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Debreu's apologies for mathematical economics after 1983

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Abstract: When reassessing the role of Debreu's axiomatic method in economics, one has to explain both its success and unpopularity; one has to explain the "bright shadow" Debreu cast on the discipline: sheltering, threatening, and difficult to pin down. Debreu himself did not expect to have such an influence. Before he received the Bank of Sweden Prize in 1983 he had never openly engaged with the methodology or politics of mathematical economics. When in several speeches he later rigorously distinguished mathematical form from economic content and claimed this as *the* virtue of mathematical economics, he did both: he defended mathematical reasoning against the theoretical innovations since the 1970s and expressed remorse for having promised too much because it cannot support claims about economic content. The analysis of this twofold role of Debreu's axiomatic method raises issues of the social and political responsibility of economists over and above standard epistemic issues.

Keywords: Gerard Debreu, axiomatic method, theoretical practice, ideology, social responsibility

JEL Classification: A11, B31, B41, C02

1. THE INVISIBLE HAND OF FORMALISM

Gerard Debreu cast a "bright shadow" on the discipline of economics. He is known to all who had to learn his proof of the existence of a competitive equilibrium at graduate school, crowned with the Bank of Sweden Prize, yet cursed by all economists who want to say more than $x \in X$. Although economists are trained in Debreu's proof, they neither read the 1954 article nor his book of 1959, *Theory of value* (noteworthy as the last 'theory of value' in economics until today). Even if he is now peremptorily rejected or belittled as outmoded, contemporary economists have profited from Debreu since he advanced the belief in, and fostered the reputation of, economics as an incontestable science. Economics today can be sold for a higher price than before Debreu, yet current theoretical battles seem less heated than in the days of, for example, the socialist calculation debate. Furthermore, although the entire effort of Debreu was motivated by, and becomes intelligible only by reference to, his training in a specific school in mathematics—"Bourbaki"—there are but a few economists who have ever heard of that name, and even less who have read it. Debreu did, meticulously, and everything he said about his own work, as we will see, can be found almost word for word in Bourbaki.[1]

This elusive influence of Debreu is apparent in the way others have assessed his work. "Although there had been quantum leaps of mathematical sophistication before in the history of economics, there had never been anything like this", Roy Weintraub commented (2002, 114). Werner Hildenbrand called his work "scientific contributions in the most honest way possible", and Paul Samuelson "an unpretentious no-nonsense approach" (quoted in Weintraub 2002, 113). Oliver Williamson, the ambitious new-institutionalist and Debreu's colleague at Berkeley, said he "always marveled at Gerard's quiet, kind and inclusive ways—an example being his insistence on referring to me as an 'economic theorist', my protests to the contrary notwithstanding" (quoted in Anderson 2005, 4). Debreu was right to insist, and indeed, as Varian wrote after the Bank of Sweden Prize, "not only have Debreu's works contributed to mathematical economics; they have contributed to the science of economics as a whole" (Varian 1984, 4).

Debreu's elusive influence is also apparent in the way others have discussed his work. Weintraub, who has given us the most complete image of Debreu up to now, employs a distinction from the historian of mathematics Leo Corry, between the "body" and the "image" of mathematics (Weintraub 2002; Corry 1992, 1997). From the point of view of today Debreu's influence on the body of economics could be

[1] 'Nicolas Bourbaki' is the pseudonym for a collective of mathematicians founded in 1935 at the École Normale Supérieure in Paris. Debreu was taught by its oldest member, Henri Cartan, and kept in close contact with André Weil in Chicago. In a nutshell, Bourbaki's work amounts to a turn inward, a mathematization of mathematics, and thus a separation from sciences such as physics, so that Weintraub rightly speaks of an oxymoron in 'applying Bourbaki' to economics (2002, 103). One aim of this paper is to show how this oxymoron was so productive in spite of Debreu's rigor in keeping up that separation.

called zero, in that general equilibrium theory (GET) is the economics of yesterday. While GET had mirrored most analytic advances in economic theory before Debreu, after Debreu most theoretical innovations came as alternatives to GET (from game theory to complexity theory). In this search for alternatives, Debreu became the bogeyman man of both orthodox and heterodox economists deploring how unrealistic and irrelevant economic theory had become. Regarding the image of economics, however, it is easy to underestimate Debreu's influence on the method, style, institutions, intellectual culture, and professional ethos of economists. Did Debreu, in whatever murky fashion, not contribute to the immense growth and increased social status of the economics profession and its epistemic dominance in wider economic discourse? In short, concerning the body of economics, Debreu's influence can be easily belittled, but concerning the image easily undervalued. The broader the view taken, the greater yet subtler his influence appears.

At the Nobel festivities in 1983, after the King had handed over the prize, Debreu himself reflected on this puzzling influence in the few words he had chosen for his banquet speech. In order to explain his role in economics, he used the metaphor of the invisible hand.

> [A] scientist knows that his motivations are often weakly related to the distant consequences of his work. The logical rigor, the generality, and the simplicity of his theories satisfy deep personal intellectual needs, and he frequently seeks them for their own sake. But here, as in Adam Smith's famous sentence, he seems to be 'led by an invisible hand to promote an end which was not part of his intention', for his personal intellectual fulfillment contributes to promoting the social interest of the scientific community. […] It was my great fortune to begin my career at a time when economic theory was entering a phase of intensive mathematization and when, as a result, the strength of that invisible hand had become irresistible (Debreu 1984a).

Debreu's use of the metaphor of the invisible hand has subtle connotations. By using this metaphor, Debreu, first, admitted that the primary concern of his intellectual life was not the "social interest of the scientific community"—whatever that might be. Instead, he was engaged in mathematical economics *for its own sake*. Like the baker in Smith's market, the mathematical economist pursues rigor only for his own interest—for the intrinsic appeal and deep satisfaction of the mathematical experience—yet mysteriously, 'led by an invisible hand', this turns out to promote the social interest of the discipline. Debreu thereby responds to a common critique of economics, that mathematics has become an end in itself, is overused excessively, and leads economics to the edge of irrelevance, in the same way that clergymen's complaints about the use of money were once rebutted by economists with the invisible hand.

Using this metaphor, Debreu thus acknowledged that there is a *separation* between the intellectual motivations of mathematical economists and the consequences for economics. "Logical rigor, generality and simplicity" are thus not virtues that stem from an interest in economics itself. Nothing *in* economics—whatever that might be—motivated his use of mathematics, whether the epistemic issues (the complexity of the economy), ontological matters (prices are numerical things), or semantic reasons (the suitability of mathematics as the language of economics) focused on by mainstream philosophy of science. Mathematical reasoning and the treatment of economic content are rigorously separated, which is *the* tenet of Debreu's axiomatic method. Debreu was unique among economists in actually *defending* this separation methodologically. That reflects his Bourbakist background and makes him, and his neo-Walrasian community, the only true formalists in economics.

With the metaphor of the invisible hand, moreover, Debreu acknowledged the "irresistible" influence his axiomatic method had had. Since his intervention, the economics profession has moved ever further away from being able to connect to a pre-Debreuvian way of intellectual life. Whatever their intellectual motivation, economists inevitably end up reinforcing the current state, *as if* one engages in economics only for the sake of the mathematical value of rigor. Debreu's remarks can also be seen as acknowledging this tragic aspect of the influence of mathematical economics: that attempts to provide a theoretical alternative only reinforce the status quo of the profession.

With the metaphor of the invisible hand, Debreu admitted that his influence had gone far beyond his own intentions when entering the Cowles Commission in 1949. Debreu was surprised by his success, and admitted that he had never considered the possible unintended consequences of mathematical virtues in economics. Indeed, during his active intellectual life up to 1983 he never felt the need to openly defend the methodology of his work, let alone participate in debates on mathematics in economics exactly because he assumed their separation. With the Bank of Sweden Prize, however, coming after he had already largely withdrawn from active research, he had to recognize that his influence had gone far beyond the separation he assumed. Hence, I interpret the use of this metaphor as an "apology" not only in the sense of a defensive 'apologia' of mathematical economics, but also as a way of saying sorry for having caused a misunderstanding about the nature of mathematical economics. Even if Debreu would not admit this feeling to himself, insisting on the axiomatic separation of mathematical form and economic content was a way of saying: sorry, dear economist, mathematical reasoning does not suffice for economics.

With the metaphor of the invisible hand, Debreu effectively, did both: he defended the importance of mathematical economics because of its beneficial results for the profession beyond the GET he applied it to; and he apologized for it, admitting its insufficiency for any economic theory. With his defense, he acknowledged the pervasiveness of mathematics in post-Walrasian economics, while with his remorse he acknowledged its irrelevance. It was in this sense that Debreu cast a 'bright shadow' on post-war economics. It is not trivial to know whether one stands under his sheltering and threatening influence or not. What Debreu acknowledged was an *invisible hand of formalism* that accounts for the success *and* unpopularity of mathematical economics since the 1950s.

What follows is an attempt to trace this invisible hand of formalism in Debreu's methodological speeches after 1983. The core of Debreu's methodology is the axiomatic separation of meaning and structure, or as he translates, the separation of economic content and mathematical form, or as he also says, the separation of interpretation and theory. In Debreu's words, "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).

The two questions to which Debreu's speeches reply, are first what *is* the axiomatic method (section two)?, and second what is its *value* for the economist (section three)? The exposition of his methodology will show that the separation of meaning and structure frustrates any conception of a *theoretical practice* and thus any actual role for the economist. The self-limiting character of mathematical economics, which has often been noted,[2] implies the redundancy of the theoretical practices of abstracting, simplifying, explaining, isolating, or any other kind of reasoning upon a particular epistemic interest commonly discussed in mainstream philosophy of science. These terms are—as I show in section two—unsuitable for tracing the influence of the axiomatic method on the discipline, for Debreu pursued mathematical rigor for its own sake.

As a consequence, when defending the advantages of the axiomatic method, Debreu must continuously back away from his separation of mathematical form and economic content—as shown in section three. Its benefits for the economist can only be justified by smuggling other intellectual values than axiomatic rigor back into economic theory. In other words, only as long as there is no separation of theory and interpretation, can economic theory be relevant for the economist. This negative result leaves open the question about how the impact of the axiomatic method can be traced. In this respect, issues of social and political responsibility, rather than ontological or epistemological concerns, emerge from the discussion. The axiomatic discreetness of mathematical economists allows others to use their authority without their involvement. This, in turn, is the very reason why Debreu, after 1983, saw the need for setting his methodology right in the first place—saying sorry for not preventing unintended uses of his work.

2. THE SCHEME OF THE AXIOMATIC METHOD AND ITS FIFTH WHEEL: INTERPRETATION

At three prominent places Debreu had the occasion to speak out about his method: in his Nobel lecture (1984b), in his Frisch Memorial lecture (1986), and in his presidential address at the AEA (1991). These speeches strongly resemble each other and closely echo Bourbaki's methodological writings (1949, 1950). In one repeated formulation Debreu describes the 'scheme' of the axiomatic method (1984b, 275; 1986, 1256-1258; 1991, 4-5). This scheme follows step-by-step what Bourbaki had said about the concept of a mathematical structure.

> It now can be made clear what is to be understood, in general, by a mathematical structure. The common character of the different concepts designated by this generic name, is that they can be

[2] Regarding the axiomatic method see, e.g., Clower 1995; regarding GET, e.g., Kirman 1989; regarding structuralism, e.g., Hands 1985; and regarding the history of economic theory, e.g., Blaug 2003.

> applied to sets of elements whose nature has not been specified; to define a structure, one takes as given one or several relations, which these elements enter [...]; then one postulates that the given relation, or relations, satisfy certain conditions (which are explicitly stated and *which are the axioms of the structure* under consideration). To set up the axiomatic theory of a given structure, amounts to the deduction of the logical consequences of the axioms of the structure (Bourbaki 1950, 225-226; emphasis added).

The only objects of mathematics according to Bourbaki are structures, since only structures, not the meaning of the elements, can be the playground of mathematical rigor. In Debreu this appears as follows:

> An axiomatized theory first selects its primitive concepts and represents each one of them by a mathematical object. [...] Next assumptions on the objects representing the primitive concepts are specified, and consequences are mathematically derived from them. The economic interpretation of the theorems obtained is the last step of the analysis. According to this schema an axiomatized theory has a mathematical form that is completely separated from its economic content (Debreu 1986, 1265).

In order to axiomatize a theory, one is to follow a scheme consisting of five steps: selecting, representing, specifying, deriving, and interpreting.

Selecting primitive concepts

The subject of the sentence, "an axiomatized theory selects its primitive concepts", is apparently ill-expressed, yet, I suggest, symptomatic of Debreu's methodology. An axiomatized theory does not "do" anything. Debreu does. He axiomatizes GET. He makes the first selection. And this selection is crucial for the assessment of the entire effort in that it presumably expresses his theoretical interest—including, for example, the purpose for which a theory is developed, the intellectual space in which one operates, and the limits of the entire enterprise. An axiomatic analysis is an analysis *of* a theory that is *already* in place, and that limits the axiomatic discourse. So, is it possible to assess Debreu's selection of primitive concepts in terms of his commitment to a particular theoretical interest?

At this point, Debreu's alleged "Walrasianism" must be addressed. The received understanding of Debreu is that he shared a theoretical interest with Walras and transformed it into what came to be called neo-Walrasian economics. In this account, Walras formulated the theory ("the economy" as a system of *n* equations), but missed the rigorous proof (*n*-equations with *n*-1 unknowns, thus solvable) which Debreu delivered 80 years later (using a rigorous topological theorem). So, does Debreu share a theoretical interest with Walras?

It was and remains open to debate what exactly Walras's theoretical interest was (see, e.g., Walker 1983; 2006). When comparing Walras's enduring interest in pursuing a scientific social science with Debreu's motivations, one certainly must point to Walras's belief in the well-ordered reality of the economy. When attacked for the descriptive inaccurateness of his mathematical model—the measurability of utility figured prominently—he insisted that his model was an *immediate expression* of the market system (see his letter to Garnier, in Ingrao and Israel 1990, 147). Walras associated his system of equations with 'the economy' on the basis of a metaphysical belief that the economy is capable and worthy of possessing mathematical truth. That is, economic reality can be expressed, rather than merely represented, with the beauty, consistency, and simplicity of mathematics. At the heart of Walras's mathematical economics was not the belief that the analogy of Newtonian mechanics and market forces actually holds in reality, as he officially framed his theory. Rather, the mathematical expression of the economy shows that the economy is worth its reality! Walras was moved by a Platonistic belief that the meaning of the structure of mathematics was the same as the meaning of the structure of "the economy."

Debreu, in contrast, had no expressive interest in any aspect of the reality of the economy when axiomatizing GET. "In proving existence one is not trying to make a statement about the real world, one is trying to evaluate the model" (Debreu, in Feiwel 1987, 243). In Weintraub's words: "The objective was no longer to represent the economy, whatever that might mean, but rather to codify the very essence of that elusive entity, the Walrasian system" (Weintraub 2002, 121). In making Walras's theory itself the object of scrutiny, but neglecting Walras's theoretical interest, Debreu cut the tie with Walras. Blaug rightly calls this re-adaptation of Walras one of the "most remarkable *Gestalt*-switches in the interpretation of a major economic theory in the entire history of economic thought" (Blaug 2003, 150). Debreu and Walras did not share a theoretical interest. Rather, Debreu used Walras as a jumping off board for an enterprise that was quite differently motivated. Walras in Debreu is a placeholder for economic theory.

What Debreu did actually select was not a market theory at all, but 'primitive concepts', first of all 'commodity'. Did Debreu make a real choice by doing so? What did he rule out? Commodities are indeed the objects of markets: they are the things referred to in market theory because they are the things of value. The concept of commodities, and the question of how they 'have' value, was one locus of the preceding centuries' contest between theories of value (Jorland 2000). But Debreu's *Theory of value* does not even pose that question. Debreu did not select a particular meaning of 'commodity': his primitive concepts remain undefined, and undetermined. To choose 'commodity' as a primitive concept is to choose it as a concept of all *possible* market theories.

As Kant said that truth is a matter of statements, Debreu said that markets are a matter of commodities. With the word 'commodity' Debreu only points to a possible theoretical interest but does not address it. Even when Debreu introduces 'commodity' in chapter two of his *Theory of value* heuristically as something that is completely determined in physical terms regarding place, time, quantity, and quality, he does not select a particular meaning, but simply makes the concept appear to be capable of concreteness, and thus of interest for the economist. And so Debreu concludes this chapter with definitions in set-theoretical terms and adds: "All that precedes this statement is irrelevant for the logical development of the theory. Its aim is to provide possible interpretations of the latter" (1959, 35). Possible interpretations, not actual! The word commodity is for the economist like the carrot suspended in front of the donkey's eyes that it can never reach.

If there was any real choice, hidden behind Walras and the seemingly unproblematic concept of commodity, it was that economic theory has a structural character in the first place—as opposed, for example, to a conceptual, narrative, literary, or instructive character. If such was his choice, what theoretical interest does Debreu really share with other economic theorists? Can one say that Debreu does not need to be interested in the meaning of commodities in the same sense as Smith's baker does not need to be interested in moral codes but merely in his own interest? Is the meaning and structure of economic theory separated in the same sense as the constitution of commodities in markets is separated from the constitution of the value of goods? Do the formalism *of* market theories (the use of formal language) and the formalism *in* market theories (no substantive requirements) have the same roots?

Debreu, certainly, did not choose for structuralism in that sense. The analogy of the separation of meaning and structure *in* and *of* economic theory never occurred to him. For Debreu, 'structure' was a feature of market theories, not of the market: he does not discuss in what sense 'the economy' could be perceived as a 'structure' in the first place. Yet without perceiving himself as a 'structuralist', Debreu nevertheless instantiates the belief that the problem of economic theory is a matter of dealing with structures. And this is as true today as it was 50 years ago when the question of an economic theory of value disappeared in the structuralist verve of *Theory of value*.

Representing primitive concepts as mathematical objects

Whatever Debreu selected as a primitive concept, the choice is diluted when this concept is represented by something else—a mathematical object: x. Hence, the first two steps should be read within one breath: *selecting-and-representing*. Primitive concepts are not objects of selection, but appear already subjected to a representation, a displacement. Their meaning, let alone their reference, does not find its way into the formal analysis.

The notion of representation nevertheless suggests a common feature or similarity between the represented and what represents. Is there anything by virtue of which a mathematical object can represent a primitive concept? No. The very question of the conditions of representing cannot be posed in Debreu's scheme. Instead of representation, Debreu speaks more accurately of the *substitution* of primitive concepts by mathematical objects. These substituted concepts are not representations, but function as the identifiers of the mathematical objects—their nicknames. Representing primitive concepts as mathematical objects is thus an act of tagging the mathematical objects. This is to say it is not an act of abstraction, idealization, comparison, simplification, inference, deduction, induction, nor an act of abduction into another context (as Khan interprets the first two steps as a choice of metaphor).[3]

[3] See Khan 1993. The difference between Bourbakian mathematical objects and metaphors is vital for understanding the role of mathematics in economics. Metaphors *need* to be interpreted. Mathematical objects *could* be interpreted.

The primitive concept 'commodity' is thus represented as a mathematical object, an "element" in the "commodity space" $x \in X$. The remaining primitive concepts are not selected, so their meaning is not even substituted. For the structure of GET to be settled, two different relations need to be defined on the commodity space, namely 'preferences', and 'prices'. Defined by the commodity space, they are irrelevant to the question of a theory of economic value, but are rather the structural requirements for formulating the mathematical problem of consistency, the 'equilibrium'.

> The consumption of a consumer, his set of possible consumptions, and his preferences are represented respectively by a point in the commodity space, a subset of the commodity space, and a binary relation in that subset (Debreu 1986, 1265).

Thus Debreu's "universe of discourse" consisting of 'commodities', 'consumers', and 'prices' is, as he requires, "explicitly listed at the outset" (1959, 3). In such terms Debreu described what is elsewhere called 'consumer choice'. How the binary relations with the nickname 'preferences' are related to the choices we make, as their *reflections* or their *determinants*, or any other queries we may have about choosing commodities, cannot even be asked.[4]

Again, one can speculate about the consequences on the broader context of economic discourse. Was not the question of price determination originally about how prices represent or express our acts of valuation, not the consistency of economic theory? Only then can all the fuss made about GDP, economic performance, growth and wealth mean something. If the consistency of economic theory can be formulated independently of this question, what then does Debreu show of the very idea of prices representing values? Did Debreu indirectly show something about the market if its theory is independent of its meaning? Did Debreu liberate the economist from the burden of interpreting in the same sense as the economic agent in markets is liberated from the social articulation of value?

Specifying assumptions

The next step is to *specify* 'assumptions'. As students learn in their Mas-Colell, et al. course, the commodity space representing 'commodities' must be *i)* finite, *ii)* convex, and *iii)* have a lower bound; the binary

[4] See, for this distinction between reflection and determinant, Sen 1973.

relations representing 'preferences' must be *j)* continuous, *jj)* strictly monotonously increasing, and *jjj)* strictly quasi-concave; and the technology space must be *k)* strictly convex. In a textbook from 1958 *j)-k)* are listed for the first time as follows: no increasing returns to scale (producing more does not reduce average costs), at least one factor of production (things are not like 'air'), consumer wants cannot be saturated (we are all excessive if we can be) (see Weintraub 2002, 188).

After the first two steps, these two textbook specifications of assumptions must be two different things. The confusion is that between 'assumption' and 'axiom'. Assumptions are not specified on primitive concepts, let alone their referential meaning, but on the mathematical objects that substitute for these primitive concepts. To speak about 'assumptions' is misleading in the sense that Debreu does not assume something to be the case which, as with hypotheses, suppositions, or basic beliefs, could be at stake when theorizing. What Debreu calls 'assumptions' are, rather than epistemic priors, the *result* of the axiomatization—the axioms: "one postulates that the given relation, or relations, satisfy certain conditions (which are explicitly stated and *which are the axioms* of the structure under consideration)" (Bourbaki 1950, 226, emphasis added).

Debreu's axiomatization results in axioms stating the mathematical conditions of a consistent structure. Debreu, in other words, did not theorize in the face of the world, but in the face of a structure. As Anderson commented on his attitude to assumptions: "[Debreu] refused to comment on the reasonableness of assumptions, believing that his job was to make the assumptions clear, and it was the reader's job to assess them" (Anderson 2005, 6). It is sheer irony how in the neo-Walrasian community such cautious treatment of economic assumptions could be taken up, as here in Egbert Dierker's survey of neo-Walrasian economics: "Economic knowledge is not required, but especially a reader without economic background will gain much by reading Debreu's classic 'Theory of Value'" (Dierker 1974, iii).

Accordingly, the task of the neo-Walrasian research program was to show the weakest conditions of an equilibrium—'weak' in terms of the Bourbakian hierarchy of mathematical structures, the weakest of all assumptions being $x \in X$. One great result arrived at, for example, was Andreu Mas-Colell's (1974) "An equilibrium existence theorem without complete or transitive preferences"; and, as an early work by David

Schmeidler (1969) asked, are "Competitive equilibria in markets with a continuum of traders and incomplete preferences" possible?

Nevertheless, some of Debreu's axioms *could* have an economic interpretation. One economist even held that "the behavior of market economies depends on how convex the world is" (Kay, quoted in Ramrattan and Szenberg 2005, 6). While most other economists may have difficulties in imagining what a 'convex world' would look like, axioms like transitivity are indeed commonly understood as demanding something of the actual holding of preferences. Here the interpretive labor of the economics instructors begins. They literally invent narratives for mathematical objects, producing the impression of actual reference. If teachers emphasize intelligibility and keep the mathematics low, they ironically reinforce the mathematical bulwark against critical reflection. The same applies to those economists who took the undergraduate narratives literally as descriptive truths and went into behavioral economics. Most of this research confuses assumptions and axioms and actually reinforces the underlying structure that is independent of any interpretation. It was mostly by virtue of these 'secondary' interpretations of axioms that the neo-Walrasian community could achieve their success in producing a benchmark of economic theory. Axioms of choice such as independence and transitivity did not express the basic beliefs of the neo-Walrasian research program. There were none.

Again, one can speculate further on the more indirect effects of this confusion on the discursive environment of economics. If the mathematical weakness of axioms is confused with the philosophical weakness of assumptions this error may be transplanted into the political arena with very different connotations, namely with regard to *freedom*. Weak assumptions then amount to weak demands for particular behavior from economic agents, i.e., negative freedom. As Debreu only spoke about structures, other economists are relieved from having to consider whether economic agents should have a preference for particular goods. The *x* of Debreu and *the whatever you want* of the liberals—is it the same? Do weaker assumptions make for a freer society?

Ruccio and Amariglio went further with this association of Debreu's rigor with liberal politics by arguing that the absence of the "body" of economic agents allows for new "re-evaluations of the experiences and distributions of pleasure and pain, work and desire, base and refined instincts, emotions and reasons, passions and interests, sex, race, and class" (2003, 101). Ruccio and Amariglio thus turn the complaints about the insignificance of Debreu's work on its head: precisely because there is no reference to bodily agents, Debreu, and neoclassical economics in general, *frees* agents for a post-modern play of meaning.

> There is a refreshing quality to recent neoclassical thinking in that it mostly displaces the question of the body as origin. […] [We] regard with some cautious degree of approval the appearance of a body in high-neoclassical theory (as, for example in Debreu's *Theory of Value* or in Arrow and Hahn's *General Competitive Analysis*) in which bodily functions of consumption, production, distribution, choice and so forth only obliquely relate to a central, unifying dimension (Ruccio and Amariglio 2003, 110).

Is Bourbaki's refreshing liberation from meaning the virtue of a postmodern society that is only "obliquely" unified by the market? Debreu would probably blush to read such lines. He hardly ever even removed his tie! How could he have endorsed such a liberated subject? It was discreetness, not a concern for a pluralist society that made him withdraw from the 'unified body' of classical economics. Debreu's work does not touch any body, neither at its surface, nor at its origin.

Deriving consequences

The fourth step concerns the kind of consistency the axiomatic method requires. Here we enter the playground of rigor, and nothing but rigor: proceeding step-by-step from 'fully specified' assumptions to conclusions. While the first three steps did not correlate with any intellectual effort, here all the labor and affectivity of mathematical reasoning comes into play. The philosophical question at this step is how formal and mathematical logic relate. Does mathematical logic add something to formal logic, or, the other way around,[5] is the language of logic proper? This question once moved the generation of Frege, Whitehead and Russell. Debreu and Bourbaki are reluctant to enter such debates.

> Whether mathematical thought is logical in its essence is a partly psychological and partly metaphysical question which I am quite incompetent to discuss. […] It serves little purpose to argue that

[5] For a discussion of the axiomatic method in economics regarding the relation of mathematics and logic see Vilks 1992.

> logic exists outside mathematics […] Logical or (what I believe to be the same) mathematical reasoning (Bourbaki 1949, 2).

Debreu also simply identifies rigor with valid reasoning: "The theory of value is treated here with the standards of rigor of the formalist school of mathematics. The effort towards rigor substitutes correct reasoning and results for incorrect ones" (Debreu 1959, 1).

Note again that 'deriving consequences' does not describe the actual practice of axiomatic theorizing, but rather its *result*. Only when the mathematical proof is presented in its final shape does one 'derive consequences'. While the reading of a proof might have a great aesthetic appeal, its writing is a rather dirty, lengthy, and uncertain process of trial and error. Writing a proof is like groping one's way in the dark, playing with ever-weaker 'assumptions', and trying to struggle through a forest of tautologies. Here I may report an anecdote of one of Debreu's students, Mark Blaug. During a summer course in 1955 at Michigan University, a doubt arose about whether a line in the proof was correct. Instead of thinking this through in class, Debreu left the room. After some time he finally returned with the words "of course, it is correct". The actual mathematical labor, as it were, does not take place in the classroom or any other public place.

Obviously, the term 'consequences' has no causal meaning in logical derivation. To derive consequences is to work through logical implications with the aim of proving the consistency of the axioms. Debreu's existence proof did precisely that: it showed that the mathematical relations ('assumptions') he specified as the axioms did not contradict each other. Debreu's proof was an indirect as opposed to a constructive proof, showing that a disequilibrium leads to a violation of the axioms.[6] Equilibrium and consistency are thus equated.

For this reason Debreu rejected the study of the stability of equilibrium—one of the most contested issues in economic theory after Debreu. "(W)hen you are out of equilibrium, in economics you cannot assume that every commodity has a unique price because that is already an equilibrium determination" (Debreu, quoted in Weintraub 2002, 146). Debreu believed that the very notion of market disequilibrium is a misnomer since in such a state there could be no conceivable price

[6] This is the point where von Neumann's use of the axiomatic method differs from Debreu's Bourbakian use. Von Neumann always stressed the need for a positive constructive analysis. Only then could the intuition of strategic behavior enter economic theory, as it is omitted in GET (see Giocoli 2003).

system whatsoever. Economic models are only worth anything insofar as they can be shown to have a logically consistent solution, that is, as long as they are equilibrium models. And this applies today to the same extent to models of growth, innovation, or uncertainty. And so Debreu said about those who engaged in the

> theory of temporary equilibrium, a so-called theory of disequilibrium (a misnomer since it is a theory of equilibrium under new constraints). They show, if it were needed, that the concept of equilibrium is an organizing intellectual concept of great generality with which it is difficult to dispense in the social sciences (Debreu, quoted in Feiwel 1987, 253).

Interpreting

Debreu mentions interpreting as the fifth and last step of the axiomatization, although the separation of interpretations guides the axiomatization. But, is interpreting a required part of the axiomatization? Or is it rather like a *fifth wheel* that one does not need, crams into a corner and forgets? Regarding the interpretations themselves, what difference does it make if they are subjected to such separation from the process of axiomatization?

Debreu illustrates the separation of theory and interpretation with a strong image of a theory having blood, flesh, and bones. He talks about an "acid test" that economic theories have to pass in order to be called rigorous—an "acid test of removing all their economic interpretations and letting their mathematical infrastructure stand on its own" (Debreu 1991, 3). Acid is put on the body of theory, corroding all the flesh of meaning and leaving the structural skeleton behind. Our flesh is what makes us sensible beings, responsive to touch, and thus vulnerable to offence. Accordingly, an axiomatized theory might have a strong backbone, yet has nothing to carry, is insensitive to its surrounding, and immune to criticism. And so, Debreu says, "As a formal model of an economy acquires a *mathematical life of its own*, it becomes the object of an inexorable process in which rigor, generality, and simplicity are relentlessly pursued" (1986, 1265, emphasis added). An axiomatized theory may have a life, but it is a life without affection—like a skeleton haunting economic science since 1959.

Debreu's corrosion of interpretation is in fact radical. By using the terms 'content' and 'interpretation' of theories interchangeably he implies that interpretations are not interpretations of things that are independently given without, or before, being interpreted, as most scientists have believed since Bacon. Economic content is nothing but interpretation. This sounds like a hermeneutical claim. Debreu, however, equates content and interpretation not in order to highlight how the theorist is enmeshed in the material he attempts to grasp, but rather to let interpretations appear as *things*. In light of an axiomatization, interpretations are no longer *activities*. They are themselves *things* to be *discovered* "whenever a novel interpretation of primitive concepts is discovered" (Debreu 1986, 1265). The act of interpreting simply disappears from the stage of rigor. Interpretations lie around in the world waiting to be discovered: the task of economists is then to pick them up and to fill them into structures in order to give those a consistent shape—like a Taylor-designed worker, as Bourbaki did not want to admit about the 'working mathematician'.

> One could say that the axiomatic method is nothing but the 'Taylor system' for mathematics. This is however, a very poor analogy; the mathematician does not work like a machine, nor as the workingman on a moving belt; we cannot over-emphasize the fundamental role played in his research by a special intuition* […]
>
> *Like all intuitions this one also is frequently wrong (Bourbaki 1950, 227).

That little footnote reveals the full ambivalence of the status of theoretical practices in the axiomatic method. Explications are excluded from intellectual practices: in order to be rigorous, one needs to be indifferent to all expressive, descriptive, and explicative practices.[7] A 'rigorous interpretation' or 'rigorous reformulation' is inconceivable. Interpretations are not more or less suited, more or less accurate, but only vague. To be rigorous is to rigorously avoid the question 'what does this mean?' As a consequence, the sources of meaning in the practice of economics are never confronted when doing mathematical economics. This indifference to everything one usually associates with theoretical activity is the core of the axiomatic separation discussed in the preceding pages. Interpretations are not made. They are just there.

Weintraub is moderate when speaking of Debreu's "take-no-prisoner attitude when it came to specifying the 'economic' content of the

[7] Within the textual hierarchy of the axiomatic method one never arrives at interpretations, but as Bourbaki said, at "remarks": "definitions, axioms, theorems, propositions, lemmas, corollaries, remarks" (Bourbaki 1968, v). More cannot be said.

exercise" (2002, 116). But Debreu not only takes no prisoners, as though a commissar of economics, but also indirectly undermines and discourages interpretations. Not only are all interpretations potential economic theories, but all interpretations are also epistemically equivalent. Even if Debreu believed that there is no economics without interpretive efforts, he provided no means for encouraging them to be epistemically relevant. Rather than acting as a democratic, pluralist diplomat, Debreu wiped out the need for evaluating economic theories. But only by means of such evaluations could something of value be at risk for the economist.

3. THE FOUR VIRTUES OF THE AXIOMATIC METHOD AND THEIR SUPPLEMENT: THE ECONOMIST

According to this image of the axiomatic method, it must seem like a miracle that Debreu was able to justify the advantages of rigor and the axiomatic method to the economist. There is nothing left of what one would naturally associate with the practice of economic theory. In fact when it comes to selling the advantages of the axiomatic method, Debreu continuously backs away from the separation of mathematical form and economic content just described. Each advantage can only be established by smuggling other intellectual values than axiomatic rigor back into the analysis. There are four advantages Debreu repeatedly refers to: generality, weakness of assumptions, clarity of expression, and freedom from ideology.

Generality

"The pursuit of generality in a formalized theory is no less imperative than the pursuit of rigor" (Debreu 1986, 1267). How are the rigor and generality of GET related?

GET is usually called general because it is not restricted to a particular market such as the market for apples, oil, kidneys or the market for Gran-Vitara-AWD-metallic-blues. GET is general in that it encompasses all markets, since only children, politicians, moralists, and marketing experts believe that one market is independent from another. In 'the economy' everything depends on everything else. Generality as a virtue in the philosophy of science, moreover, refers to the explanatory scope of theories, and corresponds to the old ideal of explaining much by little.

Debreu gives us the impression that GET gains generality by an axiomatization in this last sense of explanatory scope—that is, that GET extends the limits of other theories and is applicable to a greater range of phenomena. "A newly discovered interpretation", he claims, "can then increase considerably the range of applicability of the theory without requiring any change in its structure" (1991, 5). In the same terms, he even presents the axiomatic method as *appropriate* to the ontic properties of the market.

> A global view of an economy that wants to take into account the large number of its commodities, the equally large number of its prices, the multitude of its agents, and their interactions requires a mathematical model (Debreu 1991, 3).

Perhaps Debreu really believed that the economy is complex. But did this belief actually inform his work?

Debreu tops this assertion of the propriety of mathematics by claiming that mathematics is "neutral" because commodities and prices *are* numerical things: "Since economics gives a central role to quantities of commodities and prices, the use of mathematics seems entirely neutral" (in Feiwel 1987, 253). But on what grounds does Debreu refer to this quantitative reality of commodities and prices? Are we now to enter a discussion of the ontology of commodities?

> [T]here is no firm evidence that prices, commodity units and money were ever constituted as numbers in some pristine ontological sense: they were (and still are) contingent upon a whole range of other social practices, might be reorganized in a myriad of ways, and exhibit no 'natural' or stable mathematical character (Mirowski forthcoming).

Did Debreu, in other words, really believe that commodities could be convex in any sensible way? In what sense is generality achieved by the axiomatic method?

The actual meaning of Debreuvian generality is not that of traditional philosophy of science. The axiomatized GET does not encompass several market theories of particular markets, but is independent of them. How could a theory with a structure that is independent of its (referential) meaning be called general? Debreu relies here on a confusion between generality as the encompassment of content, and formality as the absence of content. Abstraction and formalization are two different practices. What Debreu praises as generality is freedom from the logic of the particular and general. By 'generality' he means that one can generate a market theory out of any interpretation of the primitive concepts *immediately* and *effortlessly*, or as Bourbaki said, "without forging one's means personally" (1950, 227). As Debreu put it in his Nobel lecture: "The axiomatization may also give ready answers to new questions when a novel interpretation of primitive concepts is discovered" (Debreu 1984b, 275). The economist is able to immediately leap over from an interpretation to a fully developed 'theory' without any effort of generalizing. Debreu thus unwittingly admits that his formalism makes theoretical *efforts* redundant. With the axiomatic method the theorist can be substituted for just like the primitive concepts—effortless economics.

The example Debreu repeatedly refers to in order to illustrate, and celebrate, the virtue of generality, is markets with uncertainty (developed in chapter six of his *Theory of value*). The difference between certainty and uncertainty makes the world for a group of economists such as the Austrian, institutionalist, evolutionary, and behavioral economists. In these cases, uncertainty challenges economic theorizing since the market cannot be fully determined let alone be listed in advance within a unique 'universe of discourse'. In Debreu, however, whether commodities are certain, in the sense that we know everything about them, or uncertain because of *time*, is a matter of the interpretation of the primitive concept of 'commodity'. Uncertainty does not affect the axioms themselves. Debreu does acknowledge the importance of the difference between certainty and uncertainty, but cannot incorporate it within the axiomatic scheme: "Several important questions left unanswered are emphasized below [in chapter six]. One may stress the certainty assumption made, at the level of interpretations" (1959, x). On that level, however, nothing really happens since, "by a simple reinterpretation of the concept of commodity", one immediately leads to a theory of uncertainty (1991, 5). The problem of uncertainty is solved by moving it outside of theoretical concern. Weintraub assesses the value of such re-interpretations as follows:

> Debreu's evident enthusiasm […] over his capacity to incorporate 'uncertainty' into the axiomatized model by keeping the identical mathematical formalism but redefining the 'interpretation' of the commodity thus should not be regarded as a new contribution to the economic theory of risk or ignorance; rather, in this reading, Debreu

developed it as a ratification of the structural character of his axioms (Weintraub 2002, 121).

Debreu used uncertainty as a ratification of his method. But was a ratification needed? Could issues of uncertainty have touched Debreu's 'mother structures'? No; the application of uncertainty was rather a way to show that in fact nothing happens if one reinterprets, and that a reinterpretation is actually not needed for GET to stand. Reinterpretations do not make a difference—disillusioning, to say the least, for those who want to attack, or defend, GET on the basis of its supposed economic meaning.

The weakness of assumptions

Close to the virtue of generality is the weakness of assumptions. Recall that assumptions in Debreu are not weak in relation to a basic belief of the theory—its 'ontology'—as discussed in the philosophy of science. In Debreu, the weakness of assumptions is expressed in terms of the Bourbakian hierarchy of mathematical structures, the weakest of all assumptions being $x \in X$. To say that the assumption of transitivity is weaker than that of continuity is to say that transitivity is mathematically implied by continuity but not vice versa. In terms of cognitive capacities, for example, it could be the other way around.

Though clearly a matter of mathematical structures, Debreu gives his audience the impression that these structures are related to the domain of applicability: "The mathematician's compulsive search for ever weaker assumptions is reinforced by the economist's awareness of the limitations of his postulates", as he describes the interaction of mathematicians and economists (1986, 267). But what is the effect on the 'domain of applicability' if the mathematician "expurgated superfluous differentiability assumptions from economic theory" (1986, 267)?

The issue in the background here is what came to be known as *economics imperialism*: the infusion of economic ideas into other social sciences and economic talk in general. Economics imperialism is problematic and different from a fruitful interdisciplinary effort because economists lose their sense of an economic domain and enter other domains without caring about their characteristics, that is, without sensing resistance when passing borders. Ironically, on the grounds of the confusion of assumptions and axioms Debreu could argue that the axiomatic method limited economics imperialism.[8]

> The exact formulation of assumptions and of conclusions turned out, moreover, to be an effective guide against the ever-present temptation to apply an economic theory beyond its domain of validity (Debreu 1986, 1266).

Do we sense the irony in these lines? Debreu turned perhaps the greatest vice of post-war economics into a virtue of his method—to restrict it. How could he possibly believe that his axiomatized GET *ever* functioned as such a regulative device to maintain closeness to a particular domain of economic life?

Gary Becker could be said to have received the Bank of Sweden Prize for showing that Debreu not only proved that the logic of the market is independent of its interpretation, but that market theory is not even restricted to the phenomenon of markets. In this respect, Becker goes further than Debreu, since he uses the lack of interpretation of the market as a vehicle to turn market theory into a method. Without Debreu, however, this step would have been impossible. Mirowski puts the link between the axiomatic method and economics imperialism in the following words:

> The practical effect of the Cowles program was to "toughen" up the mathematical training of economists and thus repel anyone trying to trespass from another social science […] What Cowles ultimately sought to do was to shore up the boundaries between neo-classical economics and the other social sciences; pending that, transcendental urge was re-conceptualized as the periodic forays of the economic imperialists, bringing back home raw materials wrest forcibly from the natives as fuel for their stationary engine of analysis (Mirowski 2001, 266-267).

Contrary to his own assertion, most economists would certainly see Debreu's influence in their attempts to *regain* a sense of the economic home domain—notably by relaxing the rather strong assumptions that

[8] Debreu shares here the same hope as Koopmans who, already in 1957, had written about the 'sobering effect' of rigor: "The best safeguard against overestimation of the range of applicability of economic propositions is a careful spelling out of the premises on which they rest. Precision and rigor in the statement of premises and proofs can be expected to have a sobering effect on our beliefs about the reach of the propositions we have developed" (Koopmans 1957, 147). Sobering yes, but a 'safeguard against overestimation'?

came to be assigned to Debreu's model, such as perfect knowledge, perfect rationality, symmetric information, etc. Ironically, Debreu could present these attempts at escaping the narrowness of his GET as evidence for his method serving as a *benchmark*.

> Its role as a *benchmark* was also perceived clearly, a role which prompted *extensions* to incomplete markets for contingent commodities, externalities, indivisibilities, increasing returns, public goods, temporary equilibrium (Debreu 1986, 1268, emphases added).

The economists' sensibility for an economic domain, which they try to regain when they engage in such theorizing, could still be read by Debreu as an "extension" of his axiomatic GET—a quasi-application. Since the weakness of assumptions is measured not in ontic but in mathematical terms, relaxing the supposed economic assumptions of GET does not change the mode of theorizing. Therefore, even if market theories start with an intuition about the (ontic) domain of the market they can turn out analytically equivalent to Debreu's GET. For this reason even the alternatives to GET reinforce its underlying standards of rigor, as is most apparent in the case of behavioral economics. The more economists struggle to be 'realistic', the more they 'extend' Debreu's structure. Debreu thus brought a negative closure to economics.

Clarity of expression

Perhaps the most salient excuse for mathematical economics after 1945 was Samuelson's catchphrase that mathematics is a language, with which one says the same thing, just more clearly (Samuelson [1947] 1961). Debreu also promoted this belief:

> Still another consequence of the axiomatization of economic theory has been a greater *clarity of expression*, one of the most significant gains that it has achieved. To that effect, axiomatization does more than making assumptions and conclusions explicit and exposing the deductions linking them. The very definition of an economic concept is usually *marred by a substantial margin of ambiguity*. An axiomatized theory *substitutes* for that ambiguous concept a mathematical object that is subjected to definite rules of reasoning. Thus an axiomatic theorist succeeds in *communicating the meaning* he intends to give a primitive concept because of the completely specified formal context in which he operates (Debreu 1986, 1266, emphases added).

Whence, out of the blue, this expressiveness? How can one communicate an intended meaning with a language that is separated from any meaning? That is a sheer impossibility as long as primitive concepts are not conceptualized, but *substituted* for with a mathematical object. The only thing clear in Debreu is the separation of structure and meaning so that meaning is identified with ambiguity. Debreu of course knew that axioms are not expressive. He even made the separation of mathematical form and economic content typographically visible in his *Theory of value*. "In order to bring out fully this disconnectedness [of theory from its interpretations], all the hypotheses, and the main results of the theory, in the strict sense, are distinguished by italics" (1959, x). Only in the "(s)mall type passages", which are "irrelevant for the logical developments of the text proper", is it "permissible to draw upon an intuitive knowledge of the physical world" (Debreu 1959, 2).[9] Bourbaki preferred asterisks to mark these passages, the "omission" of which "of course, have no disadvantage, from a purely logical point of view" (Bourbaki 1968, v). The point is, however, that Debreu in this way not only substitutes the "substantial margin of ambiguity", but renders meaning altogether inexpressible.

Nevertheless, Debreu was very successful in making the economist believe that his axiomatization implies "clarity of expression". The margins of economic theories are still narratives today, while the analytical core consists of formal modeling. Intellectual efforts in economics do not take place in literary passages. But did this enhance communication among economists? To some extent, yes. The more mathematics, the less need for literary skills, and the easier to communicate beyond the cultural noise of ordinary languages. On the other hand, what is the Bourbakian Esperanto of $x \in X$ good for if it is free from expressiveness? What is the clarity of language good for if disagreements, for example, become impossible? Heilbroner and Milberg argued that one symptom of the 'crisis of vision' is that the economic "discipline appears to be less and less [...] a matter of general agreement" (1995, 15). If everything one can say scientifically in economics has already been said implicitly, how reluctant must an

[9] Note the subtlety: like a Bourbakian slip, Debreu notoriously speaks of the "physical world" when it comes to referential meaning, as though he never even considered that economic theory refers to the *economic world*.

economist be to listen to someone else? The drawback of the clarity of expression is a reluctance to engage in debates.[10]

Free from Ideology

The last advantage is the most subtle to interpret, namely that "economic analysis was sometimes brought closer to its ideology-free ideal" (Debreu 1986, 1266). Debreu illustrates this with the *interpretation* of the two welfare-theorems:

> Foes of state intervention read in those two theorems a mathematical demonstration of the unqualified superiority of market economies, while advocates of state intervention welcome the same theorems because the explicitness of their assumptions emphasizes discrepancies between the theoretic model and the economies that they observe (Debreu 1986, 1266).

Up until the formalist revolution the political meaning of the welfare implications of GET had been debated in such terms, most prominently in the socialist calculation debate. Since then, by and large, the discussion has calmed. But has the issue been resolved? Is economics free from ideological issues because it has resolved them scientifically? Did it establish an epistemic authority that all political parties agree on? Have economists ever been taken seriously as political judges? Certainly not Debreu, although in his philosophical naivety he, too, evinced the dream of rigorous blackboard politics:

> The theory that we are discussing tries to be ideologically neutral. It deals with problems that are basic and common to all economic systems, for instance the efficient allocation of resources through decentralized procedures […] Mathematical models of the economy help to analyse the optimal extent of this decentralization. The risk

[10] Note the difference to the following argument that rigor constrains the range of discussable problems: "The desire to derive arguments rigorously means that they [economists] are confining themselves to saying what these theoretical tools allow them to say. Given the state of the techniques available to economists, pursuing this form of rigor has severely constrained what economists have been able to say" (Backhouse 2005, 383). Backhouse does grant the theoretical tools of rigor expressiveness in some limited domain (we know the song: allocation instead of distribution, competition instead of industries, and so on). It is again by virtue of the secondary narratives that have been assigned to these tools that this is so. The actual problem, however, was not that rigor constrained the range of problems, but that it made the economist *forget* the economic problems since one never faces them while theorizing.

> of misinterpretation […] is lessened by the uncompromising exactness of the modelization (Debreu, in Feiwel 1987, 246).

The ideologically neutral 'decentralization of the allocation of resources'? What then is the "optimal extent of this decentralization"? 65%? And is the optimization function 'smooth'? In a late interview Debreu's actual attitude—aloof from political debate rather than concerned to resolve it—is most apparent:

> [C]onsider for example the Pareto optimum of a general equilibrium. It gave rise to conceptual discussions between the liberal economists who said: "Ah! Voila, this is the proof!" and other, rather Marxist oriented economists who said: "Ah! Voila! The assumptions that one has to make for a Pareto optimum are never met!" I simply took the following stance: You can derive whatever conclusions you want from the assumptions. If it satisfies liberal economists and Marxists, too: Perfect! There is nothing better I could ask for. Intellectually you are carried by the current of ideas, and you simply end up where this current will take you (Debreu, quoted in Bini and Bruni 1998, my translation from the original French).

Marxian? Liberal? Both? *Parfait*! Economists can argue in favor of or against capitalism 'by simply reinterpreting the primitive concepts'. *QED*, economics is a science!

The confusion Debreu's claim relies on is clear. Debreu did not *solve* a political problem by any epistemic means. He rather de-politicized economics. What Debreu celebrates as the liberation from ideology is freedom from political relevance. Only in this sense is economics a science, and *not* in accordance with any standard of the philosophy of science, as I have been arguing. Economics is systematic, yes, but not systematic *knowledge*. Debreu proved rigorously that the authority of rigor supports no political interpretation of GET. What Debreu hurrahs—that economics is not (politically) biased—others, at the same time, began to hoot at—the fact that economics is (politically) irrelevant. Debreu's work marks the turn in economics from the suspicion of ideology into the lament of insignificance.

But what then about the 'the risk of (political) misinterpretation' Debreu mentioned? Was Debreu's discreet intervention really so sobering that it disillusioned all associations of GET with political meaning? At least one economist of the neo-Walrasian community, Frank Hahn, thought so and sought to use the formalist void of GET for a critical purpose:

> [T]his negative role of Arrow-Debreu equilibrium I consider almost to be sufficient justification for it, since practical men and ill trained theorists everywhere in the world do not understand what they are claiming to be the case when they claim a beneficent and coherent role for the invisible hand (Hahn 1974, 52).

According to Hahn, GET proves rigorously, precisely in its axiomatized form, what one *cannot* argue with it.[11] GET is critical about political misunderstandings. But the political misunderstanding of what? Of GET itself! With Debreu, GET clears up the misunderstandings that happened during its own tradition, and thus to a great extent the misunderstanding *of* this very tradition, namely that there is an actual political claim to be made about the invisible hand. Debreu proved, in other words, that if GET ever was bestowed with meaning, this meaning did not stem from an epistemic concern, but from ideological motives. Taking Hahn's stance seriously, Debreu showed that GET as an economic theory could only be ideological!

So, was Debreu successful in the sense Hahn envisioned? Are all misunderstandings cleared up? It is true that the association of mathematical rigor with the full determinability of the economy, and thus scientific socialism, is outmoded. This happened, to say the least, no less because of McCarthy's violent politics than Debreu's sobering Bourbakism. But how about the other misunderstanding, that economists associate the intellectual elevation of economic theory with liberal virtues? Is it not the riddle of post-war economics that despite its internal complaint of being politically irrelevant, economics came to be associated with a neoliberal advocacy of the market? In the preceding pages I noted such ideological 'infestation' of formalism in economics at several points. What then is the logic of Debreu's 'axiomatic liberalism'?

Some, particularly Marxists, such as James Cypher, did charge Debreu directly with intentionally supporting neoliberal policies:

> Most of the orthodox modelling of the effects of NAFTA has been based on either some or all of the assumptions relevant in the construction of a proof of the existence of general equilibrium under perfectly competitive (Walrasian) conditions. […] In the briefest from, this construct assumes that all markets clear (therefore, by

[11] Blaug called Hahn's move a "ju-jitsu" defense of GET (2003, 152). Although Blaug agrees that "the best way *not* to learn how markets function […] is to study general equilibrium theory" (2003, 154), he did not appreciate that this negative role can actually be critical.

> assumption there is no unemployment), all products are divisible, there are rational maximizers of independent utility functions, all firms face competitive factor and product markets, all participants are endowed with perfect knowledge (costly attained), banking and finance operate seamlessly thanks to perfect knowledge of the future (Cypher 1993, 153).

"Assuming the mantle of scientific objectivity", economists "introduce only those assumptions which enable modellers to 'prove' that Free Trade Agreements are mutually beneficial (Cypher 1993, 146)." But since all the assumptions are either wrong or at least distortions NAFTA is ill-founded. The Marxists Resnick and Wolff argued on the same grounds that "in the award to Professor Debreu, the Nobel committee made a choice between the two traditions [class and non-class theories]" (1984, 30). The assumptions of neoclassical GET exclude the consideration of class. Yet after the preceding discussion it should be clear that Debreu never spoke about any of these 'assumptions', nor gave any public sign of considering them. The alliance between formal economics and neoliberal politics, therefore, is not a matter of ontological suppositions.

The same applies to a more modest way of linking Debreuvian rigor and neoliberalism, as attempted by Roger Backhouse.

> The conventional view is that the use of mathematics protects economists from ideology, as well as from being accused of being driven by ideology. However, there is another case that can be made. This is that the intellectual value judgments that underlie technical economics, *as it currently exists*, bias one toward conservative conclusions. […] Individual optimisation and perfect competition have been, for the most part, adopted not because economists believe them to be correct but because they permit rigorous analysis (Backhouse 2005, 382n, emphasis added).

Is rigor biased towards neoliberalism? Why? Because, according to Backhouse, theories easily utilizable for neoliberal politics are *by chance* just the same as those that are easily amenable to the intellectual value of rigor. Other values and other techniques (e.g., simulation as opposed to axiomatics) could be biased in another political direction. Are economists neoliberals merely by virtue of *analytic convenience*? Are they irresponsible enough to pay the costs of supporting this or that politics merely for the sake of maintaining rigor? No! At least, this charge cannot be directed at Debreu. Backhouse does not consider that the same intellectual virtue of rigor can also evoke beliefs in the ontological transparency and determinability of the economy, and indeed has been biased for most of the 20th century towards socialism. Rigor does not support a particular political ideology for methodological reasons.

The logic of axiomatic liberalism, as suggested in the previous pages, is rather that of a contingent association: scientific aloofness and discreetness—the distance one takes *from* politics—plays out *in* politics as a *particular* political program for *freedom*. Only there, on an *associative* level, can one link the separation of content and form with the separation of politics and markets. The feeling of formal aloofness nourishes neoliberal imaginaries of the harmless self-policing of markets. In Mirowski's words:

> A mathematized world—say, a mathematized economy—by extension then also seems capable of policing itself, since it is being portrayed as existing independently of the way any analyst might characterize it, puttering along on its own terms (Mirowski forthcoming).

The obvious question is how this "by extension" takes place. One thing became clear in the preceding discussion: Debreu cannot be blamed. His Bourbakism does not justify an alliance between scientific monism and neoliberal hegemony. By not making any economic claims Debreu did not claim a truncated version of liberalism. He separated mathematical form and economic content as a way to avoid that association. Blame should be laid on others—those, who outside the production centers of mathematical economics are nevertheless able to utilize it as the epistemic authority for this or that political interest. Identifying these others is not the present task.

Important for the role of the axiomatic method is rather whether the Debreuvian economist can possibly take *responsibility* for such utilizations. Does the axiomatic method allow economists to reflect on, and possibly prevent, the ideological use of their work? As long as the self-understanding of economists is to be prior, beyond, aloof, or otherwise separated from the political meaning of their work, the political use of their authority will always work against them. And this is the irony of Debreu's assertion that he freed economics from ideology: precisely because Debreu felt himself to be free from it, others could freely find some murky ways to mobilize the aloofness of rigor as a symbol of the superiority of markets. The problem of 'Debreuvian economics' is that economists do not feel responsible for the many uses of economics in messy political and social discourse. And this applies as much today, after GET and Bourbakian rigor, as it did half a century ago.

After 1983 Debreu himself was confronted with such misunderstandings which his work, as opposed to Hahn's hope, could not clear up. After he celebrated the beneficial consequences of the invisible hand of formalism at the Nobel festivities he must have felt its rather painful consequences. The suspicion of ideology (that makes Gerard Debreu a tragic character) was not stilled, but to the contrary, was reinforced by the appearance of being free from it. After all, are cultures of suspicion not reinforced by the presence of those who declare they are beyond them? After Debreu had avoided political questions for his entire life, following 1983 they fell upon him with the unbearable weight of the Nobel ethos: 'Mister Debreu', he was asked by the entire world, '*What does that mean*?' What does it mean that you have proven that "the market works automatically" (as the Nobel committee announced in its press release)? Should we position more or less rockets toward the East? Debreu was addressed as an authority of meaning, not of structures. For nobody in economic talk was ever interested in structures! It was because of such questions that Debreu became explicit about his methodology after 1983. And it was for this reason that he insisted on the strict separation of mathematical form and economic content, excusing himself with a voice of guilt: 'Sorry, I did *not mean* that'.

REFERENCES

Amariglio, Jack, and David Ruccio. 2003. *Postmodern moments in modern economics*. Princeton: Princeton University Press.

Anderson, Robert. 2005. Gerard Debreu. *The Econ Exchange: News and Notes from the Department of Economics University of California*, Berkeley, 8 (1): 6-10.

Arrow, Kenneth, and Gerard Debreu. 1954. Existence of an equilibrium for a competitive economy. *Econometrica*, 22 (3): 265-290.

Backhouse, Roger. 2005. The rise of free-market economics: economics and the role of the state since 1970. *History of Political Economy*, 37 (Supplement 1): 355-392.

Bini, Piero, and Luigino Bruni. 1998. Intervista a Gerard Debreu. *Storia del Pensiero Economico*, 35: 3-29.

Blaug, Mark. 2003. The formalist revolution of the 1950s. *Journal of the History of Economic Thought*, 25 (2): 145-156.

Bourbaki, Nicolas. 1949. Foundations of mathematics for the working mathematician. *The Journal of Symbolic Logic*, 14 (1): 1-8.

Bourbaki, Nicolas. 1950. The architecture of mathematics. *The American Mathematical Monthly*, 57 (4): 221-232.

Bourbaki, Nicolas. 1968 [1939]. *Elements of mathematics: theory of sets*. Reading (MA): Addison-Wesley.

Clower, Robert W. 1995. Axiomatics in economics. *Southern Economic Journal*, 62 (2): 307-319.

Corry, Leo. 1992. Nicolas Bourbaki and the concept of mathematical structure. *Synthese*, 92: 315-348.

Corry, Leo. 1997. The origins of eternal truth in modern mathematics: Hilbert to Bourbaki and beyond. *Science in Context*, 10 (2): 253-97.

Cypher, James M. 1993. The ideology of economic science in the selling of NAFTA: the political economy of elite decision-making. *Review of Radical Political Economics*, 25 (4): 146-164.

Debreu, Gerard. 1959. *Theory of value: an axiomatic analysis of economic equilibrium*. New Haven, London: Yale University Press.

Debreu, Gerard. 1984a. Banquet speech. In *Les Prix Nobel*, ed. Wilhelm Odelberg. Stockholm: Nobel Foundation.

Debreu, Gerard. 1984b. Economic theory in the mathematical mode. *The American Economic Review*, 74 (3): 267-278.

Debreu, Gerard. 1986. Theoretic models: mathematical form and economic content. *Econometrica*, 54 (6): 1259-1270.

Debreu, Gerard. 1991. The mathematization of economic theory. *The American Economic Review*, 81 (1): 1-7.

Dierker, Egbert. 1974. *Topological methods in Walrasian economics*. Berlin: Springer.

Feiwel, George. 1987. Oral history II: an interview with Gerard Debreu. In *Arrow and the ascent of modern economic theory*, ed. George Feiwel. London: Macmillan, 243-257.

Giocoli, Nicola. 2003. Fixing the point: the contribution of early game theory to the tool-box of modern economics. *Journal of Economic Methodology*, 10 (1): 1-39.

Hahn, Frank. 1974. *On the notion of equilibrium in economics*. London: Cambridge University Press.

Hands, D. Wade. 1985. The structuralist view of economic theories: a review essay, the case of general equilibrium in particular. *Economics and Philosophy*, 1: 303-335.

Heilbroner, Robert L., and William S. Milberg. 1995. *The crisis of vision in modern economic thought*. New York: Cambridge University Press.

Ingrao, Bruna, and Giorgio Israel. 1990. *The invisible hand: economic equilibrium in the history of science*. Cambridge, MA: MIT Press.

Jorland, Gerard. 2000. The coming into being and passing away of value theories in economics (1776-1976). In *Biographies of scientific objects*, ed. Lorraine Daston. Chicago: University of Chicago Press.

Khan, M. Ali. 1993. The irony in/of economic theory. *MLN*, 108 (4), 759-803.

Kirman, Alan. 1989. The intrinsic limits of modern economic theory: the emperor has no clothes. *The Economic Journal*, 99: 126-39.

Koopmans, Tjalling. 1957. *Three Essays on the state of economic science*. New York: McGraw-Hill.

Mas-Colell, Andreu. 1974. An equilibrium existence theorem without complete or transitive preferences. *Journal of Mathematical Economics*, 1 (3): 237-246.

Mirowski, Philip. 2001. *Machine dreams: economics becomes a cyborg science*. Cambridge: Cambridge University Press.

Mirowski, Philip. Forthcoming. The unreasonable efficacy of mathematics in economics. In *Elsevier Handbook of the Philosophy of Science*, *13: Philosophy of Economics*, ed. Dov M. Gabbay, et al. Amsterdam: Elsevier.

Mirowski, Philip, and E. Roy Weintraub. 1994. The pure and the applied: Bourbakism comes to mathematical economics. *Science in Context*, 7 (2): 245-272.

Ramrattan, Lall, and Michael Szenberg. 2005. Gerard Debreu: the general equilibrium model (1921-2005) in memoriam. *American Economist*, 49 (1): 3-13.

Resnick, Stephen A., and Richard D. Wolff. 1984. The 1983 Nobel Prize in economics: neoclassical economics and Marxism, *Monthly Review*, December: 29-46.

Samuelson, Paul. 1961 [1947]. *Foundations of economic analysis*. Cambridge (MA): Harvard University Press.

Schmeidler, David. 1969. Competitive equilibria in markets with a continuum of traders and incomplete preferences. *Econometrica*, 37 (4): 578-85.

Sen, Amartya. 1973. Behaviour and the concept of preference. *Economica*, 40 (159): 241-259.

Varian, Hal. 1984. Gerard Debreu's contribution to economics. *Scandinavian Journal of Economics*, 86 (1): 4-14.

Vilks, Arnis. 1992. A set of axioms for neoclassical economics and the methodological status of the equilibrium concept. *Economics and Philosophy*, 8: 51-82.

Walker, Donald A. (ed.). 1983. *William Jaffe's essays on Walras*. Cambridge and New York: Cambridge University Press.

Walker, Donald A. 2006. *Walrasian Economics*. Cambridge and New York: Cambridge University Press.

Weintraub, E. Roy. 2002. *How economics became a mathematical science*. Durham and London: Duke University Press.

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Uncertainty and identity: a post Keynesian approach

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Abstract: Marshall's asset equilibrium model provides a way of explaining the identity of entrepreneurs. Keynes adopted this model but transformed it when he emphasized the short-period and volatile character of long-term expectations. This entails a view of entrepreneur identity in which radical uncertainty plays a central role. This in turn deepens the post Keynesian view of uncertainty as ontological in that entrepreneurs' survival plays into their behavior. This paper explores this role-based view of individual identity and uses the analysis to comment on Keynes's ideas for the socialization of investment and euthanasia of the rentier in the last chapter of *The general theory*.

Keywords: identity, uncertainty, Marshall, Keynes, post Keynesian economics, expectations

JEL Classification: B13, B22, D53, D84, E12

> When the capital development of a country becomes a by-product of the activities of a casino, the job is likely to be ill-done (Keynes 1973 [1936], 159).
>
> […] though this state of affairs would be quite compatible with some measure of individualism, yet it would mean the euthanasia of the rentier (Keynes 1973 [1936], 375-376).

Keynesianism in the tradition of Keynes is a theory of a monetary economy in time guided by individuals' expectations of the future. There is no permanent state of rest in a monetary world, and the equilibria that emerge are temporary and transient. But this does not

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imply that everything in the economy is in flux. Not often appreciated is that this view of the economy implies a conception of economic agents as enduring through change. Economic agents act today on expectations about an economy that they themselves expect to face tomorrow. Whether or not their expectations about the economy are fulfilled, and however the economy changes, they nonetheless act on the assumption that they continue as essentially who they are—else it would make little sense to make plans for tomorrow. That is, though the economy is a system of change, and though much also changes in the characteristics of economic agents, including the disappearance of some (through bankruptcy or voluntary withdrawal), when economic agents act, they act as if they retain their respective identities through time. The concept of agent identity, then, is an implicit tenet of Keynesianism and a correlate of the idea of a monetary economy as a system of change. From this perspective, Keynesianism is thus a theory of the economy in time based on the idea that there are agents who survive through time by managing the consequences of time.

Post Keynesians, of course, have extensively investigated the role of expectations in the economy, but relatively little post Keynesian research investigates the properties of economic agents specifically seen as enduring beings, particularly those agents under the greatest burden of negotiating time, namely, entrepreneurs and investors (in contrast to consumers and workers who are generally treated as largely passive agents). However, it can be argued that the theory of uncertainty in Keynes's later thinking and in post Keynesianism offers a basis for explaining the identity of agents when its ontological dimension is emphasized.

Uncertainty in an ontological sense means that what occurs in the world is not predetermined by some set of economic 'fundamentals' underlying behavior (Davidson 1996). This entails that what individuals do today can have an impact on what happens tomorrow, leading them to form expectations about what effects they can have on what happens to them tomorrow. Thus expectations in an ontologically uncertain world have a dual character in that they refer to both identity (of economic agents) and change (in the economy). Accordingly, the basis for investigating the nature of economic agents as enduring beings can be found in Keynes's thinking about individuals' formation of expectations in an uncertain world.

This paper attempts to develop a modest analysis of this last proposition. It does so by looking back to the roots of Keynes's thinking about time in his inheritance of Marshall's thinking about time and subsequent critique of that thinking. The argument builds on my previous work on the identity of individuals, where I proceed in an ontological-criterial manner, evaluating different candidate conceptions of the individual in economics according to whether 'individuals' as they are described can indeed be regarded as distinct and re-identifiable, as is required by the concept of an individual (Davis 2003; Davis forthcoming). I use that framework here, but focus not on individuals in general but rather on the particular type of individual responsible for the central role investment plays in Keynesian and post Keynesian thinking, namely, the individual/entrepreneur, whose identity is explained in both Marshall and Keynes in terms of asset holdings. My general conclusion is that the departure Keynes made from Marshall's view of the identity of the individual/entrepreneur is important for understanding investment in monetary economies guided by individuals' expectations of the future. Thus identity matters to our understanding of the economy.

Of course neither Marshall nor Keynes reasoned explicitly in terms of agent identity. Yet they both made claims about the nature of the entrepreneur that bear on what the identity of the entrepreneur involves. Both their conceptions, moreover, satisfy my individuation and re-identification criteria, though Keynes's view of time and uncertainty in a post-Victorian, post-'fundamentals' world has altogether different consequences for our understanding of the economy. Let me add that an additional implication of the approach taken in the paper is that different types of agents have different types of identities, so that the functioning of the economy needs to be understood in terms of the interaction of identifiably different (or heterogeneous) types of economic agents. I do not discuss this implication here, but restrict the paper to the topic of the identity of the individual/entrepreneur.

The organization of the paper is as follows. In the first section, I briefly review Keynes's thinking about uncertainty and its appraisal in recent post Keynesian economics in connection with the emphasis placed on ontological as opposed to epistemological uncertainty. Here I also attempt to explain why the investigation of agent identity may be of value to post Keynesianism, in order to motivate interest in the argument of the paper. The second section discusses the antecedents of

Keynes's thinking about entrepreneurs in time in Marshall's early theory of asset market equilibria dating back to his 1871 essay on "Money", which Keynes praised. It then uses this discussion to reconstruct a Marshallian theory of the agent identity of the individual/entrepreneur. The third section turns to Keynes's own approach to asset market analysis, emphasizes its departures from Marshall's understanding, and then constructs an alternative view of the agent identity of individuals/entrepreneurs appropriate to Keynes's view of the economy. Section four offers brief summary remarks regarding the status and nature of individual/entrepreneur identity in a world in which Keynesian economic policy dominates.

1. KEYNES ON UNCERTAINTY

Keynes's thinking about uncertainty originally derives from his thinking about the concept of probability and the weight of arguments in his 1921 *Treatise on probability*. In the *Treatise*, uncertainty has both epistemological and ontological dimensions. Regarding his concept of probability, understood to mean the degree of belief individuals may have in uncertain propositions, he distinguishes four cases:

> There appear to be four alternatives. Either in some cases there is no probability at all; or probabilities do not all belong to a single set of magnitudes measurable in terms of a common unit; or these measures always exist, but in many cases are, and *must remain*, unknown; or probabilities do belong to such a set and their measures are *capable* of being determined by us, although we are not always able so to determine them in practice (Keynes 1973 [1921], 33, original emphases).

The first case clearly concerns an ontological claim, and was famously emphasized many years later in Keynes's 1937 defense of his *The general theory of employment, interest and money* (1936), in the *Quarterly Journal of Economics*. There Keynes asserted that with respect to long-term investment decisions, "there is no scientific basis on which to form any calculable probability whatever" (Keynes 1973 [1937], 113). The three other cases are more epistemological in nature. The second concerns non-comparability and accordingly the limits of our knowledge in regard to how probability is to be measured; the third concerns what can and cannot be known regarding probabilities that exist; the fourth concerns the limitations imposed on knowledge associated with our practices regarding data generation and estimation procedures.

Regarding the weight of arguments, Keynes is there concerned with the amount and completeness of the relevant evidence an agent has regarding the probability of a given outcome. Low weight refers to insufficient and/or incomplete evidence, which is an epistemological concern. This concept of weight re-appears in *The general theory* in connection with Keynes's emphasis on how the 'state of confidence' affects investment: "It would be foolish, in forming our expectations, to attach great weight to matters which are very uncertain", whereas, "It is reasonable […] to be guided to a considerable degree by the facts about which we feel somewhat confident" (Keynes 1973 [1936], 148; see Runde 1990).

That Keynes understood uncertainty to be both epistemological and ontological, and placed special emphasis on the latter, is argued by Davidson and others to be particularly important for understanding Keynes's view of the economy (Davidson 1996; also McKenna and Zannoni 2000-2001; Rosser 2001; Dequech 2003, 2004, 2006; Wilson 2009). Where epistemological uncertainty is involved it is possible that individuals may learn the probabilities relevant to their decision-making, but where ontological uncertainty is involved no such learning is possible. In that instance, Davidson follows Shackle (1972) in saying this implies that some states of the world are not predetermined but remain to be determined as a result of the actions we undertake. The economy is nonergodic. Or as Dequech puts it, "under fundamental uncertainty," that is, ontological uncertainty, "the innovator creates new opportunities and new states of the world" (Dequech 2003, 527).

Important to this argument is whether states of the world that are not predetermined are nonetheless possible under the laws of nature, since it can be argued that if they are possible then the innovator cannot really create them. Creativity can still be maintained in a weaker sense as the idea that innovators help bring about particular possible states of the world—which ones depending on their actions—and thus help realize the future under conditions of fundamental uncertainty (see Wilson 2009). Another issue is whether people moderate and reduce uncertainty through recourse to rules of thumb and conventions which tend to determine future states of the world, a matter emphasized by Keynes in connection with his account of investment behavior (Keynes 1973 [1936], 152). In effect, strategies for reducing epistemological uncertainty also reduce ontological uncertainty.

However we assess these issues, it is still fair to say that Keynes's thinking about uncertainty gave special emphasis to ontological uncertainty, and that this gives the economy a nonergodic, historical, or even evolutionary character in which agents' actions play a creative role. Note, then, that this 'creative' role can explain dynamic growth in the economy when 'animal spirits' are high, and it can also lead to quite destructive economic consequences when long-term expectations are disappointed or there is damaging speculative behavior that depresses output. A nonergodic world has no predetermined pathway, and thus our interest lies in what the effects of agents' actions are. Keynes's interest, of course, was in their consequences for output and employment. Yet he certainly knew that behind these aggregate phenomena individuals are also affected, even if this was not a subject he often specifically addressed. Thus, taking economic agents as relatively enduring, might we also ask in parallel fashion how their identities are affected as a result of their actions? If there is no predetermined pathway for the economy, then it seems there is also no predetermined identity pathway for its agents. It follows that we must include in our analysis of undetermined possible future states of the world what may happen to the individuals as well.

I suggest there are two rationales for this extension. One is that it potentially offers a deeper understanding of the nature of long-term expectations. Long-term expectations are often simply treated as subjective, or as perhaps depending on group dynamics and average expectation as in Keynes's beauty contest explanation. But it may be that we can add to this understanding if it can be argued that agents' orientation toward the future reflects a concern regarding the extent to which their identities as entrepreneurs are at risk.[1] The second rationale lies in the possible advantages of better understanding Keynes's reliance on and revision of Marshall's early asset market equilibrium thinking, which Lawlor argues "became Keynes's basic supply and demand meta-theory for asset markets" (Lawlor 2006, 28). My suggestion is that our understanding of this too can be enhanced with a better understanding of the agents concerned with portfolio management. In the following

[1] In my earlier discussion of how Keynes's philosophical thinking developed (Davis 1994) I make interdependent belief expectations central to Keynes's *The general theory* (Keynes 1936) understanding of conventions and average expectation. That argument, however, is not framed in terms of entrepreneur identity, but rather in terms of Keynes's rejection of his own early philosophical thinking as inspired by G. E. Moore's intuitionism (Moore 1903).

section I begin from this latter vantage point, and argue that it offers an early framework for explaining the agent identity of entrepreneurs/individuals. The section after looks at how Keynes revised this asset market equilibrium framework, and comments on the implications this has for thinking about the personal identity of entrepreneurs as creative agents.

2. MARSHALL'S ASSET MARKET EQUILIBRIUM AND ENTREPRENEUR IDENTITY

The 1871 essay on "Money"

The earliest source of Marshall's asset market equilibrium analysis is his 1871 essay on "Money", later published by Whitaker (Whitaker 1975; see Lawlor 2006, 108ff.). Marshall began with a complaint about the monetary theory of his time. He pointed out that the standard explanations of the value of money were not formulated in terms of the same systematic supply-and-demand analysis used in the determination of the exchange value of commodities, but were rather formulated in terms of such things as money's rapidity of circulation or its cost of production. He then argued that it was individuals' determination to hold a stock of money that determined its value, and that these decisions were not made in isolation from their decisions to hold other assets. Consequently, since the demand for all assets involves a balancing of the opposing advantages the individual expects to derive from each, the value of money needed to be determined in terms of its relative advantages and disadvantages compared to all other assets individuals held. Marshall put this in terms of the simple choice one might face between owning a productive asset—his example is a horse—and holding a stock of non-interest bearing coin. Whether one wants the horse or the coin depends on how one chooses to apportion one's wealth given the respective 'conveniences' and 'inconveniences' of these two assets at the margin. The value of money, then, was established in the same way as the value of any other asset through supply-and-demand and marginalist reasoning.

From an equilibrium perspective, individuals are consequently seen as being in a state of equilibrium with respect to their portfolio choices over different wealth holdings. At the same time, however, individual-level equilibrium analysis needs to be accompanied by a market-level equilibrium analysis, since the market values of all assets individuals hold are equalized by the forces of supply and demand in the trading between individuals. Thus Marshall's general asset market equilibrium analysis sees each entrepreneur as being in individual equilibrium, and equilibrium simultaneously obtaining between all entrepreneurs with respect to all the different possible assets people can hold. Moreover, as an analysis of assets the framework is intertemporal. Productive assets can generate returns in the future, and money provides the means for transactions people wish to carry out today (one liquidity motive, as we would call it). Thus as individuals make their portfolio choices they do three connected things: they determine what combination of assets best suits their own individual situations, they make their own positions consistent with those of others, and they do all this over time.

Entrepreneur identity

Let us then treat this analysis as a framework for explaining entrepreneurs' agent identities as manifested by their asset holdings. In the most basic sense, identity analysis is simply an accounting system for keeping track of some kind of distinguishable entity through a process of change that is believed to be important for the purposes of some explanation. If you claim you can refer to some type of distinguishable, persisting entity you think important to your analysis, in principle you need to be able to show what makes that entity a separate and distinct thing in terms of how you have described it, and then show how you can track it as that separate and distinct thing through a process of change that may alter many of its characteristics. Explaining the identity of that entity then makes it possible to go on to argue how it may or may not function as a causal agent, able to affect its environment as well as be affected by it. In economics, of course, we are concerned with economic agents, and in Keynesian and post Keynesian economics we are interested especially in one particular type of economic agent, the entrepreneur, or, in Marshall's framework, the individual managing a set of asset holdings. Thus, explaining individuals'/entrepreneurs' agent identities as manifested by their asset holdings involves mobilizing some essential description of individuals/entrepreneurs that allows us to individuate and track them over time despite change in their non-essential characteristics, such as which particular assets they hold in their portfolios, who they trade with and when, and the like.

I suggest, then, that the 'essential description' of the individual/entrepreneur that Marshall offers in his 1871 essay includes three connected things entrepreneurs do when they make portfolio choices, and which are instrumental to their characterization as distinguishable, enduring entities with agent identities. First and most basically, individuals are distinguishable independent beings in virtue of their exclusive identification with the assets which they own. That is, the system of private ownership for stocks, bonds, real estate, bank deposits, and so on, provides a straightforward means of distinguishing entrepreneurs as independent agents. Second, it nonetheless goes too far to say that entrepreneurs are isolated atomistic beings, since for Marshall the actual values of the assets they own are determined in interaction with other entrepreneurs. As what they are made up of is not just a matter of the assets they hold but also obviously a matter of the value of those assets, entrepreneurs are only relatively autonomous and thus both independent and also interdependent beings. Third, Marshall's entrepreneurs are also enduring, re-identifiable beings in that, whatever the mix of assets they happen to own, their wealth portfolios are always seen as being in equilibrium at any point in time and thus through time as well.

This equilibrium property is crucial because it elicits what is essential in entrepreneurs' identity as asset-holders when there is continual change in the mix of assets they own. Were they to be identified solely as collections of assets without the equilibrium principle, they would then be collections of multiple selves, each different from moment to moment according to changes in their portfolios. But here individuals are enduringly distinct beings, because their identities are tied to their ability to exercise an equilibrium principle regarding the management of their asset holdings—the idea of balancing the conveniences and inconveniences of different assets at the margin. With these three components in mind, then, let us go on to see what further interpretation we can give to this agent identity conception by looking at Marshall's later treatment of time and his distinction between short-term and long-term expectations in his *Principles* (1920).

In the *Principles* Marshall provides a 'real' theory of interest in terms of the demand and supply of capital. Long-term expectations are driven by the productivity of capital which motivates investment decisions, whereas short-term expectations are determined by current production. Further, long-period values, or 'normal' values, reflect the deep underlying factors such as the marginal productivity of resources, marginal disutility of saving, and so forth, that Marshall believed ultimately explain the functioning of the economy. Short-period phenomena depend on other more transient factors, and accordingly adjust in the long run to the former. Applying this to the asset market equilibrium characterization of individuals above, it follows that individuals ultimately guide their lives by long-term expectations regarding their durable investments. That is, the mix of assets in their portfolios reflects thrift and steadfastness in their preference for holding long-term investments, at the expense of liquidity and frequent adjustment to one's holdings.

Hedging and speculation cannot pay off in the long run for Marshall because they are responses to transitory phenomena out of keeping with the fundamentals underlying the economy. Consequently, entrepreneur/individual identities are, as it were, highly secure in that stability in their personal portfolios through time gives their identities an enduring nature. Put differently, as their identities are securely distributed across time by this long-term orientation, despite the continuous process of transitory change in markets, they are effectively 'out' of time. Their equilibrium identity principle, that is, allows them to defeat time by organizing their identities around the deep, timeless values residing in fundamental scarcity relationships that for Marshall hold between human life and nature. We thus might say that this late nineteenth century concept of entrepreneur identity is classically Victorian in that the established values of thrift and hard work associated with that era underlie the pre-eminent role that long-period 'normal' values in Marshall's economic analysis play in individuals' organization of their lives *vis-à-vis* time.

3. KEYNES ON ASSET MARKET EQUILIBRIUM AND ENTREPRENEUR IDENTITY

Keynes on Marshall and asset market equilibria

To understand Keynes's thinking on asset market equilibria, I begin with his adoption and re-assessment of Marshall's distinction between the short-period and long-period. As a Marshallian, Keynes used Marshall's time distinction, but his development of the idea of the economy as a monetary economy made short-period equilibrium the key concept, and not a temporary state ultimately overcome by the gravitational pull of long-period forces as was the case for Marshall. This inversion of Marshall's thinking followed from Keynes's changed view of the character of long-term expectations. Thus for Keynes, as essentially with Marshall, short-term expectations are concerned with the price the entrepreneur can get for finished output, and are generally fulfilled, or revised in a predictable way, in light of market performance. But in contrast to Marshall he believed that long-term expectations, which are concerned with future returns on additions to the entrepreneur's capital stock, were often disappointed, and moreover it is often unclear to the entrepreneur why this was the case. Keynes inferred from this that long-term expectations consequently never really settle down and, absent a rational basis in the calculation of expected returns, are driven by investors' animal spirits. Part of the reason for this was that the rise of stock markets, associated with the historical shift in capital holdings away from privately-held family/entrepreneur firms toward rentier-type investors, made long-term expectations more changeable and unpredictable. The development of stock markets also gave rise to speculative behavior. In contrast to Marshall's late nineteenth/early twentieth century experience, then, it was simply no longer clear what drove long-period expectations. Keynes recognized this historical development, and consequently shifted the focus of Marshallian analysis from the long-period to the short-period to give "the theory of a system in which changing views about the future are capable of influencing the present situation" (Keynes 1973 [1936], 293). In fact, for Keynes there is really no longer any long-period as everything occurs in the present. Rather the long-period, as Lawlor says, is "just a succession of changing regimes of long-period expectation" that impacts us from one present to the next (Lawlor 2006, 19).

Given this, Keynes still held a high opinion of the basic ideas involved in Marshall's monetary theory as well as of the asset equilibrium model on which it depended. In his biography of Marshall (Keynes 1925), the content of the 1871 essay and Marshall's early monetary thinking in general were discussed quite favorably. (Indeed, Keynes specifically requested a copy of the essay from Mary Paley Marshall in order to write the biography.) But Keynes's later development of this framework in *The general theory* also significantly changed it by expanding upon the reasons individuals might find some assets to be 'convenient' to include speculative expectations regarding their possible appreciation.

The idea of speculative expectations, of course, was entirely foreign to Marshall's thinking since it allows for expectations not grounded in real factors but rather in transitory phenomena. It also introduces a dimension into the determination of asset values altogether at odds with Marshall's thinking about individual behavior, since speculation allows opinion to influence individuals (such as Keynes described in his beauty contest example), and draws them away from the economy's fundamentals. For Marshall, the deep factors that determine economic behavior lay in the relationship between human beings and nature, not between human beings per se. That is, social relationships for him needed ultimately to be somehow 'naturalized'.

Keynes on entrepreneur identity

How, then, does all this change the Marshallian entrepreneur identity conception for Keynes? In Marshall's asset equilibrium model of entrepreneur identity, the entrepreneur's identity is sustained across change in the variety of assets that make up the entrepreneur's portfolio through the entrepreneur's preference for holding long-term investments. In effect, if we look at entrepreneur identity in terms of how entrepreneurs position themselves towards time, the particular interpretation Marshall gives to this, by favoring long-term investments, gives individuals an identity through time largely through their minimizing the significance of time. People endure as entrepreneurs because they make choices with respect to their holdings that make time unimportant. However, in inverting Marshall's expectations analysis, Keynes produces quite a different view of entrepreneur identity. As the short-period becomes the only period and time contracts to the present, entrepreneurs shift their portfolios away from long-term commitments, constantly revising the mix of assets that they own. The unsettled character of long-term expectations, then, removes their ability to be 'out' of time, forcing them to be ever 'in' time in the sense that they are ever changing what they own and thus who they are. Accordingly, in Keynes's world entrepreneurs cease to be enduring, re-identifiable agents. Rather entrepreneurs fragment into successions of unconnected episodic selves, where the most that can be said to link each entrepreneur's multiple selves is their common desire for short-term portfolio gain.

Moreover, on Keynes's view entrepreneur identity is always at risk. When entrepreneurs are identified with the assets they own, then, since they no longer maintain long-term positions as the core of their portfolios, should they sustain serious losses they are threatened with elimination as agents and individuals altogether. On an asset identity model of the entrepreneur, that is, their losses are not to a financial portfolio separate from the individual but in fact losses to the individual identified with that portfolio. Thus, just as a financial portfolio might go bankrupt and cease to exist, so might the entrepreneur identified as a portfolio go bankrupt and cease to exist. In our ordinary way of thinking, of course, we maintain a separation in our minds between individuals and what they own, allowing us to imagine that individuals continue and may somehow sustain their identities should they go bankrupt and cease to be wealth owners. But the analysis here does not distinguish between agent identity and personal identity, and indeed in the economic world as Marshall and Keynes described it individuals are subsumed by the roles they play in the economy, so that difference arguably does not exist there either. Thus, in a world that has become thoroughly economic in nature, the risk that Keynesian entrepreneurs face in losing their 'identity' portfolios makes the unsettled character of long-term expectations an even more serious matter.

It is not just an institutional change in the way market economies began to work in the early twentieth century with the rise of stock markets and speculative investing that then underlies Keynes's shift of focus to the present and changed view of long-term expectations. When we take the basis for entrepreneurs' agent identity to be the Marshallian asset equilibrium model, Keynes's changed view of the world also signals a different understanding of the culture of the market system whereby uncertainty becomes a deeply ontological concern for entrepreneurs themselves. For them, accordingly, radical uncertainty is not only about what entrepreneurs cannot know about the future (an epistemological uncertainty), but also ultimately about whether they themselves may even exist in the future (an ontological uncertainty).

Part of Keynes's view, we saw, is the special emphasis he places on the role that opinion plays in the determination of entrepreneurs' asset choices. Given the analysis of entrepreneur identity above in terms of independence, interdependence, and enduringness, what more does this then imply about the identities of entrepreneurs? Note that since opinion is not grounded in timeless Marshallian 'fundamentals' but is rather the product of a social interaction that can produce swings in investor sentiment, it can generate both bull and bear markets. In the former entrepreneurs profit when they go long and hold assets for extended periods, whereas in the latter they profit when they go short by borrowing and selling assets forward. Thus whether entrepreneurs take a long-term perspective going long or a short-term perspective going short is in large part a matter of the state of opinion. And, ironically, opinion-driven bull markets inadvertently produce a Marshallian-like world from the point of view of entrepreneur identity, since they encourage individuals to hold long-term positions.

But Keynes had no confidence that such a circumstance would prevail over any significant period of time. It should not be overlooked accordingly that, in the last chapter of *The general theory*, Keynes argued for 'socializing investment' and for the 'euthanasia of the rentier' as means by which he hoped stability and growth might be brought to capitalist market systems. Of course he was not advocating socialism or state take-over of the economy—"[i]t is not the ownership of the instruments of production which it is important for the State to assume" (Keynes 1973 [1936], 378). Rather, he was interested in whether the state could develop policies and strategies which might influence the nature of entrepreneur behavior by encouraging long-term holding of capital assets and reducing short-termism in the way entrepreneurs approached their asset portfolios. That is, Keynes essentially sought the state's assistance in ensuring a more stable climate of opinion that would channel entrepreneurs' animal spirits in the direction of a more Marshallian-like world.

In terms of the view of entrepreneur identity set forth here, Keynes hoped public authorities might help stabilize the opinion-influenced interdependence component of entrepreneur identity and thereby reframe entrepreneur independence in such a manner as to restore their status as enduring, re-identifiable agents. Markets themselves already threatened to euthanize the rentier. Keynes was willing to lend his assistance, particularly as a step in the direction of ensuring the survival of the entrepreneur as the key economic agent in the capitalist market economy. But this required more realism regarding the social side of the entrepreneur as well as practical measures aimed at changing how interdependence figured in entrepreneur identity.

4. KEYNES AFTER MARSHALL

Thus Keynes is still a Marshallian, albeit one who learned from the history he experienced. He shares the Marshallian entrepreneur identity conception implicit in the asset equilibrium model, but his understanding of the early twentieth century evolution of asset markets caused him to think more deeply than Marshall had about entrepreneur interdependence. This reflects two ways in which Keynes's ontological view of the world was different from Marshall's. First, though the structure of Marshall's model is retained, and though Marshall's long-run normal values are preserved, they are not retained as 'natural' normal values but rather as socially-produced normal values. Contrary to Marshall, human beings play an important role in determining the relationship between the economy and nature and do not find that relationship naturally given to them in the form of a collection of pre-given 'fundamentals'. Second, in a Keynesian world with socially desired economic policy in command, individuals are again 'out' of time, and thus confident in making long-term commitments that ignore the risk of time, but they are so only when they produce consensus in opinion regarding employment and output goals—a matter strictly 'in' time in the sense of requiring social recognition of the need to manage time. That is, as post Keynesians argue, we only succeed in managing the consequences of time and uncertainty when we see the economy as inescapably in time, that is, as a monetary economy.

The Victorian world Marshall inhabited ended in 1914 when it could no longer be said that the values of thrift and hard work explained an economic process embedded in a world of conflict and power. The Victorians saw the world as benign and beneficent, as befit the privilege and illusions of Britain's upper classes which benefited from decades of ruthless colonial expansion that had made its victims invisible. The war that began in 1914 was in part a product of this nineteenth century history, which afterward wrought further damage on the national economies that fought it in the form of economic depression and a second world war.

Keynes was raised in this Victorian world, but by 1918 and Versailles he was immune to most of its illusions, including that thrift and hard work were the natural foundations of economic life. By the end of his life he was even more aware of the nature of the kind of world that had succeeded Marshall's. One aspect of this was his worry about the fragile state of human society, famously expressed in his cautions in "My early beliefs" (1933), and later given more tangible expression in his important contributions to the postwar deliberations at Bretton Woods in 1944. From an uncertainty perspective, more was involved here, I suggest, than his concern about the well-being of the international economic system. Implicitly, he was also concerned with whether individuals were likely to be able to live 'in' time in a world in which they so increasingly identified with the roles they occupied in economic life. It is an important concern, but one that has gone largely unaddressed by economists since Keynes's time.

REFERENCES

Davidson, Paul. 1996. Reality and economic theory. *Journal of Post Keynesian Economics*, 18 (4): 479-508.

Davis, John. 1994. *Keynes's philosophical development*. Cambridge: Cambridge University Press.

Davis, John. 2003. *The theory of the individual in economics: identity and value*. London: Routledge.

Davis, John. Forthcoming. *Individuals and identity in economics*. Cambridge: Cambridge University Press.

Dequech, David. 2003. Uncertainty and economic sociology. *American Journal of Economics and Sociology*, 62 (3): 509-532.

Dequech, David. 2004. Uncertainty: individuals, institutions, and technology. *Cambridge Journal of Economics*, 28 (3): 365-378.

Dequech, David. 2006. The new institutional economics and the theory of behavior under uncertainty. *Journal of Economic Behavior and Organization*, 59: 109-131.

Keynes, John M. 1921. *Treatise on probability*. London: Macmillan; reprinted in *The Collected Writings of John Maynard Keynes, Vol. VIII*. New York: St. Martin's, 1972.

Keynes, John M. 1925. Alfred Marshall, 1842-1924. In *Memorial of Alfred Marshall*, ed. A. C. Pigou; reprinted in *The Collected Writings of John Maynard Keynes, Vol. VIII*. New York: St. Martin's, 1972.

Keynes, John M. 1933. My early beliefs. *Essays in biography*; reprinted in *The Collected Writings of John Maynard Keynes, Vol. X*. New York: St. Martin's, 1972.

Keynes, John M. 1936. *The general theory of employment, interest and money*. London: Macmillan; reprinted in *The Collected Writings of John Maynard Keynes, Vol. VII*. New York: St. Martin's, 1973.

Keynes, John M. 1937. The general theory of employment. *Quarterly Journal of Economics*, 51: 209-223; reprinted in *The Collected Writings of John Maynard Keynes, Vol. XIV*. New York: St. Martin's, 1973, 109-123.

Lawlor, Mike. 2006. *The economics of Keynes in historical context: an intellectual history of the general theory*. Houndsmill, Basingstoke, Hants (UK): Palgrave Macmillan.

Marshall, Alfred. 1871. Money. In *The early writings of Alfred Marshall*, ed. John Whitaker. London: Macmillan, 1975.

Marshall, Alfred. 1920. *Principles of economics*, 8th ed. London: Macmillan.

McKenna, Edward, and Diane Zannoni. 2000-2001. Post Keynesian economics and nihilism. *Journal of Post Keynesian Economics*, 23 (2): 331-347.

Moore, G. E. 1903. *Principia ethica*. Cambridge: Cambridge University Press.

Rosser, Barkley. 2001. Alternative Keynesian and post Keynesian perspectives on uncertainty and expectations. *Journal of Post Keynesian Economics*, 23 (4): 545-566.

Runde, Jochen. 1990. Keynesian uncertainty and the weight of arguments. *Economics and Philosophy*, 6 (2): 275-292.

Shackle, George. 1972. *Epistemics and economics: a critique of economic doctrines*. Cambridge: Cambridge University Press.

Wilson, Matthew. 2009. Creativity, probability and uncertainty. *Journal of Economic Methodology*, 16 (1): 45-56.

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Adam Smith and the contemporary world

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Abstract: This paper argues that many of Adam Smith's insights, particularly those in his *Theory of moral sentiments*, have a relevance to contemporary thought about economics and ethics that is currently underappreciated. In economics, for example, Smith was concerned not only with the sufficiency of self-interest at the moment of exchange but also with the wider moral motivations and institutions required to support economic activity in general. In ethics, Smith's concept of an impartial spectator who is able to view our situation from a critical distance has much to contribute to a fuller understanding of the requirements of justice, particularly through an understanding of impartiality as going beyond the interests and concerns of a local contracting group. Smith's open, realization-focussed and comparative approach to evaluation contrasts with what I call the "transcendental institutionalism" popular in contemporary political philosophy and associated particularly with the work of John Rawls.

Keywords: moral sentiments, Adam Smith, prudence, self-interest, equality, justice, John Rawls.

JEL Classification: A13, B12, B31, D63, O10

It is a great privilege for me to join others here at Adam Smith's home base in Glasgow University to celebrate a profoundly important book first published 250 years ago. The influence of *The theory of moral sentiments* (1759) on philosophy, politics, sociology, and economics over the last few centuries has been quite remarkable. I shall have a bit to say on the nature of that influence, but my primary concentration in this lecture is on the contemporary relevance of Smith's thoughts and analyses—presented no less than a quarter of a millennium ago. While the impact of Smith's *Wealth of nations* (1776) is very widely acknowledged, the far-reaching relevance of Smith's ideas in *The theory of moral sentiments* is quite often comprehensively missed in

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discussions today. It is that neglect that makes the case for addressing Smith's contributions urgent as well as important.

In what way are Smith's contributions of contemporary relevance? This question is hard to answer mainly because there are so many ways in which Smith's ideas have insights to offer to the world today. There are a great many departures that were proposed by Smith that have not been fully taken up yet, despite the frequency with which Smith has been quoted in the literature over the last two centuries and more. The importance of those proposed departures is the principal theme of this lecture.

1

The particular contribution of Adam Smith that is most clearly celebrated today—and has certainly *not* been neglected—is the way he helped to reshape the subject of economics. Smith is standardly accepted as "the father of modern economics", and it is widely acknowledged that he has contributed more than almost anyone else to the emergence of the scientific discipline of economics. I am, of course, aware that to talk about the "scientific discipline" of economics might seem to be a little out of place at this time, given the way the profession is faring right now. Science, in fact, is not quite the first word that comes to our mind given the way economists have bungled in anticipating the gigantic crisis in which we are caught today, and in identifying how we can rapidly rescue the badly botched economic world.

This new scepticism feeds into the old doubts about the possibility of having a "science" called "social science". Economics or sociology may be worthy subjects for speculation and reflection, but can they actually be taken to be a part of the discipline of science? It is difficult not to recall W. H. Auden's (1947) advice to the aspiring academic:

> Thou shall not sit
> With statisticians nor commit
> A social science.

Smith definitely did *commit* a social science—indeed more than one social science. And we certainly know many things about the social sciences in general—and about economics and the market economy in particular—from his work that were far from clear earlier, and which remain of great value today. The debt to Smith is handsomely acknowledged in contemporary economic writings, but unfortunately some of his central ideas are not very well grasped in many of the presentations by his alleged admirers who want to see Smith just as the guru of the market economy: a one-idea man propagating only the excellence and self-sufficiency of the market. I have had the occasion to grumble in a recent essay in the *New York Review of Books* that the popularity of *quoting* Smith seems to far exceed that of *reading* him (Sen 2009a). The one-idea capsule summary of Smith is, of course, very far from what Smith in fact said.

Even as Smith's pioneering investigations explained why (and particularly how) the dynamism of the market economy worked, they also brought out the support that the markets need from other institutions for efficacy and viability. He identified why the markets may need restraint, correction, and supplementation through other institutions for preventing instability, inequity, and poverty.

2

One of the more subtle points of Smith that seems to have been fairly widely missed is his pointer to the impossibility of thinking of poverty without going, at the same time, into inequality. For each person, the income and resources needed for achieving the same minimal functionings and for having the same capabilities continue to grow with the overall progress of an economy and the rise in other people's incomes. For example, to be able to "appear in public without shame" may require higher standards of clothing and other visible consumption in a richer society than in a poorer one, Smith noted. The same applies to the personal resources needed for taking part in the life of the community, and, in many contexts, even to fulfil the elementary requirements of self-respect.

The large modern literature on the sociology of "relative deprivation" essentially develops a point that Smith identified in the *Wealth of nations*.[1] This has important implications for policies for poverty removal and indeed for assessing the process of economic development. An increasingly common tendency in public economics—to say that we should concentrate on removing poverty whereas inequality is a quite different matter—is an unviable position for good

[1] See Smith 1976 [1789], 351-352. On the relation between relative disadvantage and poverty, see the works of W. G. Runciman (1966), and Peter Townsend (1979).

Smithian reasons, and that is a recognition of some importance for policy debates today.

On a different kind of issue, it is striking how insightful Smith was in identifying the destructive influences of those whom he called “prodigals and projectors”. That analysis is, in fact, deeply relevant today in understanding what has just happened in the financial world. The implicit faith in the wisdom of the market economy, which was largely responsible for the removal of the established regulations in the United States, tended to assume away the activities of prodigals and projectors in a way that would have shocked the pioneering exponent of the rationale of the market economy.

It is interesting in this context to note that Jeremy Bentham wrote to Smith a long letter, questioning this part of his analysis and disputing in particular Smith’s remarks about the so-called “prodigals and projectors” (Bentham 1843a). Bentham argued, among his other points, that those whom Smith called “projectors” were also the innovators and pioneers of economic progress. As it happens, Bentham did not manage to persuade Smith to change his mind on this indictment, even though Bentham kept on hoping to do just that, and on one occasion convinced himself, with little evidence, that Smith’s views on this had become the same as his.[2] Smith knew the distinction between innovating and projecting well enough, and gave little evidence of changing his mind on this subject. Now, more than two centuries later, the distinction remains sadly relevant as we try to understand the nature and causation of the crisis that has hit the world of finance.

3

Smith did not take the pure market mechanism to be a free standing performer of excellence. Nor did he take the profit motive to be all that is needed. The importance of motives other than the pursuit of one’s own gain, going beyond even the more refined motivation that Smith called “prudence”, was first outlined by Smith with much force and clarity in *The theory of moral sentiments*. There are really two distinct propositions here. The first is one of epistemology, concerning the fact that human beings are not invariably guided only by self-gain or even prudence. The second is one of practical reason, and involves the claim that there are good ethical and practical reasons for encouraging

[2] See Bentham 1843b, paragraph 426 and footnote.

motives other than self-seeking—whether in a crude or in a refined form.

The latter proposition is one of the strongest concerns in the current debates on the debacle just experienced. It finds perhaps its strongest expression in one part of *The theory of moral sentiments*, when Smith argues that while "prudence" is "of all the virtues that which is most useful to the individual […] humanity, justice, generosity, and public spirit, are the qualities most useful to others" (Smith 1975 [1790], 189-190). The nature of the present economic crisis illustrates very clearly the need for departures from unmitigated and unrestrained self-seeking in order to have a decent society: even John McCain, the Republican presidential candidate in the United States complained constantly of "the greed of Wall Street" in his campaign speeches in the summer of 2008. Indeed, much evidence has emerged powerfully in recent years in that direction, in addition to what we already knew from past studies of the failings of motivational narrowness.

Despite Smith's frequent discussion of the importance of motivations other than self-interest, he has somehow developed the reputation of being a champion of the unique importance of self-interest for all human beings. For example, in two well-known and forcefully argued papers, the famous Chicago economist George Stigler has presented his "self-interest theory" (including the belief that "self-interest dominates the majority of men") as being "on Smithian lines".[3] Stigler was not being idiosyncratic in that diagnosis—this is indeed the standard view of Smith that has been powerfully promoted by many writers who constantly invoke Smith to support their view of society. A great many economists were, and some still are, evidently quite enchanted by something that has come to be called "rational choice theory" in which rationality is identified with intelligently pursuing self-interest. Further, following that fashion in modern economics, a whole generation of rational choice political analysts and of experts in so-called "law and economics" have been cheerfully practising the same narrow art. And they have been citing Adam Smith in alleged support of their cramped and simplistic theory of human rationality.

While some men are born small and some achieve smallness, it is clear that Adam Smith has had much smallness thrust upon him.[4] One

[3] See particularly Stigler 1971, 237; and Stigler 1981, 176.
[4] This issue of misinterpretation is more fully discussed in "Adam Smith's prudence" (Sen 1986); and in *On ethics and economics* (Sen 1987).

reason for the interpretational confounding is the tendency to confuse the question of rationality and the adequacy of self-interest as a motivation with a much narrower question: what motivation is needed to explain why people seek exchange in a market economy? Smith famously argued that to explain the motivation for economic exchange in the market we do not have to invoke any objective other than the pursuit of self-interest. In his most famous and widely quoted passage from the *Wealth of nations*, Smith wrote:

> It is not from the benevolence of the butcher, the brewer, or the baker that we expect our dinner, but from their regard to their own interest. We address ourselves, not to their humanity but to their self-love (Smith 1976 [1789], 26-27).

The butcher, the brewer, and the baker want to get our money in exchange for the meat, the beer, and the bread they make, and we—the consumers—want their meat, beer, and bread, and are ready to pay for them with our money. The exchange benefits us all, and we do not have to be raving altruists to seek such exchange. This is a fine point about motivation for trade, but it is not a claim about the adequacy of self-seeking for economic success in general.

Unfortunately, in some schools of economics the reading of Smith does not seem to go much beyond those few lines, even though that discussion by Smith is addressed only to one very specific issue, namely *exchange* (rather than distribution or production), and in particular, the *motivation* underlying exchange (rather than what makes normal exchanges sustainable, such as trust and confidence in each other). In the rest of Smith's writings there are extensive discussions of the role of other motivations that influence human action and behaviour. For example, Smith argued:

> When the people of any particular country has such confidence in the fortune, probity, and prudence of a particular banker, as to believe he is always ready to pay upon demand such of his promissory notes as are likely to be at any time presented to him; those notes come to have the same currency as gold and silver money, from the confidence that such money can at any time be had for them (Smith 1976 [1789], 292).

Smith discussed why such confidence need not always exist. Even though the champions of the baker-brewer-butcher reading of Smith, enshrined in many economic books, may be at a loss about how to understand the present economic crisis (since people still have excellent reason to *seek* more trade even today—only far less *opportunity*), the devastating consequences of mistrust and lack of mutual confidence would not have puzzled Smith.

Smith also made the point that sometimes our moral behaviour tends to take the form of simply following established conventions. While he noted that "men of reflection and speculation" can see the force of some moral arguments more easily than "the bulk of mankind" (1975 [1790], 192), there is no suggestion in Smith's writings that people in general systematically fail to be influenced by broader considerations—broader than sheer pursuit of self-interest—in choosing their behaviour. What is important to note, however, is Smith's recognition that even when we are moved by the implications of moral arguments, we may not see them in that explicit a form and may perceive our choices in terms of acting according to some well-established practices in society. As he put it in *The theory of moral sentiments*:

> Many men behave very decently, and through the whole of their lives avoid any considerable degree of blame, who yet, perhaps, never felt the sentiment upon the propriety of which we found our approbation of their conduct, but acted merely from a regard to what they saw were the established rules of behaviour (1975 [1790], 162).

This focus on the power of "established rules of behaviour" plays a very important part in the Smithian analysis of human behaviour and its social implications. However, neither specifically reasoned choice nor the following of established rules of behaviour takes us, in Smith's analysis, to the invariable pursuit of self interest. This has huge implications for practical reason in addition to its epistemic merits. Both individual reasoning and social convention can make a real difference to the kind of society in which we live. We are not imprisoned in any inflexible box of the unconditional priority of self-love. The pillaging bosses of perverse businesses (such as AIG) are not doomed to any inescapable pursuit of plunder; they *choose* to plunder in line with their inclinations, making little use of rational scrutiny, not to mention moral reasoning.

4

While Smith's thoughts are of much relevance in explaining the present global crisis and in suggesting ways and means of not only overcoming it but also of building a tolerably decent society in the world, there are other parts of Smith's analyses that throw light on such grand notions as justice and impartiality, subjects of lasting importance. Since I have just completed a book on justice, called *The idea of justice* (Sen 2009b) which draws very substantially on Adam Smith's ideas, I could perhaps be forgiven for spending a bit of time on the lines of analysis that I believe I get from Smith.

Even though the subject of social justice has been discussed over the ages, the discipline received an especially strong boost during the European Enlightenment, in the eighteenth and nineteenth centuries, encouraged by the political climate of change and also by the social and economic transformation taking place then in Europe and America. There are two basic, and divergent, lines of reasoning about justice among leading philosophers associated with the radical thought of the Enlightenment. The distinction between the two approaches has received far less attention than, I would argue, it richly deserves.

One approach, led by the work of Thomas Hobbes in the seventeenth century, and followed in different ways by such outstanding leaders of thought as John Locke, Jean-Jacques Rousseau, and Immanuel Kant, concentrated on identifying perfectly just institutional arrangements for a society. This approach, which can be called "transcendental institutionalism", has two distinct features. First, it concentrates its attention on what it identifies as perfect justice, rather than on relative comparisons of justice and injustice, and it tries to identify social characteristics that cannot be transcended in terms of justice. Its focus is not on comparing feasible societies, all of which may fall short of perfection. The inquiry is aimed at identifying the nature of "the just", rather than finding some criteria for one alternative being "less unjust" than another.

Second, in searching for perfection, transcendental institutionalism concentrates primarily on getting the institutions right, and it is not directly focused on the actual societies that would ultimately emerge. The nature of the society that would result from any given set of institutions must, of course, depend also on non-institutional features, such as the actual behaviours of people and their social interactions. In elaborating the likely consequences of having one set of institutions rather than another, some specific behavioural assumptions are made (of quite a demanding kind). With those assumptions in place, the search in the approach of transcendental institutionalism is for perfectly just institutions, rather than for the ways and means of bettering what actually happens in a society.

Both these features relate to the "contractarian" mode of thinking that Hobbes in particular had initiated, and which was further pursued by Locke, Rousseau, and Kant. The hypothetical "social contract" that is assumed to be chosen is concerned with an ideal set of institutions as an alternative to the chaos that would otherwise characterize a society. The overall result was to develop theories of justice that focus on the transcendental identification of ideal institutions and rules.

In contrast with transcendental institutionalism, a number of other Enlightenment theorists, of whom Adam Smith was perhaps the principal analyst, took up a variety of comparative approaches that were concerned with social realizations (resulting from actual institutions, actual behaviour, and other actual influences), and did this from a comparative perspective. Different versions of such comparative thinking can be found, for example, in the works of Adam Smith, and those of the Marquis de Condorcet (the founder of the mathematical discipline of social choice theory who was much influenced by Smith's work), Jeremy Bentham, Mary Wollstonecraft, Karl Marx, John Stuart Mill, among a number of other leaders of innovative thought in the eighteenth and nineteenth centuries. As it happens, they were all very familiar with Smith's approach. Marx even chastised Mill for daring to say that he agreed with Smith: how far would a little man go, Marx wondered, in trying to place himself in the company of the great.

Even though these authors, with their very different ideas of the demands of justice, proposed quite distinct ways of making social comparisons, it can be said, at the risk of only a slight exaggeration, that they were all involved in comparisons of societies that exist or could emerge, rather than confining their analyses to transcendental searches for a perfectly just society. Focused on realization-focused comparisons, they were often primarily interested in the removal of manifest injustices they saw in the world, such as slavery, or policy-induced poverty, or cruel and counterproductive penal codes, or rampant exploitation, or the subjugation of women.

The distance between the two approaches—*transcendental institutionalism* on the one hand and *realization-focused comparison* on the other—is quite momentous. As it happens, it is the first tradition (that of transcendental institutionalism) on which today's mainstream political philosophy largely draws in its exploration of the theory of justice. The most powerful and momentous exposition of this approach to justice can be found in the works of the leading political philosopher of our time, John Rawls.[5] Indeed, Rawls's "principles of justice" in his *A theory of justice* (1971) are intended entirely for identifying perfectly just institutions.

A number of the other pre-eminent contemporary theorists of justice have also, broadly speaking, taken the transcendental institutional route. I think here of Ronald Dworkin, David Gauthier, and Robert Nozick, among others. Their theories, which have provided different—but respectively important—insights into the demands of a "just society", share the common aim of identifying just rules and institutions, even though their identification of these arrangements come in very different forms. The characterization of perfectly just institutions has become the central exercise in modern theories of justice.

This entire tradition is very non-Smithian in approach. Smith's focus is on actual realizations (not just institutions and arrangements), and on comparisons rather than on transcendence. The difference between the two approaches is reflected in the questions that have to be answered by a theory of justice. The primary concentration in the Smithian approach is on such questions as: "how could justice be advanced?" rather than on, as in Rawlsian theory: "how could we identify perfectly just institutions?" Smith's approach has the dual effect, first, of taking the comparative rather than the transcendental route, and second, of focusing on actual realizations in the societies involved, rather than only on institutions and rules. Given the present balance of emphases in contemporary political philosophy, the Smithian approach demands a radical change in the formulation of the theory of justice.

I shall not go further into the working out of such a theory of justice here, since I have tried to do this in my most recent book on justice (2009b). However, I will separate out for discussion one particular

[5] As Rawls explained in *A theory of justice* (1971, 10): "My aim is to present a conception of justice which generalizes and carries to a higher level of abstraction the familiar theory of the social contract as found, say, in Locke, Rousseau, and Kant". See also his *Political liberalism* (1993). The "contractarian" roots of Rawls's theory of justice were emphasized by him already in his early—pioneering—paper: "Justice as fairness" (1958).

feature of the Smithian approach, on which I have not yet commented, and which is quite central to the theory I present in my book. The issue involved concerns the domain of points of views that a theory of justice should try to accommodate. How far should we have to go to get the impartiality that a theory of justice must demand?

5

Adam Smith's thought experiment on impartiality invokes the device of an "impartial spectator" who can come from far as well as near, and this differs substantially from the admissible points of view that a social contract concentrates on, to wit the views of the people within the polity in which the contract is being made. Even though in John Rawls's discussion of what he calls a "reflective equilibrium", distant perspectives can be invoked, in his structured theory of "justice as fairness" the relevant points of view are those of the people in the society in which the so-called "original position" is being contemplated (Rawls 1971). Smith's device of the impartial spectator leans towards an "open impartiality" in contrast with what can be called the "closed impartiality" of the social contract tradition, with its confinement to the views of the parties to the social contract and therefore to fellow citizens of a sovereign state.

To be sure, both Smith and Kant had much to say about the importance of impartiality. Even though Smith's exposition of this idea is less remembered among contemporary moral and political philosophers, there are substantial points of similarity between the Kantian and Smithian approaches. In fact, Smith's analysis of "the impartial spectator" has some claim to being the pioneering idea in the enterprise of interpreting impartiality and formulating the demands of fairness which so engaged the world of the European Enlightenment. Smith's ideas were not only influential among those "enlightenment thinkers" such as Condorcet, who wrote on Smith. Immanuel Kant too was familiar with *The theory of moral sentiments*, as we know from his correspondence with Markus Herz in 1771 (even though, alas, Herz referred to the proud Scotsman as "the Englishman Smith").[6] This was somewhat earlier than Kant's classic works, *Groundwork*, 1785, and *The critique of practical reason*, 1788, and it seems quite likely that Kant was influenced by Smith.

[6] See Raphael and Macfie 1975, 31.

In the present discussion I am not so much concerned with the similarities between Smith on one side and Kant—and Rawls—on the other, but with their differences. The internal discussion among the participants in the Rawlsian original position would appear to Smith to be inadequately scrutinized, since we have to look beyond the points of view of others, all in the same society, who are engaged in making the social contract. As Smith argued:

> We can never survey our own sentiments and motives, we can never form any judgment concerning them; unless we remove ourselves, as it were, from our own natural station, and endeavour to view them as at a certain distance from us. But we can do this in no other way than by endeavouring to view them with the eyes of other people, or as other people are likely to view them (1975 [1790], 110).

Rawls's focus is on removing biases of the kind that are related to vested interests and personal slants within a given society, and he abstains from invoking the scrutiny of (in Smith's language) "the eyes of the rest of mankind". Something more than an "identity blackout" *within* the confines of the local focal group would be needed to address this problem. In this respect the procedural device of closed impartiality in "justice as fairness" can be seen as being "parochial" in its construction.

We could ask: why is this a problem? Indeed, since many of the criticisms of Rawls have come from philosophers who are communitarians and cultural particularists, it could even appear that this localism of Rawls is a virtue, not a barrier to be overcome. There are, in fact, two principal grounds for requiring that the form of public reasoning about justice should go beyond the boundaries of a state or a region, and these are based respectively on (1) the relevance of other people's *interests*—far away from as well as near a given society—for the sake of preventing unfairness to others who are not a party to the social contract for that society, and (2) the pertinence of other people's *perspectives* in broadening our own investigation of relevant principles, for the sake of avoiding an underscrutinized parochialism of values and presumptions in the local community.

The first ground, related to the interdependence of interests, would have been obvious to Smith. For example the misdeeds of early British rule in India, including the disastrous famine of 1770, engaged Smith greatly, and there could not have been any notion of adequate justice based only on a social contract among the British that could do the job of assessment adequately (in terms of Smith's analysis). Similar issues remain very alive today. How America tackles its economy influences not only the lives of Americans but also those in the rest of the world, and if there is one motivation that is central to the G-20 meeting recently held in London (April 2009), it is the importance of taking appropriate steps in the light of the interdependence of the global world. Similarly, how America responded to the barbarity of 9/11 in New York has affected the lives of many hundreds of millions elsewhere in the world—not just in Iraq and Afghanistan, but also well beyond those direct fields of American action. Further, AIDS and other epidemics have moved from country to country, and from continent to continent, and also, on the other side, the medicines developed and produced in some parts of the world are important for the lives and freedoms of people far away. Many other avenues of interdependence can be identified, for example the challenge of environmental policies for the world to tackle such issues as global warming.

The interdependences also include the impact of a sense of injustice in one country on lives and freedoms in others. "Injustice anywhere is a threat to justice everywhere" said Dr. Martin Luther King, Jr., in April 1963, in a letter from Birmingham Jail.[7] Discontent based on injustice in one country can rapidly spread to other lands. Our "neighbourhoods" are now effectively spread across the world. Our involvement with others through trade and communication are remarkably extensive in the contemporary world, and further, our global contact involving literary, artistic and scientific connections, make it hard for us to expect that an adequate consideration of diverse interests or concerns can be plausibly confined to the citizenry of any given country, ignoring all others.

6

In addition to the global features of interdependent interests, there is a second ground—that of avoidance of the trap of parochialism—for accepting the necessity of taking an "open" approach to examining the demands of impartiality. If the discussion of the demands of justice is confined to a particular locality (a country or even a larger region than that) there is a possible danger of ignoring or neglecting many challenging counterarguments that might not have come up in local

[7] For the background to King's judgement on the relevance of global justice for local justice, see *The autobiography of Martin Luther King, Jr.* (2001).

political debates, or been accommodated in the discourses confined to the local culture, but which are eminently worth considering in an impartial perspective. It is this limitation of reliance on parochial reasoning, linked with national traditions and regional understandings, that Adam Smith wanted to resist by using the device of the impartial spectator, in the form of the thought experiment of asking what a particular practice or procedure would look like to a disinterested person—from far or near.

Smith was particularly keen on avoiding the grip of parochialism in jurisprudence and moral and political reasoning. In a chapter in *The theory of moral sentiments* entitled "On the influence of custom and fashion upon the sentiments of moral approbation and disapprobation" Smith gives various examples of how discussions confined within a given society can be incarcerated within a seriously narrow understanding:

> [...] the murder of new-born infants was a practice allowed of in almost all the states of Greece, even among the polite and civilized Athenians; and whenever the circumstances of the parent rendered it inconvenient to bring up the child, to abandon it to hunger, or to wild beasts, was regarded without blame or censure. [...] Uninterrupted custom had by this time so thoroughly authorized the practice, that not only the loose maxims of the world tolerated this barbarous prerogative, but even the doctrine of philosophers, which ought to have been more just and accurate, was led away by the established custom, and upon this, as upon many other occasions, instead of censuring, supported the horrible abuse, by far-fetched considerations of public utility. Aristotle talks of it as of what the magistrates ought upon many occasions to encourage. The humane Plato is of the same opinion, and, with all that love of mankind which seems to animate all his writings, no where marks this practice with disapprobation (1975 [1790], 210).

Adam Smith's insistence that we must *inter alia* view our sentiments from "a certain distance from us" is, thus, motivated by the object of scrutinizing not only the influence of vested interests, but also by the need to question the captivating hold of entrenched traditions and customs.

While Smith's example of infanticide remains sadly relevant today, though only in a few societies, some of his other examples have relevance to many other contemporary societies as well. This applies, for example, to Smith's insistence that "the eyes of the rest of mankind" must be invoked to understand whether "a punishment appears equitable" (Smith 1982 [1762-1763], 104). I suppose even the practice of lynching of identified "miscreants" appeared to be perfectly just and equitable to the strong-armed enforcers of order and decency in the American south, not very long ago. Even today, scrutiny from a "distance" may be useful for considering practices as different as the stoning of adulterous women in the Taliban's Afghanistan, selective abortion of female fetuses in China, Korea, and parts of India,[8] and plentiful use of capital punishment in China, or for that matter in the United States (with or without the celebratory public jubilations that are not entirely unknown in some parts of the country). The United States is, by the way, the country with the fourth largest number of executions in the world today, behind China, Iran, and Saudi Arabia, and just ahead of Pakistan. Closed impartiality lacks something of the quality of intellectual engagement that makes impartiality—and fairness—so central to the idea of justice.

The relevance of distant perspectives has a clear bearing on some current debates in the United States, for example that in the Supreme Court not long ago on the appropriateness of the death sentence for crimes committed in a person's juvenile years. The demands of justice being seen to be done even in a country like the United States cannot entirely neglect the understanding that may be generated by asking questions about how the problem is assessed in other countries in the world, from Europe and Brazil to India and Japan. The narrow majority judgment of the Court, as it happens, ruled against the use of the death sentence for a crime that was committed in juvenile years even though the execution occurs after the person reaches adulthood. In condemning that decision, Justice Scalia in his dissenting note complained that the majority of the Court was influenced by their tendency to "defer to like-minded foreigners". The majority of judges did refer to views from countries other than the United States, and it could be asked whether they were right to do so, rather than looking only at American points of view. Central to this debate is the relevance of Smith's insistence on the need to scrutinize from "a distance" which is an integral part of the device of the impartial spectator.

The apparent cogency of parochial values often turns on the lack of knowledge of what has proved feasible in the experiences of other people. The inertial defence of infanticide in ancient Greece, on which

[8] On this see my "The many faces of gender inequality" (2001).

Smith spoke, was clearly influenced by the lack of knowledge of other societies in which infanticide is ruled out and yet which do not crumble into chaos and crisis as a result of not permitting such killing. Despite the undoubted importance of "local knowledge", global knowledge has some value too, and can contribute to the debates on parochial values and practices.

To listen to distant voices, which is part of Adam Smith's exercise of invoking "the impartial spectator", does not require us to be respectful of every argument that may come from abroad. Willingness to consider an argument proposed elsewhere is very far from a predisposition to accept all such proposals. We may reject a great many of the proposed arguments—sometimes even all of them—and yet there would remain particular cases of reasoning that could make us reconsider our own understandings and views, linked with the experiences and conventions entrenched in a particular country, or culture. Arguments that may first appear to be "outlandish" (especially when they do actually come, initially, from other lands) may help to enrich our thinking if we try to engage with the reasoning behind these locally atypical contentions. Many people in the USA or China may not be impressed by the mere fact that capital punishment is not permitted in many other countries, for example in the bulk of Europe and much of the American continents (in fact the United States is the only country in the American continents that has systematic civil executions). And yet if reasons are important, there would be, in general, a strong case for examining the justificatory arguments against capital punishment that are used elsewhere.[9]

7

I must end here. We can examine Smith's ideas for the way they are related to the world that he saw around him, but also for their relevance to the nature of human society in general and thus to our world today. I have pursued the latter inquiry in this presentation. I never cease to be impressed—indeed astonished—by the reach of Smith's ideas across the centuries. I am sure I would be accused of being over the top when I compare, in this respect, Smith with Shakespeare. But there is something in common between the two in their reaching over to people across the

[9] There would, of course, be a similar case for continuing to examine the arguments in favour of using capital punishment that may emanate from the USA or China, or any other country that makes substantial use of that system of punishment.

barriers of time. If there is unusual profundity in this, there is reason for us to give it the acknowledgement that it would seem to deserve.

REFERENCES

Auden, Wystan Hugh. 1947. Under which lyre: a reactionary tract for the times. *Harper's Magazine*, June: 508-509.

Bentham 1843a. To Dr. Smith, on projects in arts, &c. In *The works of Jeremy Bentham*, ed. John Bowring. Edinburgh: William Tait, vol. 3: Letter XIII.

Bentham 1843b. Of wealth. In *The works of Jeremy Bentham*, ed. John Bowring. Edinburgh: William Tait, vol. 3: Chapter III.

King, Martin Luther. 2001. *The autobiography of Martin Luther King, Jr.*, ed. Clayborne Carson. New York: Werner Books.

Raphael, D. D., and A. L. Macfie. 1975. Introduction. In Adam Smith's *The theory of moral sentiments*, eds. D. D. Raphael, and A. L. Macfie. Oxford: Clarendon Press.

Rawls, John. 1958. Justice as fairness. *Philosophical Review*, 67 (2): 164-194.

Rawls, John. 1971. *A theory of justice*. Cambridge (MA): Harvard University Press.

Rawls, John. 1993. *Political liberalism*. New York: Columbia University Press.

Runciman, W. Garry. 1966. *Relative deprivation and social justice: a study of attitudes to social inequality in twentieth-century England*. London: Routledge.

Sen, Amartya. 1986. Adam Smith's prudence. In *Theory and reality in development: essays in honour of Paul Streeten*, eds. Sanjaya Lall, and Frances Stewart. London: Macmillan, 28-37.

Sen, Amartya. 1987. *On ethics and economics*. Oxford: Blackwell.

Sen, Amartya. 2001. The many faces of gender inequality. *The New Republic*, September 7; and *Frontline*, 18.

Sen, Amartya. 2009a. Capitalism beyond the crisis. *New York Review of Books*, 56 (5).

Sen, Amartya. 2009b. *The idea of justice*. London: Allen Lane, and Cambridge (MA): Harvard University Press.

Smith, Adam. 1975 [1759, 1790]. *The theory of moral sentiments*, eds. D. D. Raphael, and A. L. Macfie. Oxford: Clarendon Press.

Smith, Adam. 1976 [1776, 1789]. *An inquiry into the nature and causes of the wealth of nations*, eds. R. H. Campbell, A. S. Skinner and W. B. Todd. Oxford: Clarendon Press.

Smith, Adam. 1982 [1762-1763]. *Lectures on jurisprudence*, eds. R. L. Meek, D. D. Raphael, and P. G. Stein. Indianapolis: Liberty Press.

Stigler, George. 1971. Smith's travel on the ship of state. *History of Political Economy*, 3 (Fall): 265-277.

Stigler, George. 1981. Economics or ethics? In *The Tanner lectures on human values*, vol. 2, ed. Sterling M. McMurrin. Salt Lake City: University of Utah Press, 1-31.

Townsend, Peter. 1979. *Poverty in the United Kingdom*. Harmondsworth: Penguin.

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Ethics, economics, and markets: an interview with Debra Satz

DEBRA SATZ is the Marta Sutton Weeks professor of ethics in society and professor of philosophy and (by courtesy) political science at Stanford University. She also directs Stanford's Bowen H. McCoy Family Center for Ethics in Society. Prior to coming to Stanford in 1988, Satz taught at Swarthmore College. She has also held fellowships at the Princeton University Center for Human Values and the Stanford Humanities Center and was the Marshall Weinberg distinguished visiting professor at the University of Michigan in 2002. Satz grew up in the Bronx and received her B.A. from the City College of New York and her PhD from MIT in 1987.

Professor Satz's research interests range widely including social and political philosophy, philosophy of social sciences, philosophy of economics, and feminist philosophy. Her work has appeared in *Philosophy & Public Affairs*, *Ethics*, *Journal of Philosophy*, and *World Bank Economic Review*. Her main research interest for the last decade concerns the limits of the market: Are there some things that should not be for sale? Kidneys? Sex? International weapons? Should the reach of markets be limited for reasons other than efficiency and distributive justice? Her new book addressing these issues is entitled *Why some things should not be for sale: the moral limits of markets* and will be published by Oxford University Press in June 2010.

EJPE interviewed professor Satz in early October 2009 when she visited Erasmus University Rotterdam to present material from her new book at the Research Seminar of the Erasmus Institute for Philosophy and Economics.

EJPE: ***Professor Satz, perhaps you could start by saying something about the trajectory of your career as a philosopher and the events, people, or writings that have had a particular influence on the development of your interests.***

EJPE'S NOTE: This interview was conducted by Thomas Wells and Rutger Claassen. Wells is a doctoral student at the Erasmus Institute for Philosophy and Economics (EIPE), Erasmus University Rotterdam, and a co-editor of the Erasmus Journal for Philosophy and Economics (EJPE). Claassen recently completed a PhD thesis on *The market's place in the provision of goods*, and is now an assistant professor in political philosophy at Leiden University, The Netherlands.

DEBRA SATZ: I was from a poor family but did well in school, especially in math. My love of math took me to MIT to study logic as a graduate student with George Boolos. During the time I was at MIT a number of things were going on in the world around me. The United States was playing a largely negative role in the wars in Central America. I had always been political and I had always worried about the state of the world—some of that comes from being from a working class family—and I realized that I was spending more time thinking about politics (and acting) and less time on logic.

At that point, I happened to sit in on a course by John Rawls and enrolled in another by Joshua Cohen at MIT. I began to think that maybe there was a way to bring my intellectual interests closer to my political interests—Rawls and Cohen showed me that philosophy could have practical aims. Not only can philosophy help a person to reason clearly and cogently about an issue, but it can also help her explore the bounds of the possible; teach mutual respect as the basis of argument; and help her discover what she really cares most deeply about. But to do some of these tasks—to explore the bounds of the possible, for example—political philosophy has to be in conversation with empirical social science. I found this mutual interaction between social science and philosophy very exciting, and it also opened up the possibility that some of my mathematical training could be of use. When Gerry Cohen wrote *Karl Marx's theory of history: a defence* (1978), I saw the possibility of exploring further the nature of social science explanation as a philosopher and I wound up writing a dissertation that examined the role of moral values in Marx's empirically based theory of history.

What do you consider to be the role of a philosopher, and perhaps specifically of a philosopher of economics? For example, should it be limited to academic analysis or take a broader form, and if so what?

I believe that philosophical work and reflection arises from problems that emerge in everyday thinking that everyday thinking cannot resolve. For example, when the dominant form of everyday thinking was religious, philosophers of the medieval period naturally wrote a great deal on religion, and since science has become dominant it is no surprise that so much of philosophy has turned to reflections on science. I have the same view of political philosophy and philosophy of economics: it speaks to questions that arise in our everyday thoughts about the world. For example, a political philosopher who today wrote a defense of slavery, no matter how analytically consistent and coherent it was, would be speaking to no one; at least I hope that is so. Debates about slavery are settled parts of our culture.

This does not entail shutting off more utopian ways of thinking, but even utopian ways of thinking have to connect in some way to the world of the possible. And they have to connect with history and aspirations.

So does that also mean that philosophers should bring their philosophical answers to the audience—should they write in newspapers and appear on television and radio shows, or confine themselves to publishing their answers in journals that, to put it kindly, nobody reads or perhaps even has access to?

To some extent a division of labor makes sense because people who are good at one thing are not good at another thing: some people are good at raising issues and drawing our attention to new distinctions but not good at writing for a general audience. At the same time, I think philosophy—especially political philosophy—removes itself from the world at its own peril. Not only is it harder to justify practically, but also since I think political philosophy is nurtured by real living debates and aspirations, I think it gets stale if it is done in isolation from public concerns. And I do think that philosophers—and all academics—have an obligation to share their important findings with a wider public.

There is a quote of Gramsci's that I have always liked which goes something like this: "An original idea that remains the property of a few people is a less important intellectual event than the dispersal of an old idea among masses of people who never knew it before". We should not devalue the activity of trying to involve lots of different people in philosophical conversation. Gramsci's point was that that was a philosophical contribution too. In fact, I think the most important thing I do is to teach, because that is probably the most direct way I can reach lots of people, people who will go on and do many different things in society. If you write for a journal that only ten people read, it does not mean that what you are doing is not important, but if what you write is important and it only stays with those ten people that is a problem. Somebody else ought to be trying to spread that idea, and there are people who popularize the ideas of other people, which I think is an invaluable thing and hard to do well. Neither of my parents went to college and so I have my own test for my writing—I have probably failed it—but I try to write in a way that does not set the bar so high that unless you have a PhD in philosophy you cannot read what I write.

Do you feel any particular identity as a philosopher of economics, and does anything follow from that?

Often when people talk about philosophy of economics they are really thinking about it as a branch of philosophy of science. The questions that understanding raises are not ones that I am expert in or where my main interests are located. I think a lot about markets, which are some of the most important economic institutions of our time. So, naturally I have to engage with economic reasoning about markets and reflect on the limits of that reasoning. I am interested in the places where moral and political questions about markets arise that economics itself is not designed to answer. I am interested in whether economic theory has all the tools it needs to assess markets as institutions. This leads me to think about the minimal presuppositions in economic theory about human psychology, about the relationship between markets and moral motivations, and about the nature of concepts such as that of an externality. So I would say I am a political philosopher with strong interests in economic theory and in how economic institutions play a role in political and social life.

But aren't theoretical economics and political economy quite different areas of study? How do you bring them together?

Of course, but they were not always distinct. And many theoretical models have presuppositions that turn out to be problematic when you put them into practice or consider their larger context. Consider the question of what counts as part of the economy. If you look at how the ILO (International Labour Organization) and a lot of economists have approached child labor, at who they counted as a child laborer, it turns out that in some countries 40% of the children were missing. This is because when you add up the kids employed in the formal market sector and the kids in school you only get about 60% of the kids that population statistics predict. Admittedly, data collection is bad in many poor countries but still that is an enormous percentage of missing children. It turns out that they were missing because no-one had counted children being employed in the home or kept at home to care for siblings so that their mothers could work. Most of these missing children were girls. So here is a point where an assumption of economic theory can blind us to something that is practically important, that has all kinds of ethical ramifications, that has policy ramifications.

How far do you think academic economics is significant for the real world? You have used the example of Lawrence Summers's infamous memo on the welfare efficiency of shipping toxic waste to poor countries[1] as an example of economic logic with problematic implications, but of course such proposals were never taken seriously.

Well there was the Washington Consensus where the IMF really took over the Chicago School's economic theory lock, stock, and barrel and then applied it to developing countries. For a long time, economists proceeded as if there was a fixed recipe for economic development and it was the same for every country. Then they saw that the recipe did not produce development in many cases—or if it produced development it produced it unevenly and with a lot of social suffering—and in places like China where the recipe was not followed there was enormous development. So, now the discipline is more open to considerations about institutions, governance, free press and women's rights. But harm was done by the previous attempt to impose the recipe. So I think that bad theory can have very bad consequences.

Even apart from the particularly direct influence on the IMF, mainstream economics has had a big although sometimes indirect influence. Many students take away two points from introductory economics courses: that regulations are inefficient and that markets are efficient. But these points are generalizations and over-simplistic. And that again is why teaching matters.

Of course, many bad social outcomes are not caused by bad ideas. In my paper with John Ferejohn, "Rational choice and social theory" (1994), we looked at the role of institutions in producing outcomes. Sometimes institutional constraints matter more for explanation than individual ideas or motivations.

In that paper on rational choice theory with John Ferejohn you argued against the naive view that rational choice theory was a psychological theory, and suggested that it was useful only in cases of agents with clear goals and severe external constraints. That work attracted lots of attention from economic methodologists.

[1] Summers was chief economist of the World Bank when he sent the memo in December 1991.

Nevertheless since then you seem to have turned away from the analysis of economic method and back towards a wider-ranging critique of contemporary political economy (particularly with regard to the moral limits of markets). Is that a fair assessment? Does this reflect changes in your understanding of philosophy of economics? How do you see the paper now?

Dan Hausman wrote a response to that paper soon after we published it, also in the *Journal of Philosophy* (Hausman 1995). We have been wrestling with a response to that and other criticisms for many years. We have published one follow up paper (Satz and Ferejohn 1995), and we have others in draft form that neither of us is happy with. So, it is still an ongoing interest of mine but it has been on the back-burner. I would still defend the basic idea of that paper: the abstraction of "rational economic man" can be helpful in some contexts, but not in others. Consistency and transitivity are not always necessary. Similarly, markets work well in some contexts but not well in others. They tend to work well when we are dealing with arms lengths transactions between strangers, with many participants, none of whom has power over the others. They tend to work best in environments where choices are constrained. We have to be careful about our generalizations.

There has been an ongoing debate about 'economics imperialism' in academia at least since Gary Becker's 'economic approach'. Would you characterize the ongoing expansion of economic analysis beyond traditional economic subjects and concepts as imperialist or as something else? Does it have important consequences and if so should it be resisted?

I think "imperialism" has to be evaluated on a case-by-case basis. Unification of domains that have been previously viewed as distinct is sometimes positive: it can help you understand things better that you did not understand before. Sometimes the application of economic methods to political science has yielded insights that some phenomena that people thought were random have an underlying structure that allows us to make some predictions and understand them better. But it is more problematic when what you are unifying has distinct characteristics that you now overlook.

Richard Posner's book *Sex and reason* (1992), is an example of problematic unification. There, he analyses sex in terms of an economic model. While that has some plausibility when we are dealing with malefemale sex ratios and certain social practices, it can be taken to absurd levels. For example, he conjectures that women wear high heels to signal to men that they will be faithful and not run out on them with other men. The danger is that all social practices will be viewed as having a rational explanation. In his book, economic reasoning and Darwinian evolution are used to explain an enormous variety of human practices, but many of these practices may owe their origins to contingencies, or to power, or to other causes.

Whether or not using an economic approach yields insights varies case-by-case. I am all for intellectual imperialism if it generates some useful knowledge. I do not have an a priori view that it is only in one domain that economic methods will generate insights. But I suspect that economic analysis is going to be better at generating insights in domains analogous to the one it was designed for: arms-length transactions between large numbers of strangers.

Some philosophers, for example Ingrid Robeyns, argue that philosophers should get more involved in economics, because economists are making philosophical mistakes. But what do you think really drives the course of academic economics? For example, the internal institutional values and publishing 'incentive structures' of economics departments, real world problems or events, or the intellectual issues with which philosophers are concerned? Is economics different from other social sciences in this regard?

I share Ingrid's concern that economics is much less pluralistic than other social science disciplines. I have a courtesy appointment in political science and if you look there, or at sociology, anthropology, or psychology, there are different methods for approaching the phenomena that are being studied. Although some methods have more prominence in each field than others, there is a pluralistic space. And I think economics does not have as much pluralistic space. Work in experimental economics, and even in behavioral economics, has not been well represented in the elite economics departments. Few, if any, courses look at the assumptions behind the discipline and there is little attention to the history of the discipline.

Of course, this is just a sociological description. It could be that all these other disciplines are methodologically pluralist because they are not yet scientific disciplines, but economics is. But I don't think that is the likely explanation because I just don't think we know enough about how the economy works—and I think recent events show that—to be confident about any models that we have. Economists would do better to be more open to questioning basic assumptions, and collaborating with other disciplines including philosophy, anthropology, political science, biology and the like. Economists studying poverty have a lot to learn from these other disciplines and vice versa. While economics has made some important advances, I think that we do not know enough to settle on one framework and therefore we should be more open

It is also unfortunate that economists have paid so little attention to ethics. Think, for example, about welfare economics. You can get a PhD in economics, with a focus in welfare economics, and yet have no idea about what *welfare* is, what the competing views of welfare are, and what values might not be captured by measuring it in standard ways. Or never think about the assumptions that go into defining GDP. Or take another example. Economists use the concept of externality to analyze why markets sometimes fail. But a little reflection will show that almost any transaction in a complex economy generates an externality: high rise apartments block the sunlight of their neighbors; cigarette smoke circulates; some people disapprove of the lifestyle purchases of others. If the concept of externality is to do any work, we have to separate harmless externalities from harmful ones. Nothing in economic theory gives us guidance about how to do this.

Do you think that formalism is part of the problem?

I think that formalism has driven out alternative approaches (approaches that may be harder to formalize), because it is *so* highly valued. I think that some policymaker once said something to the effect that "If we can't count it then it doesn't exist". So things that cannot be precisely quantified or modeled tend to be ignored, but that does not mean that they are not real or important or influencing what happens in the economy.

You have identified some philosophical problems with economics, but thinking about the future and how they might be fixed, do you think that can be done philosophically through the kind of approach you have been taking in your work, or do you think more institutionally focused reforms would be needed, for example with regard to the incentives in many economics departments to only publish in certain highly orthodox and formal journals?

I think you do have to have some institutional changes because—at least in the US—if a graduate student in economics published in *Economics and Philosophy* or *Philosophy and Public Affairs* they would get zero credit for it. And students know that, so they know that it is not really worth their time to take courses in ethics or political philosophy, and so they don't, and that just reproduces a narrow discipline. So institutionally I would try to open that up, but it is very very hard to change that culture, or any culture.

On the other hand, work in development economics, mechanism design and behavioral economics do open up the possibilities of collaboration between economists and other disciplines that might lead to broader approaches.

There does seem to be a large restructuring of mainstream economics underway, but the new approaches coming to prominence seem to come more from other sciences, notably biology and psychology, than from the 'traditional' heterodox traditions such as institutionalist, Marxian, or feminist economics which would seem to have more in common with your concerns. Are you happy with this new direction economics is taking?

Well it depends on what you think of experimental economics, for example, as showing. Take the kind of studies by Ernst Fehr and others on cooperation. I think that work is very exciting. They show that when we look at how people behave in dictator and ultimatum games in different societies they do not behave the way economic models would predict. They are not rational maximizers; they are guided by norms, including norms about what is right or just. Abstracting away from norms does not give us good models or predictive theories. That is not just bringing physics in—this work opens up the question of what the relationship is between economic reasoning and norms. Indeed, there is work by experimentalists suggesting that studying economics undermines certain norms—for example, that it makes students less altruistic—and that is pretty important if it is true. There again we have to think about the interplay between economic systems, methods and norms. So I think that some of the experimental work opens up in a direction that is not unfriendly to what some feminists and radical economists were saying earlier on.

In your previous work on markets in women's reproductive labor (Satz 1992) and sexual labor (Satz 1995), you argued that we should reject the economic approach to the moral limits of the market, which holds that in principle all goods should be for sale. Can you explain what is wrong with the economic approach with respect to these markets?

I think I should have said "an" rather than "the" economic approach, leaving open the possibility that there is more than one way of using economics to illuminate the limits of markets. But what I meant by 'economic approach' is the idea that we can approach any good from the point of view of given this supply and this demand here is how this market will behave, here is the price at which it will clear, and so on, apparently without any normative assumptions. These papers argue that if we just think about sex markets like that, as if they were apple markets, then we are missing some important features of those markets.

Earlier political economy was very sensitive to the differences between labor markets and apple markets. If you go back to Adam Smith, Ricardo, Marx, they all thought of markets as heterogeneous and, in particular, they thought of some markets as having endogenous effects on the agents who were transacting in them. Smith worried that the people working in the pin factory would become pinheads because that is all they did all day, and that without intervention in that market the working population would sink to a level where they could not function as citizens. You do not have to worry in an apple market, in general, about the effects of the market on the apples, but you do have to worry in markets where human labor or sex are traded about the effects of those markets on people. Although it is not as if you could not build economic models that take into account these other features such as endogeneity, the formalism of contemporary economics tends to preclude that.

So how could contemporary economics incorporate that endogeneity and, if it did, wouldn't that deal with your objections and render the different approach you take in those articles redundant?

Even if economists took endogeneity into account that still would not fully deal with my objections, because you would still need political philosophy or ethics to tell you when the endogeneity effects matter: when they are troubling, and when they are not. That is not within the scope of the tools that economics has and so you have to look to other disciplines to answer that.

In these articles, your concern with contract pregnancy and prostitution is that these sales are made against a background of gender inequality. Defenders of the economic approach could respond that they can accommodate this concern, since market exchanges need to be voluntary for them to be genuine market exchanges. And voluntariness depends on the availability of alternative options. How would you respond?

There is a quote from Milton Friedman where he says that markets have (Paretian) optimality properties provided that the transactions are voluntary and informed. But economists pay too little attention to what it means for a market to be voluntary despite the fact that it is such a critical notion in economics. And what counts as voluntary is indeed a vexed issue. The fact that there is inequality underlying a transaction does not show that there is not voluntariness—at least on most views about voluntariness. At any rate you need a background theory to show when some choices are voluntary and when they are not. In a sense, even the choice to hand over your money to someone who says "your money or your life" is a voluntary one. To show that the choice of prostitution is involuntary you would have to show that there is something especially problematic about the set of alternatives women in prostitution face. That is unlikely to always be the case. In my articles, I concede the point that prostitution and commercial surrogacy can be voluntary but argue that there are other criteria for the legitimacy of choices other than the fact that they are voluntary.

My argument about prostitution does not deny that the choices are voluntary but it simply says that that is not enough to generate an argument that they are legitimate. I argue that in practices like prostitution there are likely to be ethically significant externalities that affect other people who do not choose to participate in the practice. The gender inequality that I hypothesize may arise in a prostitution market or a surrogacy market—operates via third party effects. It is what the market does to the way a group—women—are viewed that is objectionable although the group itself is not a participant in the market, only single individuals are.

That is still a hypothesis because it depends on empirical evidence. If you can show that widespread markets in women's reproductive labor and sexuality have effects on the way women are viewed and view themselves, then it is troubling. If you cannot show those third party effects, then I argue that prostitution is not necessarily troubling, although some instances of prostitution may be troubling because the women are controlled and beaten by their pimps, desperately poor, and so on.

In effect, I make a Parfit inspired point about third party effects: that an act that in a single case is acceptable might not be acceptable if it is held as a general rule. Think about child labor. Child labor for one family can be an improvement insofar as it provides the family with needed income, but if you allow child labor as a practice then you will drive down the price of adult labor and families that do not want to send their children to work will no longer be able to afford not to. Similarly, if you allowed prostitution as just another form of work—and it was linked with employment so that it became part of the job description for many jobs that you would have to have sex with the boss because it is just another kind of work—you would be imposing a cost on people who do not want to engage in that. They are going to have a different set of employment opportunities than they would have had had this market been closed off.

Now it is true that lots of markets have these third party effects. But at the least this point undermines the Pareto defense because it shows that a Pareto defense does not apply here.

That last point sounds similar to that of those philosophers, such as Elizabeth Anderson, who argue that having markets in certain goods will distort or corrupt the qualities of those things even for non market participants.

My argument here does not directly link to the qualities of the good, but to the choice sets of other people. This is another point about endogeneity: sometimes including an element in a choice set changes the options that are available for everyone, so that although it looks like you have added an element, you have actually taken away some other elements by making them unsustainable.

So again, for child labor this argument is not about changing the meaning of childhood, it is that you have added the element for the option to have child labor and what that has done is change the set of options for parents who do not want to have their children work, because now the adults make less money so it is much harder for them to effectively have the option not to send their kids to work. Whereas if you had closed off that option, it would have been easier for them not to send their children to work.

Nonetheless I do agree that a society with widespread child labor changes the meaning of childhood. There are probably third party endogenous effects with child labor in terms of how children are viewed by themselves and others. Some have argued that child labor itself reflects a certain view of children, or at least the children of the poor, as expendable and inferior.

But take a case for restricting a market that does not look at all like it depends on the meaning of the good—minimum wage laws. On the one hand, you could say that what minimum wage laws do is block some exchanges that people would want to make if they could. But arguably one of the things that will happen when you do not have minimum wage laws is that there are some options that people will not have in their set. For example, when there are minimum wage laws, employers have an important incentive to develop labor capacity and make labor more productive. This may lead them to invest in worker training programs, and care about the health of their employees. But when labor is cheap they may find themselves without this incentive. So even though minimum wage laws deprive people of an option, they also make possible other options. Workers as a group are arguably better off when employers have reason to invest in the development of their capacities.

In your article on markets for human kidneys (Satz 2008) you argue that prohibiting the sale of kidneys may be autonomy-enhancing for society as a whole. Could you explain?

The argument is essentially analogous to the argument I just made about minimum wages. You can think of it as an argument on behalf of a kind of collective paternalism—society closes off a choice to individuals to preserve other choices within the society. Closing off kidney markets may lead to more freedom for people overall, even though you have closed off this one kind of freedom.

This point bears on some early economists' objections to Titmuss's argument about blood markets. Titmuss argued in his book *The gift relationship* (1970) that if you have a market in blood, it will undermine altruism and you will have less gift-giving of blood and blood of an inferior quality since people will have an incentive to conceal any health problems they have. In his 1972 review, Ken Arrow accepted the point about the lower quality of blood through a market system but denied the point about quantity being diminished. Since, he wrote, all you are doing when you allow a market is giving people an additional option; this should not effect the decision of altruists. They can still donate their blood and others can sell it.

Titmuss did not have any mechanism for showing why his result would obtain. There is now a good deal more support for the fact that adding an option sometimes makes other options less available. Bruno Frey is probably the person who has done the most work on this (see e.g., Frey 1997). The classic example is the "natural experiment" of an Israeli day-care center that charged parents fines to discourage them from coming late, but found that lateness actually increased—the parents did not feel guilty anymore because they just paid the fines (Gneezy and Rustichini 2000). Payment "crowded out" their altruistic concern for the daycare workers.

In these articles, but especially in your article on 'noxious markets' (Satz 2004) and your new book, you propose a theory of democratic equality to assess the moral limits of the market. Can you explain the main tenets of this theory?

The starting point of my article—and my book—is that people respond quite differently to markets in certain goods than to other markets. Think about markets in "blood diamonds", international debt, child labor, kidneys, and prostitution. These markets elicit unease even among those who are otherwise enthusiastic about the market system. The question is whether there is anything we can say systematically about these reactions, or whether they simply reflect a primitive "repugnance". I think that there is something more that we can say. These markets all have some important departures from the markets of ideal theory. I analyze problematic markets in terms of four parameters, some of which are consistent with contemporary economic frameworks and some of which will take us outside economic frameworks.

Two of the parameters involve the sources of the market. The first is what I call 'weak agency', which happens in cases where people either lack important information about the nature of the market (as can happen when the consequences of the market extend into the distant future) and/or they are not participants in the market (as when dictators trade resources on the international markets that do not then benefit the populations of their countries). The second parameter concerns vulnerability—that some markets emerge when people are desperate or have very unequal needs for the goods at stake. When we see that kind of desperation and inequality, the market inherits it, and that is part of what makes us respond to the market as problematic. We do not think people should be so poor that they have to sell their kidneys to put food on the table.

The other two parameters concern the consequences of markets: some can be extremely harmful for individuals, others can be extremely harmful for social relations in a democratic society. So, for example, consider markets in toxic waste. Some of the effects of storing and transporting toxic waste are likely to be very bad for people—either people who are alive now or for future generations. Markets in blood diamonds are used to wage bloody civil wars where thousands die. In these cases, the harms brought about as a result of such markets rightly elicit our concern and even outrage. Other markets have deleterious social effects. Think about a market in votes. Although it is easy to show that a vote market might be efficient, a democratic society depends on the prohibition of such a market. (It also depends on prohibiting the free exchange of votes.) Or think about the ways that certain markets shape us—who we are, what we can do and hope for. In this category I would put labor markets, child care markets, and markets in education.

What ties these four criteria together? The parameters do not always go together—some markets are very bad because they have bad effects for individuals, but they do not arise from the weak agencies or underlying vulnerabilities of the transacting agents. But I believe that all noxious markets involve high scores along at least one of these parameters.

One way that these parameters go together is through their relationship to democracy. You cannot have a democracy, I argue, without important restrictions on noxious markets. And some markets, if not regulated can become noxious. If education were treated solely as a market good instead of also as a right, this would undermine democracy. In my 2004 paper and forthcoming book I develop what I call a 'democratic theory of citizenship', taken from T. H. Marshall: there are some basic requirements for being a citizen and markets which depress people below those requirements are worrying. That does not necessarily mean we should block such markets, but at the very least we have to supplement markets in these goods with non-market provision.

Democratic citizenship is a demanding standard. And while I think it is appropriate to analyze markets using that standard, I want my theory of noxious markets to cover non-democratic contexts such as child labor in Pakistan where it is not about democratic relations, it is about extremely harmful consequences for children. But in a democracy, we have reasons to care even more about the scope of markets and their operation, and so I take the more minimal criteria specified by my four parameters and build it up into a more robust theory that says that democracies have special reasons to worry about the domain of the market.

The most hotly debated issues in the Netherlands with respect to the introduction of markets—as in many other countries—are (higher) education, health care, and social welfare services (e.g., job reintegration). What would your theory of democratic equality say about these types of cases, where what is at stake is not so much the sale of goods or services from the personal sphere, but the privatization or marketisation of services belonging to the public sector?

I think you can evaluate any of these markets along the parameters I have given. As I mentioned, these markets have endogenous effects: education markets and childcare markets really make people in certain ways. So you have got to pay attention to how you are making people, and what kind of people you are producing by these markets. These are not apple markets: we have reason to want our education system to produce more than docile and servile adults.

Let me elaborate on the education example. If we just treated education like any other kind of consumer preference you could, for example, educate children to be pets. But we have a lot of reasons not to allow this and therefore reasons to prevent consumer preferences from driving the content of education. There is also weak agency in education, because children's interests are at stake but they are not the consumers in the market—generally it is their parents making decisions. The state has an interest in making sure the interests of children are protected over the consumption interests of the parents.

At the same time, I do not think my arguments about limiting the market can specify the institutional answer to these issues because different institutional arrangements can be compatible with democratic citizenship. Think about markets in healthcare. One could imagine a system in which all healthcare is publicly provided by a single payer system, and one in which a market system is supplemented with publicly provided healthcare.

My theory is compatible with different modes of provision and I think you would have to look at the effects of those modes on the values that I stake out. But in the case of democracy, there are other values at stake besides extreme harm to individuals. There are values like equality of opportunity that are built into the idea of equal citizenship—no one as a result of their birth alone deserves more opportunity than anyone else. So you have to think about how the provision and distribution of education, whether through a market or a non-market or some combination, interacts with the political requirements of equal citizenship. There are lots of reasons to be worried about *how* education is provided. For example, one of the striking facts about education in the United States is that there are more children now being educated by home-schooling than through private schooling. This is an enormous movement and I would call it gift-giving because the parents are giving out their labor. It has all kinds of worrying effects from the point of view of a democratic, egalitarian society because parents are often home-schooling their children for religious reasons: because they do not want their children hearing about Darwin in the schools, they do not want them hearing about different ideas about sexuality, they do not want them learning about different religions. There are all kinds of worrying things about that sort of provision, even if the kids can pass some multiple-choice tests. All your examples—education, health care, social welfare services—involve cases where the endogeneity effects are very significant and have to be taken into account when we look at the provision of these goods, whether by markets or by other institutions.

While most people never engage in receiving or providing prostitution or the sale of kidneys, nearly everyone in developed countries is significantly exposed to public services such as the education and health care systems. So shouldn't your theory of the market be more ambitious in what it can say about those cases?

My theory does not say whether market provision is acceptable or not in the abstract, but it says that if there is a market, it will have to be limited in some way to safeguard or guarantee the values at stake. To what extent do you rely on the market, to what extent do you rely on the public sphere, to what extent do you rely on altruism in the provision of healthcare and education? I do not think my theory or any philosophical theory, can deliver determinate answers to that question. That seems to me a political question, not a political philosophy question. The political philosophy question is to try and point out the constraints on the domain of decision-making. What values ought to constrain our decisions? What values are important to promote in our institutional designs? But even once we have set those constraints, there are going to be a lot of close-to-the-ground empirical factors that have to be taken into account, and there is going to have to be some room for publics to reasonably disagree about different trade-offs. But you do want to see when you have reason to accept a constraint, that says here are the reasons not to trade off over here.

Many political philosophers set up an opposition between "the market" and "the state". One has the impression that you do not see that contrast; that you see markets merely as instruments for whatever we decide politically should be done, and in accordance with the political values we decide should be important. But don't you somehow assume that a country's politics will already meet some minimal requirements, for example of democratic institutions?

I think of markets as political institutions that depend on regulation and property rights. On my view, true *laissez faire* is not an option: all markets depend on background rules that are enforced. I am not even sure that we really have markets in some developing countries, because they often do not have the preconditions for a market society, such as enforceability of contracts. There are also *de facto* monopolies in many markets in poor countries, certainly in the credit market. Markets have certain political preconditions, but I would not go so far as to say that markets can only exist in democratic societies.

Nonetheless, I do not see market and state as rivals, the way they are often portrayed. Moreover, there are diverse markets (as I have been arguing) so "the market" is itself a heterogeneous domain. Finally, there are a lot of modes of provision between market and state. We need an expanded menu of modes of provision between those two: gift, philanthropy (which can involve a more collective gift-giving), self-provision, lottery, compulsory service. There are a lot of different modes of provisioning that we have to consider.

The last chapter of my book is actually about the policy implications of the approach that I defend. I argue that there is no way to deduce from the fact that a market is problematic to what we should do institutionally. In part that depends on which of the parameters are in play. If the problem is vulnerability, for example, you might leave the market in place but try to diminish vulnerability using a tax and transfer system. If the problem is weak agency, you might use mechanisms to increase information, like informed consent. What you do with a noxious market is going to vary case-by-case and also be the subject of a lot of political debate—quite correctly—because there may be a trade off of values.

So in the case of a weak country like Nigeria, you would say that it is not the untrammeled "market" causing the corruption, poor public services provision, and so on, since in fact there are no proper markets in the first place because of the lack of a proper democratic politics?

That is too quick. As I said, markets can exist in non-democratic contexts. But a lot of what looks like a market transaction is actually theft—as when dictators sell their country's resources on the world market and pocket the results of the sale. And in countries like Nigeria, you have weak enforcement of property rules, a weak banking system, and widespread corruption. These all undermine the ability of a market to work. But clearly markets can exist and still work to the benefit of dictators. And some markets help keep dictators in power—here I have in mind child labor and bonded labor markets which keep the population passive and uneducated.

Are state-imposed and enforced measures to ensure that workers voluntarily choose to enter into contracts genuine 'limits' to the market? Or are they rather measures to install a genuine ('ideal') market in the first place? So is debt peonage or child labor compatible with an ideal market, given the ideal market's prerequisite of the absence of asymmetric bargaining power? And more generally, what exactly is the concept of a 'limit' to the market that underlies your work?

I think true *laissez faire* is impossible. The existence of a market system depends on there being limits, on there being enforcement of property rules for example, i.e., on taking some options off the table.

My first line of criticism is that in reality in a lot of these cases in the developing world we do not have actual markets, and so a lot of the problems we see are not so much problems with the market, but problems of the absence of an established market. But even if we created the conditions for a market in child labor—which could be done—there are still reasons for concern about that market.

My second line of criticism is that, even if these markets involved free exchange between equals, we would have reasons to object and that goes back to what I said earlier about not accepting the fact that an exchange is voluntary as sufficient for its legitimacy. Some things that are voluntarily exchanged are still wrong (see also, Satz 2007). Even if vote-trading is voluntary and involves trading between equals, we have reasons to object to it.

A lot of the markets that I look at are very non-ideal markets, and that is why my approach is not so hostile to economic reasoning. Standard economic reasoning can take a lot of my framework on board. It is already concerned with externalities and what I refer to as weak agency. At the same time, although I use the economic term “externality”, I am quite specific about the kinds of externalities I think are troubling. I think it is a problem in economics that the notion of externality is ill-defined—it is an empty term that needs filling in, since, as I said, almost every exchange generates some kind of externality.

Where do you think the debate about the limits of the market in political philosophy is heading?

I think the debate has moved to particular issues such as markets in the media, or education or healthcare. I think there is a huge amount of work to be done by political philosophers thinking about alternative modes of distribution in those domains. How, for example, should we think about the distribution of media time; what should we do when the production of for profit print news no longer seems viable; how should health care be distributed. How should education be funded? The environment will be another place where debates about the market become salient. There have already been debates about carbon markets, but what about the cultural services that nature provides? How should a community’s way of life as fisher-people be taken into account in our environmental decision making? I also think the recent economic shock is going to make people more skeptical about financial markets, markets in derivatives, and the like. So, I suspect there will be attention to that.

More generally, I suspect that there will be an increased focus on institutional delivery systems—centralized versus non-centralized modes of provision; underlying property rules, especially about intellectual property; and forms of public service provision in poor countries.

REFERENCES

Arrow, Kenneth. 1972. Gifts and exchanges. *Philosophy and Public Affairs*, 1 (4): 343-362.

Cohen, Gerald A. 1978. *Karl Marx's theory of history: a defence*. Princeton (NJ): Princeton University Press.

Frey, Bruno. 1997. *Not just for the money: an economic theory of personal motivation*. Boston (MA): Beacon Press.

Gneezy, Uri, and Aldo Rustichini. 2000. A fine is a price. *Journal of Legal Studies*, 29 (1): 1-18.

Hausman, Daniel M. 1995. Rational choice and social theory: a comment. *Journal of Philosophy*, 92 (2): 96-102.

Posner, Richard. 1992. *Sex and reason*. Cambridge (MA): Harvard University Press.

Satz, Debra. 1992. Markets in women's reproductive labor. *Philosophy and Public Affairs*, 21 (2): 107-131.

Satz, Debra. 1995. Markets in women's sexual labor. *Ethics*, 106 (1): 63-85.

Satz, Debra. 2004. Noxious markets: why should some things not be for sale? In *Economic theory, philosophy and contemporary social issues*, eds. Prasanta Pattanaik, and Stephen Cullenberg. New Delhi: Oxford University Press, 11-38.

Satz, Debra. 2007. Liberalism, economic freedom and the limits of markets. *Social Philosophy and Policy*, 24 (1): 120-140.

Satz, Debra. 2008. The moral limits of markets: the case of human kidneys. *Proceedings of the Aristotelian Society*, 108 (3): 269-288.

Satz, Debra, and John Ferejohn. 1994. Rational choice and social theory. *Journal of Philosophy*, 91 (2): 71-87.

Satz, Debra, and John Ferejohn. 1995. Unification, universalism and rational choice theory. *Critical Review*, 9 (1-2): 71-84.

Summers, Lawrence. 1991. Untitled Memo. Reprinted in Daniel M. Hausman, Michael S. McPherson. 2006. *Economic analysis, moral philosophy, and public policy*. New York: Cambridge University Press, 12-13.

Titmuss, Richard Morris. 1970. *The gift relationship: from human blood to social policy*. Sidney: Allen & Unwin.

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<http://ethicsinsociety.stanford.edu/>

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Review of Julian Reiss's *Error in economics: towards a more evidence-based methodology*. London: Routledge, 2007, 272 pp.

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The ancient debate between deduction (model-based inference) and induction (which I shall identify with evidence-based inference) is still with us. But it does not seem to take as visceral a form as it once did. Indeed, the nuances of the debate are subtle enough to escape simple characterizations, which may signal a growing consensus within the field about what good practice consists of. If so, Julian Reiss's fine new book offers a case in point.

Reiss flies under the "evidence" banner. However, the label is given a rather diffuse definition.

> Evidence-based economics is the conscientious, explicit, and judicious use of sound evidence in making decisions about the welfare of societies. The practice of evidence-based economics means integrating individual socio-economic expertise with valid external evidence from systematic research relevant for the purpose at stake (Reiss 2007, 13).

No one is likely to take offense at this definition. But it is not entirely content-free, either. In the following paragraphs I shall try to give the reader a sense of the most salient characteristics of this methodology, based on—but also extending upon and intuiting from—Reiss's lucid but cautious prose.

To be clear, this book offers few grand statements about how economists should or should not behave. No general philosophy or methodology is offered. Instead, the topic is approached patiently, by way of case studies. Reiss's tour of economics is not a breezy tour, but an immensely detailed one, with a well-informed and friendly tour guide. Some might regard this as a book of essays. However, I think there is a consistent viewpoint expressed throughout and one that is wedded to the general idea of evidence-based knowledge. This will be my focus, as I suppose it is the one of greatest interest to readers of this journal.

A recurring theme of Reiss's book is the *inextricability* of theory, values, and evidence. Each is embedded in the other. For example, in the first empirical chapter Reiss explores the conundrum of the US Consumer Price Index (CPI). Here, he shows that the debate over how to measure prices is closely entwined with the debate over how to understand the meaning and value of goods (food, shelter, et al.) to people. Thus, we shift from "what is the right index number?" to the broader question of "what is the right concept of 'cost of living'?" (p. 31). Concept and measurement, along with whatever causal theory is being tested, are wrapped up in a single index. It is no wonder that some debates are impossible to resolve in a definitive fashion.

In a wide-ranging series of case studies—e.g., on consumer-price indices, radio spectrum auctions, and the minimum wage—Reiss exposes the assumptions embedded in economic findings and shows that these assumptions are often difficult to defend. This is not to say that they are wrong, simply that they present a partial view of the subject matter. Reiss delights in opening up these boxes. One might say that he *deconstructs*, except that his purpose is clearly constructive.

The larger project envisioned here is a systematic and continually renewed critique of economics, one that involves not only professional social scientists but also philosophers, policymakers, and citizens. Reiss stands against the tyranny of experts, though he also (implicitly) acknowledges the limitations of lay citizens in directly engaging with these highly technical debates. In any case, the purpose of this critique is not to discredit economics but rather to reach better truths, and more relevant truths, i.e., truths that are relevant to the everyday needs and purposes of citizens (the ultimate consumers of economics).

Questionable assumptions are not limited to abstract ("deductive") economic models; they also inhere in empirical models, as employed to test the nature of reality. Instrumental variable analysis (the topic of chapter seven) is one case in point. Given the assumption-ridden nature of the enterprise, we ought to become comfortable with the intrinsic uncertainties of the social science enterprise, rendering the sources of uncertainty transparent wherever it is unrealistic to try to eliminate them (which is usually the situation we find ourselves in). For Reiss, "evidence-based" thus implies a triple social science immersion: in the evidence, in the relevant theories about a subject, and in societal needs and values.

Reiss's view of evidence-based economics extends beyond the usual purview of experimental and quasi-experimental designs to include a wide variety of inferential techniques. Employing a distinction drawn from Nancy Cartwright, Reiss distinguishes between "clinchers" and "vouchers". The former (e.g., randomized trials, natural experiments) are definitive, if highly restrictive assumptions hold; however, their purview (range of external validity) is often very limited, or at least highly questionable. The latter (e.g., process tracing, pattern matching) are suggestive, but can often claim greater range of applicability (p. 122).

A principal motivator for Reiss's evidence-based economics is the need to make the field of economics relevant to policy concerns. This is not to say that theory is irrelevant, but rather that it is insufficient and often poorly suited to answering highly specific policy questions in highly specific policy contexts. Here, the weight of evidence is generally superior to whatever weight might be allocated to general theory. Given the heterogeneity of contexts in which humans find themselves, we are better advised to explore *particular* settings and *particular* questions with great care and attention to detail rather than to rely on 'general theory' to pull us through.

A final reason to treat 'general theory' skeptically is that there is usually more than one theory that can be applied to a given set of facts. To this conundrum, Reiss endorses a pragmatic solution: inference to the best explanation—which is another way of saying, use theories as tools and let the facts (such as we understand them) determine which tool is most appropriate in a given setting. This solution would make little sense if economics is viewed as an enterprise whose purpose is to develop theory. But if the purpose is to answer concrete questions of interest to policymakers and citizens, then a problem-centered (and evidence-centered) approach to theory is commonsensical.

A similarly pragmatic approach is taken to choices among methods. Here, one might begin by noting a growing rhetorical commitment to methodological pluralism within the social sciences. And yet, Reiss notes a curious division within the discipline of economics. When acting as policy advisors, economists make ample use of a wide variety of empirical approaches, including highly contextual ("a-theoretical") facts of a loose, qualitative nature. However, when acting as scientists (i.e., when publishing in academic venues), they appear to rely solely on formal theories and formal research designs. It would appear that background knowledge of a more informal nature informs scientific practice but it does not have an honored place in the higher counsels of economics, and is therefore routinely stripped from austere academic publications. Again, the appeal to societal utility serves the function of justifying an evidence-based approach to knowledge.

Before concluding, I must reiterate that my brief review focuses on only one element of this diverse volume, which stretches across many subjects that I have scarcely alluded to: issues of measurement (chapters 2-4), the potential and limits of experiments (chapter 5), mechanism-based explanations and inferences, including social capacities (chapters 6, 8-9), natural experiments and instrumental variables (chapter 7), and policy counterfactuals (chapter 10). Since these rich and detailed chapters are not amenable to quick summary, I leave the reader to explore them.

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Review of Russell Hardin's *How do you know?: the economics of ordinary knowledge.* Princeton: Princeton University Press, 2009, 256 pp.

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Although some people are curious and enjoy learning things about the world even if they are of only remote interest to their daily lives, most people are not very inquisitive and set out to gather information only on a need-to-know basis. In his latest book Russell Hardin argues that people are perfectly rational when they refrain from embarking on a quest for knowledge. Ignorance turns out to be bliss, not because possessing knowledge would be disturbing, but because we maximize our utility by not investing much in knowledge acquisition.

The core of what Hardin calls his 'economic theory of ordinary knowledge' consists of the thought that costs and benefits influence what we come to know. His main thesis is that cost-benefit analysis explains why most non-scientists know very little about anything. Hardin puts this thesis to a somewhat unexpected use—to defend the idea that human beings are rational agents. The charge is that we often do stupid things and that this goes to show that we are not very rational. Hardin's response is that the stupidity is only apparent. Allegedly stupid actions may be due to our ignorance and ignorance is usually rational since for practical purposes it is often not worth the trouble to learn more (i.e., the returns on investment in knowledge are frequently negative). In light of the rationality of our ignorance, it would be foolish to insist that the actions really are irrational. Hardin applies these ideas to several domains including medicine, culture, and religion, which I only mention here, and morality and politics, on which I shall elaborate.

In relation to politics Hardin uses his thesis of the rationality of ignorance—as I will call it—to explain why so many people defy the logic of collective action. His main example is voting. The chance that your vote will make a difference is very small. Even when the cost of voting is small, the negligible chance of making a difference makes voting irrational for most, if not all of us. In spite of this, many people do vote.

Does this mean that they are indeed irrational? Not necessarily. They might fail to understand the logic of collective action. And the expected benefits to understanding it are so low that it is rational for them not to invest in trying to understand it. The rationality of their ignorance in turn reflects on the rationality of their voting behavior. It would be irrational for them to vote, if they understood the logic of collective action. Given that they do not understand it, however, voting may be rational for them. The reason for this is that the rationality of an action has to be evaluated relative to the beliefs the agent has. After all, according to rational choice theory, people maximize their *expected* utility. One might object that voting is irrational for them after all, because it is due to ignorance or to beliefs that are mistaken or unjustified. In response, it is worth pointing out that it is far from obvious that the irrationality of a belief affects the rationality of an action within the context of rational choice theory. To the extent that it does, the objection is, once again, answered by his thesis of the rationality of ignorance.

What should we make of this rather ingenious line of reasoning? Hardin does not present any systematic evidence in favor of his explanation. The evidence he presents is anecdotal. On numerous occasions he has experienced difficulty in explaining the logic of collective action. From this he infers that it is indeed very difficult to understand. Although he admits it is not quite as difficult as quantum mechanics, he surmises that for most people it is not comprehensible at all. Hardin explains this in turn by claiming that since the costs of voting tend to be small, most people have little incentive to try to understand the logic of collective action (p. 73).

The main problem I see with this argument is that the core idea is in fact very easy to grasp. The only thing people need to know in order to realize that voting does not result in any direct benefit is that the chance that their vote will make a difference to the outcome is small. Most people are well aware of this (even if they overestimate it). In light of this it is implausible that, as Hardin implies, many people who vote do so because their cost-benefit calculations turn out positively. Note also that an alternative interpretation of Hardin's conversations is readily available. His interlocutors simply do not believe that the logic of collective action should be the decisive factor insofar as voting is concerned. They assign, and they believe other people assign, more significance to reasons for voting that Hardin is inclined to dismiss as irrelevant.

Hardin admits that some people might vote because they regard not voting as inappropriate, perhaps even immoral, and that such people might justify their attitudes and actions by rhetorically asking 'What if everyone did that?'—a question that appears to lie at the heart of what might be called 'the logic of moral action'. However, he plays down the importance of this explanation by suggesting that people often experience feelings of guilt and regret for not voting when their party loses and that such feelings should be independent of the fate of their party if the issue were really a moral one for them (p. 73). He takes this to imply that the feelings are non-moral and that they are based on the (fallacious) idea that individuals can reasonably expect to influence elections after all.

Matters might be slightly more complex. Perhaps the people Hardin describes rationalized their not-voting by thinking to themselves that their vote would not make a difference anyway. This rationalization would presumably survive if their party were to win since there would not really be an occasion for guilt or regret in the first place. The rationalization may break down, however, if their party loses. They might then realize that they—just as many others—succumbed to a line of reasoning to which they should not give in. Their feelings of guilt and regret might have a social aspect in that they feel bad about falling foul of a line of reasoning that is damaging if too many people give in to it. From a moral perspective the logic of collective action might in fact put extra pressure on individuals to vote: people might regard the very fact that not voting is tempting for all as a consideration that ultimately counts in favor of them voting. This somewhat more complex line of reasoning, which revolves around the tension between the logic of collective action and the logic of moral action, suggests the feelings Hardin discusses might well be sensible even on a moral interpretation.

The basic idea underlying this interpretation is that moral considerations and cost-benefit analysis may pull in different directions. Hardin devotes a chapter to moral knowledge in which he refers to this as "the dualism of practical reason" (which he traces back to Henry Sidgwick). He seems to have a somewhat ambivalent attitude towards this idea. He criticizes the thought that moral norms serve our collective benefit by providing an example of a destructive social practice—the inhabitants of St. Kilda followed a norm that required them to spread a mixture of fulmar oil and dung on the wound where the umbilical cord was cut loose, which decimated the island's population (p. 115). In addition to this he maintains that children are provided with both moral and prudential reasons against lying and breaking promises: such actions are wrong, but also harm our long-term self-interest, because they make us untrustworthy. Hardin goes as far as claiming that acting morally typically coincides with acting from self-interest (p. 111). A few pages later, however, he argues that people have little incentive to acquire moral knowledge, because we rarely need it and because acting on such knowledge is often costly. My problem with this chapter is not so much that I disagree with particular claims, but that I am somewhat lost as to what the overall point is supposed to be.

A question that remains is why Hardin talks about moral knowledge rather than moral beliefs. The idea that there is such a thing as moral knowledge is rather controversial as it appears to presuppose that there are objective moral truths. But perhaps it is due to Hardin's skepticism about the idea that ordinary people make a sharp distinction between knowledge and belief in general. Hardin is rather dismissive of the philosophical literature on knowledge and its relation to belief, which he regards as "arcane" (p. 101). He also maintains that the distinction between belief and knowledge is practically useless. It is not obvious, however, that this is true. Suppose I have misplaced my keys, but I know they are somewhere in my house (I even know that I know this). Surely my search behavior will be different as compared to the situation in which I merely believe they are somewhere in my house. Only in the latter case would I consider retracing the steps I made outside of the house earlier today.

Hardin seems to believe recourse is needed to some 'super knower' if the traditional philosophical distinction between knowledge and belief is to be maintained. It is, however, not clear why. The idea that what you know is true, whereas what you merely believe need not be, is deeply ingrained in the common sense conception of knowledge (the question Hardin has used as the title of his book is often used to express skepticism about the epistemic status of a claim someone has just made, and in those cases presupposes the very distinction Hardin is skeptical of). The distinction may well be a genuine one even if in particular cases we have no means to establish with certainty whether someone knows something or merely believes it, and even if there is no 'super knower' who knows this. So Hardin's skepticism concerning the distinction seems unjustified (which may not bother him, because he is not convinced that justification is a useful notion). Hardin's dismissal of the philosophical literature is in fact somewhat annoying. He claims that the whole distinction between knowledge and belief is unimportant for his purposes (p. 26). In light of this, it would make more sense to sidestep the issue altogether.

I also find Hardin's dismissal of psychological research about constraints on decision-making too glib. Here is his characterization of it: "[I]ts arguments and conclusions range from the often dazzling and powerful to the ordinary and merely commonsensical. That literature is full of ad hoc labels for countless patterned behaviors" (p. xii). What Hardin presents us with is a series of somewhat speculative and loosely connected arguments and observations based on armchair cost-benefit analysis. Ideally science provides theoretical understanding of phenomena that is supported by empirical evidence. Hardin may be right when he suggests that psychology often gets stuck in piecemeal empiricism. However, armchair rationalism does not provide for a very satisfactory alternative.

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Review of *Darwinism and economics*, edited by Geoffrey M. Hodgson (The International Library of Critical Writings in Economics Series, vol. 233). Cheltenham and Northampton: Edward Elgar, 2009, 457 pp.

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The reorienting of economics has by now gained full momentum, although one can still barely see how its different themes are converging. The *Critical Writings in Economics* collection provides a window into the course of this hard to follow trend thanks to its auspicious combination of a publisher's committed interest, the patronage of Mark Blaug as series editor, and exemplary paper anthologies. Volume 233 comes out of the efforts of Geoffrey M. Hodgson, a leading thinker in the institutional and evolutionary current in economics and an authoritative advocate of the historical specificity of economics. It consists of a selection of 24 contributions previously published between 1990 and 2008 that build the case for understanding economic behaviour as ultimately a reflection of evolutionary principles.

The schema the editor proposes for reading this selection is based on a dichotomy between theories of interaction and of communality, to reflect causal interaction between the natural and the social world, and concepts and assumptions shared by both domains, respectively. In spite of overlapping themes, mostly resulting from the underlying genotype-phenotype distinction that may render that dichotomy redundant, the book's schema forcefully points to the influences currently feeding into economics across established disciplinary boundaries, from biology, genetics, history, anthropology, psychology, and management theory. 'Imperialist economics' is apparently no more; the dominant feeling now is an inferiority complex.

John Nightingale ("Universal Darwinism and social research", 2000) reminds us about the admittedly pejorative picture of the economist as a "scavenger digging around the mullock heaps of the more developed sciences, searching for scraps of sustenance". The same feeling is evoked by Jack J. Vromen ("The human agent in evolutionary economics", 2001), when he voices the concern that one serious shortcoming of mainstream economics is that "it simply ignores insights and ideas developed in adjacent disciplines".

Two of the three parts of the volume are engaged in explaining how evolutionary theory changes the basic economic tenets of rationality and causality. Part one takes on the former and expounds the biological shaping of individual decision-making at the level of individual and group selection, preferences and beliefs. Equating selection with market competition bears the mark of "the banality of mathematics and logic applied to survivability" leading to a "deducible theorem of 'competitive exclusion'", advises Paul Samuelson ("The economics of altruism", 1993). The mind's development engenders all economic behaviour by fitting together the parts of a decision-making machinery, such as computational brain modules that adapt themselves to the simultaneous needs of "reasoning instincts" to express intentionality (Leda Cosmides and John Tooby, "Evolutionary psychology and the invisible hand", 1994); "informational inputs" to account for cultural diversity (Jack J. Vromen); or maladaptive traits such as altruistic behaviour for the survival value they confer on populations (Herbert A. Simon, "A mechanism for social selection and successful altruism", 1990).

Facing a domain that has long left behind its pioneering explorations, the reader may find the near absence of real-world data puzzling in a volume that aspires to be "a useful guide for empirical enquiry". To be sure, innovative working hypotheses lure the economist into an exciting research environment: cultural group selection fits human evolution better than rational choice based explanations (Joseph Henrich, "Cultural group selection, coevolutionary processes and large-scale cooperation", 2004); an evolutionary environment is ideally suited to bring to light collaborative/defective patterns of human behaviour (Theodore C. Bergstrom, "Evolution of social behaviour", 2002; Samuel Bowles and Herbert Gintis, "Can self-interest explain cooperation?", 2005). However, game theory exercises, laboratory and thought experiments, plus stories of mice cooperation in haystack models, instructive though they may be, serve poorly as a practical reference to the way economies (may) function.

The two papers that are exceptions to this pattern raise questions of their own. In the first, Henrich, et al. ("In search of homo economicus", 2001) report the results of field experiments in 15 *small-scale societies* (i.e., foraging, nomadic herding, horticulturalist, and sedentary agriculturalist groups). Their study targets patterns of cooperation, reciprocity, or punishment to investigate the relevance of the canonical model of monetary-payoff-maximizing actors. The contrast between the case of primeval man's societies and modern behaviour seems justifiable on evolutionary grounds alone as long as both *society* and *population* are perceived as a collection of atomistic individuals. Dispensing with the canonical model altogether, to which the common thread of volume contributions in fact leads, would normally invite reflections of a different nature, for example on the cultural or historical significance of the sample and hence on the possibility of generalizing their findings. The second exception (J. W. Stoelhorst, "The naturalist view of universal Darwinism", 2007) looks through evolutionary lenses at the case of Intel's impressive advance to world dominance and concludes that selection in the marketplace operates in a similar manner as in biology. From this analysis, we learn that differential success is primarily a result of competition for resources rather than reproductive behaviour, one contending point the author wants to clarify, but remain unsure about the value of substituting the evolutionary mindset for good old competitive analysis in the first place.

Part three moves one step further from the use of analogy and metaphor and looks for an economic theory proper based on ontological communalities between social and biological evolution. The core Darwinian concepts of variation, inheritance, and selection apply to a very broad class of phenomena in the social realm, even though the selection criteria and mechanisms have nothing to do with biological survival and growth (Richard Nelson, "Evolutionary social science and universal Darwinism", 2006). The economist is instead challenged to identify preferences and values that are operative in social environments such as technology, business, and science.

A series of three papers written by Geoffrey M. Hodgson and Thorbjørn Knudsen ("Why we need a generalized Darwinism, and why generalized Darwinism is not enough", 2006; "The nature and units of social selection", 2006; "Information, complexity, and generative replication", 2008) proposes a virtually complete account of *generative selection* in the social domain. At a general level, its lesson is that appropriating Darwinian thought as a universal way of theorizing should not distract us from seeing that the ontologies of the human social world and biological structures are different. Accordingly, the model attempts to identify replicators and interactors that are *relevant* 'genotypes' and 'phenotypes' for socio-economic and cultural evolution.

The logic of social generative selection is founded on four "definitional features". Firstly, causality gets an evolutionary meaning on the premise that, citing David Hull, "interactions cause replication to be differential". A genotype or replicator is associated with a potential for differentiation at multiple levels of social interaction. It is for this reason that the selection process must be understood in terms of "corresponding pairs of replicators and interactors" which hold the key to understanding the observed behaviour. Secondly, the replicator should in its own capacity ensure copying fidelity among the social units of selection. Thirdly, the selective pressure is triggered by a transfer of generative information between interactors, like firms and institutions, and replicators, which act as "stores of social dispositions, rules and knowledge". A fourth and final feature is summed up by the conditional generative mechanism, a material entity which is required in order to "turn input signals from an environment into developmental instructions". It is thus possible to analyse the objects of our study as full-fledged "generative replicators", like habits and routines, that satisfy all four criteria, or as only "emergent expressions", like ideas, that fall short of developing a reproductive capability of their own.

Part two opposes the historical and nomological modes of evolutionary explanation, in contrast to the emphasis on common ground in parts one and three. On one side, Joseph Fracchia and R. C. Lewontin ("Does culture evolve?", 1999) are sceptical that "substituting the metaphor of evolution for history" may be of any use and think of it rather as the malign consequence of "the grand twentieth-century movement to scientize all aspects of the study of society". They disentangle natural selection explanations from cultural alternatives with possibly similar implications by analysing the restrictiveness of the contingent pattern of differential reproduction, which varies from "very strong constraints on which states may succeed each other" in evolutionary contexts to "purely random differential survival" in historical frames. The copying process may actually unfold in such unpredictable patterns that causation becomes impossible to discern. Two other complementary studies (Dan Sperber, "An objection to the memetic approach to culture", 2000; John S. Wilkins, "The appearance of Lamarckism in the evolution of culture", 2001) reinforce the point that a sensible distinction between the social actor and the biological individual rests on cultural attributions and learning.

On the other side, evolution is viewed as a lawful process of selection and adaptation, with in-built directionality and developmental stages. "We must search for a causal explanation", argue Geoffrey M. Hodgson and Thorbjørn Knudsen ("Dismantling Lamarckism", 2006), and natural selection is "the only possible" one. They and other contributing authors (e.g., Jack J. Vromen) find no evolutionary role for *accidents* of evolution. The selective process leads only to novelty; residual developmental errors are expurgated of "useless and injurious characteristics" which may prove detrimental to the potential to increase complexity.

The nature of the debate in part two returns us to the introductory text that serves to introduce Darwinism in the guise of a "middle-range theory" for reorienting economics towards the social realm. Much of that debate is inspired by Darwin himself who "hinted that his ideas may apply to other evolving systems, including language and social institutions" (p. xv). But it is probably safest to argue that he left the issue unsettled. For scholars who emphasize the historical nature of evolution, Darwin well understood that the mechanism of natural selection in fact applies poorly to the causes of social change (Gould 2007, 547). A rereading of primary sources can conflate the controversial nature of the originator's idea. In 1859, Darwin was "convinced that Natural Selection has been the main but not exclusive means of modifications" (Darwin 1859, 6). Is this a cautious remark, mindful of other possible explanations, or is it just a marginal note, being the very last sentence of its prefatory text? In Darwin's 1882 *The descent of man*, after six revised editions of his *Origin*, carefully crafted phrases validating the universal character of selection, such as "When a poor man becomes moderately rich, his children enter trades or professions in which there is struggle enough, so that the able in body and mind succeed best" (Darwin 1882, 135), continue to conflict with ambiguous assertions about man's social and material progress that is made possible "due to his power of speaking and handing down his acquired knowledge" (Darwin 1882, 79).

Hodgson's introduction cautiously eschews appropriating evolutionary theory in the blunt way that ideas from physics were previously imported, giving birth to neoclassical economics. The book nevertheless falls short of convincingly substantiating its claim that

Darwinian thought be construed as supportive of both interaction and communality theories. As has been suggested by some contributors to this very book, doubts persist as to the pivotal role causal explanation supposedly plays in providing a realistic view of the social world. It may just prove too elusive to pin down in a meaningful manner why things occur the way they do.

REFERENCES

Darwin, Charles. 1859. *On the origin of species by means of natural selection, or the preservation of favoured races in the struggle for life*. London: John Murray.

Darwin, Charles. 1882. *The descent of man and selection in relation to sex*, 2nd edition. London: John Murray.

Gould, Stephen Jay. 2007. *The richness of life: a Stephen Jay Gould reader*, eds. Paul McGarr, and Steven Rose. London: Vintage.

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Review of Dennis C. Rasmussen's *The problems and promise of commercial society: Adam Smith's response to Rousseau.* U. Park (PA): Pennsylvania State University Press, 2008, 208 pp.

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As its title suggests, Rasmussen's short but elegant book is about the relationship between Adam Smith and Rousseau, a relationship about which, Rasmussen notes, only a "handful" of articles and book chapters have been written (p. 6n. 8). Rasmussen's book will surely go a long way towards filling this scholarly gap, although it certainly will not (and probably should not) be the last word on the subject.

Rasmussen organizes his book by way of objections Rousseau makes to commercial society and Smith's responses to them. What emerges from Smith's replies to Rousseau, Rasmussen says, is a "deeper, more thoughtful Smith" (p. 6), one who both "unreservedly advocate[s] commercial society", but who also "accepts—indeed, insists—that many problems are associated with it" (p. 7). Rousseau saw commercial society as unhappy, and so too did Smith, for many of the same reasons. But Smith, on Rasmussen's telling, also sought to rebut or at least diminish the force of Rousseau's objections.

What does Rousseau find wrong with commercial society? Briefly, commercial society produces great inequalities of wealth, makes people weak and indolent, leads them to rely on the opinions of others for their sense of themselves and for their well-being, and creates desires in people that it (commercial society) cannot satisfy. The bottom line—for Rousseau—is that with commercial society we may have procured prosperity, but "at the cost of our goodness and our happiness" (p. 40). "Commercial society, in short, produces people who are good neither for themselves nor for others" (p. 40).

Although it would later be carried on, magnified, and given additional nuance by others (Marx, Nietzsche, Thoreau, and even Smith), Rasmussen maintains that Rousseau's critique was the most comprehensive and still "represents the greatest challenge for someone who hopes to defend commercial society" (p. 49). Rasmussen believes that Smith was able to make this defense of commercial society, and make it successfully. The majority of Rasmussen's book is taken up in showing how.

The key move of Smith's defense—according to Rasmussen—is to demonstrate that the advantages (prosperity, liberty, and security) of commercial society outweigh its flaws, which anyway are not "as numerous or as great" as the flaws of other, earlier forms of society (p. 6). As Rasmussen summarizes in his final chapter, "Rather than simply claiming that commercial society is good or bad, Smith constantly asks, 'In comparison to what?'" (p. 158). Commercial society, Rasmussen concludes, was for Smith (as it should be for us) "the worst form of society except for all the others that have been tried" (p. 175).

Before turning to Smith's response we might want to pause and consider what the goal of Rousseau's critique of commercial society was, precisely. It is probably wrong to think of Rousseau's portrait of primitive man in his second *Discourse* as urging us to return to an earlier state of nature. Surely in this regard we must take seriously Rousseau's starting point in that piece, viz., that he is going to begin by "setting all the facts aside" and that his researches should not be taken for "historical truths". We were never in paradise.

So Rousseau is not necessarily arguing that we can or should forsake commercial society because we were better off (happier, more equal) earlier. What Rousseau may be arguing, instead, is that we should refuse to be *content* in commercial society; that the proper attitude toward commercial society should not be one of endorsement, but one of detachment and alienation. Rousseau at many places in his work could simply be saying that we may never be happy, given our plight (pp. 47-48).

If this is so, then we might wonder whether Rasmussen's (and according to Rasmussen, Smith's) strategy of showing that commercial society is better off than pre-commercial societies were in terms of wealth and the distribution of wealth might not directly be to the point. Merely challenging Rousseau's empirical claim that commercial society results in inequality of wealth does not seem the best way to rebut Rousseau on the damaging effects of commerce (especially if the problem is luxury itself: an equal distribution of luxury does not solve *that* problem). Nor will it do to say that the riches of commercial society will enable it to pay for education for the poor, thus making up for the stultifying effects of the division of labor on them (p. 110). This latter claim is akin to allowing a company to pollute on the ground that the taxes it pays will go towards environmental clean-up. This may persuade the pragmatic environmentalist, but not the one who is pure of heart. (The pure of heart will not be persuaded to sell his soul because the benefits outweigh the costs.)

A better sort of response to Rousseau might be to show that, despite the evident disadvantages of commercial society, we can still reconcile ourselves to living in such a society, and that there is nothing morally wrong about being so reconciled. Rasmussen and Smith approach this kind of response when dealing with Rousseau's critique that in a market society we only worry about *seeming* to care about other people, or *seeming* to be virtuous, not about actually being caring, or being virtuous. Smith's reply is that seeking the approval of others can give us a powerful incentive to actually *be* moral. Moreover, in order to get ahead in commercial society, one must really care for the interests of others and not just seem to; one must also exercise the real virtue of prudence (p. 122). Finally, although citizens must still depend on the opinions and actions of others in commercial society, citizens are not dependent on any *one* person, which gives them "an independence that the serf, the spaniel, and the ambitious poor man's son lack" (p. 124).

This response to Rousseau strikes me as more promising than simply totaling up the benefits of commercial society and discovering that they outweigh the costs (see, pp. 91, 129). Rasmussen has Smith saying that morality may depend on the opinions of others, and that commercial society may actually *foster* a certain type of virtue, not destroy it. Here the argument is not, or not entirely, that while commercial society corrupts us, it nevertheless has other, countervailing advantages. Rather, the argument is that commercial society in fact can *better* people, and better them morally.

In chapter 4, by far the most interesting and novel chapter in the book, Rasmussen sets out to solve an apparent paradox in Smith. Smith defends a kind of society that encourages people in the mad pursuit of material goods, while simultaneously insisting that material goods cannot and will not make us happy (here Smith obviously echoes Rousseau). Rasmussen puts the paradox carefully and pointedly, calling it "one of the most fundamental and puzzling questions of Smith's thought". Why does Smith "advocate commercial society if it undermines people's happiness?" (p. 131).

Rasmussen's attempt at a solution to this puzzle is unsatisfying, although one cannot fault him for trying. For Smith, Rasmussen writes, commercial society is defensible because it provides liberty and security for "more than a relatively small number of individuals" (p. 144), and liberty and security are the prerequisites for happiness. Putting it another way, while commercial society may not guarantee happiness, it nonetheless removes two chief obstacles to happiness: dependence and insecurity (p. 131).

But the problem with this answer is that elides the fact that commercial society, on its way to removing dependence and insecurity, itself produces its own barriers to happiness. In order for commercial society to survive and prosper, people must pursue the baubles and trinkets that cannot in the end satisfy them. "People typically think they would be happier if they had more money", Rasmussen writes in an earlier chapter, "but Smith argues that this false belief actually tends to lead to *un*happiness" (p. 86). Although liberty and security may be goods in their own right and moreover goods which commercial society supplies, commercial society also fosters conditions which make us unhappy *in spite of* being free and secure. We are back to being faced with the paradox: commercial society undermines people's happiness, yet Smith advocates it.

Rasmussen's work is useful because it forcefully shows us the manifest tensions in Smith's thought (chapter 4 may be the best example of this). It is perhaps this paradoxical quality that makes Smith's work so engaging, and why he deserves to be wrestled with as a moral philosopher. Rasmussen may not have resolved the tensions in Smith's work that he illuminates so well, but he puts those tensions into sharp relief, a necessary step in resolving them—if such a resolution is to be had.

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Review of Paul Dragos Aligica and Peter J. Boettke's *Challenging institutional analysis and development: the Bloomington School*. London and New York: Routledge, 2009, 176 pp.

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This book is about the school that was built by Vincent and Elinor Ostrom at Indiana University, Bloomington (USA). It provides an insight into the origins and philosophical foundations of the 'institutional analysis and development' (IAD) framework of the so-called Bloomington School, which is at the core of Elinor Ostrom's work on governance for which she received the 2009 Nobel Memorial Prize in Economics (jointly with Oliver Williamson). However, this book is actually mainly about the work of Vincent Ostrom as he was the one who provided the philosophical foundations of the Bloomington School.

The book is divided into three parts, which inform the reader about the origins, thesis, and concepts of the IAD framework (part 1); its social philosophy—the most important part—(part 2); and its 'intellectual context' (part 3). At the end of the book short interviews with Vincent and Elinor Ostrom are presented.

THE ORIGINS

The history of the development of the IAD framework starts in the 1960s in debates over the reform of American municipal government. It was generally supposed that the cause of many administrative problems was the existence of a large number of independent public jurisdictions within a single metropolitan area. That was perceived as a recipe for chaos that required replacement by a single coordination centre. The orthodox view that large bureaucracies were more efficient in providing public goods and services and solving administrative problems within an area was based on a well established traditional paradigm of centralization in political science.

THESIS

Vincent Ostrom developed an alternative 'political economy approach' that questioned the traditional view that large bureaucracies are more efficient in providing public goods than decentralized coordination arrangements. He focused on coordination through patterns of inter-organizational arrangements which may induce 'self-regulating tendencies'. In order to be able to test the hypotheses of the political economy approach, the Ostroms developed the general IAD framework over several decades—a set of theories, concepts and methods to operationalize and measure variables and performance. This book does not aim at providing a comprehensive overview of all the contributions of the Bloomington School, but focuses on the ideas of Vincent Ostrom as the philosophical foundations of the school.

Inspired by the institutional economist Michael Polanyi, polycentrism and monocentrism became central concepts for the Bloomington School. A monocentric political system is one where "the prerogatives for determining and enforcing the rules are vested in a single decision structure that has an ultimate monopoly over the legitimate exercise of coercive capabilities" (Aligica and Boettke 2009, 21). A polycentric political system has many centres of decision-making that are formally independent of each other: "No one has then ultimate monopoly of the legitimate use of force. All rulers are constrained by the 'rule of law'. This makes the rule systems central in the study of polycentric systems" (p. 21). When developing hypotheses about the efficiency of the two political systems the Ostroms used Polanyi's insights about the advantages of polycentric systems with respect to their built-in mechanisms for self-correction and institutional innovation.

THE LOGIC OF POLYCENTRIC SYSTEMS

The structure of the polycentric system is a function of the presence of polycentricity in the governance of each basic type of social activity: governmental arrangements, economic affairs, political processes, and judicial affairs in constitutional rule. For the system as a whole to function well there should be a certain degree of polycentricity in the different domains of the system, that is to say there should be a certain coherence, a certain 'logic' between the political, economical, judicial and social domains. Polycentricity describes a complex system of powers, incentives, rules, values, and individual factors combined in a complex system at different levels. When the logic is disturbed because of, for instance, technological innovations or changes in one of the related domains, then the resulting imbalance will be an incentive for actors to initiate adaptations in related parts of the system. The dynamics of these changes can be understood in terms of the 'logic of the system'.

THE NATURE OF THE GOOD

The nature of a good or service in terms of excludability is basic to the theory of public economics. When a good is not suitable for private production because consumers cannot be excluded from consumption, or only at high costs, then a 'collective consumption unit' has a variety of options to organize the production, such as establishing its own production unit, contracting with a private firm, or contracting with another governmental unit. Centralized production by the state is also an option, but certainly not the only or most likely one.

The nature of the good in most cases is not an ontological given: technological and institutional arrangements have an effect on the degree of choice and accordingly on the way the nature of the good is perceived and its production organised. There is a complex interplay between goods, technology, and institutions. New technology and institutions can destroy forms of exclusion, thereby initiating an institutional change. The dynamics are not only driven by the interaction between technology and institutions, but also the power play between different interest groups. The existing structures serve certain interests better than others would and so changing those structures implies that those interests will be harmed, creating resistance, conflicts and struggle.

THE FOUNDATIONS OF SOCIAL ORDER

In part 2 of the book, Aligica and Boettke discuss the core of the Bloomington School's social philosophy of social order and change, an issue which has not been very well discussed elsewhere and so is considered by the authors to be their major contribution.

Vincent Ostrom developed a theory of human actors that was not based on the abstract formal attributes of an actor, like full rationality, but on stylized facts derived from an anthropological and historical understanding of the central issue in social science: choice. Choice is loosely defined as actors being able to consider alternative possibilities and to select a course of action after comparing and assessing the consequences of different alternatives. Choice is thus a form of selection.

The analysis in part 2 concentrates on the connection between the theory of choice and the theory of institutions via a theory of learning, knowledge, ideas, and language. According to this approach 'reason-based choice' is not relevant to understanding how actors choose, but rules, routines and institutions become crucial. For Vincent Ostrom, 'threats', or more general 'problems', are the starting point for the analysis of how and what actors choose. Threats like tyranny, potential chaos, and uncertainty demand solutions that are to be found in institutional arrangements that constrain actors in their behaviour, coordinate that behaviour, and reduce uncertainties.

CHOICE AND THE IDEA-CENTRED APPROACH

Central to the work of Vincent Ostrom is the concept of choice. Not the kind of choice familiar from neoclassical models, in which actors are modelled and put into situations where 'no choice is left', but the so-called 'epistemic choice', that "illuminates the various choice dimensions—operational, public and constitutional—but at the same time emphasizes that choice in institutional matters is ultimately a choice of ideas and is intrinsically linked to learning and knowledge" (p. 131). Actors choose on the basis of ideas, defined as covering "a broad class of beliefs, worldviews, values, motives, intentions, causal beliefs, operational codes, etc." (p. 91). Ideas both reflect and create social order. They are design concepts and represent both the ontological and epistemological keys of social order. Choice implies selection and when that takes place under constraints one can expect that patterns will emerge. Rarely do institutions emerge as accidents and rarely as the result of deterministic forces: the largest part of them are the result of human construction, of deliberation, reflection, and choice.

THE NORMATIVE DIMENSION IN SOCIAL SCIENCE

In the Bloomington School the value of a theory is its capability to indicate the consequences that can be expected from specific structural conditions. If those consequences are not appreciated by the members of the community they will undertake action to change the structural conditions, like institutional arrangements. In other words, the 'is' are compared with the 'ought'—consequences are assessed in the light of their contribution to 'human welfare'—and so one must engage in normative analysis (p. 113). A change in decision-making arrangements can transform patterns of human interaction from "unproductive pathological relationships to productive relationships" (p. 113). But then one also needs solid criteria to determine what is pathological and what is not. In other words, the "science of norms" that Vincent Ostrom advocates should be seen not only in a positive way (as the rules of the game and the mechanical working out of their implications), but also as an exercise in thoroughly reflecting about human values and the criteria for decision and action (p. 113). These foundational aspects rule out the possibility of a value-free social science that is just a replica of natural science. "Value terms are at the core of rule-ordered relationships, and rule-ordered relationships are at the core of political order and social relationships" (Vincent Ostrom, quoted in p. 114).

To understand problems and threats in a community and to design effective policies it is crucial to have a deep knowledge of the institutional environment, and to have a deep understanding of the local knowledge, perceptions, and ideas of the actors. What are the values and norms that drive their actions? The task of the policy analyst is not only to use values as entry points or vehicles for analysis but to apply them. "These values are not 'given'. One has to derive criteria for choosing one alternative over the other and to assess their consequences" (p. 118). In other words, the applied dimension presses up against the problem of norms not only at the analytical level but also at the decision making one. The world of policy analysis is in the end the world of implementation—i.e., the world of action and decision—and therefore a world of normative commitments.

INTELLECTUAL CONTEXT

The Bloomington School is often incorrectly associated with mainstream thinking on rational choice. So far there has not been a thorough account of the foundational core of the program, but this book fills that gap. Based largely on an examination of the work of Vincent Ostrom the authors demonstrate how over several decades a school of thought was constructed, and a new framework of theories and methods developed, to provide the tools to analyse problems from the perspective of polycentrism. In part 3 of the book, the Bloomington School is discussed in relation to its intellectual context. It is connected to its predecessors in the spontaneous dynamics of social order like M. Polanyi, F. Hayek, the German ordoliberalism and the Scottish Enlightenment. Aligica and Boettke explain the links (and differences) with Nozick and Buchanan, and show the links with modern authors like Avner Greif. They show how the Bloomington School differs from neoclassical economics and traditional public choice (as it developed over the years), but has clear connections to the Austrian School, Alexis de Tocqueville, and Adam Smith.

It is interesting to note that even facts about the Bloomington School do not ‘speak for themselves’, but are analyzed and constituted by the theoretical lens of the authors. Aligica and Boettke see many connections to the Austrians but surprisingly no connections with the American Institutionalists (except for a short reference to John Dewey). It is, no doubt, my own specific theoretical lens that shows me how the work of Vincent Ostrom connects intellectually to the world of American Institutionalism. The Bloomington School’s ideas about interaction between actors and institutions, of language being constitutive, of normative theory, of facts being theory laden and theories being value laden, about the driver of ‘threats’ and assessment of consequences (instrumental value theory), of the need for a participant-observer approach, and so on, are all to be found in the work of American Institutionalists like Veblen, Commons, and Ayres, and in the work of their successors (see, e.g., Bush 2009). This does not make the analysis in the book any less worthy. On the contrary, the book is a very valuable contribution to understanding the foundations of an important school in institutional economics and highly recommended to students who are already rather familiar with the world of institutions and interested in more fundamental issues concerning modelling actors in relation to the structures surrounding them.

REFERENCES

Bush, Paul D. 2009. The neoinstitutionalist theory of value: remarks upon the receipt of the Veblen-Commons Award. *Journal of Economic Issues*, 43 (2): 293-304.

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Review of *Intellectual property and theories of justice*, co-edited by Axel Gosseries, Alain Marciano, and Alain Strowel. London: Palgrave Macmillan, 2008, 296 pp.

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More than a quarter-century ago property scholars interrupted the hegemony of a law and economics discourse focused exclusively on efficiency with broader theories about property and social relations. As the New Jersey Supreme Court declared in 1971 in the historic case of *State v. Shack*, "[p]roperty rights serve human values". Modern property law balances plural values beyond efficiency to consider personhood, health, dignity, liberty, and distributive justice.

In contrast, at the start of the 21st century intellectual-property scholarship remains moored to a singular economic account. In the modern day, intellectual property (IP) is understood almost exclusively as being about *incentives*. Its theory is utilitarian, but with the maximand simply creative output. Law's goal is to calibrate the optimal length of copyright and patent terms to promote efficient innovation. Critiques of the recent expansion of intellectual-property law's breadth, scope and duration resonate in the same language. Progressive law and economics scholars argue that too much IP can impede innovation, locking up the building blocks necessary for further innovation.

It should be noted that understandings of intellectual-property law were not always this way. Copyright law emerged out of the Enlightenment in England in the early 18th century; limited rights to authors broke the perpetual monopoly in intellectual works held by printers, encouraging the creation of new works and their broad dissemination to a democratically engaged public. Patent law has always sought to encourage access to knowledge, requiring owners to share knowledge of their inventions in exchange for limited monopoly rights, rather than protecting the knowledge as a trade secret. And trademark law originated in theories of unfair competition and tort, not property law. But over the last few decades law and economics scholars have reimagined intellectual-property law, portraying it as solely an instrumental mechanism to incentivize creativity (copyright), invention (patents), and industry (trademarks). Because information is assumed by its nature to be nonrivalrous and nonexcludable, the concern is that free-riding will eliminate any incentive to produce information. The insertion of property rights, the theory goes, incentivizes the production of information, which will then inure to society's benefit through the market mechanism, with those willing and able to pay being permitted to consume the information. Others might free ride, but only where high transaction costs would make marketplace exchanges unlikely. In short, market failure is cited as the *raison d'être* for intellectual property, explaining copyright, patent, and even trademark.

But intellectual property today is more than simply a tool for incentivizing creative production in the form of more things, from *i*Pods to R2D2. Intellectual-property laws bear considerably upon central features of human flourishing, from the developing world's access to food, textbooks, and essential medicines, to the ability of citizens everywhere to democratically participate in political and cultural discourse, to the capacity to earn a livelihood from one's intellectual contributions in making our world. And yet, to date much scholarship in this area insists that law's focus is efficiency alone. The dominance of this singular, narrow economic discourse has rarely been challenged.

This is now beginning to change. Emergent social movements, around access to HIV drugs and other essential medicines, have combined forces with open source advocates in the software and Internet fields to insist upon "access to knowledge" as a human right. Highlighting the constitutive role of knowledge in promoting central human capabilities, from health to education to the right to participate in and enjoy culture, these social movements are beginning to influence theoretical understandings of intellectual-property law, as well. The result has been increased interdisciplinary engagement with intellectual-property law, from fields as diverse as anthropology and science and technology studies to philosophy. Each of these disciplines brings an important lens to contemporary intellectual-property law, and challenges the dominance of the singular economic vision. Anthropology helps us to consider more deeply a central purpose of this law: the promotion of culture. Anthropology suggests that law's current focus on the production of more things misunderstands the essence of culture itself—participatory community and shared meaning. Science and technology studies reveal that technology is not merely science, but also a social and political artifact.

Philosophers attend to the moral questions raised by intellectual property. Such questions are legion today with the exponential growth of intellectual property to cover everything from medicines to seeds, and with the steady march of this law into every corner of the globe, including the poorest countries on Earth. Even in the least developed countries, the dominant approach has remained law and economics, relying upon the market to spur creation. But this leads to the mistake that drugs for baldness are more important than drugs for malaria because the former enjoys a multi-billion dollar market, while those who need the latter are too poor to offer much to save their own lives. Understanding intellectual property as the incentive-to-create reduces to the claim that the ability to pay, as evidenced in the marketplace, should determine the production and dissemination of knowledge and culture.

Intellectual property and theories of justice (Gosseries, et al. 2008) is a much-needed intervention into current debates over intellectual property and social justice, a topic once thought irrelevant to IP.[1] The book considers the theoretical foundations of intellectual-property claims—are these *rights* rooted in Lockean claims, or are they merely *tools* to promote innovation? Utilitarians, who seek to maximize the overall social welfare, show little concern for the distribution of social welfare, but the contributors to this volume ask whether IP law ought to attend to maldistribution of resources and wealth that flow from IP law, from pricing medicines out of the reach of the poor to the redistribution of wealth from the IP-consuming South to the IP-producing North. Perhaps most importantly, the book is focused on plural values, for example, not just efficiency or equality, but also freedom.

As Axel Gosseries writes in the introduction, “Not having enough money to buy non-generic drugs clearly raises problems of both equality and freedom. Therefore, redistributing resources, even at the cost of taxing people, amounts to redistributing real freedom” (p. 9). Gosseries argues that while efficiency concerns are important, they “are not the end of the matter. They need to be plugged into theories of justice” (p. 16). Scholars in this volume consider not only the relevance of Locke and Nozick for understanding intellectual-property rights (some argue they are less relevant than many think), but also of Rawls and G. A. Cohen.

The strength of the volume is not so much in introducing new visions of intellectual property (for example, as a human right, or as a

[1] See generally, Chander and Sunder 2007.

tool for promoting central human capabilities). Rather, the essays here question and probe deeply the oversimplified justification of modern intellectual-property law as incentives-to-create. Theorists in this volume recognize they can take efficiency seriously "in ways that go beyond merely defending a trade-off or convergence", in Gosseries words (p. 16). Giovanni Battista Ramello, for example, uses economic analysis to show that exclusive rights in knowledge decrease the overall productivity of knowledge by undermining its social purpose. Ramello argues that the concerns of efficiency and social justice advocates converge because both want knowledge to be put to its most productive use: to serve societies (p. 86).

It is helpful to push beyond the traditional incentives theory from both within and without economics. Economic theories of knowledge as a unique good, of development and human capabilities, and of distributive justice can helpfully broaden existing law and economics approaches to IP. But interdisciplinary accounts of intellectual property that go beyond economics are also necessary. Indeed, we must insist on plural accounts of this law, which regulates culture, freedom, democratic participation, and equality itself. Most of the contributions to this volume do not go this far, largely taking the tack that theories of justice may be added on to the dominant approach.

Still, there is much to be learned and gained from the critiques of IP's incentive theory herein, and many are quite powerful. Seana Shiffrin provocatively asks: even if incentives are necessary, *are they just?* She distinguishes between various demands for incentives, finding some more fair than others. Claims that incentives are necessary to recoup costs or maintain a livelihood are fair—she says—but they would justify only weak IP rights and nothing like the maximalist rights that exist today (p. 96). In contrast, the demands for stronger and more far-reaching rights we hear today offer a different incentive argument: that creative people will refuse to make or share their works in the absence of a monopoly reward. In the case where "talented people ransom their talents, withholding their creative products in order to demand greater compensation", Shiffrin concludes that such arguments are inconsistent with the tenets of a Rawlsian vision of a just society, in which "a just citizen accepts that social and natural talents as well as one's market position are arbitrary from a moral point of view" (p. 101). Even if one does not endorse a Rawlsian vision of justice, the question of inconsistent or unfair motives remains. For example, says Shiffrin, we ought to be skeptical of those who seek to use copyright to protect their works against critique and comment, because these norms are inconsistent with our society's commitment to free speech. Shiffrin further asks whether a legal system that acquiesces to immoral demands is not itself unjust (p. 102).

At the end of the 20th century too few legal decision-makers asked such questions and intellectual-property rights were expanded with abandon, propelled by the simple elegance of a dominant law and economic understanding of intellectual property as incentives-to-create. Now, at last, the dominance of that account is being challenged by scholars in a variety of disciplines questioning the assumptions, effects, and goals of this law, which reach far beyond incentives. The analyses here are sophisticated and compelling, teaching much about the ways in which philosophy can illuminate and enrich economic analysis of law.

REFERENCES

Chander, Anupam, and Madhavi Sunder. 2007. Is Nozick kicking Rawls' ass?: IP and social justice. *U. C. Davis Law Review*, 40 (3): 563-580.

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PHD THESIS SUMMARY: The usefulness of truth: an enquiry concerning economic modelling.

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This thesis attempts to justify a normative role for methodology by sketching a pragmatic way out of the dichotomy between two major strands in economic methodology: empiricism and postmodernism. It is important to understand that my thesis is *about* methodology and this means that I do not add another recipe with prescriptions as to how economics needs to change in order to become a “better” or “proper” science. Instead, I discuss several methodological approaches and assess their aptness for theory appraisal in economics.

I begin with the most common views on methodology (i.e., empiricism and postmodernism) and argue why they are each ill-suited for giving methodological prescriptions to economics. Then, I consider positions that avoid the errors of empiricism and postmodernism. I specifically examine why the two major strands of methodological criticism fail to give helpful methodological advice to economists and sketch out a pragmatic approach that *can* do this.

Basically, there are two different demands from empiricists: the first requires that economic models become more falsifiable and their *results* must be more severely tested. The second demand claims that the behavioural basis of economics (i.e., the rationality assumption) must be enriched or replaced by more empirically founded theories of human behaviour. These two attacks are the most common and best known forms of criticism against mainstream economics. I counter these attacks by showing that the theoretical core of mainstream economics can be defended as fruitful and largely unempirical heuristic device. This does not mean that more empirical approaches are ruled out, but it *does* mean that the state of economics is not as hopeless as the empiricist critic suggest.

After rejecting the empiricist position I turn to postmodern relativism. I first present the general idea and then turn to some of the best-known relativistic positions in economic methodology: Bruce Caldwell's pluralism and Deirdre McCloskey's rhetorical approach to economics (Caldwell 1982; McCloskey 1985). I discuss critically Paul Boghossian's recent work "against relativism" (among others) which is a systematic approach to refute basic postmodern convictions. In my discussion I show why his arguments fail to hit the target. The main reason is that constructivism is an irrefutable position. In total, it turns out that the postmodern rejection of 'Global Truth' cannot be refuted, but this does not necessarily lead to giving up prescriptions at the *local level.* In economics however, the two most prominent postmodern authors fail to give useful and accepted advice even at the local level; they fail to achieve their self-set goal of improving the critical discussion of economic models.

The last main section tries to overcome the dichotomy of empiricist and postmodernist methodological positions by offering a pragmatic way out. Where postmodern methodologies are often *based* on their rejection of empiricist positions, there is no principal reason why empiricist arguments should play no role on a local level. The most promising way towards a useful concept of theory evaluation seems to look first for a characterisation of economics that economists can accept and then search for quality criteria that are in line with that description. This rules out fundamental criticism, of course, but if the aim of theory appraisal is improving a critical discussion about models, fundamentalism does not lead very far but is rather a rejection of the basic premises. A pragmatic point of view that focuses on evaluating the quality of solutions for given *problems* is much more likely to settle a discussion about models than general methodological arguments derived from philosophical positions such as empiricism.[1]

My pragmatic approach to theory appraisal draws inspiration from a reinterpretation of Milton Friedman's (1953) classic. I extend his views to an economic approach for theory evaluation. This leads to two separate developments that can contribute to a new pragmatic way of normative reasoning in theory evaluation. The first idea is to apply the concept of cost-benefit analysis to theory choice. Economic theories of science have rendered Friedman's claim to "explain much by little" more precise by offering a radically problem dependent way of assessing theories. Economic philosophy of science theory accepts that there is no

[1] Note that I take 'problems' in their broadest sense, so that even philosophical problems are *valid* problems.

single right criterion for judging science and so the only evaluative question that makes sense is whether a theory is the best and most efficient way to solve the problem it attempts to solve. The second idea taken from economics and applied to the evaluation of science is not directly related to economic *theories* but argues that the *institutional structure* of science is the best starting point for improving the quality of a science. From this point of view, science is seen as a collective process of individuals that maximise their reputation and do not necessarily care much about good theories. This twist allows for accepting that various "irrational" social factors are interfering with science and still argue for an epistemic privilege of scientific knowledge. In such a way, normativity has shifted from single theories to a meta-level of analysing and improving the organisational structure of science.

In concluding, I try to answer the crucial question at hand: what is left for theory evaluation? To be sure, rule based single-criterion methodologies are rejected because methodologists are *not* in a privileged position to tell economists what to do. However, if they have any knowledge about economic methods, they can assess (as well as good economists can) whether a model is a genuine contribution for solving the problem it was set out to deal with. The criteria to judge this are often implicitly given by the description of the problem itself: for example, theory-ladenness is less of an obstacle, when science is not supposed to deliver objective description, but answers to problems. This is because trying to solve a given problem already presupposes and accepts much theoretical background. If one deals with explaining GDP growth, the theory needed for measuring the GDP is already presupposed and out of question. In a nutshell, problem-orientation accepts that science does not start in empty space but is always embedded in a context that defines problems, background knowledge, and the actual aims of science. This does not lead to relativism, but brings about a discussion concerning the particular means for arriving at a given end in the first place.

REFERENCES

Caldwell, Bruce J. 1982. *Beyond positivism: economic methodology in the twentieth century*. London: Allen & Unwin.

Friedman, Milton. 1953. The methodology of positive economics. In *Essays in positive economics*, Milton Friedman. Chicago: University of Chicago Press, 3-43.

McCloskey, D. N. 1985. *The rhetoric of economics*. Madison: University of Wisconsin Press.

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PHD THESIS SUMMARY: Intellectual paths and pathologies: how small events in scholarly life accidentally grow big.

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The aim of this work is to examine the conditions under which ideas in the history of economics do not evolve or continuously develop towards a steady state of perfection. Evolution in the intellectual world is characterized by various forms of discontinuity. The key notion that is used to explain this specific type of evolution is *intellectual path dependence*. The perspective of (intellectual) path dependence shows a history of ideas with punctuations that mark shifting pathways. When such pathways become sufficiently mature, they represent the same phenomena in different ways.

HOW DOES THE PROCESS WORK?

The briefest way to re-tell the mechanism that makes intellectual path dependence work is the following: initial conditions of certain ways of thinking sometimes lock us in to particular pathways. Such pathways occur when the follow-up to particular small events catches intellectuals irreversibly in a complex web which then grows bigger. The distinctive property of such pathways is that the evolution of ideas does not necessarily lead to any pre-defined end point. Small events trigger shifts in the course of events and this leads to (extra-) positive or (extra-) negative consequences that move the system away from its original direction.

After small events take place, the complex webs of scholarly life function in either of two ways: (i) as a short-cut that moves the system to a better state and elevates it to higher levels of order which without interruptions could only have been reached within longer time spans; or (ii) as a hindrance that breaks down the system and prevents intellectuals from proceeding further and achieving intellectual progress. Small historical events become a hindrance (ii) when a small uncorrected error feeds back a negative cumulative effect for the progress of scientific knowledge. Small historical events can operate as a short-cut (i), however, in conditions that turn the event into a starting point for a new pathway at the expense of an old one—by unlocking a previously blocked pathway—and thereby perhaps lead to more complex evolutionary pathways which move the system to more coherent and sophisticated levels.

WHAT DOES ALL THIS MEAN?

In less technical terms, intellectual path dependence holds the view that the main reason why we have come to take the same evolutionary path as our predecessors is that people follow established habits of thought according to which they think, behave, and act. A path-dependency world view suggests that we are not really entitled to begin talking about intellectual and practical problems in the terms that we are accustomed to, especially when we are more knowledgeable than past generations about the shortcomings and imperfections of the constructions that we continue to construct.

Historians of economics are within the same circle: we do not need a depiction of economics expressed in the terms (and the ideology) introduced by *Utopia.* We do not need *one* theory of economics providing us with solutions to *all* the worldly problems of human societies that have existed in history and all around the globe. There should also be no presumption that "progress" in the history of economics would cure all the imperfections in and of the past (thus irreversibility). In other words, "markets for ideas" often fail to fully reverse the consequences of errors because of a complex set of reasons that I have tried to set out in this book. We should underline, additionally, that errors *and* corrections, considered together, are two of the non-eliminable constituents of the evolutionary history of human institutions. The relationship between the two is complex and, as they interact upon each other, they generate further irreversible and unpredictable outcomes.

The dissertation is built upon a case study of the 'Coase theorem'. This shows that the *cause* of the emergence of an idea (the assumption of zero transaction costs) was too small to give rise to a big consequence (the 'Coase theorem'). An error in the history of the "theorem" turned into an intellectual pathology when Stigler's (1966) representation of Coase (1960) functioned as a tipping point in the evolution of the perception of Coase's main argument. The reason why the "Coase theorem" has not been corrected for so long is principally economical. In other words, the economics of this particular case has prevented correction from happening. The "market for ideas" in a positive transaction costs world does not allow negative externalities to disappear quickly: due to the high epistemic costs of retesting previously published findings of scholarly research, economists failed to replicate the original results of Ronald Coase in 1960 and onwards. Retesting the original contribution would have changed the fate of the 'Coase theorem' long ago. But running experiments about the validity of past findings requires time and this has been the scarcest "commodity" for university researchers.

Path dependence, in general terms, is regarded as resulting from blind processes that do not consummate at a certain end-point. In medicine, blindness is usually considered to be a pathological situation that causes a person to lack visual perceptions. In fact, blind processes, from a philosophical point of view, can be considered pathological, too, in the sense that, in nature and society, they lead to path dependent circumstances in which individuals exercise their capability for error and their capability to repeat it in the general course of events. The lesson to be drawn from intellectual path dependence is that we should develop new vocabularies and metaphors. We should do this not because we wish to break with the old. We should do this primarily because we *can* do it. It is pragmatically possible and definitely fruitful.

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PHD THESIS SUMMARY:
Diversity and economics.

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By and large, the mainstream contemporary economics literature assumes that economic behaviour can be explained by means of a singular process (usually a choice process) applied to all individuals. In discussing specific issues, it is occasionally assumed that individuals exhibit quantitative differences—either parametric or functional—in their properties, as summarised by preferences, constraints and endowments, expectations or cognitive abilities. In my thesis I argue that such an analytical differentiation of individuals (i.e., *heterogeneity*), does not exhaust the theoretical needs or the methodological possibilities of economic science. Specifically, my thesis aims to show that insufficient attention is currently being given to another form of difference, that produced by societal dynamics (i.e., *diversity*).

Some heterodox approaches, for example institutional, feminist, and radical economics, as well as other social sciences such as sociology, often proceed by conveniently aggregating individuals in a number of groups or classes, each subject to its own laws of behaviour and fundamentally influenced by the socioeconomic environment. These individual and group differences emerge as a consequence of market or institutional developments, i.e., they emerge as a consequence of the division of labour, and imply a group-specific set of constraints and rules of conduct (either legal or social norms) for individuals' behaviour. Thus, it is clear that the issue of diversity (as opposed to heterogeneity) is intertwined with the methodological issue of aggregative analysis and with the theory of individual and collective behaviour. These topics are investigated in the first chapter of my thesis, especially by comparing the views by John Stuart Mill and Gustav von Schmoller.

In the next two chapters, my argument regards a relevant example of diversity: gender inequality. I argue that in contemporary capitalist societies men's and women's economic performance is best understood when the two genders are perceived as diverse, that is, largely affected by differences in status due to institutional dynamics, rather than simply as heterogeneous, that is, as direct a consequence of differences in their endowments or preferences.

This argument is put forward in the second chapter of the thesis through a historical analysis of Mill's and Schmoller's works. The two authors are among the few economists who included a discussion as well as several references to the debate on the origin and legitimacy of gender inequality (i.e., well beyond the discussion of specific policy issues such women's employment and wages) in their major economic works. A historical analysis of Mill and Schmoller's works enables one to identify a number of factors significantly affecting men's and women's socioeconomic status: the sexual division of labour (that is not exclusively produced by market forces); the role of public policies; the relevance of unfair competition (or what might be termed gender-based discrimination); the role of law, custom, and gender roles.

In the third chapter, Mill's and Schmoller's analyses, both in terms of the theory of behaviour and the interpretation of gender inequality, constitute the basis of an applied analysis, investigating men's and women's contemporary labour market experiences in Italy.

The empirical analysis makes use of multi-level modelling, allowing us to consider individual, household and context factors simultaneously as determinants of individuals' employment status. I interpret all quantitative differences arising from a singular process (e.g., ageing) that can be reasonably considered as independent from society as instances of heterogeneity. Instead, a method compatible with the investigation of diversity is the definition of separate and different models (and estimates) of individuals' behaviour, possibly affected by totally different variables.

The analysis demonstrates that a number of variables affect only men or women, while others show opposite effects on the two. This implies that there is a significant difference in terms of theoretical models and policy implications, when defining men and women's employment as heterogeneous, thus resulting from a singular choice process, or diverse, that is, crucially affected by context and family variables, in a society where the context is clearly different for men and women.

Education is confirmed as the single most relevant variable in determining individuals' probability of employment. The analysis suggests that it cannot be assumed that aggregate growth or the expansion of the 'services sector' will automatically lead to higher women's employment rates. Rather, culture (in terms of gender roles), discrimination and unfair competition (in terms of barriers to entry to specific professions and qualifications) play a crucial role, together with social policy.

The most relevant result with respect to the main aim of this work concerns the concept of gender diversity. From the analysis, it emerges that the behaviour of each gender cannot be represented as equal or simply as parametrically heterogeneous. These empirical findings suggest that diversity might be a relevant theoretical category for economics and that its application to applied analyses may bring to light a number of topics currently not thoroughly developed in the literature.

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PHD THESIS SUMMARY: An evolutionary approach on knowledge coordination and theories of mind.

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The aim of this thesis is to provide a wide analysis of knowledge coordination from the point of view of cognitive evolutionary economics, and to connect this issue with interdisciplinary fields of research, in particular biological anthropology, behavioural economics, and philosophy of mind.

Knowledge coordination concerns the human social and cognitive capacity to delineate rules of conduct and social practices which become patterns of behaviours, tacitly codified and spontaneously emergent, without any external enforcement. Social norms and conventions are examples of these tacit rules. From an historical perspective, Friedrich A. Hayek's epistemology has been considered a remarkable point of departure for this research. In addition knowledge coordination involves the ability to formulate expectations about other people's behaviour via a *mentalizing or mindreading* skill, which consists in the capacity to attribute mental states (i.e., intentions, beliefs, goals, motivations) to other social actors, also referred to as "theories of mind".

Behavioural economics has shown that *mentalizing* is involved in processes of market exchange and the division of labour, while anthropological studies consider theories of mind as the necessary ingredient for social learning. Moreover, a wide literature in the philosophy of mind provides a clear understanding about how *mentalizing* can occur. Finally, knowledge coordination is strongly related to cultural evolution and knowledge transmission: how can skills, practices, or rules of behaviours be transmitted and spread, and thus become generally recognizable for the members of a group? They are spread through cultural evolution: social norms which have been beneficial for a group are "selected" in certain contexts and maintained in the same population across generations. A crucial question then becomes: how can economic theory deal with this issue?

Chapter 1 of the thesis examines if, and how, Hayek's theory of cultural evolution can be compatible with the current literature in biological anthropology. In particular, Boyd and Richerson's "dual inheritance theory" highlights the importance of cultural group selection in social learning dynamics: frequency-dependent bias (i.e., conformist transmission and normative conformity), prestige bias transmission, and punishment are the social learning processes which allow for cultural group selection. Hayek, then, correctly predicted the importance of this evolutionary force in the emergence of social norms. However, we must be careful in addressing similarities and differences between the cultural and biological theories of evolution: any deterministic and teleological metaphor has to be avoided since natural selection is not the ultimate factor driving cultural evolution. The importance of imperfect perception is noted as a methodological tool typical of the cognitive evolutionary paradigm: human knowledge and transmission are error-prone.

Chapter 2 analyses the methodology of by cognitive evolutionary economics. I reject the orthodox version of methodological individualism in favour of a "sophisticated", "revolutionary", and "subjectivist" version of individualism. The debate on whether Hayek's theory of cultural evolution could be inconsistent with his individualistic methodology is misplaced. Cognitive evolutionary economics is compatible with the emergent properties of social facts and with the biological concept of altruism.

Chapter 3 defines the problem of knowledge coordination as an approach combining behavioural economics and philosophy of mind. When multiple stable equilibria are present in a pure coordination game, there is no player has any material interest to choose one equilibrium over the others. Scholars disagree about how coordination can occur under such circumstances. Two models of the mind known as *theory-theory* and *simulation-theory* are investigated as possible cognitive mechanisms involved in pure coordination games. *Theory-theory* posits that in understanding other people's behaviour subjects have a "folk psychology", a tacitly codified theory about how people feel, believe, and act. By contrast, *simulation-theory* posits that attributing mental states involves a simulation and imagination process characterized by "putting oneself in other people's shoes", without any codified theory. Rather than defend one model over the other, I argue that we should associate *theory-theory* with coordination characterized by non-derivative salience, and *simulation-theory* with viable mental processes in coordination games having derivative salience.

Finally, I describe an economic behavioural experiment that has been run at ALEX, the cognitive economics laboratory at the University of "Piemonte Orientale". This experiment consists of a coordination game followed consecutively by a two-step battle-of-the-sexes game. Including an image next to the decision options aims at revealing the effects of context and learning.

It is worth pursuing further interdisciplinary research on the mentioned topics, in particular on the role of derivative and non derivative salience and the possible application of *theory-theory* and *simulation-theory* in behavioural games.

Chiara Chelini received her PhD in economics of complexity and creativity at the University of Torino under the supervision of Salvatore Rizzello, professor of economics and history of economic thought at the department of economics and legal affairs, University of "Piemonte Orientale". She has also held a research fellowship at the University of "Piemonte Orientale" (March-August 2009). Her research interests the emergence of social norms, cultural evolution, and the cognitive basis of economic behaviour. She is particularly interested in developing an interdisciplinary approach to these issues.
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PHD THESIS SUMMARY: The phenomenology of economics: life-world, formalism, and the invisible hand.

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PhD in philosophy and economics, May 2009
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What makes it so attractive to economists to be granted the rank of scientists? What role did political ideologies play in the scientification of economics? How could formal reasoning function so prominently in establishing scientific authority? And how is it that economists today hold considerable power in shaping social discourse although inside the profession the complaints about its irrelevance do not cease?

This thesis tackles such issues surrounding the modern project of an economic science with a fresh look from phenomenology. In a grand narrative of the scientification of economics and a piecemeal biography of the mathematical economist Gerard Debreu, it tracks the relation of economics and the "life-world"—that is, economists' sensibility for problems and responsibility for answers. Dwelling upon Husserl's late philosophy, this thesis makes the case for the 'forgetfulness of the life-world': practicing economics, at least since the formalist revolution of the 1950s, makes economists insensible to the very motives for their practice, and irresponsible for their claims. Instead of envisioning a phenomenological economics, the thesis shows that this forgetfulness was necessary for the formation of economics as a science. It suggests a new tone of criticism away from the deploring of its present state. Today, the question is no longer 'Which economic science?', but 'When does it disappear?'

Technically speaking, the thesis explores the "subjective constitution of economics": What kind of person does one have to be in order to be interested in an economic science? What kind of character, or "ethos", is induced by, or allowed to express itself in economics? What limits does the pursuit of scientific authority in economics impose on the conduct of an intellectual life?

This approach is unique in the field of today's commentary on science. As opposed to traditional approaches in the philosophy of science, it does not consider the epistemic principles of science, but considers science as the practices of claiming scientific authority in economic discourses. As opposed to works in science studies, however, it does not try to "explain" these practices, but considers their condition in the "life-world".

Three different meanings of the notion of the "life-world" give structure to this project: a discursive, a historical, and a biographical. I first describe in an informal way the public, professional, and pedagogical ethos of present-day economists. Second I recount the social history of the scientification of economics from early modern Europe until the present. Third I write the intellectual biography of Gerard Debreu, which illustrates the existential challenge of scientific authority for economists.

Besides the consideration of economics in its historical whole, the distinct contribution of this thesis is to explicitly discuss the conditions for the significance of economics, which are implicitly assumed in most sceptical judgements regarding the irrelevance of economics. Since the 1970s, it is common even among leading economists to lament the irrelevance of economics and yet it is difficult to establish an actual alternative. For this literature this thesis sets a new tone, if not standard: if the historical conditions for the renewal of scientific optimism are no longer met—as is the case—then economic science is free to dissolve.

The introduction connects with the widely perceived "crisis" of economics since the 1970s and proposes a different take beyond the constraint of envisioning a better economic science. The preliminaries ("Phenomenology of the life-world: hermeneutics, history, and transcendentality") introduce the philosophy of a phenomenology of science (and economics in particular). The notion of the life-world is operational for this philosophy and gives structure to the thesis according to three different meanings. First, a "hermeneutic" notion of the life-world, which conceives of the life-world as a pragmatic and discursive order, within which economics is situated. This notion informs the description of the discursive ethos of economists in the first part. Second, a "historicist" notion of the life world which considers the handing over of economics as a tradition—its "sense-history". This notion informs the social history of the scientification of economics. Third, a "transcendental" notion of the life-world, which considers the "sense-accomplishment" of economics, and how economics can be incorporated intellectually. This notion informs the life-writing of Gerard Debreu in the third part.

Part 1 ("Discourse: the public, professional and pedagogical ethos of economists") introduces the character of the economist from an outsider's point of view, that is, for an audience not trained in economics. As a benchmark, I presume a pragmatic notion of economic services, which is dismantled step-by-step. In this fashion, I introduce the scepticism that is common in today's commentary of economics—namely the charge of its political irrelevance from inside the profession, and the charge of its political bias from outside the profession. Part 1 is organized in three chapters: first, I describe the public ethos of economists in relationship with the general public; second, the professional ethos of economists in relationship with those paying for their services; third, the pedagogical ethos of economists in relationship with their students.

Part 2 ("History: economic science from the oikonomia to 'the economy'") presents a social history of the scientification of economics. The main question is what has led economists to seek science, and how this motive developed over the course of the last centuries. The chapter is structured chronologically; first a chapter on pre-modern economic discourse when there was no scientific authority; then the establishment of economic science in the 17th century mercantilist milieu; third high modernism in economics when its scientificity was politically contested (1850-1950); and last, economics today under the influence of the formalist revolution of the 1950s. This part shows that formalism has always been crucial in establishing scientific authority, and that accordingly the formalist revolution was not merely a detour in the history of scientification, but actually its end.

Part 3 ("Biography: Gerard Debreu from Nicolas Bourbaki to Adam Smith") is an exercise in life-writing as the genuine locus of phenomenological criticism. It illustrates the experiential problem of incorporating scientific authority through the elusive character of Gerard Debreu. I recount his life in the form of a "parable"—that is, a moral dilemma, a questionable decision, and the suffering of the consequences of this decision. The moral dilemma for Debreu was to be fascinated by his mathematical experience of Nicolas Bourbaki, though he recoiled from it for its inadequacy towards the end of WWII. Instead of choosing for economics, he entered it by chance and remained discreet throughout his entire intellectual life. He never saw himself as an economist. In 1983, he finally faced this ambivalence when receiving the Noble Prize for 'having proven the invisible hand of Adam Smith'.

The thesis concludes pessimistically regarding the possibility of a significant economic science, but adds, in contrast, an optimistic note regarding the prospects of a post-epistemic culture in economic talk.

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PHD THESIS SUMMARY:
Sensemaking in economics: economic activity from a social-philosophical perspective.

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PhD in philosophy, August 2007
FernUniversity in Hagen

The purpose of this project is to formulate the theoretical fundamentals of an economic philosophy that is understood as a component of a practically oriented social philosophy. I start by recapitulating the major critiques of the traditional framework of economic theory, in particular of the concepts of rational choice and 'methodological individualism'. The limited scope of these concepts for explaining social phenomena such as markets, cooperation, and trust is emphasized. These phenomena come about as results of the mutually dependent interactions of multiple actors and cannot be adequately analyzed with the agent-centered concepts of traditional economics. For example, it remains unclear how a market could arise from the isolated pursuit of individual interests.

Against the backdrop of this critique, ways of theorizing about economic activity as genuine social activity are sought on the level of economic philosophy. As the agent-centered rational choice model cannot serve as the starting point for such an economic philosophy, I opted for 'sensemaking' (the creation of meaning) as an alternative key concept. In the first part of this dissertation, however, my analysis of sensemaking as an isolated activity demonstrates that not every sensemaking concept fulfills the requirement of understanding economic phenomena as genuinely social phenomena. For example, Max Weber's 'subjective meaning', Alfred Schütz's 'construction of meaning' and the hermeneutical understanding of sense, as well as their relation to economic theory, are explored in detail. All conceive of sense as an ends-means relationship or as an individual intention and are incapable of providing an adequate tool for analyzing an economic action as a social one.

In the second part of the dissertation, the concept of 'social sensemaking' is introduced against the backdrop of systems theory and post-structural—particularly performative and practice-theoretical—assessments which allow sense to be shown as a relational concept. For example, Luhmann, in determining the social dimension of sense, focuses on the relationship between Ego and Other as two interdependent sensemaking centers.

In post-structuralism, sense is determined through relationships between symbols, as a mutual establishment of difference. These relationships are not stable but dynamic. Sensemaking is an open, restless process that occurs not only in texts but also in the social sphere. In performative assessments and in practice-theory, it is a process of co-creativity or "making-together". Sense arises from within social interactions, primarily in the contexts of practices; it is experienced, developed further, and creatively co-formed. It is a matter of the practical act which is characterized by the continual interlocking of individual calculations. Sense arises without a creator within perpetual social processes: a continual transformation of sense occurs, and it is not to be understood as an intentional, conscious accomplishment of an autonomous individual. Examples that relate to economics can be found in the works of Karl Weick as well as in some approaches to economic sociology.

Given that 'sense' is a relational term, an economic philosophy that uses the concept of sensemaking as its basis should conceive of economic activity as a kind of social activity, understood in terms of the mutual dependency of actors in relationships. The social is not just intersubjectivity, but a complex, restless network of relationships. If economic philosophy is a part of social philosophy, it can apply the categorical apparatus of social philosophy. This apparatus is sufficiently complex to be able to conceptualize the social in such a way that the individualistic perspective is overcome. An approach to solving the problem of grasping the interdependence of the calculations is made possible by linking the social-philosophical categories of the 'third person' and the 'stranger' with the concept of social sense. Here I refer to the work of Georg Simmel, Michel Serres, and Bernhard Waldenfels.

The categories of social philosophy allow social interdependencies to form the theoretical basis of economic events. The starting point for thought within the framework of economic philosophy is shifted from an individual to a relationship. It has to do with a relationship in the presence or absence of the third party or of the stranger, a triad that exceeds a mere mutuality of two actors. From a social-philosophical standpoint, such a relationship is conceived as a dynamic foundational element of the social. Therefore, methodological 'relationism' is suggested as an alternative to methodological individualism.

In the last chapter of this dissertation, I discuss in detail an example of the triadic relationship in the consumer market: 'producer-consumer-product'. I show that a mere entrepreneurial idea is not enough to create a new commodity. A produced object or service becomes a part of the unintended signification process in the market. The preconditions for the creation of a new commodity are developed in the relationship between producers and consumers in the presence of a manufactured object as an active third party. Producers and consumers tell each other stories, use products, and develop or reject them. Only in these processes of co-creation, do market participants discover which objects represent commodities at all, which needs are satisfied, how preferences are sorted, and which consequences correspond to each action alternative. Preferences, markets, innovations, and trends are not given facts but phenomena that arise and continually change within social relations. Here I also suggest an approach to the problem of economic novelty that is the other central theme of this dissertation.

Ekaterina Svetlova obtained her PhD in philosophy from the Institute of Practical Philosophy at the FernUniversity in Hagen, Germany, in August 2007. Her thesis is entitled *Sensemaking in economics: economic activity from a social-philosophical perspective* (Sinnstiftung in der Ökonomik: Wirtschaftliches Handeln aus sozialphilosophischer Sicht). She was supervised by Professor Kurt Röttgers (Dean of the Institute of Practical Philosophy, FernUniversity in Hagen). Currently, the author is a postdoctoral researcher at the Buchanan Institute of Finance and Entrepreneurship, Zeppelin University in Friedrichshafen, Germany.
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