real world" (see also Verreault-Julien 2017, 310). Verreault-Julien points out that although the model has "how-possibly" explanations (HPEs) rather than how-actually ones, the fact that it can help answer what-if questions shows that it can be used to gain understanding about the world.

Relevantly for our purposes, Verreault-Julien argues that the Arrow-Debreu model leads to "mathematical explanations" -- that is, explanations based on mathematical, not causal, dependencies. To see what a mathematical explanation can be, consider that the fact that 23 is not divisible by 3 explains why a person with 23 strawberries cannot distribute them evenly to three people without cutting any (Verreault-Julien 2017, citing Lange 2013). The central fact of divisibility is a mathematical one. While the possibility and nature of distinctively "mathematical" explanations is contested (see Baker 2005 and Bangu 2024), what is relevant here is that in harmony with Boylan and O'Gorman's analysis, Verreault-Julien says that the seemingly mathematical explanations in the Arrow-Debreu model are how-possibly explanations (HPEs), not how-actually ones (HAEs). In contrast to the strawberries example, the explanation in the Arrow-Debreu model fails to provide "an actual mathematical explanation" because 1) the mathematical conditions the model identifies are only sufficient and not necessary and 2) the model "does not identify actual mathematical structures or properties that map onto the world." "Mathematical facts," he says, "can only provide an actual explanation of phenomena if there is a mapping or correspondence between the mathematical facts and the physical ones" -- and in this case, "it is rather dubious whether there is such a phenomenon as the general equilibrium in the first place and to what extent the conditions for a competitive economy are satisfied" (2017, 306). Thus the model provides only a mathematical HPE not a mathematical HAE.

From this point of view, the fact that the equilibrium is only possible and not actual does not render the explanations "vacuous": the HPEs contribute to our understanding despite not being actual. And the fact that the equilibrium is derived in a formalism that does not provide a construction for that equilibrium is not grounds for criticism because the model's usefulness lies in other directions.

However, we then face the following question: if we adopt this view of equilibrium models as providing how-possibly explanations and providing insight into counter-factual circumstances, how can we evaluate the application of a certain set of mathematical tools and approaches? As we've seen in the discussion of "fit" above, the suitability of applications is typically based on intra-scientific factors such as empirical adequacy and successful prediction, but if we interpret the model as yielding how-possibly (not how-actually) explanations, then this strategy is unavailable. Note that if a model were embedded in a background theory, we could apply these criteria to that theory, but in the interpretation we've seen here, equilibrium models are what Reutlinger et al. (2018) call "autonomous" models -- they are not embedded in a well-confirmed framework theory, and thus cannot derive justificatory support from the framework. So a new perspective is needed.

Views in applied mathematics such as the mapping account are of limited usefulness, as they are typically focused not on the question of how to evaluate a given application, but rather on questions such as what we learn from established, successful applications. However, I propose that we may draw on Maddy's account described above as a way of framing the central questions. Specifically, recall Maddy's claim that applied mathematicians "take great care" to determine when their "various idealizations and simplifications ... can be counted both beneficial and benign" (2008, 14). Adapting this view, we may say that the relevant question for an instance of mathematical applicability in our context would be to consider and evaluate the idealizations that make possible the application of the given piece of mathematics. Such a framing harmonizes with Davide Rizza's observation that "the selection of salient formal properties of an empirical setup" may be a crucial initial step in applying a given mathematical structure (2013, 402).

In the context of axiomatization and equilibrium models, the crucial question then becomes: in the absence of a "secure experimental base," how can we evaluate the idealizations relevant to the application of a particular bit of mathematics? There is no answer analogous to that in the fluid dynamics example above, because the context of models with how-possibly explanations is one in which we cannot appeal to empirical adequacy, predictive success, and the results of experiments. Of course, some parts of economics do have experimental and empirical justification through "natural experiments," the use of data sets, and so on; in those cases, we may find a strategy like that used for physical theories to be useful. But for formal theoretical reasoning, as general equilibrium theory is often interpreted, it is not obvious how we should determine whether the idealizations needed to apply the mathematics are suitable ones in the given context. The philosophical question of mathematical application in the economics context is therefore whether relevant idealizations are apt in the context in which they are deployed, and we lack a way of answering this question. This framing leads to a conclusion that is less strong than Boylan and O'Gorman's, but it supports the overall claim that there are unresolved questions about how various applications of mathematics in economics can be shown to be appropriate in context.

An advantage of this framing is that it links questions of mathematization to longstanding debates over the general aptness of idealizations in concepts such as rationality and utility in a fruitful and interesting way. When we ask whether the model of the person as "homo economicus" is a dismal misrepresentation or a helpful idealization, we encounter a rich literature exploring the implications of the given idealization in various contexts and how it does or doesn't fit with experimental information, and we find a well-developed alternative theory of behavioral economics. When we consider the idealizations of equilibrium theory -- such as voluntary exchange and perfect information -- it is possible to consider whether they are more suitable for some contexts than others. Insofar as an idealization is necessary for application of a given mathematical tool, we may draw on these multifaceted ways of thinking to consider whether those idealizations are beneficial and benign. In some cases, mathematical tractability is introduced as an explicit reason for introducing a given idealization; in the proposal I am suggesting here, such cases are especially ripe for examining the relevant idealizations.

This perspective on mathematization shifts our attention from the nature of the mathematics being applied to the methods we have for evaluating theoretical models and whether the idealizations they contain are apt for a given modelling context. But it still focuses our attention on mathematics rather than axiomatization generally, thus addressing the difficulties raised in the previous section. My proposal is therefore that a proper formulation of what it would mean for an economic theory to be "too mathematized" or "wrongly mathematized" is that it would mean that the idealizations needed to apply the given mathematical tools are not beneficial and benign in a given context. This framing supports the significance of the question of mathematization: we need reason to believe the idealizations work well. It also supports the coherence of asking whether, e. g., mathematical tools based on computable foundations rather than real analysis are more suitable for economics. But it entails a different way of answering from the one proposed by Boylan and O'Gorman, offering instead a way of answering focused on idealizations, rather than mathematics and its foundations.

Conclusion

I've argued that recent attempts to locate problems of mathematization in economics in "formalism" misstep when they center issues in foundations of mathematics and fail to connect critiques of axiomatization to challenges about mathematization. However, I have argued for a new framing for questions about mathematization in economics: where the methodology deploys theoretical models and uses mathematical tools requiring idealizations, the question is how we can determine which idealizations are beneficial and benign in the given context. In some contexts, such as real analysis used in General Equilibrium Theory, there is no obvious answer, so there are unresolved questions about the mathematization of economics.

With respect to the broader context of debates over economic methodology, this framing could help explain why debates over formalization and mathematization in economics often intersect with normative and socio-political topics. Whether an idealization is apt for a given context can depend on what the modellers' purposes are and what the research problem is; and as the phrase "dysfunction of the real world" suggests, evaluating idealizations in the economics context often involves normative as well as descriptive evaluation. We may disagree on what our modelling purposes are, or should be, and we may disagree over whether an idealization is innocuous or troubling in a given context. Topics for future research thus include not only how the idealizations enabling the application of various mathematical tools should be evaluated in the context of formal and modal models, but also what the stakes are of adopting one set of idealizations over another.

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References

Athreya, Kartik B. 2013. *Big Ideas in Macroeconomics: A Nontechnical View*. MIT Press.

Baker, Alan. 2005. "Are there Genuine Mathematical Explanations of Physical Phenomena?" *Mind* 114: 223-238.

Balaguer, Mark, "Fictionalism in the Philosophy of Mathematics." 2025. *The Stanford Encyclopedia of Philosophy* (Spring 2025 Edition), Edward N. Zalta & Uri Nodelman (eds.), URL = <https://plato.stanford.edu/archives/spr2025/entries/fictionalism-mathematics/>.

Bangu, Sorin. 2024. "Mind the Gap: Noncausal Explanations of Dual Properties." *Philosophical Studies*: 1-21.

Blaug, Mark. 2002. "Ugly Currents in Modern Economics." In Uskali Mäki, ed., *Fact and Fiction in Economics: Models, Realism and Social Construction*. Cambridge University Press. 35-56.

Boldyrev, Ivan, and Alexey Ushakov. 2016. "Adjusting the Model to Adjust the World: Constructive Mechanisms in Postwar General Equilibrium Theory." *Journal of Economic Methodology* 23: 38-56.

Boylan, Thomas A., and Paschal F. O'Gorman. 2007. "Axiomatization and Formalism in Economics." *Journal of Economic Surveys* 21: 426-446.

Boylan Thomas and Paschal O'Gorman. 2018. *Philosophy of Mathematics and Economics: Image, Context, and Perspective*. Routledge Press.

Bueno, Otávio, and Mark Colyvan. 2011. "An Inferential Conception of the Application of Mathematics." *Noûs* 45: 345-374.

Chiribella, Giulio, and Robert W. Spekkens, eds. 2016. *Quantum Theory: Informational Foundations and Foils*. Dordrecht, The Netherlands: Springer.

Colyvan, Mark. 2014. "The Undeniable Effectiveness of Mathematics in the Special Sciences." In *New Directions in the Philosophy of Science*, eds., M. C. Galavotti, et al. Springer, 63-73.

Debreu, Gérard. 1959. *Theory of Value—An Axiomatic Analysis of Economic Equilibrium*, London, John Wiley.

Debreu, Gerard. "Random Walk and Life Philosophy." 1991. *The American Economist* 35: 3-7.

Eichner, Alfred S. 1983. "Why Economics Is Not Yet a Science." *Journal of Economic Issues* 17: 507-520.

Grüne-Yanoff, Till, and Philippe Verreault-Julien. 2021. "How-Possibly Explanations in Economics: Anything Goes?" *Journal of Economic Methodology* 28: 114-123.

Hallett, Michael, 2024. "Zermelo's Axiomatization of Set Theory." *The Stanford Encyclopedia of Philosophy* (Spring 2024 Edition), Edward N. Zalta & Uri Nodelman (eds.), forthcoming URL = <https://plato.stanford.edu/archives/spr2024/entries/zermelo-set-theory/>.

Hausman, Daniel M. 2007. *The Philosophy of Economics: An Anthology*. Cambridge University Press.

Ingrao, Bruna, and Giorgio Israel. 1990. *The Invisible Hand: Economic Theory*. MIT Press.

Iemhoff, Rosalie, 2020. "Intuitionism in the Philosophy of Mathematics." *The Stanford Encyclopedia of Philosophy* (Fall 2020 Edition), Edward N. Zalta (ed.), URL = <https://plato.stanford.edu/archives/fall2020/entries/intuitionism/>.

Kaldor, Nicholas. 1975. "What is Wrong with Economic Theory." *The Quarterly Journal of Economics* 89: 347-357.

Lange, Marc. 2013. "What Makes a Scientific Explanation Distinctively Mathematical?" *The British Journal for the Philosophy of Science* 64: 485-511.

Lesk, Arthur M. 2000. "The Unreasonable Effectiveness of Mathematics in Molecular Biology." *The Mathematical Intelligencer* 22: 28-37.

Linnebo, Øystein, "Platonism in the Philosophy of Mathematics." 2024. *The Stanford Encyclopedia of Philosophy* (Summer 2024 Edition), Edward N. Zalta & Uri Nodelman (eds.), URL = <https://plato.stanford.edu/archives/sum2024/entries/platonism-mathematics/>.

Maddy Penelope. 1997. *Naturalism in Mathematics* (Oxford: Oxford University Press).

Maddy, Penelope. 2007. "Three Forms of Naturalism." In *The Oxford Handbook of Philosophy of Mathematics and Logic* (Oxford: Oxford University Press).

Maddy, Penelope. 2008. "How Applied Mathematics Became Pure." *The Review of Symbolic Logic* 1: 16-41.

Marquis, Jean-Pierre. 2012. "The Structuralist Mathematical Style: Bourbaki as a Case Study." In *Objects, Structures, and Logics: FilMat Studies in the Philosophy of Mathematics*, pp. 199-231. Cham: Springer International Publishing.

Mitsch, Chris. 2022. "Hilbert-style Axiomatic Completion: On von Neumann and Hidden Variables in Quantum Mechanics." *Studies in History and Philosophy of Science* 95 (2022): 84-95.

Oddan, Jessica. 2024. "Reconstructions of Quantum Theory: Methodology and the Role of Axiomatization." *European Journal for Philosophy of Science* 14: 20.

Pratten, Stephen. 2004a. "Mathematical Formalism in Economics: Consequences and Alternatives." *Economic Affairs* 24: 37-42.

Pratten, Stephen. 2000b. "Reclaiming History: a Reply to Weintraub." *Economic Affairs* 24: 50-52.

Reutlinger, Alexander, Dominik Hangleiter, and Stephan Hartmann. 2018. "Understanding (with) Toy Models." *The British Journal for the Philosophy of Science* 69: 1069–1099.

Rizza, Davide. 2013. "The Applicability of Mathematics: Beyond Mapping Accounts." *Philosophy of Science* 80: 398-412.

Sargent, Thomas J. 1987. *Macroeconomic Theory*, 2nd edn, London, Academic Press.

Shapiro, Stewart. 2000. *Thinking about Mathematics: The philosophy of Mathematics*. Oxford University Press.

Sugden, Robert. 2000. "Credible Worlds: the Status of Theoretical Models in Economics." *Journal of Economic Methodology* 7: 1-31.

Sugden, Robert. 2013. "How Fictional Accounts Can Explain." *Journal of Economic Methodology* 20: 237-243.

Verreault-Julien, Philippe. 2017. "Non-Causal Understanding with Economic Models: the Case of General Equilibrium." *Journal of Economic Methodology* 24: 297-317.

Weintraub, E. Roy. 2002. *How Economics Became a Mathematical Science.* Duke University Press.

Weintraub, E. Roy. 2004. "Making Up History: A Comment on Pratten." *Economic Affairs* 24: 46-49.

Wilson, Mark. 1994. "Can We Trust Logical Form?" *The Journal of Philosophy* 91: 519-544.

Wilson, Mark. 2000. "The Unreasonable Uncooperativeness of Mathematics in the Natural Sciences." *The Monist* 83: 296-314.

Velupillai, K. Vela. 2005. "The Unreasonable Ineffectiveness of Mathematics in Economics." *Cambridge Journal of Economics,* 29: 849-872.

Velupillai, K. Vela. 2012. "Bourbaki's Destructive Influence on the Mathematisation of Economics." *Economic and Political Weekly* 47: 63-68.

Verreault-Julien, Philippe. 2017. "Non-Causal Understanding with Economic Models: the Case of General Equilibrium." *Journal of Economic Methodology* 24: 297-317.

Ylikoski, Petri. and N. Emrah Aydinonat. 2014. Understanding with Theoretical Models. *Journal of Economic Methodology*, 21: 19-36.

Zach, Richard. 2023. "Hilbert’s Program", *The Stanford Encyclopedia of Philosophy* (Winter 2023 Edition), Edward N. Zalta & Uri Nodelman (eds.), URL = <https://plato.stanford.edu/archives/win2023/entries/hilbert-program/>.

Zalta, Edward N., "Gottlob Frege." 2025. *The Stanford Encyclopedia of Philosophy* (Summer 2025 Edition), Edward N. Zalta & Uri Nodelman (eds.), URL = <https://plato.stanford.edu/archives/sum2025/entries/frege/>.