**The Changing Look of the Market Model in Mainstream Economics:
Gérard Debreu and the Influence of the Hilbert Programme**

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Background

I have always been interested in the way in which economists have placed the basic intuition of the market model at the heart of their explanation of all sorts of social phenomena. This goes back to my time as a student on an economics degree programme back in the early 1990s, and in hopefully rather more sophisticated form it currently acts as the core animating theme of my ongoing ESRC Professorial Fellowship project. I suspect, over time, my interest in this issue has shifted from 'what's wrong with economics?' to 'what's different about economics?' and even to 'what's different about economics today compared with itself in earlier times?' This does not mean that I have stopped worrying about the first of these three questions, only that it might be possible to suggest more subtle answers to it by approaching it through the perspective of the latter two.

For instance, the answer 'because its mainstream proponents wish it to be treated as a mathematical science' can plausibly be given to all three of these questions, but it leads to very different implications depending on which of the three questions has actually been asked. If this answer is offered as a diagnosis of the wrong turns that economists have taken in the past, then it does little to foster further understanding of what makes economics a seeming law unto itself within the social sciences. Moreover, given the increasing level of concern being expressed across the social sciences about the encroachment of both the methods and models of economics across disciplinary borders – the so-called economics imperialism phenomenon – the need for that understanding has arguably never been greater. That is why in my increasing dotage I now prefer to come at the issue from another angle. Simply dismissing economics because its mainstream proponents think mathematically and express themselves mathematically is no longer good enough (if, indeed, it ever was).

At the very least, it is necessary to note that economic theory has always been mathematical since the first faltering steps towards marginalism in the 1850s were fleshed out as a thoroughgoing theoretical system twenty years later. In the second edition of his *Theory of Political Economy* published in 1879, for instance, William Stanley Jevons (2013 [1871/1911]: lxiii) wrote that "the mathematical treatment of Economics is coeval with the science itself". But what he meant by this was that economic theory dealt with categories that could be quantified, and if they could be quantified they should be to make sure that the theoretical claims were constrained by the presence of corroborating observational data (Jevons 2013 [1871/1911]: 3). When Jevons wrote about 'mathematical treatment', then, what he had in mind was numbers. One of the things that often puts non-economists off economics today is that when they begin to read it they feel somewhat besieged by the prevalence of numbers. However, we should not jump to too swift a conclusion that this means that nothing has changed in the intervening nigh-on 150 years.

Numbers continue to adorn the analysis of econometric pieces of work, but the models that propel economic *theory* have a very different look to them. They also make a very different set of demands on the mathematical literacy of the reader. It is algebraic relationships expressed often within a set theoretical framework that dominate the content of economic theory and, indeed, have done so since the so-called formalist revolution that was in full swing by the 1950s. Econometrics and economic theory now conceive of mathematical treatment in very different ways. When numbers appear in economic theory today they typically do so as little more than window dressing, with the real action taking place in the algebraic working through of a number of abstract behavioural axioms using well-defined logical rules. It would be very odd to try to claim that Jevons was anything other than an innovative mathematical economist when judged by the standards of his time, but his whole idea of what was mathematically important for economic theory has been entirely transcended between then and now. As a consequence, the same must be said about the way in which the market model is expressed mathematically in economic theory, even if a number of its basic building blocks remain fundamentally intact. We are still familiar today with the marginalists' language of demand and supply and with the faith that they vested in the concept of equilibrium, but in mathematical terms these underlying components of the market model now have a very different meaning.

In the words of the historian of mathematics Leo Corry (1989: 418), the important thing to have changed in this regard is the underlying 'image' of mathematics. This is the collectively negotiated agreement amongst practising mathematicians about what it means to be doing mathematics at the cutting edge of the profession. It is not about the content of individual propositions as they are worked through in logical form so much as what it means for the mathematical endeavour to be thinking in terms of that type of proposition in the first place (Corry 1992: 317). As the outer parameters of 'good' mathematics changes because of the rewriting of the prevailing image of mathematics, so too must the definition of 'good' mathematical economics (Weintraub 2002: 2). In particular in this regard, the definition of what counts as a rigorous argument has changed in a way that has had a profound effect on economists' underlying market model. I have argued for some time now that one of the most distinctive things about this model when read politically is the language spill-over effects that have taken purely abstract arguments from the pages of economic theory and reworked them as political arguments to be used within the public sphere. It is by no means a one-to-one correspondence, and we should never expect it to be so. Nonetheless, an intimate relationship exists between the arguments that are rehearsed in political debate about 'the market' and the means through which an abstract market essence is distilled in economic theory. It therefore matters greatly in political terms that the underlying definition of mathematical rigour through which this essence is derived theoretically has been entirely divorced from the type of empirical corroboration that Jevons had in mind when declaring that economics was necessarily a mathematical subject field.

In brief, we have seen a shift in the metamathematical concept of rigour from that associated with Augustin-Louis Cauchy, the French mathematician who was most active in the mid nineteenth century, to that associated with David Hilbert, the German mathematician who was most active in the early part of the twentieth century. Cauchy thought that mathematical rigour arose when models were successfully tethered to things that were known to be true about the physical world because of the existence of observational data to that effect. Jevons learnt his mathematics at UCL from Augustus De Morgan, and De Morgan was fully invested in Cauchy's metamathematical claims. This meant linking mathematics to the physical world and envisioning successful mathematical models only on the basis of what could be said with any degree of certainty about that world. The mathematical economics that Jevons practised was cast in this way. As the various writings of Philip Mirowski (1989: 12) have shown, economic theory throughout the period of marginalism flirted openly with physical analogy, taking mathematical models that had been pioneered in the physical sciences and using them largely unchanged for their own purposes. At all times there remained some sort of deferential nod towards anchoring the market model in observational data. It should be said that this pursuit was nothing other than eventful and equally nothing that really resembled successful, but still it remained the touchstone of how to do mathematical economics at that time.

Hilbert's conception of what made a mathematical argument rigorous was very different. He removed altogether the need for external validation from the physical world if it was to be possible to say that a mathematical model had worked as intended. His revolutionary metamathematical premise was that mathematics itself could act as the judge of mathematics. In the time since the heyday of Hilbertian metamathematics in the early 1930s, mathematical economics also seems to have displayed a similar shift from physical analogy to mathematical analogy. This means that its mathematical models are now appraised for their truth values in their own terms and not in terms of the presence of corroborating data. Hilbert started a trend towards formal axiomatisation that has largely done away with Jevons's dream that economics could become a science grounded in rich empirical detail. Instead, the new route to rigour involved the construction of theoretical models that were immune to challenge from within their own structure. Was the system of behavioural axioms on which the model relied complete and internally consistent? Had it been transposed into a series of theorems that were worked through according to the prevailing standards of logic? If the answer to all of these questions was 'yes', then the underlying mathematical model passed all of the tests of rigour. But what does this shift do to the way in which political claims can be made on behalf of 'the market'? After all, if mathematical economics was now to proceed on the basis of the dominant Hilbertian metamathematics then there was no need at all for the underlying market model to any longer be tethered to conditions that were known to exist in actual markets. Yet the language derived from the underlying market model itself still has profound effects when used politically.

The Problem, Simply Explained

The argument is perhaps most straightforwardly explained by way of an example. Nothing could be more suggestive of a Hilbert-inspired shift within economic theory than comparing Jevons's definition of 'the market' with that of Gérard Debreu. Debreu is without doubt the most important of all of the formal axiomatisers whose work has left an impression on the way in which economists conceive their market model. He was a follower of the French mathematics collective, Nicolas Bourbaki, whose metamathematical approach is so thoroughly Hilbertian that they have often been accused of proceeding as if subsequent qualifications to the Hilbert programme had never seen the light of day. He has also been described by Paul Samuelson, himself a key figure in bringing formalist methods to bear upon economic theory, as the "mathematical economist's mathematical economist" (Samuelson 1983: 838). Debreu's work has proved too difficult for the vast majority of economists to be able to replicate. Indeed, when he and Kenneth Arrow published the first existence proof of a general equilibrium system displaying perfect market-clearing properties across an infinite number of markets instantiated in an infinite number of time periods (Arrow and Debreu 1954: 265), the level of sophistication demanded of the reader was such that the editors of *Econometrica* could not find suitably mathematically trained economists to act as referees (Weintraub and Gayer 2001: 427). The piece was accepted on trust given the mathematical reputation of the authors rather than on the basis of the normal standards of peer review. Throughout his career similar standards were applied to Debreu's work as he liberated economists' basic market model from the need to have any association with actual market outcomes. Jevons and Debreu were doing resolutely the same thing in asking how markets work and appealing to an underlying mathematical instinct to provide the answer. However, what they meant by a 'market', what they meant by 'mathematics' and what they considered to be a suitable test of rigorous mathematical models of the market could not have been any more different.

To take Jevons first. His *Theory of Political Economy* still seems to grate with non-economists who are looking for an answer to the question 'what's wrong with economics?' There are good reasons for this, as throughout the book they can find evidence of Jevons's hedonistic calculus that paved the way for the subsequent fleshing out of the modern homo economicus character in the manner of a socially abstracted utility maximiser. At the same time, though, it is clear that his mathematical training at the hands of De Morgan had had a lasting effect on him. Economic theory, Jevons asserted, was not worth the paper that it was written on if it was so thoroughly divorced from the realities of the physical world that the relationships it talked about were not instantly recognisable when referring to the experiential realm of everyday life (Mosselmans 2007: 34). His elaboration of a concept of the market for use within economic theory followed exactly the same pattern.

Jevons used that concept explicitly, employing the verbal reasoning that found favour with those who were committed to Cauchy's mid nineteenth-century understanding of mathematical rigour when stating: "I shall mean [by 'market'] much what commercial men use it to express" (2013 [1871/1911]: 84). The passages in which Jevons outlined his conception of 'the market' remain replete with intimations of mathematical intuition and it would be a mistake not to give this fact due attention. Yet here an important distinction between mathematical intuition à la Jevons and mathematical expression à la Debreu becomes very important. Jevons's attempts to show how "the word ['market'] has been generalised" stopped significantly short of full-on mathematical expression: "In Economics we may usefully adopt this term with a clear and well-defined meaning. By a market I shall mean two or more persons dealing in two or more commodities, whose stocks of those commodities and intentions of exchanging are known to all" (Jevons 2013 [1871/1911]: 84, 85). To emphasise that mathematical models of the market needed to remain constrained by observational data, he gave the names of actually existing markets so that his readers would know immediately to what he was referring when he used the word. "In London," he wrote, "the Stock Market, the Corn Market, the Coal Market, the Sugar Market, and many others, are distinctly localised; in Manchester, the Cotton Market, the Cotton Waste Market, and others" (Jevons 2013 [1871/1911]: 85).

Now fast forward to Debreu, writing four generations later. Could his definition of an economy based on a perfectly clearing market apparatus be any more divorced from Jevons's once mathematical intuition had given way to formal mathematical expression? Or perhaps that is the wrong question to ask and it is better to think in relation to the distance that has been travelled from Cauchy's definition of mathematical rigour and its deference to physical analogy to Hilbert's definition of mathematical rigour and its deference to mathematical analogy. Either way, Debreu suggested that:

> A *state* of the economy E … is an $(m+n)$-tuple $((x_i), (y_i))$ of points R^l. It can be represented by a point of $R^{l(m+n)}$. Formally: An economy E is defined by: for each i=1,…,m a non-empty subset X_i of R^l completely preordered by $<\tilde{\imath}$; for each j=1,…,n a non-empty subset Y_j of R^l, a point ω of R^l.
> (Debreu 1959: 75)

Whilst the concept of the market does not appear once explicitly in Debreu's 1959 masterpiece, *Theory of Value*, he does provide a definition of the equilibrium condition whose explanation lays at the heart of all attempts to mathematise the basic market model.

> Formally: A state $((x_i^*), (y_j^*))$ of E is an equilibrium relative to the price system p in R^l if:
> (α) x_i^* is a greatest element of $\{x_i \in X_i | p \cdot x_i \leq p \cdot x_i^*\}$ for $</\tilde{\imath}$, for every i,
> (β) y_j^* maximizes $p \cdot y_j$ on Y_j, for every j,
> (γ) $x^* - y^* = \omega$. (Debreu 1959: 93)

The two methods of saying what is implied when talking about the market are completely unrecognisable from one another's perspective. Indeed, it is possible to go further than that by saying that their most distinctive feature is just how different they *look* to one another. Jevons described using plain words what he thought a market was by drawing links to the actually existing markets the like of which his readers would have experienced on a day-to-day basis. Debreu, by contrast, starts with a series of behavioural axioms and then elaborates these using formal logic into something that can be said to stand in for a market. It hardly needs stating, though, that nobody can be expected to have the same level of familiarity on an experiential plane with Debreu's system of equations as they might have with Jevons's account of actually existing markets in London and Manchester.

Debreu's fundamental reworking of the mathematised market model has also presented economists with two very important dilemmas. The first is that his use of the mathematics of Bourbaki provided economics with the most general account of a market system ever developed, but very few economists share Debreu's mathematical skills to really truly know what it all means. Bourbakism might well have travelled from France to the United States in the immediate post-Second World War period but relatively quickly fell out of favour. It has not been taught as the mathematical underpinnings of graduate economics programmes in the US since the 1970s (Weintraub 2002: 123). There are consequently very few economists who are able to follow directly in Debreu's footsteps, even though it remains a deeply embedded professional marker to defer to the logical tightness and analytical precision that Debreu imprinted in his economic theory via the hands of Bourbaki. Viewed through the perspective of Hilbert's metamathematics, Debreu's mathematisation of the market model remains much more mathematically reputable than Jevons's, but so few economists can actually directly engage with him on his own level.

Economists might thus be characterised today as being in hock to the generality that Hilbertian metamathematics regards as a methodological virtue, but most economists are only capable of working with a pale imitation of the most exacting examples of generality in practice. This leads directly to the second dilemma. This is that the embrace of generality passes through the commitment to a style of formalism that separates the form of economic theory from its content. The shadow of Bourbakism continues to loom large in this regard, even if there are almost no card-carrying Bourbakists left amongst practising economists. The economics profession consequently cannot have it both ways. It cannot maintain commitments to the mathematical expression of the basic market model and still expect the concept of the market to be rooted in the observational data that tells us how people actually behave in real life. In general it has chosen the former over the latter, at least for theoretical purposes. This is despite Debreu's Bourbakist mathematics being instrumental in the demonstration in the 1970s that the basic market model became increasingly inoperative when scaled up from the actions of one individual to the modus operandi of an entire market system. The choice was then whether to go all out in the search for new foundations for economic behaviour or to cherry-pick the findings of mathematical expression so as not to invalidate the use of the basic market model. Here the general consensus has been to opt for the latter over the former.

The Methodological Issue, Not So Simply Explained

Hilbert's work can be seen as an end-point of two thousand years of striving for a genuinely axiomatic treatment of geometry that might be extended to encompass the whole of mathematics (Chaitin 1995: 89). His role as an unequivocally successful revolutionary in this regard might have been somewhat nipped in the bud by the nigh-on simultaneous publication of Kurt Gödel's famous incompleteness theorems in 1931 (Wang 1987: 168). However, it is the attempt that matters more to the subsequent evolution of the market model in economic theory, not the need for a more qualified version of Hilbert's arguments to fit the post-Gödel world. Moreover, nothing that has come later in the field of metamathematics has done anything to challenge the validity of Hilbert's starting point. Discontent gathered pace in the nineteenth century with Euclid's original path-breaking attempts to ground an axiomatic account of geometry in ordinary language statements. His theory spoke to mathematical propositions regarding geometric principles that were taken to be self-evidently true because they could be expressed in terms that were visually 'obvious' to the mind's eye (Hodel 1995: 228). It is possible to know what, say, a point, a straight line and a plane are without having them defined from scratch every time their names are used, because everyday experience enables us to call up relatively easily their unique visual features and how they differ from one another. But these are intuitive definitions that fit really rather poorly with the axiomatic work to which they had typically been put. The related proofs led to "foggy inferences" (Tappenden 2013: 323) that, for their own purposes, appeared to be "unsatisfactory and imprecise" (Jacobs 1992: 29).

Hilbert's idea for transcending these shortcomings was to initiate a decisive break between the treatment of axiomatic systems and the requirement to link their meaning to underlying physical processes that might be deemed to exist in nature. In short, he tried to redefine the meaning of 'meaning' as a metamathematical principle. In his *Grundlagen der Geometrie* of 1899 Hilbert removed all ontological requirements from Euclid's sense of what made a point a point, a straight line a straight line, a plane a plane, etc. In his own words he embarked upon "a new attempt to choose for geometry a *simple* and *complete* set of *independent* axioms and to deduce from these the most important geometrical theorems in such a manner as to bring out as clearly as possible the significance of the different groups of axioms and the scope of the conclusions to be derived from the individual axioms" (Hilbert 1950 [1899]: 1). Note that there is no mention at all here to the content of the axioms or to what they might refer in substantive terms. 'Points', 'straight lines' and 'planes' are no longer important in themselves as points, straight lines and planes. The system of axioms is thus liberated from the intuitionist framework of ordinary language statements about primitive objects, which means that the basic concepts related to a system of axioms can be named in whatever way is chosen, just as long as the formal structure of the axioms is protected (Mainzer 1996: 368).

Edmund Husserl, Hilbert's fellow professor at Göttingen in the immediate aftermath of the publication of the *Grundlagen der Geometrie*, was heavily critical of the possibility that Hilbert was turning mathematics into a mere formal game that would be forever cut off from the physical processes he thought it should describe (Jagnon 2006: 67). At the very least, it is clear that he was in the process of changing what was meant by rigour in mathematics. The question that formed the backdrop for mathematicians' prior understanding of how rigour was to be attained had been whether a theory could be realised in a concrete domain of knowledge (Kneebone 2001 [1963]: 203). A mathematical proof came to life, as it were, only when observational data fleshed out its substance in terms of something that was recognisable from the physical world. Hilbert's new standard, however, asked merely whether there was an in-principle objection to the assertion that the theory would ever prove realisable (Iglomitz 2012: 333). This meant that the test for rigour was contained within the mathematical structure itself, and that if a system of axioms could prove its own internal consistency then this was enough to say that a proof had been demonstrated. There was no longer to be deference to the idea of referential truth claims that required external corroboration, nor yet to what now looked like the old-fashioned criterion of evidence. Under the influence of Hilbert's *Beweistheorie*, or 'proof theory', a consistency proof was all that was needed in the realm of formal mathematical theory (Menzler-Trott 2007: 315). Mathematical proof-making could therefore be understood merely in the abstract and with no necessary relationship to empirical content. A proof that survives contradictory tests in its own terms can be said to admit of realisation, even if it is unclear what its primitive objects refer to when translated into empirically observable objects.

The most readily repeated story about Hilbert comes from the pen of his former student Otto Blumenthal. It appears in a biography that he wrote of his mentor in 1935, which presumably Hilbert would have seen before it was published and would therefore also have given at least tacit approval to its contents. Hilbert, as is well corroborated, had attended a lecture on foundational issues in geometry delivered by Hermann Wiener in Halle in 1891. During his presentation to the assembled audience of the German Mathematical Society, Wiener had argued forcefully that geometry not only could but also should be studied at one stage removed from the visual images that helped to make otherwise less than fully defined geometric propositions 'obviously' true within the mind's eye (Gandon 2016: 48). What Wiener proposed instead was a radical break with the tradition handed down by Euclid. He told his fellow mathematicians to go out and think about how best to replace all visual formulations of geometric principles, ones whose familiarity comes from their origins in ordinary language statements, but whose familiarity might also have diverted mathematicians from a more important task for the best part of two millennia. Hilbert clearly took to heart Wiener's suggestion that it should be possible to re-found the whole of geometry using abstract axiomatic methods alone (Gray 2000: 51). Blumenthal (1935: 403) has it that Hilbert made the following remark to his travelling companions when reflecting on the significance of Wiener's insights: "Man muß jederziet an Stelle von 'Punkte, Geraden, Ebenen' 'Tische, Stühle, Bierseidel' sagen können". This is usually translated into English as something akin to: 'You can say at any time instead of 'points, straight lines, planes' 'tables, chairs, beer mugs''.

There is some dispute in the history of metamathematics literature about exactly what Hilbert meant by this observation. The more extreme interpretation is that he had in mind the vision of a purely contentless mathematics, in which formal proofs will be "no different in basic, syntactic form from a finite sequence of strokes ||| … |" (Hallett 1994: 181). This is contrasted in less extreme interpretations, however, with a more general preference for model theory that stopped short of "the marks-on-paper formalism … [of] … the symbol-loving arithmeticians" (Grattan-Guinness 2000: 208). Hilbertian *Beweistheorie*, or proof theory, becomes a pure "free-standing" structure under the influence of the more extreme interpretation (Iglowitz 2012: 80) but merely a prelude to the "exhibition of a model satisfying [the axiomatic] system" under the influence of the less extreme interpretation (Sieg and Ravaglia 2005: 987).

What the 'tables, chairs, beer mugs' quote appears designed to do is to draw attention to the essentially arbitrary nature of the Euclidean choice of 'points, straight lines and planes' as the place to begin surveying the foundations of geometry (Corry 2007: 782). And a choice it was. The name that was given to the primitive objects might usually become significant to the communicative practice of illustrating the principle in question, but it was mathematically insignificant to the derivation of the principle itself. As Constance Reid (1996: 60) explains, whatever the primitive objects of an axiomatic system were ultimately called, "they would *be* those objects for which the relationships expressed by the axioms were true. In a way", she continues, "this was rather like saying that the meaning of an unknown word becomes increasingly clear as it appears in various contexts. Each additional statement in which it is used eliminates certain of the meanings which would have been true, or meaningful, for the previous statements". There is a purging process in operation here, then, but what is being removed might not necessarily be content per se so much as content that was acquired through the process of physical analogy.

It remains unclear whether Hilbert's remark about 'Tische, Stühle, Bierseidel' literally meant 'tables, chairs and beer mugs'. The smart money is to presume that he did not. A more plausible explanation of what Hilbert appears to have had in mind is that the names of the primitive objects was so unimportant to what followed that the naming process could have been entrusted to a random letter generating machine. Hilbert wanted geometry to become a formal deductive system (Jagnon 2006:

67), and to achieve that then the names of concepts needed to be separated from the operation of the deductive structure (Corry 2004: 127). The primitives had to remain undefined whilst the mathematical processes took place, so that the axioms could be expressed purely mathematically rather than as a reflection of some ostensibly self-evident physical truth (Kline 1972: 1010). The axioms driving the mathematical practice were thus to be written down solely by using the symbols of formal logic and without the potentially corrupting influence of words (Breger 2000: 228). Anyone who has ever wondered about the general absence of words from the proofs that are to be found in the pages of leading economics journals is likely to have had a moment's recognition at this point. Primitive objects are to be treated as "bare 'things'" that can be assumed to come to life only after the satisfaction of the system's axioms can be shown to have distinguished between those theorems that are true and those theorems that are false (Rodin 2014: 40). The primitive objects remain undefined terms with no essential meaning during the process of proving the theorems. 'Tische, Stühle, Bierseidel' remain mere letter sequences for the duration of this process. It is only when all proofs are at hand that meaning is then assigned to the concepts of a system that is already known to be true in its own terms (Sentilles 1975: 94). At this point it is almost certain that 'Tische, Stühle, Bierseidel' will not mean 'tables, chairs, beer mugs' at all.

This discussion – abstract, complex and from a different domain of knowledge as it clearly is – can nonetheless be seen to have important implications for the conduct of economic theory. Jevons's market concept relies on being able to talk about demand and supply, and it also revolves around the idea of demand and supply being brought into equilibrium. But the demand and supply that are operative here are the demand conditions that the actors working within specified market environments are familiar with in everyday terms, and likewise for the supply conditions. Debreu's market concept also relies on notions of demand, supply and equilibrium, but they do no generative work in enabling the realisation of the model. All of that work takes place within the mathematical structure itself, because the behavioural axioms with which he began have been worked through unflinchingly and unrelentingly according to the prevailing standards of logic. Debreu, as the remarkably competent mathematician he was, found himself perhaps uniquely well placed to come good on the potential contained within the mathematical structure.

However, it is not entailed by that structure that the primitives with which Debreu operated should be called 'demand', 'supply' and 'equilibrium'. It was his choice to name them in this way and his choice alone. Whereas 'demand', 'supply' and 'equilibrium' for Jevons were direct analogues to observable economic behaviour in the manner required by Cauchy's definition of mathematical rigour, as soon as Debreu effected the shift to Hilbert's definition of mathematical rigour 'demand', 'supply' and 'equilibrium' became nothing more referentially real than letter sequences. He could have chosen to call the primitives contained within his model anything he liked and still have left the mathematical structure intact and the accompanying model would have remained true in its own terms. The model is not of an economy as it appears visually in the mind's eye, because this would have been all too suggestive of the Euclidean intuitionism that Hilbert strove to leave behind. It only appears to be about the economy because the primitives are given names in a post hoc fashion that in other usages infer something economic. Debreu could just as easily have called 'demand', 'supply' and 'equilibrium' 'Tische, Stühle, Bierseidel' (or, as would perhaps have been more likely in his native French, 'tables, chaises, chopes à bière'). Indeed, with suitable theoretical rearrangement his mathematical model would have been equally as true had the primitives been called 'accumulation', 'exploitation' and 'final crisis of capitalism'. Hilbert's liberation of mathematical rigour from physical analogy means that Debreu's mathematical model could have been infused with all of these different economic meanings without ever once stopping being true in its own terms.

The irony here is that Debreu's work resonates as deeply as it does only because his predecessors who were operating to a very different metamathematical standard of rigour had already established in plain language terms the meaning of economic concepts that Debreu latterly appropriated. From Debreu's perspective, and given his mathematical skills, Jevons's *Theory of Political Economy* looked decidedly old hat. However, Debreu would not have been in a position to have others believe that his *Theory of Value* contained the logical working out of the market model were it not for the fact that theorists like Jevons had already done the preparatory work in establishing how the components of the basic market model fitted together in relation to one another. Debreu's 'demand', 'supply' and 'equilibrium' are little more than letter sequences in their own terms, but they are passable for concrete economic categories because of the prior meaning that earlier theorists had attached to them. Debreu's theory therefore sits on the shoulders of Jevons's in ways that have never really been recognised before.

What happened to Debreu on receipt of his Nobel Prize in 1983 is indicative in this regard. Always one to shy away from the limelight, he was caught seriously off guard by members of the press seeking to ascertain what his work might reveal about the major economic policy disputes of the day (Düppe 2012: 439). He believed that Bourbaki's mathematics was a natural home for him because it allowed him to lead an intellectual life without being required to reveal any of his inner thoughts. But this did not stop the press intrusion, because not understanding the nature of the Hilbert programme to which he subscribed they were insistent that his commitment to the axiomatic treatment of a market economy rendered within a general equilibrium framework translated into endorsement of the Reaganomics of the day. The journalists were simply not in a position to know that by making this unsubstantiated jump on Debreu's behalf they were harking back to the need for a physical anchor for his mathematical treatment of the market model that his metamathematical commitments deny.

And so finally I can bring the discussion back to where I started. It matters politically, I have long argued, how economists use a market model to imagine a world in which exchange relations have some mysterious self-regulating power. It matters because it is the language that is derived from economic theory that subsequently frames discussions of 'how much market' should be allowed into various aspects of everyday life. Even if this takes place at one or two stages removed from the way in which the mathematical models are established in their own terms, the fact that the same words are used gives additional respectability to political arguments that seem to mirror those of economic theory. The real world is more easily imaginable as a series of interlocking market institutions governed by their own internal logic the more that economists can be thought to have described such a world at an abstract level. Debreu's work might well now be half a century old, but nobody has done more than him to create an economic theory that operates at this level and is about markets and nothing other than markets. Yet it is a complete misunderstanding of his work to suggest that it provides any sort of rationalisation for the political use of market ideology. It is just not this sort of theory. As I hope to have shown, this is primarily because it is built upon metamathematical foundations that disqualify the move from abstract theory to claiming that the theory describes actually existing economic institutions.

Misunderstandings, however, can still have sizeable political effects, and I would want to argue that this is seen very clearly here. It is by no means intuitively easy to grasp the distinction between 'demand', 'supply' and 'equilibrium' as used by Jevons in search of referential truth claims and 'demand', 'supply' and 'equilibrium' as used by Debreu as random letter sequences to name the primitives in his mathematical model. For as long as it remains more convenient simply to assume that these two things are the same, Debreu is likely to be two things at once. He certainly still deserves to be known as the foremost exponent of Hilbert's programme of formal axiomatisation in economic theory, and in this regard he has had a significant influence on economists' mathematical treatment of the market model. In addition, though, he is also likely to be remembered as a wholly unwitting supporter of a market ideology that continues to be used politically in all sorts of regressive ways.

Postscript

Perhaps I should have said a couple of words right at the beginning about how I have come to be interested in such an odd topic for a political economist as the history of metamathematics. I am currently getting to the end of the first draft of a book from my ESRC Professorial Fellowship project that is ostensibly about the economics imperialism phenomenon but treats this as a matter of the mathematisation of the market model. When economists plant their flags in other people's subject fields, it is always through the suggestion that the market model offers a framework for 'rigorous' analysis of a 'mathematical' nature, therefore ridding social science of the disturbing features of author subjectivity. As hopefully is clear from the above, though, in the course of drafting this book I have become increasingly convinced that the image of 'mathematical rigour' has changed appreciably during the time that economists have been trying to establish their subject field as a mathematical science. What counted as mathematical rigour was not the same in 1970 as it was in 1870. Explicit advocacy of economics imperialism by mainstream economists only really began in the late 1970s, which means that the pre-history of that phenomenon has a lifespan that almost perfectly maps onto the changes in mathematical economics sparked by the replacement of Cauchian metamathematics with Hilbertian metamathematics. That is the story that I hope the book will reveal, because it becomes a means of showing how the economics imperialism project is riddled with internal tensions. Its justifying rhetoric of what it promises to bring to other social science subject fields relies on an image that is consistent with Cauchian metamathematics, but its declared sense of superiority over those other fields invokes a loyalty to Hilbertian metamathematics that remains beyond the mathematical skills of all but a tiny handful of economics imperialists.

I have also committed to giving a keynote address at the Karl Polanyi Institute Annual Conference next year on the 'Geographies of Markets'. I plan to organise that around Hilbert's distinction between 'concrete' and 'formal' axiomatisation and show how this translates in mathematical economics to a shift in focus in the market concept from something that is by definition place-bound to something that is essentially placeless.

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