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A failure to communicate: the fact-value divide and the Putnam-Dasgupta debate

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Abstract: This paper considers the debate between economists and philosophers about the role of values in economic analysis by examining the recent debate between Hilary Putnam and Sir Partha Dasgupta. It argues that although there has been a failure to communicate there is much more agreement than it seems. If Dasgupta's work is seen as part of the methodological tradition expounded by John Stuart Mill and John Neville Keynes, economists and philosophers will have a better basis for understanding each other. Unlike the logical-positivist tradition, which treats facts and values as two mutually exclusive concepts, the Mill-Keynes tradition recognizes that facts and values are intertwined. Unlike the Smithian tradition, which blends the study of facts and normative rules, it divides economics into a science that studies "what is" and an art which considers "what ought to be done".

Keywords: methodology, logical positivism, values, positive, normative

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In thinking about the on-going debate between philosophers and economists about the place of values in economics, one cannot help but be reminded of that famous line in the movie *Cool Hand Luke*, "What we've got here is a failure to communicate". Despite attempts to resolve the debate, there seems to be little agreement, with many economists continuing to believe that economics should study and indeed does study facts, not values; many philosophers continuing to

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believe that economists are hopelessly confused; and neither side recognizing the other's position as defensible.

A recent flare up of this debate can be seen in the on-going exchange between Hilary Putnam—writing together with Vivian Walsh (2007a; 2007b; 2009; 2012)—and Sir Partha Dasgupta (2005; 2007a; 2009), both representative of the best in their field. The debate between them began in an unusual manner. In his book *An inquiry into well-being and destitution* (1993, 6-7), Dasgupta cited Putnam (1981; 1989) to the effect that an entanglement of facts and values is unavoidable and that that entanglement would influence the way he argued. Based on that citation, and a reading of Dasgupta's work, Putnam saw Dasgupta as an example of how economists can do economic policy analysis right—i.e., by explicitly including ethical judgements in their work.

If Putnam believed that he and Dasgupta were in the same camp, that belief was shattered when, in a 2005 article 'What do economists analyze and why: values or facts?' published in the journal *Economics and Philosophy*, Dasgupta took issue with claims that Putnam had made about how he was including values in his economic analysis. Dasgupta argued that what economists do is analyze facts, and that in professional debates on social policy economists differ primarily on their reading of the facts, not on their values. He further claimed that "Ethics has taken a back seat in modern economics not because contemporary economists are wedded to a 'value-free' enterprise, but because the ethical foundations of the subject were constructed over five decades ago and are now regarded to be a settled matter" (Dasgupta 2005, 221-222). Dasgupta suggested that Putnam was promoting the false impression that modern economics is an "ethical desert".

Dasgupta's paper led to a strong response by Putnam and Walsh in *Economics and Philosophy* (2007a)—to which Dasgupta replied (2007a)—and a longer response in the *Review of Political Economy* (2007b). That ultimately led to a co-edited book (2012), which reprinted their articles together with others by philosophers on their side of the argument. In all these works Putnam and Walsh argue forcefully that Dasgupta has failed to understand Putnam's account of the entanglement of fact and value.

Neither side was persuaded by the other's arguments; despite their exchange in the pages of *Economics and Philosophy* in 2007, both Dasgupta's and Putnam-Walsh's positions remained unchanged. One can see this because Dasgupta published an adapted version of his original

2005 paper in *The Oxford handbook of philosophy of economics* in 2009, under the new title 'Facts and values in modern economics'. Despite the new title the argument remained basically the same as in 2005. The new version made some clarifications in the introductory sections, added a discussion of why Sen's capabilities cannot be seen as primitive ethical notions, and included a short section on estimating poverty. But these changes amplified and clarified his points; they did not change his position. Likewise, Putnam and Walsh did not change their position when revisiting the debate in *The end of value-free economics* (2012) by reprinting their original contributions (2007a; 2007b, 2009). Given the lapse of time, both sides clearly had the chance to amend their published positions if they wanted to. They chose not to. By examining the debate this paper attempts to clarify the issues in dispute and facilitate communication between philosophers such as Putnam and economists such as Dasgupta.

The paper is organized as follows. In Section 1 we review the origins of the debate between Putnam and Dasgupta. In Section 2 we identify two different issues in relation to the debate—the concept of value and the methodology of economics—and argue that these two issues need to be treated separately. We examine the first issue in Section 3 by placing the Putnam-Dasgupta debate in the context of more recent debate about the role of facts and values in the philosophy of science and the philosophy of economics. We examine the second issue in Sections 4 and 5, arguing that the methodology of economics advocated by Dasgupta does indeed belong to a broad classical tradition as Putnam suggested, but to a Mill-Keynes tradition rather than to the Smithian approach presumed by Putnam and Walsh. In Section 6 we conclude by arguing that seeing Dasgupta as a follower of the Mill-Keynes tradition makes it easier to see precisely where Putnam and Dasgupta disagree. Both are convincing within their own context, but outside of that context there is ambiguity and a resulting lack of communication.

1. INTELLECTUAL BACKGROUND TO THE PUTNAM-DASGUPTA DEBATE

To understand the Putnam Dasgupta debate, it is useful to review its origins. In a series of works since the 1980s Putnam has argued against the idea that there is a sharp metaphysical dichotomy between facts and values, and that facts and values are entangled in scientific knowledge (1981; 1990; 1993; 2002; 2003). The main target of Putnam's discussion is logical positivism, which holds that ethical values cannot be legitimate

subject-matter of science because they are cognitively meaningless. Putnam's fact-value entanglement arguments are applicable to all sciences, but economics has been of particular interest to him because he believes that logical positivism strongly affected the development of economics in the 1930s, and that its influence still lingers in economics today.

According to Putnam, the logical-positivist movement, combined with several other intellectual currents of the time, shaped economists' idea of economics as a scientific discipline in the twentieth century. Among the results of these influences, Putnam argued, was Lionel Robbins's position requiring a clear-cut distinction between economics and ethics, with ethical judgments having no place in the science of economics (Putnam 2002, 53-54).[1] In Putnam's view, the exclusion of ethics has impoverished economics since then. In particular, the fact-value dichotomy has impoverished the ability of welfare economics to evaluate economic well-being.

Putnam argues that just as economics was embedding a positivist methodology into its vision of itself, philosophy was moving away from logical positivism. As early as 1951 Willard Van Orman Quine launched an attack on the analytic-synthetic dichotomy which, in Putnam's view, eventually collapsed the fact-value dichotomy that lay at the foundation of the logical-positivist approach. In his works Putnam has extended Quine's insights and reinforced the argument against the fact-value dichotomy by exploring the phenomena that he has called the entanglement of fact and value.

The core of Putnam's idea of the entanglement of fact and value is that "the very vocabulary in which we describe human facts […] frequently fails to be factorable into separate and distinct 'factual' and 'evaluative' components" (Putnam and Walsh 2007b, 185). One of Putnam's own examples can help us understand better what Putnam means by this. According to Putnam, when we say a sentence like 'He is a cruel person', we do not simply 'describe' the person, but also 'evaluate' the person (Putnam 2002, 34-35). It is Putnam's view that when we describe a fact we almost inevitably make an evaluation or

[1] While Putman follows the standard way of interpreting Robbins, there is an alternative interpretation that sees Robbins's contribution differently (see Colander 2009). In this alternative view, instead of wanting to keep ethical values out of economics, what Robbins actually wanted to do was to reduce some of the most blatant blending of value judgments and supposedly scientific policy conclusions. We do not discuss such points extensively here since they involve history of thought issues rather than philosophical issues.

value judgment as well. Since making a factual judgment almost inevitably involves value judgments, description and valuation are interdependent and entangled. Note that what Putnam argues against is not the practical *distinction* between facts and values but the metaphysical *dichotomy* or dualism of fact and value (2002, 9-10). The former still considers that fact and value are not the same. Putnam refutes the dichotomy on the ground that the factual and evaluative components in the vocabulary we use are often simultaneously present. While the "cruelty" case may overstate the point, since scientific technical language is generally structured to avoid such obvious entanglements, we fully agree that if one digs deep enough, all descriptive language, and hence all language in science is inevitably value-laden. That is what might be called a base-line metaphysical entanglement that cannot be avoided. But, as a practical matter, one might still want to call a primarily logical proposition, for example, 'Given a utility function with appropriate assumptions, a derived demand curve will be downward sloping', a fact to be distinguished from a relatively more value laden proposition such as, 'Society will be better off if income is redistributed in some fashion'.

One of Putnam's goals is to enrich modern economics by getting economists to recognize not only the negative critique of the fact-value dichotomy but also the positive opportunities of the entanglement of facts and values. Entanglement demonstrates the legitimacy—indeed necessity—of ethical judgments in economic analysis. A major example cited by Putnam of how this opportunity can be taken up by economists is Amartya Sen's capability approach to studying economic well-being.

Several of Dasgupta's works can be seen as practical demonstrations of Putnam's position. His 1993 book *An inquiry into well-being and destitution*, among many other works, shows how economists can and should integrate ethical concerns into their research, and even cites Putnam's work as a justification for this approach. Thus it probably came as some surprise to Putnam that Dasgupta's 2005 article advanced a quite different interpretation of what economists, including Dasgupta himself, were doing. In the resulting exchange both sides seemed to be talking past each other.

2. THE ENTANGLEMENT OF FACT AND VALUE: THE DISAGREEMENT

In a reply jointly written with Walsh, Putnam argues that Dasgupta completely misread his position on the entanglement of facts, theories,

and values (Putnam and Walsh 2007a). In response, Dasgupta insists that he understood entanglement perfectly and had no quarrel with it (Dasgupta 2007a).

In examining why they disagree, let us start with an example where their disagreement is evident. In closing his paper, Dasgupta (2005) offers two quotations—from Reutlinger and Pellekaan (1986) and from the World Bank's 1986 *World development report*—to support his central claim that economists have shared ethical values, but differ in their reading of the facts. The same quotations are also used by Putnam and Walsh as evidence that Dasgupta had failed to understand what they meant by the entanglement (Putnam and Walsh 2007b, 185-187).[2] These two quotations are as follows:

> [L]ong run economic growth is often slowed by widespread chronic food insecurity. People who lack energy are ill-equipped to take advantage of opportunities for increasing their productivity and output. That is why policymakers in some countries may want to consider interventions that speed up food security for the groups worst affected without waiting for the general effect of long-run growth (Reutlinger and Pellekaan 1986, 6).

> The best policies for alleviating malnutrition and poverty are those which increase growth and the competitiveness of the economy, for a growing and competitive economy facilitates a more even distribution of human capital and other assets and ensures higher incomes for the poor. Progress in the battle against malnutrition and poverty can be sustained if, and only if, there is satisfactory economic growth (World Bank 1986, 7).

In this case, in saying that economists have shared values, Dasgupta means that the ethical desirability of eliminating destitution is presumed by both sets of authors. He sees the difference in policy recommendations as disagreements concerning the most effective *means* of eliminating destitution that follow from the two parties' differing views of the central causal mechanisms. In contrast, in arguing that the disagreement between the two sets of authors is of an entangled character, Putnam and Walsh mean that the apparent divergence in views regarding the most effective means is actually the result of the authors' different values. In their view, the authors of the *World development report* do not truly share the value of eliminating

[2] In fact, the two quotations also appeared in the first chapter of Dasgupta's 1993 book. It is clear that Dasgupta's standpoint did not change over time.

destitution with Reutlinger and Pellekaan: the apparent value agreement is just a disguise for their real unspeakable values (Putnam and Walsh 2007b, 186).

Our claim is that the arguments of both sides can be seen as convincing within their own context while simultaneously being seen as incomplete from the perspective of the other side. Dasgupta is clearly aware that ethical values are often the motivation for economic studies, and hence he agrees that that economics is not value-free. Moreover, he believes, rightly or wrongly, that the ethical values which motivate most economic research are widely shared by economists. There is little doubt that Dasgupta recognizes the entanglement of fact and value at the initial stage of a research project, but he seems to believe that at the later stages of the research, the evaluation of facts will not be entangled with *ethical* values, though he does not deny that other types of values may be involved (Dasgupta 2007a, 471). Putnam disagrees with him on the latter point. For Putnam, it is impossible to make a statement about facts without making an ethical value judgment. He believes that on this point Dasgupta has failed to comprehend the true meaning of his analysis of entanglement and its implications.

Putnam and Walsh argue that the values held by Reutlinger and Pellekaan are different from those of the World Bank, and that this difference in values is at the root of their different reading of the facts. Their sharp critique points out the problem that economists may use so-called 'scientific' theory as cover for ideological beliefs. But can this argument alone defeat Dasgupta's position that economists, even when sharing ends, would still have different views regarding which means would be most effective for achieving them due to their different readings of the facts? And isn't it possible that economists do genuinely agree about some ends, yet still disagree about means due to different understandings of the relevant facts, such as causal mechanisms?

We believe that it is indeed possible, and that as a practical matter good economists, such as Dasgupta, focus their applied work on an analysis of "facts", while recognizing that on a deeper metaphysical level facts and values are intertwined. In developing that applied empirical work, for example in identifying and studying specific causal mechanisms, they will come to different judgments about the facts and their real world significance, but those differing judgments do not mean that they differ about the ultimate goal.

3. VALUE-FREE ECONOMICS?

The debate between Putnam and Dasgupta is just part of a more general debate between philosophers of science. Insight can be gained into their debate by considering that broader philosophical debate, specifically the work of Andrea Scarantino (2009), who divided the relationship between science and values into three types: the 'naïve positivist view', the 'separatist view', and the 'non-separatist view'. The naïve positivist view is that values should not play any role at any stage of the activities of scientific economists and that, if they do, economists have violated the methodological conventions that make economics a science. Neither Putnam nor Dasgupta holds those views. Where they differ is that Dasgupta is more of a separatist, and Putnam is more of a non-separatist.

Following Scarantino (2009), in order to distinguish the separatist and non-separatist views we need to distinguish both between epistemic values and non-epistemic values, and between internal activities and bordering activities. The epistemic/non-epistemic distinction is similar to the distinction made by Mark Blaug between 'methodological values' and 'normative values' (Blaug 1992, 114; 1998, 372). The term 'epistemic value' is used by philosophers of science to refer to those values which govern the meaning and formulation of scientific knowledge. For instance, accuracy, consistency, and simplicity. In contrast, 'non-epistemic value' is used to refer to all other values that may be involved, i.e., values which are not instrumental to the establishment of scientific knowledge. Ethical, political, and socio-cultural values belong to this category. Internal activities are the core activities that economists do—the research that determines what will be considered economic facts (Scarantino 2009, 465-466). They relate to what philosophers call the context of justification. Bordering activities refer to the selection of which economic problems to investigate, or what philosophers call the context of discovery, and to the use made of economic knowledge once acquired.

According to Scarantino, the non-separatist view holds that "both epistemic and non-epistemic values have a legitimate role to play in the 'internal activities' of scientific economists" (2009, 466).[3] Putnam can thus be seen as a non-separatist. For him, it is impossible to exclude

[3] Other scholars who hold this view include Phyllis Rooney (1992), Peter Machamer and Heather Douglas (see Machamer and Douglas 1999; Douglas 2007), and Helen Longino (1990).

values—both epistemic and non-epistemic—from either the internal or the bordering activities of economists.

The separatist view lies in between the naïve positivist view and the non-separatist view. While the naïve positivist view represents the ideal of science as free from all values, the separatist view represents the ideal of science as free only from non-epistemic values because it recognizes the inevitability of epistemic values in scientific activities. Moreover, as Scarantino points out, it is compatible with separatism to see the bordering activities of scientific economics as laden with non-epistemic values. But the legitimate influence of non-epistemic values is restricted to the prior and posterior stages of the pursuit of economic knowledge, such as choosing socially significant problems to work on and interpreting the policy relevance of results.

Using Scarantino's classification, the disagreement between Putnam and Dasgupta about Dasgupta's position can be better understood. Putnam sees Dasgupta as a naïve positivist whereas the view Dasgupta actually holds seems closer to separatism. This understanding of their debate by no means allows us to resolve the ongoing disagreement between non-separatism and separatism. Nevertheless, the removal of an apparent misunderstanding can be a first step to more effective communication between them, since they would at least be in agreement about what it is they are disagreeing about.

Putnam is fully aware of the distinction between epistemic and non-epistemic values. But he does not put much weight on it, because he considers that both types of values are ultimately inseparable (Putnam 2002, 31-33). Indeed, it is likely that non-epistemic values would indirectly influence economists' research by influencing how epistemic values are taken up. But the distinction does help us to clarify that whether economics is value-free is not the key point in the debate between Putnam and Dasgupta: both believe that economists' bordering activities are laden with non-epistemic values and that their internal activities are laden with epistemic values. The real disagreement between them is about whether any part of economic analysis can be free from *ethical* value judgments, or, more precisely, whether economists can avoid making ethical judgments in their internal activities. In our view, Putnam does not respond to this question adequately in his reply to Dasgupta, even if his non-separatist view is the right one.

Several outstanding economists and economic methodologists have advocated a careful study of the impact of values on the scientific activities of economists. For instance, back in the 1930s Gunnar Myrdal (1953 [1930]) argued that economists' personal traits, disciplinary traditions, and the interests and prejudices of the society they lived in would inevitably influence their research through influencing the approach they chose, their explanatory models and theories, the concepts they used, and the procedures they followed in making observations and drawing inferences. In 1973 Myrdal reiterated his argument, emphasizing the importance of studying the sociology and psychology of economists (Myrdal 1973). However, until recently the exploration of these fields remained a "neglected agenda" (see Backhouse 2005). How the formation of economic knowledge is influenced by non-epistemic values acting through epistemic values is indeed an important question. But in addition to pursuing a full account of such issues, there might be some other ways in which economists can improve the quality of economic studies. We argue that Dasgupta believes so and that this is the key message of his 2005 article.

4. DASGUPTA'S MISSED MESSAGE ABOUT ECONOMIC METHODOLOGY

The title of Dasgupta's 2005 paper 'What do economists analyze and why: values or facts?' implies the dichotomy of facts and values rather than their entanglement, as Putnam and Walsh commented. It reinforces the puzzle of why Dasgupta would insist that economists study facts not values if he accepts the entanglement of facts and ethical values, at least to some degree. We believe that Dasgupta had an important message to convey but failed to communicate it clearly, and we suggest that Putnam and Walsh's failure to understand him was partly due to their reading of him as under the influence of the logical-positivist tradition with its demarcation between fact-based science and value-based ethics. Dasgupta's position cannot actually be understood in this logical-positivist tradition.

For Dasgupta, the main challenge for policy analysis in the economics profession at present is not the lack of ethical foundations. The much more pressing issue for economists is to improve their understanding of the factual side of social problems. In our view, Dasgupta's claim that economists share many ethical values is an overstatement, but one that can be justified as a reasonable simplification that explains and justifies why economists try to

structure their debates so as to focus on issues where their ethical differences are not in play. The simplification is a useful idealization because it allows Dasgupta to focus on the more important claim that refining our understanding of the factual aspects of a social phenomenon can benefit the policy debate regardless of what one's ethical views are. In our view, this key point in Dasgupta's argument did not receive enough attention from Putnam and Walsh. As an economist, and perhaps especially as a development economist, Dasgupta's main concern is with how to refine our understanding of facts for policy analysis. That is a question about the pragmatic methodology that economists should use. Dasgupta's aim is mainly practical, not theoretical or philosophical. He does not so much downplay the significance of ethics as play up the significance of *operational* solutions that improve policy analysis. As he put it bluntly, "I am a practicing economist, not a philosopher" (Dasgupta 2007a, 370).

Dasgupta is not alone. The goal of improving the reading of facts for practical purposes has a long history in economics. Pursuing this goal does not really distinguish him from other contemporary economists. What makes Dasgupta unusual is his practice of economics, which, as recognized by Putnam and Walsh, distances him from mainstream neo-Walrasian theory and puts him more in line with classical economic theory (Putnam and Walsh 2007b, 195). We also see Dasgupta's approach as in line with the classical tradition. But unlike Putnam, who associated Dasgupta with Adam Smith, we argue that Dasgupta's approach to economic policy analysis is better placed in the Mill-Keynes tradition. Looking through this lens, what Dasgupta is doing is consistent with what he claims he is doing.

5. DASGUPTA AND THE MILL-KEYNES TRADITION OF METHODOLOGY

Putnam and Walsh (2007b, 193-195) quoted extensively from Dasgupta's discussion of destitution to demonstrate that Dasgupta's work belonged to the classical tradition. Using the same passages quoted by Putnam and Walsh, we will provide an alternative reading of Dasgupta.

> [A]ll the equilibria in the timeless economy are Pareto-efficient […] This means, among other things, that there are no policies open to the government for alleviating the extent of undernourishment other than those that amount to consumption or asset transfers. A common wisdom is that such policies impede the growth of an economy's productive capacity because of their detrimental effect

> on saving and investment, incentives, and so forth. But this is only one side of the picture. Our model will stress the other side, which is that a transfer from the well-off to the undernourished can enhance output via the increased productivity of the impoverished (Results 7 and 8). We don't know in advance which is the greater effect, but to ignore the latter yields biased estimates of the effects of redistributive policies. […]
>
> By developing the economics of malnutrition, I will offer a final justification for the thesis that it is the singular responsibility of the State to be an active participant in the allocation mechanism guiding the production and distribution of positive and negative freedoms. This justification is built on the idea that in a poor economy markets on their own are incapable of empowering all people with the opportunity to convert their potential labour power into actual labour power. As a resource allocation mechanism, markets on their own simply aren't effective. The theory I will develop below also shows how a group of similar poor people can become fragmented over time into distinct classes, facing widely different opportunities. Risk and uncertainty will play no role in this. It is a pristine theory of class formation (Dasgupta 1993, 476-477).

Putnam and Walsh used these passages as evidence of the fact-value entanglement in Dasgupta's work and the concordance between Dasgupta's and Smith's economic writings. But reading Dasgupta through the Mill-Keynes lens gives us what seems a better view of his true intentions. We suggest the similarities of Dasgupta's approach with the Mill-Keynes tradition can be identified from the following two aspects.

> *a) The knowledge of 'what ought to be' is distinct from, but based on, the knowledge of 'what is'.*

Dasgupta's work suggests that he would accept the science-art distinction proposed by John Stuart Mill. On the one hand, science and art are distinct (Mill 1967 [1844], 312). Science, which concerns the knowledge of 'what is', is different in nature from art, which concerns the knowledge of 'what ought to be'. On the other hand, science and art are closely interrelated. Art assigns ends to science; science informs art of the means available for achieving those ends; based on the knowledge provided by science, art decides what ought to be done to achieve the ends (Mill 1974 [1872], 944-945). Note that the science-art distinction is not equivalent to the fact-value dichotomy. A key difference between the

two is that while the latter implies that science deals with facts and art deals with values, the former does not.

From the second passage cited above, we can see how Dasgupta intends to base his normative judgment on the knowledge of facts provided by science. The statement that "it is the singular responsibility of the State to be an active participant in the allocation mechanism guiding the production and distribution of positive and negative freedoms" is a normative one. It is clear in Dasgupta's writing that this normative judgment "is built on" the idea that "in a poor economy markets on their own are incapable of empowering all people with the opportunity to convert their potential labour power into actual labour power", which is a reading of fact derived from his scientific economic analysis of malnutrition (Dasgupta 1993, 477). Dasgupta would not deny that his claim that markets are incapable of empowering all people might involve a value judgment, but for him the statement is a positive statement, not a normative one. The statement does not indicate what ought to be done. It alone cannot tell us why the State rather than non-governmental organizations should be the remedy for the failure of markets. It does not even suggest that leaving the markets alone should not be an option, unless we already consider it desirable to try to empower all people to convert their potential labour power into actual labour power and this aim is not trumped by other aims.

b) It is necessary to adopt an interdisciplinary approach to reading facts to remedy the limitations of mainstream models relating to their unrealistic assumptions.

Despite being critical of mainstream economic models, Dasgupta does not deny their contribution. He has issues with them because he believes they present an unrealistic view of the world—because their construction neglects crucial *facts*, such as basic needs and physiological phenomena—and hence they are unable to provide an accurate reading of economic phenomena. For Dasgupta, the mainstream models can be a poor guide to the causal mechanisms involved because of inappropriate assumptions and construction. The ethical values held by economists might be the cause of the problem, but not necessarily. In his 2005 article, Dasgupta shows that as a practicing economist he aims to deal with those cases in which ethical values are *not* the cause of economists' mistaken reading of causality.

In view of the limitations of the standard models, Dasgupta includes scientific knowledge from outside economics in his analysis of policy. In his research, the knowledge provided by disciplines such as physiology, the science of nutrition, ecology, and so on, plays an important role in understanding the factual side of social phenomena.[4]

At the very beginning of chapter 16 of his 1993 book, Dasgupta points out that the standard theory of resource allocation fails to take into account the *fact* that meeting physiological maintenance requirements is a precondition of labour power. The term 'economic disfranchisement' is used by Dasgupta to point out the illusion, suggested by the standard theory, that every labourer is on an equal footing in terms of converting potential labour power into real labour power in the labour market. He therefore attempted to construct a theory that took human physiology into account.

It is true that the ethical values held by Dasgupta may have contributed to his interest in the phenomenon of economic disfranchisement and redistributive policies. Yet it is also true that although concluding that "models that are dissonant with physiological truths are hopelessly incomplete" (1993, 475), Dasgupta does not attack the standard theory from an ethical point of view, but from a factual point of view. From the first passage cited above, we can see that Dasgupta intends to disprove the "common wisdom" by showing that the outcomes derived from the standard model will not come about if the positive effects on productivity of a transfer from the well-off to the undernourished are greater than its negative effects on saving and investment. The approach he took to refute the standard theory is very much 'scientific' in Mill's sense, rather than 'ethical' or 'normative'.

According to Mill, social science is a deductive enterprise, but one which follows the model of the physical sciences, rather than that of geometry. Social science, he wrote,

> infers the law of each effect from the laws of causation on which that effect depends; not, however, from the law merely of one cause, as in the geometrical method; but by considering all the causes which conjunctly influence the effect, and compounding their laws with one another (Mill 1974 [1872], 895).

[4] See, for instance, Dasgupta 1990; 1997; 2003; 2007b; 2008; Dasgupta and Ray 1987; Dasgupta and Mäler 2000.

In Mill's view, the complexity of social phenomena does not arise from the number of the laws, but "from the extraordinary number and variety of the data or elements—of the agents which, in obedience to that small number of laws, co-operate towards the effect" (Mill 1974 [1872], 895).

Dasgupta's approach to asset transfer policies is a good example of Mill's deductive method. Dasgupta identifies two main effects of a transfer: decreasing savings and investment on the one hand while increasing the productivity of the impoverished on the other hand. These two tendencies can be seen as co-existent intermediate mechanisms which will have different effects on economic growth. According to the physical 'deductive method', the final result of the transfer policy should be estimated by summing up the individual effects of the co-existent intermediate causes. In contrast, the approach adopted by the standard model is equivalent to the 'geometrical method' because it does not admit the modification of the presumed psychological law (the behaviour of saving and investing will be negatively affected by the transfer) by another law (the improvement in nutrition will increase productivity).

It is worth noting that Mill does not pretend that it is possible to calculate the aggregate result of many co-existent causes with complete precision. In his view, it is beyond human faculties to take into account all the causes which happen to exist in one case (Mill 1974 [1872], 898). But, as a practical science, if economics can provide us with knowledge of tendencies, it gives us a considerable power to "surround [our] society with the greatest possible number of circumstances of which the tendencies are beneficial, and to remove or counteract, as far as practicable, those of which the tendencies are injurious" (Mill 1974 [1872], 898).

From the above discussion, we can see that the scientific aspirations of Dasgupta's economic writings are clearly in line with the approach explicitly stipulated by Mill. This scientific dimension is absent from Smith's work. Indeed, Mill's proposal of the science-art distinction specifically took Smith as a target. In Mill's view, the title and arrangement of Smith's book *An inquiry into the nature and causes of the wealth of nations*, despite being suitable for the purpose of his work, had caused a general misunderstanding of the nature of economics as a science. Smith's approach tended to mix up what makes a nation rich (what is) with what a nation ought to do to increase its wealth (what

ought to be done). For Mill, the latter is not an appropriate subject for scientific economics; it should be the subject of political economy as *art* (Mill 1967 [1844], 312). Moreover, according to Smith the object of political economy is firstly to enable the country's people to provide sufficient necessaries and conveniences of life for themselves and secondarily to supply the state with a revenue sufficient for the public service (Smith 1976 [1776], book 5, Introduction). For Mill, the desirability of these objects is determined by art, not by science (Mill 1967 [1844], 312).

Dasgupta is not the only economist whom Putnam and Walsh have held up as a paradigm of Smithian methodology, and not the only one who turns out not to fit that model quite as well as they supposed. Putnam and Walsh have also suggested that Sen's work, and especially his capability approach, is in the Smithian tradition (Putnam 2002, 2003; Putnam and Walsh, 2007b). In terms of Sen's methodology, we do not see it that way—Smith blended normative and positive analysis without separating normative and positive economics in any logical way. Sen does the opposite; he carefully specifies what in his analysis is normative and what is positive, and explains why his normative analysis is much more consistent with most people's normative views than are the implicit normative judgments in standard analysis. This, in our view, puts him in the Mill-Keynes methodological tradition, which evolved from Smith's partly by criticizing Smith for his lack of clarity about the difference between what economics studies and what the ends of economics and economic policy ought to be.

In the first chapter of his book *On ethics and economics* (1987), Sen identifies two origins for economics in ethics and engineering. Sen groups Smith and Mill together in the ethics-related tradition, which is correct in the sense that both Smith and Mill see economics as a branch of moral philosophy (i.e., the ultimate end of economic knowledge is to make life better, and hence ultimately economics cannot be independent from ethics). But we would add an extra distinction to Sen's classification that allows us to distinguish Smith and Mill in terms of their methodology. Whereas Smith blended his normative and positive analysis together, Mill carefully attempted to distinguish art from science. Thus, like Putnam and Walsh, we see Sen as following Smith's (and Mill's) ethical tradition—in the sense of seeing economics as a branch of moral philosophy. But unlike them we see Sen's *methodology* as deriving from the more sophisticated Mill-Keynes tradition rather

than Smith's. This is what we mean by saying that Sen belongs to the Mill-Keynes approach, not the Smithian approach.

It is intriguing to note that enriching the nation, the major goal of Smith's political economy, has been implicitly taken over by many modern economists as a value-neutral goal, while equitable distribution, which is less directly addressed by Smith, is considered as value-laden and hence as an illegitimate subject for economics. Mill's distinction between science and art could in effect support Putnam's intention of revealing the biased attitude of some economists towards different ethical values that leads to biased readings of facts.

Dasgupta rarely if ever refers to Mill in his work. However, it is not entirely surprising to find similarities between their methods of doing economics. Daniel Hausman once commented that "[t]he temper and character of modern economics still embodies the Millian vision of the discipline as a separate science" (Hausman 1992, 225). Modern economics may not have developed in quite the way Mill had hoped, but it is fair to say that his analysis of the nature and methodology of economics was indirectly and partially inherited by contemporary economists through the influence of John Neville Keynes and Robbins.

In *The scope and method of political economy* (1917 [1890]), J. N. Keynes took up Mill's distinction between positive science and normative art and further developed it into a tripartite division of economics in accordance with his classification of knowledge According to this classification, a positive science is a body of systematized knowledge concerning what is; normative or regulative science is a body of systematized knowledge relating to the criteria of what ought to be; and an art is a system of rules for the attainment of a given end. Each has its own distinct objectives: for a positive science the objective is to establish uniformities; for a normative science it is to determine ideals; for an art it is to formulate precepts. Accordingly, investigations into economic uniformities, economic ideals, and economic precepts can be categorised respectively as the positive science of political economy, the ethics of political economy, and the art of political economy (see 1917 [1890], 31-36).[5]

In our view, the Millian approach did not end with J. N. Keynes. In particular, we have argued elsewhere (Colander 2009) that Robbins is best interpreted as working within this tradition, and that that sheds

[5] For a detailed discussion of Keynes's tripartite division of economics, see Colander 1992.

a quite different light on his message. Specifically, we argue that Robbins (1945 [1932]) advocated not only the importance of separating positive economics from ethics but also a separate, non-scientific branch of economics to deal with issues of values. Robbins noted that the majority of classical economists used the term political economy to cover "a mélange of objective analysis and applications involving value judgments" (1976, 1; 1981, 7). In his 1981 Ely Lecture and in the introduction to his 1976 book *Political economy, past and present*, Robbins suggested that the use of the term 'political economy' should be revived, to maintain a space in economics where ethical values play a central role (1976, 2-3; 1981, 7-8).[6] According to Robbins, this political economy is not part of economic *science*, but it is an integral part of economic studies.

Mill's call for economics as a science separate from art has been largely realized in the economics profession over the past 150 years, but the line of descent from Mill through Keynes and Robbins to today took various turns. Each inflexion caused some changes to the direction of the development of economics, and the final outcome is very different from what Mill would have expected. We do not deny the problems of modern economics that emerged during its formation as a separate discipline. But, with a correct understanding of the Mill-Keynes tradition of methodology, and particularly by recovering the integral role of art in economic studies, the economics profession could do a much better job than it does now to highlight the way values are integrated into economic analysis.[7]

Specifically, we believe that when Dasgupta's arguments are interpreted through the Mill-Keynes lens, rather than a Smithian one, his arguments make much more sense philosophically. They are not deep philosophical arguments but pragmatic arguments about how to move forward in tentatively separating positive truths from normative rules, even while accepting that on a deep level they may not be fully separable. Instead of letting fact-value entanglement lead one to an

[6] Robbins uses the term in a narrower sense than Smith: Robbins uses the term to designate only the prescriptive part of economic investigation, whereas Smith's political economy concerned both what we have been calling positive science and normative art.

[7] We have discussed elsewhere how the economics profession can improve by reintroducing the Mill-Keynes methodological tradition (see Colander 1992, 2001, 2013; Su 2012). It involves distinguishing separate methodological approaches for applied policy economics and for the pure science of economics, along the lines suggested by J. N. Keynes.

impasse, one distinguishes those factual judgments and normative judgments that are most separable, accepts that others are not, and gets on with one's analysis.

We are not especially concerned with whether Dasgupta is actually a follower of either Smith or Mill. Our argument is that seeing Dasgupta within the Mill-Keynes tradition helps clarify his methodology. The Mill-Keynes interpretation allows us to understand how Dasgupta considers himself able to integrate ethical considerations into his economic policy analysis without sacrificing the scientific character and objectivity of his economic analysis. In the Mill-Keynes methodological tradition, the scientific branch of economic studies is separated from applied economic policy analysis. The separation is meant to enhance the quality of the latter by improving the understanding of economic phenomena through adopting appropriate scientific methods. Putnam may disagree with the Mill-Keynes methodology, but we believe his criticisms would be better understood by Dasgupta, and other economists, if they took explicit account of the pragmatic art-science foundations of his methodology, and did not reduce them immediately to the fact-value dichotomy associated with the logical-positivist tradition, and which the Mill-Keynes economic tradition did not embrace.

6. CONCLUSIONS

The debate between Putnam and Dasgupta was perceived by Putnam to be about whether economics is value-free or not, as indicated by the title of his recent book with Walsh about their side of the debate, *The end of value-free economics*. We have suggested in this paper that this was a misperception. The fact-value divide is problematic, but it is not the key to the Putnam-Dasgupta debate. We have argued that Dasgupta was mistakenly understood by Putnam and Walsh as holding a naïve positivist view, which insists on a dichotomy between fact-based science and value-based ethics and argues that economics should be free from all sorts of values. In our view, the confrontation between Putnam and Dasgupta is actually between a non-separatist view and a separatist view. More specifically, the disagreement between them is about whether it is possible for economists to avoid making ethical value judgments when they try to explain observed economic phenomena in an objective factual way.

The philosophy of science debate between the non-separatist view and the separatist view is on-going. The implications of these two views for scientific activities require more investigation. In particular, if ethical value judgments cannot be avoided even in internal scientific activities—as the non-separatist view claims—then it is important for economists to understand how this entanglement occurs in order to know how to minimize the resulting biases in their research, as much as one can. However, real-world economic problems are pressing and cannot wait for solutions until we have a satisfactory answer to these profound questions. Moreover, even if it is true that economists' reading of facts is inevitably influenced by their personal values, it is not necessarily the case that their different readings of the facts can be solely explained by differences in their *ethical* values. For these reasons, the value of Dasgupta's call for refining the reading of facts should be acknowledged, and the Mill-Keynes tradition rediscovered.

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The relation between economics and theology in *Caritas in Veritate*

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Abstract: *Caritas in Veritate* is the latest in the series of papal 'social encyclicals' beginning with *Rerum Novarum* (1891). Like its immediate predecessor *Centesimus Annus* (1991), it presents a body of economic doctrine favourable to the market economy that is superimposed on an underlying body of older doctrine that is deeply hostile to it. This article investigates the possibility that this incoherence results from a corresponding incoherence in the theological framework of the recent encyclicals. The doctrine of the encyclicals is then contrasted with an eighteenth-century, Anglo-Scotch tradition of thought that showed the compatibility with Catholic moral theology of a privately owned, competitive economy driven by *self-love*. This tradition is the intellectual origin of modern economics, yet it has not been available to the Church of Rome because of an historical accident. The article concludes by speculating upon the reasons for this.

Keywords: history of economics, moral theology, political economy, social policy, economic development, self-love

JEL Classification: B19, B29, H10, I30, O10, O20

Caritas in Veritate (*CV*) of Pope Benedict XVI, published in June 2009, was the latest in a series of 'social encyclicals' that begins with *Rerum Novarum* (*RN*) of Leo XIII, issued in 1891. As *Quadragesimo Anno* (*QA*) of Pius XI was intended to celebrate the fortieth anniversary of *RN*, so *CV* looks back to *Populorum Progressio* (*PP*) issued by Paul VI in 1967 (*CV*, 10). Though papal social doctrine has often been promulgated through other documents, the series of social encyclicals from *RN* to *CV* epitomizes that doctrine, and enables us to mark change and

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development since 1891. In what follows, I shall first analyse the treatment of economic matters, with special attention to *Centesimus Annus* (*CA*) and *CV*, showing that there is—or has been since 1991—a serious internal contradiction. A body of doctrine favourable to the market economy is superimposed on an underlying body of older doctrine that is deeply hostile to it. Next I shall explore the possibility that this incoherence results from a corresponding incoherence in the theological framework of the recent encyclicals. In the third section I report the achievement of Anglican thinkers in the eighteenth century, building on the French Jansenist theodicy of the previous century, in showing the compatibility of (Catholic) moral theology with a privately owned, competitive economy driven by *self-love*. But because of an historical accident this solution has not been available to the Church of Rome. In a final section I speculate on the reasons for this.

ECONOMIC IDEAS IN *CARITAS IN VERITATE*

The encyclical recognises explicitly several fundamentally important ideas, acknowledged by economists to be necessary conditions of sustained development and growth. These are 'social capital', human capital, the internalization of environmental costs, the rule of law, and individual initiative.

By 'social capital' is meant "the network of relationships of trust, dependability, and respect for rules, all of which are indispensable for any form of civil coexistence" (*CV*, 32). And indeed "without internal forms of solidarity and trust, the market cannot completely fulfil its proper economic function" (*CV*, 35); i.e., "*The economy needs ethics in order to function effectively*" (*CV*, 45; all italics are from the original unless otherwise stated) a position which closely resembles what I have elsewhere called the 'Folbre-Morse thesis' (see Waterman 2003b). These undeniable propositions—sometimes forgotten or neglected by present-day economists—are associated with more controversial yet arguable claims: social capital is weakened or eroded by the "systemic increase of social inequality" (*CV*, 32), therefore the market requires not only commutative justice but also distributive justice (*CV*, 35).

The importance of human capital is clearly acknowledged: the "*primary capital to be safeguarded and valued is man, the human person in his or her integrity*" (*CV*, 25), wherefore "the most valuable resources in countries receiving development aid are human resources; herein lies the real capital that needs to accumulate in order to guarantee a truly

autonomous future for the poorest countries" (*CV*, 58). Economists might substitute "all" for "the poorest" and be sceptical about "a truly autonomous future" for *any* country in today's world, but all would agree that human capital is the fundamental economic resource.

The encyclical touches on the problem of environmental degradation resulting from economic growth, and correctly maintains that "the economic and social cost of using up shared environmental resources" be "recognized with transparency and fully borne by those who incur them" (*CV*, 50). Noting that some adverse economic effects of growth are "the result of impoverishment and underdevelopment", it points out that "When incentives are offered for their economic and cultural development, nature itself is protected" (*CV*, 51).

Aid to developing countries should include "reinforcing the guarantees proper to the *state of law*: a system of public order […]" (*CV*, 41), which appears to mean what is more usually known as the 'rule of law'—and which has been understood at least since the eighteenth century to be a necessary condition of a viable economy under any possible assignment of ownership rights. Thus "corruption and illegality" in both rich and poor countries (*CV*, 22) impairs development. Some problems of third-world economies are self-inflicted, and caused by "political irresponsibility" (*CV*, 26), indeed "grave irresponsibility within the very countries that have achieved independence" (*CV*, 33).

In addition to the observations about incentives and the rule of law, there is some slightly more explicit awareness that economic activity is driven by individuals: "The peoples themselves have the prime responsibility to work for their own development" (*CV*, 47; citing *PP*, 77). This necessary self-reliance can be undermined by foreign and domestic paternalism:

> At times […] those who receive aid become subordinate to the aid-givers, and the poor serve to perpetuate expensive bureaucracies which consume an excessively high percentage of the funds intended for development (*CV*, 47).

These true and important insights imply that economic activity is driven by purposeful individuals and firms, dependent upon publicly sanctioned and privately ratified law and order, having self-determined goals and therefore responsive to incentives. They are also consistent with the assumption that agents in the public sector are motivated in

the same way, by their own private goals. Yet they coexist with many passages in *CV* which appear to deny that individuals "motivated by purely selfish ends" (*CV*, 36) can produce socially beneficent outcomes as unintended consequences, and which rest instead on a naïve reliance upon the wisdom and goodness of public functionaries. The underlying assumptions of these dissonant doctrines are what I shall call *organicism* and *constructivism*.

By 'organicism' I mean a conception of human society as a living body, necessarily governed by a 'head' which all other 'members' must obey if it is to remain viable. I have analysed the centrality of organicism for papal social teaching in previous work (Waterman 1999) and shall return to it below. Its most obvious signs in *CV* are the continual hypostatizing of such abstractions as "nations", "the political community", and "the international community"; its frequent references to human "solidarity" (*CV*, 19, 21, 25, 38, 39, 43, 44, 54, 58); and its authors' desire for "a true world political authority", with "authority to ensure compliance", which would "manage the global economy" and "seek to establish the common good" (*CV*, 67).

'Constructivism' is a term coined by F. A. Hayek to denote the assumption "that we have it in our power so to shape our institutions that of all possible sets of results that which we prefer to all others will be realized" (Hayek 1967, 85). Hence the recollection in *CV* of John-Paul II's call in 1991 for "a comprehensive new plan for development" not only in Eastern Europe "but also in the West and in those parts of the world that were in the process of evolving" (*CV*, 22-29). Benedict XVI speaks of "constructing [sic] a new order of economic productivity" (*CV*, 41); of the need to "replan our journey" (*CV*, 21) and for "comprehensive new plans for development" (*CV*, 23); and contemplates a "reason" that is capable of "knowing and directing" globalization (*CV*, 33) so as to achieve "*distributive justice* and *social justice* for the market economy" (*CV*, 35). It is evident that the drafting committees and authors of these encyclicals envisage a world in which those in political authority might have the *knowledge* and the *power* not merely to affect but actually to determine the structure and operation of the economy; and have the *wisdom* and *goodness* to do so in a way that may achieve the common good.

These assumptions were common among economic thinkers at one time, especially in the seventeenth century when *économie politique* emerged as a set of recipes for running France as the manorial fief of

le roi soleil. But since the early eighteenth century increasing scepticism about the four attributes, particularly those of *knowledge* and *goodness*, has gradually led economists to a very different view of the world. The key figure is David Hume (1711-1776), whose radical scepticism about the possibility of human knowledge—inspired perhaps by Joseph Butler (1692-1752), Bishop of Durham—brought him and his successors in the so-called 'Scottish Enlightenment' to see that

> every step and every movement of the multitude, even in what are termed enlightened ages, are made with equal blindness to the future; and nations stumble upon establishments, which are indeed the result of human action, but not the execution of any human design (Ferguson 1767, 187).

David Hume, Adam Ferguson (1723-1816), Adam Smith (1723-1790), and John Millar (1735-1801) came to see human societies not as *bodies*—and certainly not as *machines*, as some socialist theory later implied—but rather as quasi-biological *habitats*, in which what John Stuart Mill (1874) later called a "spontaneous order of nature" emerges as the unintended outcome of a myriad of private, self-regarding acts by individuals. In market economies individual, self-regarding transactions are coordinated by prices for goods and services. When there is enough competition, an 'invisible hand' will produce the optimum pattern of production and consumption, given any initial distribution of assets.

If economic activity is driven by the self-regarding acts of individuals, we must abandon the assumption that political decision-makers are better than the rest of us. A business executive does not become altruistic merely by accepting a job in government. There is no reason to expect that ministers and civil servants will be any less self-interested than business managers and trade union officials; nor any reason to suppose that managers will subordinate their own private interests to those of their shareholders or union bosses to those of their rank-and-file (see *CV*, 25).

In the view of economists therefore, papal confidence in beneficent, 'top-down' governance is misplaced on two counts. First, 'the sovereign' must not be charged with

> a duty, in the attempting to perform which he must always be exposed to innumerable delusions, and for the proper performance of which no human wisdom or knowledge could ever be sufficient;

> the duty of superintending the industry of private people, and of directing it towards the employments most suitable to the interest of the society (Smith [1776] 1976, 687).
>
> Secondly,
>
> that, in contriving any system of government and fixing the several checks and controuls of the constitution, every man ought to be supposed a *knave* and to have no other end, in all his actions, than private interest (Hume [1752] 1994, 21).

Whether this radically different view of society is more or less accurate than the organicism and constructivism of papal social teaching is of secondary importance. What matters here is that the latter is fundamentally incompatible with what economists would accept as the valid insights of *CV*. For if 'the market' actually works at all, which the encyclical certainly assumes it does (e.g., *CV*, 25), it does so because economic activity arises from the private acts of individuals "motivated by purely selfish ends" (*CV*, 36), responding to incentives (*CV*, 51), and with a productivity augmented by their human capital (*CV*, 58). The duties of "the sovereign" (i.e., the government of a sovereign state) are thus confined to national defence, the provision of public goods, education (*CV*, 61), and the maintenance of the rule of law (*CV*, 41; see Smith [1776] 1976, V.i). This conception of the economy contradicts the idea of any "comprehensive new plans for development" (*CV*, 23) or of a human "reason" that is capable of "knowing and directing" globalization (*CV*, 33); and vice versa.

It is noteworthy that in its correct insistence on the overriding necessity of "the network of relationships of trust, dependability, and respect for rules" (*CV*, 32) the encyclical seems implicitly to recognize the fact that individuals, both in the private and the public sector, are normally motivated by self-interest; and that it is this that actually drives all economic activity. Now unless the self-seeking propensities of individuals in each sector are disciplined by individually internalized ethical imperatives—unless most people normally obey the rules of the game even when the referee is not looking—the market game quickly ceases to be worth playing, and society descends into the Hobbesian anarchy of a Somalia or a Côte d'Ivoire. Which is why "*The economy needs ethics in order to function effectively*" (*CV*, 45).

SOME THEOLOGICAL ELEMENTS OF *CARITAS IN VERITATE*

It is not reasonable to expect that this encyclical should contain a complete theological rationale of its doctrine since it is explicitly one in the series of 'social encyclicals' and continually cites the authority of its predecessors. Yet it may be useful to identify some of the more important theological themes in *CV* as a preliminary to the discussion in part III. These are *Caritas*, the Holy Trinity, the unity of the human race, Nature, the prophetical mission of the Catholic Church, original sin, and Providence.

Caritas, which signifies more than 'charity' or 'love' in ordinary English usage, is that *αγαπη* identified by St Paul (I Cor 13:1-4) as a necessary condition of the Christian life. It is known by Christ's giving of himself for the redemption of the world (I John 4:7-12), for "God is love" (*'ο Θεος αγαπη εστιν*, I John 4:8). Thus the encyclical can affirm that "everything is shaped by it" (*CV*, 2). *Caritas* is therefore "an element of fundamental importance in human relations, including those of a public nature" (*CV*, 3). Combined with Truth (*Veritas*), *Caritas* "shows us the way to true development" (*CV*, 52).

The doctrine of the *Holy Trinity*, described as a "revealed mystery", is invoked in *Caritas in Veritate* (*CV*, 54) to illustrate the hyper-economic, transcendent conception of "development" proposed by *PP* and *CV*. This conception "can be identified with the inclusion-in-relation of all individuals and peoples within the one community of the human family, built in solidarity […]" The Trinity-in-Unity shows that for human beings too "true openness does not mean loss of individual identity but profound interpenetration", hence God desires (John 17:22) "that they may be one even as we are one" (*CV*, 54). Therefore "The Christian revelation of the unity of the human race" (*CV*, 55) "does not submerge the identities of individuals, peoples and cultures, but […] links them more closely in their legitimate diversity" (*CV*, 53).

Nature "is a gift of the Creator who has given it an inbuilt order". It "speaks to us of the Creator" and "*expresses a design of truth and love*" since it is "not the result of mere chance or evolutionary determinism" (*CV*, 48). It might appear from this that the encyclical asserts the traditional, pre-Darwinian view of the 'book of Nature', which we may read, as did Sir Isaac Newton and Archdeacon William Paley, to discover evidence of the *design* of the universe by an all-knowing, all powerful, all-wise and all-good Deity. However it is possible (though they do not tell us) that the authors believe they have defensible

philosophical grounds for accepting organic evolution as a useful scientific theory on the one hand, without having to abandon teleology on the other. 'Nature' and its cognates are often used narrowly and loosely in *CV* to mean nothing more than the human environment (e.g., *CV*, 48, 49, 50, 51); and at one point it is asserted that there is "a covenant between human beings and their environment" (*CV*, 69). But it is also used metaphysically: because we are made in the image of God we may discover "the inviolable dignity of the human person and the transcendent value of natural moral norms" (*CV*, 45). For in

> all cultures there are examples of ethical convergence, some isolated, some interrelated, as an expression of the one human nature, willed by the Creator; the tradition of ethical wisdom knows this as the natural law. This universal moral law provides a sound basis for all cultural, religious and political dialogue (*CV*, 59).

It also provides a basis for the populationist, anti-birth-control doctrine of *Humanae Vitae* (*HV*, 4) issued by Paul VI in 1968, and reasserted by Benedict as "highly important for delineating the *fully human meaning of the development that the Church proposes*" (*CV*, 15).

Echoing *Quadragesimo Anno* (*QA*, 41), this encyclical acknowledges that "The Church does not have technical solutions to offer" and does not claim to "interfere in any way in the politics of States" (*CV*, 9; quoting *PP*, 13). But

> She does, however, have a mission of truth to accomplish, in every time and circumstance, for a society that is attuned to man, to his dignity, to his vocation […] For this reason the Church searches for the truth, proclaims it tirelessly, and recognizes it when it is manifested (*CV*, 9).

Upon the assumption, which virtually every Christian would accept, that the Church will be led by the Holy Spirit into all truth (John 16:13)—recognizing that 'truth' in this sense is spiritual and theological, not scientific—the Church has both the right and the duty to proclaim to the whole world those divinely revealed facts about human existence which must govern private and public morality if humans are to flourish. As a twentieth-century Archbishop of Canterbury once put it in a homely example, he might have to say to the Prime Minister:

> No; I cannot tell you what is the remedy; but I can tell you that a society of which unemployment […] is a chronic feature is a diseased

> society, and that if you are not doing all you can to find and administer a remedy, you are guilty before God (Temple [1942] 1976, 45).

The Church thus has a prophetical office. Like the prophets of ancient Israel, those who speak in her name must sometimes declare unpalatable truths to the rest of society.

A vitally important passage in *Centesimus Annus* warned us of "the wound of original sin" and of its consequences for social order:

> Man tends towards good, but he is also capable of evil. He can transcend his immediate interest and still remain bound to it. The social order will be all the more stable, the more it takes this fact into account *and does not place in opposition personal interest and the interests of society as a whole, but rather seeks ways to bring them into fruitful harmony*. In fact, where self-interest is violently suppressed, it is replaced by a burdensome system of bureaucratic control which dries up the wellsprings of initiative and creativity (*CA*, 25; italics added).

This passage is alluded to in *Caritas in Veritate* (*CV*, 34, note 85) but, unlike in *CA*, no inferences whatsoever are drawn from "the presence of original sin in social conditions" (*CV*, 34) for any need to allow "personal interest" to operate for the benefit of "the interest of society as a whole". If anything, the paragraph in *CV* seems to tend in the opposite direction. (I shall return to this topic in detail below.)

A key element in the theological analysis of the place of *original sin* in social and economic affairs is the idea of *Divine Providence*. It is mentioned once, briefly, in this encyclical: "development requires […] reliance on God's providence and mercy" (*CV*, 79). But the concept does no work in that context and is merely decorative. (It too will be studied more carefully in part III.)

Each of these theological elements is important, and three of them—*Caritas*, *original sin*, and the *Holy Trinity*—lie at the heart of Christian orthodoxy. But as employed in *Centesimus Annus* and *Caritas in Veritate*, especially the latter, they are left undeveloped. There is no theological rationale of self-interest and spontaneous order which would validate those passages which appear to recognise the efficacy of competitive market institutions, and which would deliver Papal social teaching from its reliance on the 'Romantic categories' (Waterman 2003a) of organicism and constructivism. Why should this be?

Part of the answer may lie in the fact that the idea of *original sin*, as developed authoritatively from biblical and earlier patristic sources by St Augustine (who seems to have coined the term), may be in conflict with other themes that *CV* wishes to assert, such as "the fundamental values of *justice* and *peace*" required for human solidarity (*CV*, 54). For Augustine taught that, because of sin, true justice (*vera justitia*) is impossible in this life; and that no state can exist without positive injustice (Deane 1963, 118-126; citing many scattered examples from Augustine's unsystematic writings). And because sin has destroyed the fraternity natural to human society, no true peace is possible in this Earthly City (*Terrena Civitas*), only the absence of overt conflict that a state having a monopoly of coercion can forcibly impose (Deane 1963, 95-100). It seems clear that for Augustine human *solidarity* can only be realized in the City of God (*Civitas Dei*), and is not to be looked for in any conceivable this-worldly set of political arrangements.

There is therefore a fundamental theological dissonance in Papal social teaching, which corresponds to some extent with the contradictory economic ideas identified in Part I above.

The Christian organicism that runs through the social encyclicals like a *leit-motif* conceives of human society in terms borrowed from the Pauline doctrine of the Church as the mystical body of Christ, as the "body politic" or "body social". In *Quadragesimo Anno*, for example, the faithful are taught that "it will be possible to say in a sense even of this body what the Apostle says of the mystical body of Christ: 'The whole body (being closely joined and knit together…) derives its increase to the building up of itself in love'" (*QA*, 90). It is in this sense that *CV* can affirm that *Caritas* "is at the heart of the Church's social doctrine (*CV*, 2), reflecting a commonplace of sixteenth-century political thought: that love is the cement that holds society together.

> [...] yf al the partys of the cyty wyth love be not knyt togyddur in unyte as membres of one body, ther can be no cyvylyte [...] [but] [...] there ys perfayt cyvylyte [...] where [...] al the partys [...] be knyt togyddur in perfayt love & unyte, evey one dowying hys offyce & duty [...] & wythout envy or malyce to other accomplysh the same [...] (Starkey [1538] 1989, 37).

Because of an accident of history (Waterman 2004, chapters 11 and 12), that social doctrine was shaped at its outset by Leo XIII's commitment to Thomistic philosophy, promulgated in *Aeterni Patris* (1879). In 1888, three years before *RN*, Leo issued the encyclical *Libertas*

Praestantissimus which mounted a frontal assault on nineteenth-century liberalism: the sovereignty of the people, democracy, and the so-called "liberties" of religion, speech, the press, and teaching (*LP*, 15-25). According to the Thomistic apparatus of that encyclical:

> The eternal law of God is the sole standard and rule of human liberty, not only in each individual man, but also in the community and society which men constitute when united. Therefore the true liberty of human society does not consist in every man's doing as he pleases, for this would simply end in turmoil and confusion and bring on the overthrow of the State […] (*LP*, 10).

There is no possibility in this theological framework of accommodating the idea that individuals might bring about a socially benign state of affairs as the unintended consequence of pursuing their own private ends. The *Body* must be held together by love; and all its members must obey the eternal law of God.

Original sin throws all this into doubt. Because of sin, love often fails; and because of sin we continually disobey the eternal laws of God. Yet—save in exceptional and temporary circumstances such as those now prevailing in some Arab dictatorships—we do not normally see "turmoil and confusion". Indeed it is precisely in those places where individuals have been freest to pursue their own private ends, notably in Britain, the USA and other English-speaking countries, that we see the highest achievements of peaceable cooperation.

Evidently there is theological work for *Providence* to perform. St Augustine confronted the problem of evil presented by his horrifying doctrine of *original sin* with a theodicy of social and political institutions. God is just and allows us to suffer the consequences of *original sin*. But He is also merciful and provides means for those very consequences themselves to become remedies for our sin. The state, private property, slavery, and the hangman are evil in themselves, but under *Divine Providence* they save us from a worse evil: destruction by our more powerful neighbours at home and abroad (Deane 1963, chapters III, IV passim).

THEODICY, APOLOGETIC, AND THE MARKET ECONOMY

Late seventeenth-century Jansenist scholars proposed an Augustinian theodicy of self-interest in economic life using the concepts of *original sin* and *Providence*. Anglican thinkers in the eighteenth century provided

an Apologetic of self-interest by developing the concept of *Caritas*. Those we now think of anachronistically as 'economists' in France and Britain made use of their work to assemble a coherent, large-scale theory of the self-regulating market economy that was acceptable to the most rigorous religious and moral sensibilities in a Christian society.

The internationally celebrated moralist Pierre Nicole (1625-1695) and the eminent jurist Jean Domat (1625-1696) taught at the Benedictine community of Port Royal. This was the home of the so-called 'Jansenist' movement within the Gallican Church, which was more purely Augustinian—less Thomistic—than was usual among Roman Catholics at that time. Acutely conscious of the pervasiveness of human sin, Nicole and Domat were forced to construct a theodicy of civil life for their students. Why does God allow humans, created in his own image but defaced by *original sin*, to be as selfish, power-hungry, predatory, and cruel as we continually observe our species to be? Their solution followed St Augustine's model. Under *Providence* the unintended consequences of our self-seeking propensities include the bringing about of the institutions of political society, which are both a *punishment* and a *remedy* for sin. Nicole and Domat extended the analysis to the market economy.

> […] when travelling […] we find men ready to serve those who pass by and who have lodgings to receive them almost everywhere. We dispose of their services as we wish. We command them; they obey […] What could be more admirable than these people if they were acting from charity? It is cupidity which induces them to act […] Think what charity would be required to build an entire house for another man, furnish it, carpet it and hand him the key. Cupidity does this quite joyfully (Nicole 1670, 204-205; cited in Faccarello 1999, 28).

For Augustine 'cupidity' or 'avarice' is one of the three primary sources of 'lust' (*libido*), "the fundamental quality of the unregenerate" (Deane 1963, 44). Yet in this context it is conceived as permitted, even as used, by God to compensate for a failure of *Caritas*.

Gilbert Faccarello (1999, 26-32) cites a range of cognate passages from both Nicole and Domat to illustrate the general Augustinian position summarized by Domat:

> […] from so evil a passion as our self-love, and from a poison so contrary to the mutual love which ought to be the foundation of

> society, God created one of the remedies which enable it to survive; for from the principle of division He constructed a link which unites all men in a thousand ways and which maintains most agreements (Domat [1689] 1828-1829, 25; cited in Faccarello 1999, 27).

Faccarello has shown how the French economic thinker, Pierre de Boisguilbert (1646-1714), who had been a pupil at Port Royal under Nicole and Domat, constructed the first complete theory of the self-regulating market economy on this basis. His *Le détail de la France* ([1695] 1966, vol. 2, 591-661) explained how, under *Providence*, the unintended consequences of the competition of agents, each motivated by self-love in response to incentives created by market prices, produced a state of "harmony" or "equilibrium". Modern economic theory, descending from Boisguilbert through Cantillon, Quesnai, and Adam Smith to the 'classical' political economy of Malthus, Ricardo, and J. S. Mill, has its origin in Augustinian theodicy.

Though Nicole was so highly regarded in England that John Locke translated three of his essays, Boisguilbert's work remained unknown. His path-breaking economic ideas were transmitted instead through Bernard Mandeville's notorious satire, *The fable of the bees: or, private vices, public benefits* ([1714-1728] 1988), which was placed on the *Index Librorum Prohibitorum* in 1744. Like the Jansenists, whose work he would certainly have known, Mandeville assumed that self-love is evil; like them he argued that the "Publick Benefits" of market exchange are driven by this "Private Vice". The multifarious economic activities of modern society arise *and can only arise* in a gradual, unplanned, accidental, piecemeal fashion in response to the incentives for individual self-regarding action created by others' needs, wants, and desires. But his work appeared to be a crude parody and was reviled as blasphemous by the godly and respectable.

The reason for this adverse response was that if self-love really is a 'vice' then we have yet another nasty case of the problem of evil. Why does God allow (or worse, 'design') a world in which good things necessary for human life and happiness require moral evil for their production? The crucial question of course is the moral and theological standing of *self-love*. The Jansenists were ultra-Augustinian in regarding it as an 'evil passion', for St Augustine himself had acknowledged that God cannot be understood to have ruled out, or even to have frowned upon, self-love. Indeed, He commands it:

> Iam vero quia duo praecipua, hoc est dilectionem Dei et dilectionem proximi, docet magister Deus, in quibus *tria* invenit homo quae diligat, Deum, *se ipsum* et proximum, atque *ille in se diligendo non errat qui Deum diligit* […] (*Civ. Dei* XIX, 14; italics added).[8]

We must therefore identify three senses or aspects of *Caritas*: $Caritas_1$ as love of God; $Caritas_2$ as love of neighbour; and $Caritas_3$ as love of self.

This theme was taken up and developed definitively by Joseph Butler, a convert from Dissent who eventually became Bishop of Durham and perhaps the most powerful theological mind of the eighteenth century. His fifteen *Rolls Sermons* (Butler [1726] 1969) were preached in the immediate aftermath of the public outcry aroused by the 1723 edition of the *Fable* (Waterman 1997, 240-241). As against the influential doctrine of Lord Shaftesbury's *Characteristicks* (1711), Butler showed that the ends of private good and public good "do indeed perfectly coincide"; that "self-love is one chief security of our right behaviour towards society"; that under *Providence* much unintended social good is produced by self-regarding actions; and that "there is seldom any inconsistency between what is called our duty and what is called interest" (Butler 1969, 32, 36, 37-38, 67). Sermons XI and XII, 'On the Love of our Neighbour' (164-202), recognize that $Caritas_3$—*love of self*—is not merely permissible for the faithful but is actually a duty commanded by Christ himself.

Jansenist theodicy rectified by Anglican apologetic cleared the way for the development by Anglophone Christians of Boisguilbert's pioneering economic insights. The first was the Dean of Gloucester, the Reverend Josiah Tucker (1713-1799), who had been Butler's chaplain when the latter was Bishop of Bristol. In *Elements of commerce* (Tucker [1755] 1993, 58) he explains how "SELF-LOVE, the great Mover of created Beings, determines each Individual to aspire after these *social* Goods, and to use the most probable Means of obtaining them"; for

> the same good Being who formed the religious System, formed also the commercial, and the End of both, as designed by Providence, is no other than this, That private Interest should coincide with public,

[8] Translation (Everyman edition): "God, our good master, teaching us in the two great commandments (the love of him and the love of our neighbours), to love *three* things: God, our neighbour *and ourselves*, and seeing that *he that loves God offends not in loving himself* […]".

> self with social, and the present with future Happiness (Tucker [1757] 1993, 73).

All the ingredients were now to hand for what was to become, two decades later, the central message of *The wealth of nations*:

> let the Legislature but take Care not to make *bad Laws,* and then as to *good ones*, they will make themselves: That is, the Self-Love and Self-Interest of each Individual will prompt him to seek such Ways of Gain, Trades and Occupations of Life, as by serving himself, will promote the public Welfare at the same Time (Tucker ([1757] 1993, 48).

Adam Smith acquired Tucker's writings for his own library (Mizuta 1996), and would also have known of Tucker and his ideas from his friends David Hume and Lord Kames. By the third quarter of the eighteenth century, English-speaking Christians had assimilated and domesticated a theological rationale of individual private interest in economic affairs that is now part of the air we breathe. Samuel Johnson, oracle of Tory high-church piety summarised it memorably: "There are few ways in which a man can be more innocently employed than in getting money" (Boswell [1791] 1960, 597).

CONCLUDING REMARKS

Caritas in Veritate is long, diffuse, and wide-ranging. I have deliberately ignored some of its most important ideas, such as those of *development* (*CV*, 11, 13-15, 17-19, 21, 29, 52, 76, 79), the *common good* (*CV*, 7, 21, 36, 41, 57), and *justice* (*CV*, 6, 35, 36, 54, 78). It correctly warns against *rights* talk and insists on the moral priority of *duty* (*CV*, 43); and correctly affirms that "Man is not a lost atom in a random universe: he is God's creature, whom God chose to endow with an immortal soul and whom he has always loved" (*CV*, 29). More controversially it asserts that "on this earth there is room for everyone" (*CV*, 50), and reiterates the teaching of *Humanae Vitae*—which rather surprisingly it supposes to be "without any direct link to social doctrine" (*CV*, 15). Each of these neglected themes would require an article at least as long as this one to do it justice.

I have focussed narrowly on the relation between economics and theology in this encyclical, and the preceding *Centesimus Annus,* because I believe that in so doing we may discover the conceptual core of papal social doctrine, and thereby throw some light on the

divergence—and partial isolation—of that doctrine from the mainstream of modern economic and social thinking. In particular I have tried to show that this divergence is not at all a consequence of the fact that whereas papal social doctrine is theologically informed, the mainstream is merely 'secular'. For though modern social theory, like all science, is indeed 'secular' in its method, its intellectual history reveals the formative role of a Christian theology based on exactly the same set of theological concepts as papal social doctrine. Divergence has come, rather, from the differing use made of that common theological material. Some of this can be explained by the growing intellectual estrangement of the Roman Church from the main currents of European thought in the eighteenth and early nineteenth centuries.

The so-called 'Enlightenment' in eighteenth-century France was anti-clerical and sometimes anti-Christian. An attack on 'superstition', identified with the doctrine of transubstantiation, was central (Waterman 2004, chapter 2). It was therefore difficult if not impossible for Catholic thinkers, whether Gallican or Ultramontane, to join in 'the Enlightenment project'. There were no major Catholic philosophers after Nicolas Malebranche (1638-1715), and even his work was placed on the *Index*. The French Revolution, which carried the French (but not English) Enlightenment attack on Christianity to its furthest extent, swept away the *ancien régime* of Church-and-State in France and elsewhere. The papacy was humiliated and almost destroyed. Though the Papal States were returned in 1815 and papal religion once again tolerated throughout Europe, the Roman communion was never restored to its commanding position as the established Church of the West. Its property was plundered and its authority ignored. For seventy years the Curia licked its wounds and bewailed "the philosophy of this age", which it blamed for "progress, liberalism and modern civilization" (Waterman 1991). When Leo XIII revived the philosophy of an earlier age in 1879 he restored the possibility of intellectual respectability, but did nothing to reunite Roman Catholic thought with the main stream. Indeed the effect of compulsory Thomism may only have made it more sectarian.

Meanwhile 'the Enlightenment project' had flourished in England. From Newton (1642-1727) and Locke (1632-1704) to Paley (1743-1805) and Malthus (1766-1834), its intellectual leadership came almost entirely from Anglicans, who so far from being bound by the doctrine of Transubstantiation were actually obliged to repudiate it. The English

Enlightenment was therefore "conservative, clerical and magisterial" (Jacob 1981; see also Pocock 1980; 1985). It was in England rather than France that the decisive theological work was done to demonstrate that the new economic ideas of competitive individualism and spontaneous order are consistent with traditional Christianity.

In principle there is no reason why Romanist theologians should not have availed themselves of this new work, even though generated by those outside their communion. For example the *Defensio Fidei Nicaenae* (1685) of the Anglican theologian George Bull (1634-1710, Bishop of St David's from 1705) was warmly commended by the great Bossuet and other French theologians, and his *Judicium Ecclesiae Catholicae* (1694) received a formal tribute of thanks from the Synod of St Germain in 1700. But by the middle of the eighteenth century the Roman Church had become inward-looking and defensive, perhaps especially in France. It remained on the defensive until the pontificate of Leo XIII, by which time it had virtually cut itself off from all outside intellectual influences.

But the world has changed since 1891. If only because all the Christian churches and institutions in the North Atlantic world are now a dwindling and embattled minority, intellectual and cultural differences between Roman Catholics and the rest seem less acute than formerly. Papal social doctrine, which has exhibited many a twist and turn over the past 120 years (Waterman 1982), could easily be made to accommodate the social-theoretic implications of theological material already in place: and thereby purge itself of incoherence.

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The role of hypothesis testing in the molding of econometric models

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Abstract: This paper addresses the role of specification tests in the selection of a statistically admissible model used to evaluate economic hypotheses. The issue is formulated in the context of recent philosophical accounts on the nature of models and related to some results in the literature on specification search. In contrast to enumerative induction and a priori theory, powerful search methodologies are often adequate substitutes for experimental methods. They underwrite and support, rather than distort, statistical hypothesis tests. Their success is grounded in a systematic effort to mold appropriate models to, and test them against, constraints of the data.

Keywords: statistical testing, hypothesis tests, models, general-to-specific specification search, optional stopping, severe tests, costs of search, costs of inference, extreme-bounds analysis, LSE econometric methodology.

JEL Classification: B41, C18, C12, C50

Economics is a modeling science. The Nobel laureate James Heckman (2000, 46) has said that, just as the Jews are the "people of the book", the economists are "the people of the model". Of course, economists are not alone in this. In the period since the mid-20th century, the model has become the dominant epistemic tool in a wide variety of sciences. The philosophy of science used to pay a great deal of attention to issues such as the axiomatic structure of formal scientific theories and to demarcation criteria between science and non-science. Increasingly,

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it has focused on how models work in science.[1] The change is part of the "naturalistic turn" in the philosophy of science—the laudable notion that, if we want to know how science works, we ought to try to understand the practices of scientists.

My own work as a methodologist derives from my work as a monetary- and macro-economist. I was interested in the role of macroeconomic and monetary policy in controlling inflation and real output, which raises questions about the causal structure of the economy. In that context, I developed a kind of interventionist or "natural-experiments" approach to causal inference (Hoover 2001). In implementing the approach in real-world cases, I was forced to characterize the data statistically and adopted the model-selection strategies of the LSE (London School of Economics) approach of David Hendry and his colleagues and co-workers (Mizon 1984, 1995; Hendry 1987, 2000). In the event, it was this approach, which relies heavily on statistical testing, and not my own contribution to causal inference that raised questions with referees. So, I came to the problems of statistical testing through the backdoor. Even now, I prefer to keep my reflections grounded in the specific problems encountered in my own practices.

Issues related to statistical testing can, I think, be subsumed to more general issues related to modeling. Frequently, inferential problems assume that the form of a probability distribution is known and the test relates to some parameters of that distribution. McCloskey and Ziliak provide a neat example of what worries me: "the accuracy of [the] estimated mean [of a regression coefficient] depends on the properties of the error term, the specification of the model, and so forth. *But to fix ideas suppose that all the usual econometric problems have been solved*" (McCloskey and Ziliak 1996, 98; emphasis added). They, like many others, ignore the larger problem: how would we justify the supposition that "all the usual econometric problems have been solved"? All my own work on causality in macroeconomics was about choosing the form of the relationships that McCloskey and Ziliak and most econometric textbooks simply take as given. In this paper, I want to consider the role of statistical tests in addressing the problem of selecting—or better, *shaping* or *molding*—economic models.[2]

[1] See Morgan and Morrison 1999, and Morgan 2012.
[2] I am echoing here Boumans' (2005, chapters 1 and 3) notion of the "mathematical moulding of economic theory".

MODELS

Let us begin with models, without supposing that they are necessarily stochastic or invoke probability. The concept of causation, on my preferred account, is one of mechanism or structure (Hoover 2001, chapters 1-4). The object of an empirical analysis of causation is to construct a model that recapitulates the salient features of the mechanism and displays its causal architecture perspicaciously. In most cases, the role of a model is to make hidden causal relationships visible. Economic data do not wear their causal relationships on their faces. But that is a matter of degree. Some modeling exercises recapitulate relationships that are, as it were, visible to the naked eye. For example, children and aeronautical engineers make models of airplanes in which the mapping from the real airplane to the model is not much of a mystery. (I do not wish to underestimate the complexity of the relationship between the modeled and the model, even in this case; see Sterrett 2005.)

Models are instruments for relating truths about the world. Although models are sometimes "approximations" in an exact sense of that word, I prefer to think of them, up to some explicit or implicit level of precision, as telling the plain truth about limited aspects of the world or from particular perspectives on the world (Hoover 2012). Models may have varying levels of precision and cast the world from various points of view, but their premier virtue is accuracy (i.e., in being used to claim what in fact happens in the world).

Models are governed by their constitutive properties, internal mechanisms or rules of operation (e.g., mathematical or logical structure). Some of these properties are specific to the model and irrelevant to the world. A wind-tunnel model, for example, need not have an internal structure that mimics an actual airplane, so long as the mass and exterior shapes are appropriate. Models may be closed systems in which deductive results are available or their operation may be only analogical with results available through simulation. In either case, the world of the model is not automatically informative about the real world. It will be informative only if there is a good mapping between model and world on relevant dimensions, which adds an interpretive relationship between model as object and its implications for the real world.

Perhaps *the* principal function of models is as engines for counterfactual analysis. We validate the mapping between real-world

and the model using observations of the real world as our guide, but the utility of the model is that manipulations of it reveal facts about the world that we have not yet or, perhaps, cannot ever observe directly. This is the source of the utility of a model for prediction or control.[3]

The general characteristics of models are evident in such transparent cases as the model airplane. In economics, however, as in many disciplines, they are typically less transparent, and we value models precisely because they clarify the actions of hidden mechanisms. Consider Project Ultra in which the British successfully read German military codes in World War II. The code-breakers constructed a working model of the German's Enigma code machine. In part, they benefitted from stealing versions of the machine. Nevertheless, a substantial part of their success arose from figuring out how the machine actually worked (how it generated the intercepted coded signals). Their model did not need to be an exact copy; it did need to be an appropriate analogue. And it served as a tool for counterfactual analysis: given that the model provides a mechanism that accounts for some observed code with a particular initial setting, the machine allowed the code-breakers to determine what any particular piece of plain text would look like with some other initial setting.

The process of modeling the Enigma machine was not a process of conjecture and refutation or of hypothesis testing of the form, "propose a hypothesis and then ask, 'accept or reject?'" Rather it was a process of molding the model mechanism to constraints—some directly from data, some from other considerations. And it is a process very unlike the philosophers' accounts of inductive logic. Typically, induction is presented as a problem of moving from specific observations to a generalization: raven_1 is black, raven_2 is black, raven_3 is black, …, raven_n is black; therefore, all ravens are black (or, very probably, all ravens are black). This kind of inference inaccurately describes the scientific or practical reasoning of the code-breakers. First, it is too simple. It may be a good strategy for finding the proportion of white beans in an urn, but it fails to come to grips with the wide range of inferential patterns found in science and everyday life. Second, it does not deal with the role of creativity in learning. We really must engage in a good deal of guessing the answers on the basis of pre-existing beliefs. This process is not, however, unfettered. It is a process in which our beliefs are

[3] See Hoover 2011 for the role of economic models in counterfactual analysis.

mutually constraining, even when those beliefs are not held with complete conviction. We gain conviction from their mutual reinforcement.

Here is a mundane illustration. In many cases when we have solved a complex crossword puzzle, our conviction that our solution is correct is nearly absolute. It is not that there may not be a possible world in which an entirely different set of answers fit the physical constraints of the puzzle grid and satisfied reasonable interpretations of the clues. We cannot rule out such a possibility a priori, but neither should we feel compelled to let it have great force over our thinking when the fact is that our solution fits together nicely and that it is extremely difficult to get any solution to fit together at all. In solving the puzzle, we have passed a severe test.

Creative imagination is essential to progress, but the limits of imagination also constrain the alternative choices that we might consider. Frequently, the imaginations of different investigators point to different solutions, which must be checked against the commonly accepted constraints or tested by generating new constraints that may not satisfy one or another alternative. We can, for example, see the Ptolemaic and Copernican models of the solar system as different imaginary solutions to the observed motions of the planets and stars. Our preference for Copernicus over Ptolemy is that ultimately, though this was not immediately obvious, it better fit the constraints. Of course, the original Copernican system is not entirely satisfactory, and our modern model has been molded to adapt to the additional constraints of later observations and our belief in Newton's laws, among other things.

ECONOMIC MODELS

The problem of empirical economics is largely one of inferring the nature and properties of the hidden mechanisms of the economy. We do that in the manner of the code-breakers: we construct analogue models of some features of the economy. Economic theory can be regarded as a set of model templates for such mechanisms, and the problem of the applied economist is to find a good template and to mold it to various constraints imposed by observed data and pre-existing beliefs.

Let me give a hackneyed example. Suppose that we want to know how the price of electricity affects the demand for electricity. We might appeal to a supply-and-demand model:

(1) $Q_E^D = a + bP_E + cT$ (Demand)

(2) $Q_E^S = d + eP_E + fP_C$ (Supply)

(3) $Q_E = Q_E^D = Q_E^S$ (Equilibrium)

In this model, Q_E = quantity of electricity; Q_E^D = demand for electricity; Q_E^S = supply of electricity; P_E = price of electricity; P_C = price of coal; and T = temperature. Figure 1 shows the model in a graphical form. Here a problem is evident: if we know only the data (Q_E, P_E, P_C, and T), we account for a single observation where the supply and demand curves cross and we cannot learn what we want to learn, namely *how* price affects the demand for electricity. If it happened that T were constant and P_C were variable and, in addition, some other assumptions held, then shifts in the supply curve (shown in the figure as grey lines) would trace out the demand curve and we would be able to identify the values for the coefficients a and b. If both T and P_C varied, then we would be able to identify all of the coefficients.

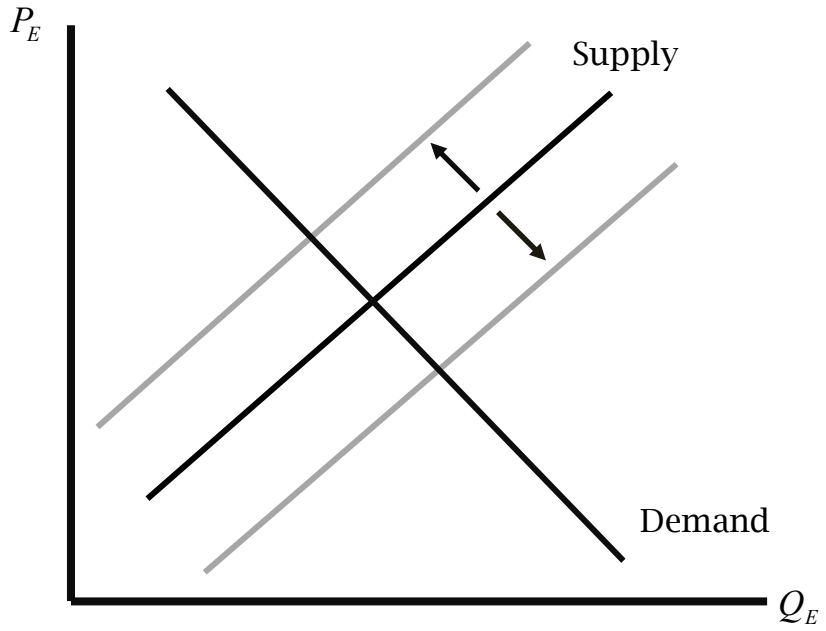

Figure 1: Supply-and-demand model of electricity

But what about the other assumptions we make? For example, variations in T and P_C are independent of each other, the underlying relationships are well modeled as linear, T does not appear in the supply equation nor P_C in the demand equation, there are no other

shifters of the equations, and so forth. That knowledge is not in the observable data. How do we know it? The standard answer to this question among economists—going back at least to Haavelmo's seminal "Probability approach in econometrics" (1944)—is that it is a priori knowledge based in economic theory. But how did we come to have such knowledge? Indeed, this question is hardly ever addressed.

The concept of a priori knowledge, which is relied upon to do a vast amount of work, has never to my knowledge been examined by econometricians or economic methodologists. The professed faith in economic theory as the source of such knowledge amounts to whistling in the dark.[4] Economic theory in its pure form generates very weak conclusions: for example, we can reasonably hold it to suggest that demand curves slope down ($b < 0$), but it certainly does not tell us that demand depends on temperature (T) and not on the price of coal (P_C) or any other factor.

Sometimes we are told that it is not theory, but subject-matter knowledge (expert knowledge) that supplies the ground for our a priori knowledge. This is, perhaps, closer to the truth, but equally unanalyzed by econometricians, methodologists, and philosophers alike. To answer a question about the nature of demand, we need to have a model with known properties that maps well onto properties of the world. Is there a systematic method for obtaining such knowledge? Would the statistical methods used in econometrics help? The answer must be, no, if econometrics, as it is presented in many (perhaps most) textbooks, is limited to the problem of statistical estimation of the parameters of structures assumed to be known in advance.

ECONOMETRIC MODELS

The problem of a priori knowledge and of identification are typically thought of as econometric or statistical problems. The supply-and-demand model shows, however, that the problem arises in deterministic systems. It is a problem of modeling and not a problem of probability or statistics per se. The problem is to find sufficient constraints that allow us to effectively mold our model into one that is strongly analogous to the hidden mechanisms of the economy. We cannot do that by armchair speculation or appeals to economic theory. The only hope is for the data

[4] Skepticism about identification has been expressed by Ta-Chung Liu (1960) and Christopher Sims (1980); see Hoover 2006.

to provide some of the key constraints in the same manner as they do in solving a crossword puzzle or breaking a code. If the world is indeterministic, either ontologically (reality is deeply stochastic) or epistemically (we are so ignorant of the full spectrum of causes that from our limited point of view reality acts as if it were deeply stochastic), we will need to account for its indeterminism in our models. We may do this by developing probabilistic models. (There may, of course, be other modeling tools applicable to indeterministic models. We are too apt to privilege our analytical creations. There is no more reason to assume that well-known treatments of probability provide the only possible resource for confronting indeterminism than there is for thinking that balsa wood is the only suitable material for model airplanes.)

The usual formal treatments of probability are, I believe, best seen as characterizing properties of models, leaving open the connection between such models and the world. Probability models grab on to the world in just the same way as other models do through analogy in specific respects useful for the particular purposes of particular agents (see Giere 2006, 60). My view is perhaps usefully expressed in Giere's treatment of models as predicates, such as "is red." For example, a classical particle system is a model of behavior that obeys Newton's laws and the law of gravity for interacting point masses. To say that our solar system is a classical particle system is to make a claim that this model provides accurate analogies for the motions of the planets around the sun (Giere 1979, chapter 5; Giere 1999, 98-100, 122; and Giere 2006, 65; see also Hausman 1992, 74). Kolmogorov's (or other) axiomatizations of probability provide just such a model of probability and can be regarded as a predicate in the same manner. The cases that most interest me are cases where the laws of probability can be accurately predicated of processes in the economy or physical world. A model can be predicated wherever it effectively captures analogous features; so I leave it as open question whether probability models can be effectively applied as descriptive or normative models of beliefs as advocated by Bayesians.

Statistical tests come into modeling on my view as measures of the aptness of the predication. A cooked example, originally due to Johansen (2006, 293-295) will help to make my point (also see Hoover, et al. 2008, 252-253). Johansen starts with the unobservable data-generating process:

(4) $$x_t = 0.9x_{t-1} + 1.0 + \varepsilon_t;$$
$$t = 1, 2, \ldots, 100;\ x_0 = 10,$$

where the ε_t are identically independently distributed (i.i.d) $N(0, 1)$. Note that $E(x_t) = 1/(1-0.9) = 10$ and $\text{var}(x_t) = 1/(1-0.9^2)$. Consider an economic theory that predicts that the mean value of x is $\mu = 10$. (Here, the theory happens to be *exactly* true, but it need not always be so.) To test the theory we need to provide a model of the probability process. One model is:

(5) $$x_t = \mu + v_t \qquad \textbf{(Model 1)}$$

where the v_t are i.i.d. $N(0, \sigma^2)$. For one simulation of equation (4), the maximum-likelihood estimate of Model 1 yields $\hat{\sigma}^2 = 5.256$ and an estimate of a 95 percent asymptotic confidence interval for $\hat{\mu}$: $\hat{\mu} \pm 1.96\hat{\sigma}/\sqrt{T} = 9.138 \pm 0.449$. Since 10 does not lie within the confidence interval, it might appear, then, that we have good grounds to reject the hypothesis that $\mu = 10$.

But is this model accurately predicated of the data-generating process? The error terms in Model 1 are i.i.d normal. Given the data-generating process (4), a simple statistical test would almost certainly show that the residuals do not conform to that assumption, but are serially correlated. We can conclude, then, that Model 1 cannot be accurately predicated of the data-generating process and that our estimate μ is unlikely to be properly analogous to $E(x_t)$, which is its target and, consequently, our theory has not been tested adequately.

An alternative statistical model is

(6) $$x_t = \rho x_{t-1} + \mu(1-\rho) + v_t \qquad \textbf{(Model 2)}$$

where again the v_t are i.i.d. $N(0, \sigma^2)$ and $E(x_t) = \mu$, if $|\rho| < 1$. Model 1 is nested in Model 2. Again omitting details, the maximum likelihood estimate of Model 2 for the same simulated data yields an estimates of $\hat{\rho} = 0.923$ and $\hat{\sigma}^2 = 0.744$, which translates into the 95 percent asymptotic confidence interval of $\hat{\mu} \pm 1.96\hat{\sigma}/[(1-\rho)\sqrt{T}] = 9.123 \pm 2.247$. On the basis of this confidence interval, we cannot reject $\mu = 10$.

Statistical tests play two different roles in Johansen's cooked illustration. First, they translate the data into constraints on the form of the model in the same way that the puzzle grid and reasonable interpretations of the clues impose constraints on the solution to the crossword puzzle. Model 1 does not display serial correlation ($\rho = 0$). It is highly unlikely that a model of that form could generate the pattern of the observed data, so we conclude that it would be inaccurate to predicate Model 1 of the data-generating process. Model 2 allows us to compare the estimated $\hat{\rho}$ to a null of $\rho = 0$. The test rejects the null, and relative to an alternative such as $\rho = 0.9$, the test is severe in the sense of Mayo and Spanos.[5] The way in which Model 1 fails actually suggests a property that any more accurate model will have—i.e., it must be able to generate serially correlated realizations.

The second role of statistical tests in the cooked illustration is the more familiar one: they are used to evaluate hypotheses conditional on the form of the model. If Model 2 is an acceptable model, then μ is not very precisely estimated, but it is consistent with the hypothesis that $\mu = 10$. This is the basis on which hypothesis testing is usually conducted. The model is given, and we are concerned entirely with the precision of the estimates.

To interpret an estimate of a parameter, we must have a model in which the parameter is meaningful. Econometricians are wont to say that economic theory provides that model. While economic theory may impose some constraints on acceptable models, it is a vanishingly small class of cases in which it provides a single, estimable model. The first use of statistical models is to draw on the resources of the data itself to

[5] The idea of "severe testing" is due to Deborah Mayo and Aris Spanos (see Mayo 1996; Mayo and Spanos 2006). This idea may be unfamiliar, so let me give a précis. In its statistical formulation, severe testing hinges on the distinction between substantive and statistical significance. Consider a test of a null hypothesis. In the typical textbook framework, one accepts the null if the test statistic is less than the critical value for a designated size and rejects it if it is greater. But is such a test severe? That depends on the alternative hypothesis. We must choose an alternative that is just big enough to matter substantively. The hypothesis retained by the test—either the null or the alternative—has been severely tested if this test outcome would have been highly unlikely had the opposite hypothesis been true. Thus, a test is severe if we give it every chance to fail and yet it still succeeds. Severity is judged on an ex post analysis, where probabilities are evaluated relative to the actual value of the test statistic rather than relative to a critical value set in advance. We are familiar with such attained probabilities: the *p*-value, for example, gives the *attained* size—that is, the greatest test size that would have led us to accept the null based on the *actual* data.

cover the weakness of economic theory in this regard. Seen this way, the first use of statistical tests in molding the model shows that Model 1 is not an acceptable starting place for the second use of statistical tests. The precision of the estimate of μ is spurious, because that estimate takes its meaning from a model that does not accurately analogize to a salient feature of the world.

Econometrics as it is taught in textbooks—and even as it is sometimes practiced—focuses on the second use of statistical tests as if we had a priori knowledge of the structure of the model to be estimated. It is as if economic theory gave us direct access to the book of nature in which God had written down almost everything important, but somehow thought that it would be a good joke on people to leave out the values of the parameters. We do not have that sort of knowledge. We have to rely on empirical observation to learn the structure of the model just as much as we must to learn the values of parameters. Econometricians have frequently resisted the first use of statistical tests with a powerful, but ultimately vague, and not-consistently-developed, fear of data mining.

SPECIFICATION SEARCH AND ITS ENEMIES

Among economists 'data mining' is a pejorative term, nearly always invoked as a rebuke. Unhappily, the metaphor has escaped them: gold mining is the sine qua non of uncovering treasure. Yet, the economists' fear does have a basis. Imagine that we have a data-generating process such as

$$x_t = \delta + \varepsilon_t, \tag{7}$$

where δ is a constant and the ε_t are i.i.d. $N(0,\sigma^2)$. Suppose that we seek to model this process with

$$x_t = \mu + \beta y_t + v_t \tag{8}$$

(Model 3)

where the v_t are i.i.d. $N(0,\sigma^2)$ and y is some element of an infinite set of mutually independent, i.i.d. variables. Most elements of that set would prove to be insignificant as the regressor (y_t) in (8) (i.e., we will not be able to reject the null hypothesis of $\beta = 0$). But with a test size $\alpha = 0.05$, one time in twenty on average we will estimate a $\hat{\beta}$ that rejects the null. If we follow a search procedure that allows us to keep searching until we find one of those cases, the probability of finding a

significant regressor is one. This illustrates the *optional stopping problem* that is often thought to be the bane of hypothesis testing.

The optional stopping problem does not require that we have an infinite set of candidate variables. Even in a finite set the probability of finding significant regressors in a search procedure may be very far from the nominal size of the test used to evaluate their significance. In some cases, the probabilities can be calculated analytically. In more complex cases, they can be determined through simulations of the search procedure. To take one illustration, Lovell (1983, 4, Table 1) considers a data-generating process like equation (7) and searches over a set of mutually orthogonal i.i.d candidate variables with a known variance for pairs in which at least one of the variables is significant in a model of the form

$$x_t = \mu + \beta_1 y_{1t} + \beta_2 y_{2t} + v_t \qquad \textbf{(Model 4)} \tag{9}$$

Table 1 shows that for a *t*-test with a size $\alpha = 0.05$, the probability of the search procedure finding significant regressors—i.e., falsely rejecting the null implied in (7)—equals the test size only when there are only two candidate variables. As the number of candidate variables rises, the "true" significance level approaches unity. Lovell suggests that we penalize search by adapting critical values in line with the "true" significance levels rather than acting as if the nominal size of a single-shot test remained appropriate.

Table 1: The dependence of the true size of a hypothesis test on search

Number of variables in pool	True significance level
2	0.050
5	0.120
10	0.226
20	0.401
100	0.923
500	0.999

Notes on Table 1: Based on Lovell (1983, Table 1). Variables in the pool are independent i.i.d and the hypothesis of no relationships with the dependent variables is true. Search procedure regresses dependent variable on pairs of variables in the pool until at least one coefficient is statistically significant at the $\alpha = 0.05$ level. The true significance level is the actual proportion of searches in which a significant regressor is identified.

Two distinct costs accrue to not knowing the true parameters of the data-generating process (see Krolzig and Hendry 2001, 833; Hendry and Krolzig 2005, C40). The *cost of inference* is the uncertainty that arises from estimation in the case that we know the structure of the model. It is illustrated by the standard error of the estimates of ρ and μ in Model 2. The *cost of search* is the cost that arises from the process of molding an econometric model into a form that accurately captures the salient features of the data-generating process. The take-home message of Lovell (1983) is that the costs of search are high, although in some cases calculable. The key lesson of Johansen's analysis of Models 1 and 2 is that the failure to mold the econometric model effectively may generate a large cost of inference: inferences based on Model 1 are systematically misleading about the likelihood of the mean of the data-generating process being close to 10. Another way to put this is that there is a *cost of misspecification* that offsets the cost of search and to evaluate any search procedure we have to adequately quantify the net costs.

In order to illustrate the failure of actual search procedures, Lovell (1983) conducts a more realistic simulation. He starts with a set of twenty actual macroeconomic variables. He then constructs nine models with different dynamic forms using subsets of the twenty as the independent variables in conjunction with definite parameter values and errors drawn from a random number generator. He then considers three search procedures over the set of twenty candidate variables: 1. stepwise regression; 2. maximizing $\overline{R}^2$; and 3. max-min $|t|$—i.e., choosing the set of regressors for which the smallest t-statistic in the set is the largest.

Table 2: Error rates for three simple search algorithms

	Error rates (percent)		
	Stepwise regression	**max $\overline{R}^2$**	**max-min $\|t\|$**
Type I error	30	53	81
Type II error	15	8	0

Notes on Table 2: Based on Lovell (1983, Table 7). The table reports the average error rates over 50 simulations of four models using three search algorithms.

Table 2 shows the empirically determined average type I and type II errors over fifty simulations of four of the models for a nominal test size of $\alpha = 0.05$. Since the relevant null hypotheses are that the

coefficient on any variable is zero, type I error can be interpreted as falsely selecting a variable and type II error as falsely rejecting a variable. Each of the search procedures displays massive size distortions. The table also shows that type I and type II errors are inversely related as intuition suggests.

It would be fallacious to suggest that because these particular (and very simple) search procedures have poor properties that we should prefer not to search but simply to write down a model and to conduct a one-shot test. Though based on a fallacy, one hears the one-shot procedure advised by colleagues from time to time. Johansen's example shows that the risks of misspecification vitiate that procedure. To his credit, Lovell does not suggest this, but instead suggests adjusting the nominal size of the tests to account for the degree of search. It also does not follow that, because these particular search procedures are poor, all search procedures are equally poor. The general prejudice against data mining captured in such phrases as "if you torture the data long enough, it will confess" are rather cavalier projections of the optional stopping problem in such simple cases as the one that Lovell examines to more complicated, but unanalyzed, situations. The problem with that analysis and with the three simple search procedures in Table 2 is that the procedures themselves do not constitute a severe test of the specification.

An alternative approach to search is found in the so-called LSE approach of David Hendry and his colleagues. Hoover and Perez (1999) were the first to automate search procedures in this family. We showed, using an experimental design similar to Lovell's, that these procedures were in fact highly effective and not subject to the massive distortions that Lovell found with the three simple procedures (see also Hendry and Krolzig 1999). Hendry and Krolzig incorporated a refined version of Hoover and Perez's search procedure into a commercially available program, *PcGets*, where the name derives from one of its key characteristics that search is conducting from a general to a specific specification (Hendry and Krolzig 2005). Working with Hendry, Doornik developed a search algorithm in the same family that uses a substantially different approach to investigating the search paths (Doornik 2009). The algorithm, *Autometrics*, is now incorporated along with the econometrics package *PcGive* into the *Oxmetrics* econometrics suite.

Different in detail, all the procedures based on the LSE search methodology bear a strong family resemblance. Omitting many of the minor details, I will describe Hoover and Perez's (2003) search algorithm:

1. **Overlapping samples:** A search is conducted over two overlapping subsamples and only those variables that are selected in both subsamples are part of the final specification.

2. **General-to-specific simplification:** A general specification includes all the variables in the search universe as regressors. A subset of the variables (five in the results for the cross-country-growth simulations reported below) with the lowest *t*-statistics serve as starting points for simplification paths. To start on a path, one variable in this subset is deleted. The path is determined by a sequence of deletions, corresponding to the lowest *t*-statistic in the current specification until all the remaining variables are significant on test with size α. At each deletion, the simplified regression is run through a battery of specification tests, including a subsample stability test and a test of the restrictions of the simplified model against the general model. If it fails a test, the variable is replaced and the variable with the next lowest *t*-statistic is deleted. The *terminal specification* is one in which either all variables are significant and the specification passes the battery of tests or in which no variable (significant or insignificant) can be removed without failing one of the tests in the battery.

3. **Selection among terminal specifications:** Tests are run among the terminal specifications to determine whether any one specification encompasses the others. If so, it is the *overall terminal specification* for the subsample. (see Mizon 1984; Mizon and Richard 1986; for a discussion of encompassing tests.) If not, a new specification is formed as the non-redundant union of the regressors of the terminal specifications, and the search procedure begins again along a single search path starting with this specification.

4. **Elimination of adventitious variables:** The *final specification* is the intersection of the regressors of the overall terminal specifications from the two subsamples.

Compared with the search algorithms investigated by Lovell, this is a complex procedure. Its general idea, however, is relatively simple. Just as Johansen's Model 2 nested Model 1, the initial general specification nests all possible final specifications. This guarantees that, if a model that adequately captures the data-generating process is nested in the

general model, it will be possible to identify it in principle. Multiple search paths reduce the likelihood that low probability realizations will lead away from the target model. A criterion for the adequacy of the model is that it supports the statistical assumptions that would be maintained for purposes of inference, which include, for example, white noise errors, homoskedasticity, normality, and subsample stability (see Johansen 2006). The statistical tests in the search procedure measure how tightly these constraints are binding, and the algorithm uses the tests to mold the final specification, by eliminating possibilities that violate them.

The anti-data-mining rhetoric that is fueled by results such as those reported by Lovell would lead one to guess that such a test procedure would inevitably lead to wild distortions of size and power. But this is not a question in which it is wise to judge from the armchair. Hoover and Perez (2003) conducted a simulation study using a subset of the data used in Levine and Renelt's (1992) study of cross-country growth regressions: 36 variables × 107 countries. The dependent variable (an analogue to the average rate of growth of GDP per capita 1960-1989, which was the target of their study) was constructed by selecting at random the independent variables. The coefficients for each variable were chosen by regressing average rate of growth of GDP per capita 1960-1989 on the chosen independent variables. The simulation then created an artificial dependent variable using error terms drawn from the residuals of this regression in the manner of a bootstrap. One hundred simulations were run for each of thirty specifications for true data-generating processes, and the true processes employed specifications involving 0, 3, 7, and 14 variables (12,000 specifications in all).

There is, of course, an irreducible cost of inference. Different simulations are parameterized with variables with wildly different signal-to-noise ratios. We know by construction that if our model were identical with the data-generating process, then the size of the test would be the same as the nominal size (assumed to be $\alpha = 0.05$ in all the simulations). The *empirical size* is calculated as the ratio of the incorrect variables included to the total possible incorrect variables. The *size ratio* (the empirical size divided by the nominal size) measures sins of commission. A size ratio of unity implies that search does not typically select variables that are not in the true model.

The power of the test depends on the signal-to-noise ratio. The *empirical power* for a given true variable is the fraction of the replications in which the variable is picked out by the search procedure; that is, it is the complement of the proportion of type II error. We determine the *true (simulated) power* through a bootstrap simulation of the data-generating process—that is, from the correct regressors without search. The true (simulated) power for a given true variable is the empirical power that one would estimate if there were no specification uncertainty, but sampling uncertainty remained. When the signal-to-noise ratio is low, the true (simulated) power will also be low; and, when it is high, the true (simulated) power will be high. The *power ratio* (the empirical power divided by the true simulated power) measures sins of omission. A power ratio of unity indicates that a search algorithm omits variables that appear in the true model only at the rate that they would fail to be significant if God had whispered the true specification into one's ear.

The two right-hand columns of Table 3 present the results for the general-to-specific search algorithm. The size ratios are very near to, or much below, unity. Far from losing control over size in the manner of Lovell's various search algorithms, the general-to-specific procedure is *more* stringent than nominal size. Power ratios are close to unity. Given that size and power are inversely related, adjusting the nominal size of the underlying tests upward until the size ratio reached unity would likely raise the power ratios towards unity as well.

The other four columns compare two other search algorithms that have been used in the literature on cross-country growth regressions and in other contexts. The two left-hand columns refer to Leamer's (1983) extreme-bounds analysis as modified by Levine and Renelt (1992). Here each variable is taken in turn to be a *focus variable*. The focus variable is held fixed in regressions that include it and every possible three-variable subset of remaining variables. A 95-percent confidence interval is calculated for the focus variable for each of the regressions with different subsets of regressors. Any variable is eliminated as not robust if any of these confidence intervals includes zero. The modified extreme-bounds analysis of Sala-i-Martin (1997) follows the same procedure, but treats a variable as not robust only if the confidence intervals include zero in more than 5 percent of the cases.

Table 3 shows that the extreme-bounds procedure and the modified extreme-bounds procedure fail in opposite ways. The two left-hand columns of the table show that the size ratios of the extreme-bounds procedure are tiny, implying that it almost never commits a sin of commission. But the power ratios are low and, in fact, fall to nearly zero when the number of regressors is large. In effect, its virtuous size is purchased with the wages of sins of omission: it simply rejects almost every regressor—the true are cast out with the false.

The problem of excessive omission of true regressors is the problem that motivated the modified version evaluated in the middle two columns of Table 3. Here the size ratios are very high, except when there are no true variables to be found. This implies that the procedure suffers from excessive commission: many variables are selected that should not be. The power ratios are better behaved, though less well behaved than for the general-to-specific algorithm.

Table 3: The efficacy of three search algorithms

	Extreme-bounds analysis		Modified extreme-bounds analysis		General-to-specific	
Models with:	**Size ratio***	**Power ratio†**	**Size ratio***	**Power ratio†**	**Size ratio***	**Power ratio†**
0 true variable	0.060		1.10		0.75	
3 true variables	0.003	0.43	5.17	0.77	0.77	0.95
7 true variables	0.030	0.13	5.89	1.10	0.81	0.93
14 true variables	0.020	0.04	5.45	0.67	1.02	0.82

Notes on Table 3: The table was originally Table 1 in Hoover and Perez 2004. The basic data are a pool of 36 variables described in Memorandum 1 downloadable from our websites:
http://www.econ.ucdavis.edu/faculty/kdhoover/research.html
http://www.csus.edu/indiv/p/perezs/Data/data.htm

For each number of true variables, 30 models are specified by choosing the indicated number of regressors at random from the pool. Coefficients are calibrated from a regression of the chosen regressors on the actual average growth rate. 100 dependent variables are created from the same regressors and coefficients and error terms constructed with a wild bootstrap procedure from the errors of the calibrating regression. Specification searches are then conducted by each of the three methods and the number of type I and type II errors are recorded. Statistics reported here average over each of the 100 simulations for each of the 30 models. Details of the simulations and the search procedures are found in Section 2 and Appendix A of Hoover and Perez 2004.

* Size is calculated as the proportion of incorrect variables included (significantly for general-to-specific) to the total possible incorrect variables. The size ratio is average ratio of the size to the nominal size (0.05) used as the critical value in all the hypothesis tests in the search procedures. A size ratio of 1.00 indicates that on average the size is equal to the nominal size (0.05).

† Power is calculated as the proportion of times a true variable is included (significantly for the general-to-specific procedure). The true (simulated) power is based on the number of type II errors made in 100 simulations of the true model without any search. The power ratio is the average ratio of power to true (simulated) power. A power ratio of 1.00 indicates that on average the power is equal to the true (simulated) power. The power ratio is not relevant when there are no true variable.

This simulation study shows that there are good and bad search procedures. A good search procedure is one in which the costs of search are low, so that all that remains are the costs of inference. The general-to-specific procedure appears to balance these costs well. And the particular results presented here have been backed up by other simulation studies as well (see Hendry and Krolzig 2005; Doornik 2009). What accounts for the superiority of the general-to-specific search compared to the alternatives (both those evaluated by Lovell and the two versions of extreme-bounds analysis)? I suggest that it is the severity of the testing procedure that arises from imposing multiple constraints on model through various specification tests. A theorem due to White (1990, 379-380) clarifies the process. Informally, the theorem says: for a fixed set of specifications and a battery of specification tests, as the sample size grows toward infinity and increasingly smaller test sizes are employed, the test battery will—with a probability approaching unity—select the correct specification from the set. According to the theorem, both type I and type II errors fall asymptotically to zero. Given sufficient data, only the true specification will survive a severe enough set of tests. The opponents of specification search worry that sequential testing will produce models that survive accidentally. Some hope to cure the problem through adjusting the critical values of statistical tests to reflect the likelihood of type I error. White's theorem, on the other hand, suggests that the true model is uniquely fitted to survive severe testing in the long run.[6] The key—as it is for breaking a code or solving a crossword puzzle—is to exploit the constraints of the data as fully as possible.

Asymptotic results are often suggestive but not determinative of what happens with fewer observations. The message, however, of the Monte Carlo simulations presented earlier is that it is possible to design

[6] Recent analytical results for some specific aspects of search algorithms have added to our understanding of when and how they reduce the costs of search to a second-order problem; see Santos, et al. 2008; and Hendry and Johansen 2011.

practical search algorithms that go a long way toward securing the promise of the asymptotic results. With models obtained through such severe search algorithms, the costs of search have been reduced sufficiently that it is reasonable to conduct inference as if we, in fact, knew the true model.

WRAPPING UP: MOLDING MODELS AND EMPIRICAL METHODOLOGY

Empirical economics has been regarded as an inductive science, a modeling science, and a science that relies on a priori theory as a substitute for experimental control. These characteristics sit uneasily together. Respect for the actual practice of empirical economics leads to skepticism of the relevance of simplistic accounts of enumerative induction and to honest recognition that a priori theory rarely provides compelling enough, or detailed enough, constraints on modeling to successfully replace experimental methods.

The solution, I have suggested, is to look again at modeling practice and to recognize how infrequently it looks like enumerative induction and how rarely empirical investigation proceeds along the simple Popperian lines of conjecture and refutation. Rather modeling is *typically* a process of molding the model to relevant constraints. These may come from prior beliefs, from general well-supported economic principles or empirical facts, and from the details of the data themselves. The ability of a model effectively and consistently to capture these constraints is a principal epistemic virtue that does the work often ascribed to induction.

In the case of empirical, particularly stochastic, models, the conformity of the model to the constraints can be checked by appropriate forms of statistical testing—especially through specification tests that are severe in the sense of Mayo and Spanos. Such tests are not used to establish economic hypotheses, but to establish that the model bears the appropriate relationship to its target. That is an essential step; for it is only in the context of such an appropriate relationship between model and real-world target that the ordinary statistical tests, which are the mainstay of econometrics textbooks, have any compelling force.

The practice of specification search (data mining—often pilloried, never quite reputable) is seen in a new light once the necessity of molding is understood and taken seriously. Since theory provides only weak constraints, the adequacy of a specification (or econometric model) can be established only with the additional constraints implied

by the data themselves. Data mining is indeed a poor practice if it is undisciplined by the imperatives of molding the econometric model to the constraints. That is the lesson of the optional-stopping problem and of the many badly performing search methodologies. But the evidence is rapidly accumulating that there are successful, powerful search methodologies that underwrite and support, rather than distort, statistical hypothesis tests. Their success is grounded in a systematic effort to mold appropriate models in which key modeling assumptions are tested rigorously against the constraints of the data.

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The oeconomy of nature: an interview with Margaret Schabas

MARGARET LYNN SCHABAS (Toronto, 1954) is professor of philosophy at the University of British Columbia in Vancouver and served as the head of the Philosophy Department from 2004-2009. She has held professoriate positions at the University of Wisconsin-Madison and at York University, and has also taught as a visiting professor at Michigan State University, University of Colorado-Boulder, Harvard, CalTech, the Sorbonne, and the École Normale de Cachan. As the recipient of several fellowships, she has enjoyed visiting terms at Stanford, Duke, MIT, Cambridge, the LSE, and the MPI-Berlin. In addition to her doctorate in the history and philosophy of science and technology (Toronto 1983), she holds a bachelor of science in music (oboe) and the philosophy of science (Indiana 1976), a master's degree in the history and philosophy of science (Indiana 1977), and a master's degree in economics (Michigan 1985).

She has published four books and over forty articles or book chapters in science studies. Some of the journals in which her articles can be found are *Isis*, *Monist*, *History of Political Economy*, *Public Affairs Quarterly*, *Daedaelus*, *Journal of Economic Perspectives*, and *Studies in the History and Philosophy of Science*. Her first book, *A world ruled by number* (1990) examines the emergence of mathematical economics in the second half of the nineteenth century. Her second book, *The natural origins of economics* (2005), traces the transformation of economics from a natural to a social science. She also has two co-edited collections, *Oeconomies in the age of Newton* (2003), with Neil De Marchi, and *David Hume's political economy* (2008), with Carl Wennerlind. She is currently writing a monograph on Hume's economics, as well as articles on the history and philosophy of bioeconomics. She is currently president of the History of Economics Society.

EJPE interviewed Margaret Schabas at the University of British Columbia in March 2013. In this interview, she recounts her earliest foray into the history and philosophy of economics, the conceptual trade between economics and natural science, and her most recent undertaking: the history and philosophy of bioeconomics.

EJPE'S NOTE: This interview was conducted by C. Tyler DesRoches, PhD candidate at the University of British Columbia in Vancouver and a co-editor of the *Erasmus Journal for Philosophy and Economics*.

EJPE: ***Professor Schabas, can you begin by describing how it is that you first became interested in the history and philosophy of economics. After all, you hold a bachelor of science in music?***

MARGARET SCHABAS: Well, you cannot take up music later in life, but I was always studying philosophy and physics along with music, and left open the option of an academic career. As a master's student in the history and philosophy of science (HPS) at Indiana University, I was encouraged to take a course from H. Scott Gordon and that got me interested in economics. Scott also taught Wade Hands and Harold Kincaid. He was about the only scholar teaching the history and philosophy of economics in a HPS department. When I decided to leave Indiana, spending a gap year in London to study music, he suggested I do my doctorate back in my home city of Toronto, which also had a HPS program. My supervisors were Sam Hollander and Trevor Levere.

At that time, the philosophy of economics as a subfield of the philosophy of science was really just emerging. Did you also realize that you were a part of this movement that included people like Dan Hausman and Alex Rosenberg?

No I did not, but I met them both within the first few years after my PhD of 1983 and am extremely grateful for their efforts to launch the field as we know it today. I had already studied the core literature in the philosophy of the social sciences, but it was not until my grad studies in economics at the University of Michigan that I undertook a systematic study of the philosophy of economics, in 1985, as a reading course with Alan Gibbard. We read Sen, Harsanyi, Tversky and Kahneman, among others. There were only a few jobs that listed the philosophy of economics as a field, but I managed to secure one of them, a two-year post at the University of Colorado-Boulder. That was my second job. My first and also temporary job was at Michigan State University, and I had the good fortune to be part of a regular seminar that Warren Samuels ran, with John B. Davis, and Zohreh Emami as members. By 1985 I had also met many of the other key contributors, Neil De Marchi, Mark Blaug, Mary Morgan, Philip Mirowski, and Bruce Caldwell.

Which thinker was most influential for you during your early formative years, during graduate studies generally? Was there any one thinker that really marked you?

That is a very hard question to answer! I suppose I would single out Thomas Kuhn, whom I had the privilege of meeting and talking to as a graduate student, and then much later as a fellow at MIT in 1995. I definitely preferred his work to Popper or Lakatos. I also learned a great deal from the work of Amartya Sen and Ian Hacking. Ian had arrived at the University of Toronto in the fall of 1983, when I defended my thesis, and he was part of the examining committee. D. McCloskey came to speak at Toronto in 1983, on the rhetoric of economics and so did Stephen Toulmin on his evolutionary epistemology. Both talks were influential and there were subsequent interactions in the years ahead.

Your doctoral thesis was on a key neoclassical revolutionary, William Stanley Jevons. This work culminated in your first monograph,* A world ruled by number *(Schabas 1990). Can you describe the main thesis of this work? What factors drew you to work on Jevons in the first place?

Well, there were no books on Jevons at that time (now there are four), but the leading Jevons scholar in the 1970s, R. D. C. Black, had just finished issuing the seven volumes of the Jevons papers and correspondence. I had also worked in the Jevons archives in the John Rylands Library at Manchester and the British Library, but Black's volumes proved invaluable. By chance, I also found a large collection of Jevons letters at Seton Hall University in New Jersey; letters that Black knew existed but had not been able to find in time for his publications.

I was drawn to Jevons because he was a contributor to logic, philosophy of science and economics, and because he was the instigator of a revolution that had Kuhnian overtones. He also published in the natural sciences, meteorology, fluid mechanics, biology, chemistry, even music theory. Before I started to work on Jevons I had assumed that he had simply tried to dress economics up as a mathematical science but I came away with a completely different appreciation and came to realize that he actually had done something quite profound. Jevons via his work in logic had tried to understand the essence of mathematical reasoning and was one of the first logicists. This means that he tried to reduce mathematical core concepts to pure logic, a position made more famous by Bertrand Russell among others. Jevons influenced many of the logicians of the late nineteenth, early twentieth century, including John Venn and Gottlob Frege. Similarly, Jevons's lengthy work, *Principles*

of science, built on the work of John Hershel. Both were fallibilitists with a strong appreciation for the role of probabilistic thinking in science.

Over time, I came to realize that Jevons was a much richer and more original thinker than I had expected, and that there was not any dose of insincerity to his efforts to see that economics, as he put it, must be necessarily mathematical. I came to believe that his argument had some merit insofar as the phenomena of economics are numerical and thus as intrinsically mathematical as those in physics. Take the interest rate, for example; it is given to us as number and does not require any mapping as would be the case in say astronomy (mapping light points using spherical trigonometry). The title of my book comes from Jevons, that as a neo-Pythagorean he truly believed that "the world is ruled by number". Not only is the world of the economist replete with numerical facts, prices and quantities exchanged in the market place, but for Jevons even our individual minds, in making the decisions in the marketplace, are essentially doing the calculus.

Jevons hoped to make economics more scientific but he was quite aware that certainty eludes our grasp. He was not just thinking "oh, physics is this wonderful science and I am going to make economics like physics" because he did not think physics was the wonderful science! He could see, as Hershel did, that our ability to know the physical world is significantly limited.

In your more recent book entitled,* The natural origins of economics *(Schabas 2005), you argue that such fundamental economic thinkers as Adam Smith and François Quesnay did not view human economic activity as located "outside" of physical nature and that only gradually did economics come to be denaturalized. What exactly do you mean by this?

Well, thank you for that. That is a nice rendition. First let me say that the word 'nature' can mean virtually whatever one wants, and so one has to place it in a historical context. And second, the process of denaturalization has not been completed. There are still ways in which the discourse of economics overlaps with and draws upon our understanding of natural phenomena. But the early modern economists did not see their phenomena as distinct from the phenomena of what they took to be the natural realm. There was no clear sense in which there was a distinct realm called 'the economy'.

Quesnay exemplifies this point of view. His *tableau* depicts a flow of material goods from one sector to another, without any deliberation. Wealth grows entirely through the gifts of nature and we are part of this natural order. David Hume's specie-flow mechanism is similar in that humans are part of the mechanism but are as governed by natural laws as the tides of the ocean.

And then, towards the middle of the 19th century, economics underwent what you describe as a denaturalization process, largely at the hands of John Stuart Mill. Can you describe this process?

Mill still wants to say that political economy is half physical and half mental. And I am sure that if you pushed an arch-rationalist, even an Austrian, they would have to say that there is some physical description to economic phenomena. But for neoclassical economists, everything stems from individual minds, from utility maximization, and in that sense is set apart from physical nature. The marketplace was redefined as information, not a physically located institution. Individuals have different predilections for consumer goods, risk, and time and, precisely because no two of us are alike, that gives rise to economic phenomena. That approach was not prevalent in the 18th and early 19th centuries—then theory was oriented around three classes with little to no differentiation internal to each class. The strong methodological holism fit well with their commitment to inexorable laws in the economy, the laws of Malthus or Ricardo.

Economists continue to borrow methods and metaphors from the natural sciences. Phil Mirowski is exactly right in saying that early neoclassical economists adopted techniques from thermodynamics. It is not that they stopped drawing on science for inspiration, but that they conceived of the phenomena differently, as the product of human deliberation.

Right, so human agency became the proximate cause of economic phenomena. But surely this is a surprising thesis given that Mill, in his* Principles of political economy*, explicitly recognizes nature's agency. How do you square this circle?

Well, I think Mill is the pivotal figure insofar as he urges readers to see the mental origin of some economic laws, but he also emphasizes the powers of nature to produce our goods. For Mill and the earlier classical economists, no one individual decides to promote the increase in the

rent, defined as a return to the natural attributes of the soil. Rents arise in a stylized and law-like fashion that is perceived to be beyond human control. Population growth is grounded in natural passions and entails diminishing returns in the agrarian sector as we cultivate, necessarily, inferior grades of land. Wages rise and profits fall. The iron law of wages best captures the sense in which human agency is impotent in comparison to what nature delivers. When you get to the neoclassical economists, however, particularly by the 1930s with the recognition of macroeconomics as a separate pursuit, one finds a strong belief that we can engineer the economy, that we are not limited by scarcity or human frailties (passions). Ricardo and Malthus sound extremely odd to our twenty-first century sensibility. Now we can use monetary easing or corporate tax cuts to solve almost any problem, or so we are told.

For the future of economics, do you envision the discipline re-establishing itself "in nature"? If so, what would this mean exactly, especially how economics might relate to the natural sciences and life sciences, such as biology and ecology?

It is hard for me to say what the future will hold but I do sense that economists are taking global warming very seriously and this has drawn them all the more to accounts in ecology and the life sciences more generally.

In the early 1990s you wrote on the history of economics as history of science. Have you changed your position?

I hold onto that same position to this day, and wrote about it again in 2002. I was invited to write on that topic by Roy Weintraub for a symposium in the Spring 1992 issue of *HOPE*. Roy asked me because I was one of the only people in the history of economics practising in a history of science department and trained in that field—in HPS. But that said, I never wanted to say that we should not do some history of economics in economics departments. I just wanted to point out that the field was not really growing and that if anything the field was losing ground in economics departments. It is prudent to think about a different patron or institutional setting, mainly history and philosophy of science or sciences studies. There is much more interest in economics within that community, say with the work of Donald Mackenzie or Nancy Cartwright. The volume that Neil De Marchi and I put out on *Oeconomies in the age of Newton* (2003) helped to situate the history

of economics within the history of science, as did Mirowski's *Natural images in economic thought* (1994). I still believe that it is better, all things being equal, to do the history, philosophy, and sociology of economics as just one science among many, that is, within the broader rubric of science studies. But I also want mainstream economists to read and take our courses, just as biologists should study the history and philosophy of biology. I think we would have more impact on economists if we were more detached from them rather than seeking their direct approval, not to mention beholden to their budget constraints.

Might there be something distinctively valuable about the work of a historian of economics working from within an economics department?

I think to do the history of economics well one has to have some good training in economics and it would be ideal if one continues to interact with economists in seminars, colloquiums, and conferences. Those who contribute well to the history and the philosophy of biology, for example, tend to interact with biologists. They tend to get to know them, go to their labs and keep up with the latest research, but are housed in separate departments of HPS or STS. This is all for the good. But it seems obvious, at least in North America, that economists have lost interest in the history of economics. I think we could rekindle an interest if we first gain some distance and cultivate ourselves more fully, if we are less beholden to the disciplinary norms that govern the profession of economics. He who pays the piper calls the tune.

The history of psychology is a good example. It used to be done by retired psychologists and was not, for the most part, done well. Those who finally shaped the field did so by gaining autonomy, within departments of the history of science, for example Robert Richards and Jan Goldstein at Chicago, or Anne Harrington at Harvard. Now most of the top programs in science studies offer the history of psychology and find it benefits from attending to intersections with the history of physics or biology. It is a field that has truly matured. The history of mathematics, by contrast, has not. It still tends to be done by retiring mathematicians in math departments and done poorly. The few historians of science who specialize in the field are always bemoaning the fact that their subject is neglected.

I am delighted that two of the most influential scholars of my generation, Phil Mirowski and Mary Morgan, fit this description. Both have become established names in HPS or science studies, precisely because they have crossed over into those worlds, publishing in the key journals and speaking at the annual meetings. Mirowski now holds a cross-appointment in the HPS program at Notre Dame, while Morgan works closely with HPS scholars at the LSE and abroad.

I understand that you are now working on David Hume's economics. When most people think of the history of economics, they immediately think of Adam Smith and not his best friend, David Hume. Why is this? What is Humean economics (if I can use such a term)?

Well, it would be hard to answer any of this in a short amount of time; that is why I need a book! Hume is very well-known in the history of monetary thought and as a proponent of the moral sciences, an eighteenth-century term that roughly corresponds to our social sciences. I would not say that Hume is ignored among the cognoscenti but there is no question that Smith is the best known economist outside academia, and that *The wealth of nations* is seen as equivalent to, say, Newton's *Principia*.

Hume wrote a number of essays on economics and he developed many rich insights about property and money in his main philosophical texts. I hope to show that there is a thread of economic thinking all the way through Hume's publications and correspondence. He is very interested in economic issues, fiscal and trade policy, money and banking. He interacts and corresponds with most of the leading economists at the time. But probably his most important contribution is his understanding of the epistemological limits of what he called the science of commerce. He believed that we have a better grasp of our ignorance in economics than we do in physics and, in that sense, economics may be superior to physics. In both fields, of course, he emphasizes how little we know and how fragile that knowledge truly is.

One thing that always struck me with Hume's economics is his definition of wealth. The wealth of any nation consists in the people and commodities that constitute that nation.

Yes, he was keen to lift the veil of money and look at the physical properties of wealth. And he was very cosmopolitan in his thinking,

placing weight on the migration of economic opportunities around the globe. Indeed, he believed there was a global justice to how wealth ebbed and flowed from one nation to the next. And, interestingly, he was not only as aware as Smith that America would be the next economic hegemony, but conjectured that China might one day surpass America.

Michel Foucault (1960) and Keith Tribe (1978) have argued that the theoretical concept of "the economy" is relatively new—having gained traction only over the last two hundred years. You (2009) recently argued for a similar thesis. Are there any methodological consequences that arise when we recognize that the economy is a mind-dependent theoretical entity?

Well, when you construct something in theory then there is always the question of whether or not you have captured the physical dimensions accurately. So, in physics one could argue that the electro-magnetic field of Maxwell's equations is just constructed on paper. The same thing is true for the economy. It is a theoretical construction made up of such leading indicators as the money supply, population, interest rate, consumer price index, gross national product, and so on. We have to stitch all of these things together and then somehow we create this economy, but it is really on paper. Furthermore, most of our indicators can only be measured with a temporal lag and slightly imperfect tools. This means we need to attend to methods and recognize what is merely conventional.

You are now embarking on a new research project on the history and philosophy of bioeconomics. What have you discovered so far and what do you hope to accomplish with your project?

The journal *Bioeconomics* is only a little more than a decade old, but conceptual and methodological trade between economic and biological discourse, to speak anachronistically, reaches back to at least the 17th century. There is also reason to believe that both are trying to make sense of how life is produced and reproduced, distributed, and so forth. In the early modern period, the most common term was the "*Oeconomy* of nature", and it included all life forms and even the earth's atmosphere and crust. Evolutionary thinking took hold in both

discourses, starting in the mid-18th century. I would like to revisit this and see to what extent both disciplines make use implicitly of the same predilections, for efficiency for example. Darwin is replete with economic metaphors and natural selection itself is a mechanism that could be construed as bringing efficiency to the distribution of life forms. Bioeconomists at present are interested in understanding non-human animals in economic terms, or understanding the biological constraints of economic processes. These are interesting pursuits that might blend with ecology and environmental science. For now, I am trying to write more articles before getting to a book.

As I understand it, the conceptual trade between economics and biology is a significant theme in this project. Are such exchanges between these sciences surprising?

Most of science feeds on analogical trade. Besides what I just said about Darwin, other examples from that period are Milne-Edwards's use of the idea of division of labour in physiology, or Marx's use of reproduction in concepts of capital. If economics is defined as the allocation of scarce resources among alternative ends, or as making the best of things, I think you can see that it is an open book as to how you configure those resources or those ends.

Let me switch gears for a moment. What, to your mind, is the purpose of doing the philosophy of economics?

Well, I think the general rule in the philosophy of science is that many scientific practitioners, with a few exceptions, cannot take the time to look at the foundational issues regarding their science, for all the reasons advanced by Kuhn among others. Why do I make these assumptions? Why do I use the particular methods? Philosophers of economics do have the time and training to ask these questions—to ask them in light of what has developed in philosophy more generally. They can also step away and assess the extent to which economic discourse is advancing or retreating on specific topics, or even recycling old ideas. Those making up economic theory and practice do not always have the time or tools to do this, although there are some exceptions, Amartya Sen, for example.

Today, philosophers of economics and methodologists are wont to say that their work should be practically relevant for economists. What is your position on this matter? Are philosophers of economics Lockean under-labourers of some kind or merely a special breed of reflective economists?

Well, I think you could be either. But again, going back to my breaking away idea, I believe that it would be good for us to be neither, to develop our discipline of the history and philosophy of economics independently of the discipline of economics. We might end up with results that would have more impact in the long run. As long as we write for economists, we will try to copy their methods (i.e., overuse mathematical models) or employ their concepts, and do ourselves disservice. I realize this sounds idealistic, because we need more resources to gain this autonomy, but it is still good to spell out that end as an ideal to aim for over time. I would still hope that the work done in the history and philosophy of economics would result in a better economics.

So have you contradicted yourself? Do you in fact share the same goal as those who would be against "breaking way"?

Well, we always want to influence the world, of course, and there is reason to think that economists have not done the best job of giving us a world of full employment or reducing significant inequality of wealth or income, for example. Maybe they should not be held accountable! But if we hope to make a better world, one possible path is to understand better how it works. That said, doing history for its own sake, or in my case, the history and philosophy of economics for its own sake, is a possible way to cultivate a kind of wisdom and a set of insights that can be used over time. I think the indirect method is better; certainly direct methods to hit economists on the head have not worked. The philosophy of biology, for example, has come unto its own in the last 20-25 years. It has grown dramatically and is mostly done by people who are not practicing biologists. And I think it really has had an impact on biology, on deep questions pertaining to the process of speciation, for example. We have to separate ourselves off and do our field well in accordance with our own disciplinary standards. We cannot predict what the results will be, but my hope is that they will have more of an effect than if we are just trying to chase after and conform to the latest fashion in economics.

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Review of Mark D. White's *The manipulation of choice: ethics and libertarian paternalism*. New York: Palgrave Macmillan, 2013, 208 pp.

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Mark White's book is a critique of the theory of libertarian paternalism popularized by Cass Sunstein and Richard Thaler's *Nudge: improving decisions about health, wealth and happiness* (2009). Sunstein and Thaler propose policymakers improve the well being of citizens by 'nudging' us to make better choices in our day-to-day lives. Take the American example of employee enrolment in 401 (k) pension plan accounts. Sunstein and Thaler argue that changing the default option to automatic enrolment would be in the interests of the majority of employees who procrastinate about investing in their retirement, while still allowing them to opt out if they so choose. Such policies are considered *libertarian* because they do not limit people's choices, and yet also *paternalistic*, because choices are framed in such a way that the choice maker will be nudged to make the decision the 'choice architect' believes is the right one.

White argues against libertarian paternalism in both theory (chapters 1-3) and practice (chapters 4-6). The first part critically analyses the theories that underpin libertarian paternalism: traditional choice models, behavioural economics, and law and economics. White's original contribution here is an analysis of the importance of principles, judgement and the will in coordinating human interests. The second part focuses on libertarian paternalism in practice with chapters making a case against it from an informational and ethical perspective and a chapter analysing the distinctions in practice between private and government nudges.

In the opening chapters of the book, White explores whether traditional economic models properly account for how individuals make choices, since it is on these models that the choice architecture of nudges is based. The first problem White identifies is that the traditional assumptions of preferences, constraints, and trade-offs are overly simplistic: they do not account for the variety of interacting

aspects that make up human interests. There are three features that traditional choice models neglect: principles, judgement, and the will.

First, traditional models overlook moral principles, which can place binding constraints on human action. The principles we endorse become part of our character and identity, producing both important and consistent influences on our choices. White explains that principles are not easily substituted for preferences which make them difficult to fit into the traditional models. Principles, for instance, have the "property of limiting our discretion to make different trade offs among preferences when circumstances change" (p. 9). White gives the example of a couple who refuse to buy a car made by a particular company because they disapprove of its business practices, regardless of how low a price they are offered.

Traditional models suppose that the economic actor is concerned only with *utility maximization*, i.e., with achieving the greatest possible satisfaction of her preference ordering given her resource constraints. White provides examples to illustrate how even the simplest consumer choices are more complex than this recognises because people routinely face conflict between their preferences and principles. This brings out the crucial role of judgement in the human decision-making process. As White puts it, "we must utilize our judgement to balance conflicting principles and arrive at an answer that maintains our moral character" (p. 14).

Finally, whether we act on our judgement or not requires the use of willpower. An individual's strength of will determines if they are able to follow through on what they have decided is best. As White argues:

> Willpower is the necessary bridge between making a decision and acting on it […] But very few economists recognize the existence of a will that either carries out the decisions that our judgement tells us is best, or leads to another action altogether (p. 18).

The second problem White identifies with traditional models is that economists view preference satisfaction as equivalent to 'well-being'. This is problematic because many of our preferences are not consistent with improving our well being, defined in the general sense of a life that is going well. (Sunstein and Thaler themselves define well-being more objectively as health and wealth.) People have a wide range of motivations for their choices, such as: other-regarding concerns, self destructive desires, moral principles, and general social ideals, which do

not always further their well-being in this general sense. In solely focusing on the satisfaction of preferences economists ignore how individuals make decisions.

In the next chapter, White extends his critique to behavioural economics, which elaborates on the assumptions of traditional choice models. For instance, Amos Tversky and Daniel Kahneman added cognitive biases and heuristics to economic models of choice. Unfortunately, while behavioural economics expands the scope of analysis—in particular accepting that people can choose badly—it does not address White's critique of traditional models. It does not recognize the role of principles, judgment, and the will as determining factors. White illustrates this point through the example of Patrick, an overweight man who eats a muffin every Sunday morning at a cafe. From the perspective of the 'choice architect', Patrick is making an irrational choice because he is contributing to his poor health. But Patrick could have many interests which eating the muffin fits well within, as White explains (perhaps his now deceased grandfather took him to the cafe every Sunday, and he eats a muffin in remembrance of the lessons and experiences they shared). Yet all the behavioural economist perceives is an overweight man eating a muffin. This introduces White's two major problems with libertarian paternalism. First, there is no way for an outside observer to know what a person's interests are; and second, even if they were known it would still be illegitimate for a policy maker to try and nudge her choice.

White begins his direct challenge to libertarian paternalism in the fourth chapter by reaffirming his original claim—that the sum of human interests is far more complex than these models allow. This poses an informational problem for libertarian paternalism: it claims to do what is in the best interests of the citizenry, but there is no way for an outside observer to know a person's best interests or what motivates their choices unless that is made explicit. The only glimpse we get of people's different interests is from their choices, which libertarian paternalists desire to alter. While claiming to do what is in our best interest, policymakers actually nudge people into acting in what they believe people's interests ought to be, substituting their values for our own.

White refers to the work of a number of influential philosophers. He quotes Gerald Dworkin's definition of paternalism: "a usurpation of decision-making, either by preventing people from doing what they have

decided or by interfering with the way in which they arrive at their decisions". White also refers to John Stuart Mill's 'harm principle' as a guide to the limits of legitimate interference with individual action, and to Immanuel Kant's view that deception and coercion are the two prominent ways that an individual's autonomy can be compromised. White's ethical case against libertarian paternalism derives from the insights of these philosophers, focused into two critiques.

His first critique, which he shares with Riccardo Rebonato (author of *Taking liberties: a critical examination of libertarian paternalism*, 2012), is that nudges are not value free, and that value substitution as a governing philosophy violates people's autonomy and the respect that is due by projecting one person's interests onto another. It violates a person's autonomy by directly interfering, albeit 'softly', in their choices.

White's second critique is that libertarian paternalism manipulates the cognitive biases and heuristics that behavioural economists identified. Here White makes a distinction between *soft paternalism* and *hard paternalism*. Hard paternalism, such as taxes and legal prohibitions, use state power to directly alter our behaviour; soft paternalism, such as libertarian paternalist nudges, wields power secretly, outside our awareness. Hard paternalism is immediately evident because it affects the constraints on our decision making. In contrast, libertarian paternalism manipulates the cognitive biases and heuristics that affect our decision-making process itself, and thus generally goes unnoticed.

This critique of nudges, and behavioural economics in general, in terms of secretive manipulation is a strong one, since it strikes at the root of the idea of nudges as a solution to poor choice making. It is shared by other sceptics of libertarian paternalism, such as Gilles Saint-Paul in his book *The tyranny of utility: behavioral social science and the rise of paternalism* (2011). White develops the point by connecting it with the learning process. Nudges exasperate the bad choice habits they claim to fix by latching onto the very same cognitive biases and heuristics that produce our objectionable choices to begin with. It is through the negative consequences of our choices that we learn how to make better ones. If choices are manipulated, this learning process is weakened or removed. Predetermined government correction to possible mistakes disrupts the environmental feedback that generates learning, and does not help agents improve their choice making in the future.

White goes on to deal with the ethical distinction between business nudges to manipulate customers and government nudges to manipulate citizens. His reasoning is that it comes down to purpose and respect. Businesses' interests are transparent and single minded—to maximize profit—and so, "they do not presume to make choices for their customers in their own interests" (p. 109). As customers we are aware of our relationship to businesses and can respond appropriately. Moreover, businesses exercise no coercive power over customers because we are always free to exit the relationship (p. 107). Yet, Dworkin's definition of paternalism, which White endorsed, would seem to cover any power, private or governmental, "interfering with the way in which [people] arrive at their decisions". This creates an inconsistency in the way paternalism is defined in the book.

Thaler and Sunstein argue that what separates libertarian paternalism from traditional ('hard') paternalism is precisely its non-coerciveness, since 'choice architects' do not prohibit choices but rather frame them in specific ways. An important counter argument to White is that, in a great many cases, a choice about framing does have to be made one way or another. The ethical question, Sunstein and Thaler argue, is not whether to become a 'choice architect', but how one should exercise that power responsibly (Thaler and Sunstein 2003, 175). Shopkeepers, school teachers, parents, politicians, and many others all face this ethical challenge. White deals with this through a framework that distinguishes between those close to us, such as friends and family, who may legitimately attempt to influence our choices because of their greater knowledge of our interests and closer connection to the consequences of our choices, in contrast to government agencies which cannot know us as individuals and which therefore end up manipulating us in ways that disrespect our interests. Thus,

> If we understand respect to be the attitude required of everybody based on our shared humanity, and care to be an appropriate attitude only for people who are close to each other and have some idea of each other's interests, then we can see the problem with paternalism (p. 117).

While I can go along with White's distinction, it could do with further explanation of how it fits with the philosophical analysis of paternalism that he cited and endorsed earlier in the book (i.e., Dworkin, Mill, and Kant). The chapter goes on to explore possible exceptions to the

general rule: acceptable government nudges (e.g., food labelling) and unacceptable business nudges (e.g., default retirement plan enrolment). It ends by returning to previous points on value substitution and distortions in learning feedback, which works well at cementing earlier points but left this reader feeling that the new concepts White introduced could have been more thoroughly explained.

The work is a solid, compelling read for anyone interested in a concise but comprehensive account of the case against libertarian paternalism and its theoretical foundations. The book is well organized: each chapter focuses on a distinct issue and this complemented by overlapping discussion of earlier points throughout, emphasising the interconnectedness of libertarian paternalism's many nuances. An excellent feature of the book is White's use of many relatable and entertaining examples which can connect with any reader, whether or not they come from an economics background. The book stands out from other recent critiques of libertarian paternalism (such as I have cited). It retains the strongest points of earlier critiques while also offering additional contributions, as noted in this review. In the course of battling libertarian paternalism and its underlying theories, White simultaneously builds a positive case for individual freedom in defence of more traditional, non-paternalistic paradigms of libertarian philosophy and economics.

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Review of *Philosophy of economics (Handbook of the Philosophy of Science, Volume 13)* edited by Uskali Mäki. North Holland: Elsevier, 2012, 902 pp.

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The primary intended audience for this Handbook is philosophers who might be enticed to consider economics as a subject for analysis. As the editor says, "Economics has characteristics that make it a particularly inviting target and playground for philosophical argument and analysis" (p. xiii). He goes on to say that a "possible source of philosophical reflection and debate is the emergence of new theories or research techniques that challenge more established ways of doing economics" and that "recently, the initiatives of experimental, behavioural and neuroeconomics have launched methodological debate and research, with philosophical arguments designed and used either to justify the new approaches or to question them" (p. xiv).

To accomplish his task, the editor has divided the Handbook into two separate parts. Part A is a collection of papers by authors who are well versed in the philosophy of economics, some of whom are also familiar with the methodology of economics (I separate these because they are not the same—for example, one can study the methodology of economics without ever discussing the philosophy of economics). Part B consists of some papers by practicing economists who are willing to consider a philosophical aspect to their field of expertise, and some papers by philosophers who have an interest in economics. Part A seems to be pleading with philosophers to take an interest in economics, as philosophers of science are interested in say physics. Part B seems to be directed at demonstrating how the philosophy of economics can be done.

Despite the intended philosophical audience, there are many good papers in this volume that are worth reading by ordinary economists without an overt interest in the philosophy of economics. Of course, I am one of that type of reader and I will here be considering it from this perspective. But before I do that, let me summarise what can be found in the two parts.

Part A is identified as "General philosophical themes" and Part B is identified as "Specific methods, theories, approaches, paradigms, schools, traditions". The "philosophical themes" of Part A include such issues as realism (a topic that few economists ever talk about), causation (a topic most economists take for granted), models versus theories (a topic that would not be understood by the younger generations of economics [see Boland forthcoming]), naturalism (usually a question about whether economics can be considered a science like physics—a topic few economists find interesting) and the associated nature of economic explanations (a topic that economists should be interested in but very few are), the role of mathematics (a topic few if any economists find interesting), feminist philosophy (a topic that the male-dominated economics academic community should be interested in but few males are), the old positive versus normative dichotomy (a hot topic among a few methodologists today, but not well understood by practicing economists), economics as ideology (another go at the topic of whether economics can be considered a science), and the role of experimentation (still another attempt to deal with the topic of the extent that economics can be scientific). As my parenthetical comments indicate, I think few economists would find a need to consider what is discussed in Part A. But the editor is probably right that many philosophers of science might.

The papers in Part B are easier for those trained in economics, such as I am. It begins with two practicing economists talking directly about "The philosophy of economic forecasting", and the "Philosophy of econometrics". A later paper, similarly, discusses the "Philosophy of game theory". Not all of the authors in the second part are practicing economists, although three have two PhDs, whereby they started with a philosophy PhD and finished with one in economics. Some are philosophers of science outright and I guess are included to demonstrate how to do proper philosophy of economics.

If the book's intended purpose lies in interesting philosophers of science to consider looking closer at economics, I am not sure the chapters by practicing economists who are willing to consider philosophical aspects of their sub-discipline will convince them. (Although, should the economists in this part do a poor job, it might convince some philosophers to try to do it better.) The reason is that most, if not all, of the practicing economists included here are not really talking about philosophy of science (beyond their introductory

observations), but are instead talking about topics of interest to *methodologists* of economics.

It is true that some philosophers think that any talk of methodology is inherently philosophical. But, as I noted already, one can talk about methodology without ever engaging in philosophical analysis. D. McCloskey (1985, 159-162) made this point thirty years ago by distinguishing between 'big-M' methodology that involves questions of interest to philosophers and 'small-m' methodology that involves questions of interest to practicing economists (usually about model building methods).

The only 'philosophical themes' discussed in Part A that are explicitly discussed by the authors in Part B are causality (briefly), the nature of explanations and the nature and use of models (versus theories). Almost all of the authors in Part B are talking mostly about small-m methodological questions with little explicit reference to philosophy. There is only one chapter in Part B that includes any significant discussion of the views of philosophers of science, though two chapters give a prominent role to the views of Karl Popper (which seems clearly to contradict the argument of Chapter 2, that Popper's views are no longer relevant to economics). It is left to some of the philosophers of science to indulge in examining non-conventional (i.e., non-neoclassical) models of economic behaviour—this seems intended to indicate to other philosophers of science that there are interesting (i.e., non-stale) questions outside of neoclassical economics that might be worthy of philosophical analysis. That includes a couple of papers that have less to do with discussing either the philosophy or the methodology of economics and more to do with the methodology of political science or sociology.

Now, I have always been suspicious of 'Handbooks of' but this one does have many chapters that many *economists* should find interesting and useful in their research. Whether this volume will be successful in its intended goal of attracting philosophers to the study of the philosophy of economics I am not so sure. Not because of any lack of quality in the included chapters (all seem good at what they do), but because of a major cultural gap between how philosophers and non-philosophers view research activity in social and natural science: the big-M versus small-m views of what matters in methodological research which distinguishes the interests of the philosophers (Part A) from the interests of economists (Part B). It is difficult to see that many

would-be new philosophers of economics would find much of interest in the small-m methodology discussed in Part B, particularly given the general absence in Part B of big-M methodology discussions that might be of interest to philosophers.

It is unfortunate that philosophers promoting the philosophy of economics too often see any discussion of the cultural gap as an attempt to reinforce disciplinary biases and divisions. But the gap is real, as we see in this volume's separation between Parts A and B, where the 'philosophical themes' which philosophers think are essential are hardly mentioned let alone discussed in Part B. Hopefully this book will be successful in attracting some new philosophers of science to the study of economic methodology, but that is only first step. Unless these philosophers want to be accused of what some might call "philosophical imperialism", they need to recognize the "big-M versus small-m" cultural gap that McCloskey was warning them about 30 years ago and that is well illustrated in this book. If one's interest in methodology stems from seeing a need for helping practicing economists with their small-m problems, particularly those problems with roots in philosophy, then the challenge is to discuss methodology in such a way that practicing economists will pay attention and maybe even learn something about philosophy of science. And, if one's interest in methodology stems from seeing interesting philosophical problems in the work of economists, then the challenge is to discuss big-M methodology in a way that contributes to the philosophy of science.

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Foundations or Bridges? A review of J. E. King's *The microfoundations delusion: metaphor and dogma in the history of macroeconomics*. Edward Elgar, 2012, 304 pp.

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J. E. King has written a timely book. The dominant mainstream of the economics profession is deeply committed to the notion that macroeconomics requires microfoundations; yet there has not been a careful book-length examination of this dogma since Maarten Janssen's (1993) methodological and James Hartley's (1999) more historical accounts. King's book is valuable, as it surveys both the microfoundations dogma itself and the methodological and philosophical accounts of reductionism and methodological individualism that are often thought to ground it. Its strength lies in the fact that King has read very widely and provides usable capsule summaries of a huge range of views of economists, philosophers, and social scientists facing cognate issues in their own disciplines.

I

Although foreshadowed earlier, a clear distinction between *microeconomics* and *macroeconomics* is a product of the 1930s. The terms were coined by Ragnar Frisch and spread through the newly formed Econometric Society. The relationship of John Maynard Keynes to this distinction—Keynes in fact used neither term—is more equivocal than the folk wisdom of economists would suggest. The nature of the relationship between micro- and macroeconomics was considered to be a critical issue from the outset, having been discussed by Frisch (1933) himself. (Frisch argued that a successful microanalysis must be grounded in a background macro analysis.) Leontief (1936) attacked Keynes's *General theory of employment, interest, and money* (1936) on the ground that his aggregate analysis violated basic principles of microeconomics.

There is considerable common ground among economists. No one really denies that the economy is a collection of individuals and that macroeconomics uses categories and relations that are not, on their

face, about particular individuals. The question about the relationship of macro- to microeconomics is, then, immediate.

One frequent answer that appeals to many economists is that microeconomics underpins, lies behind or below macroeconomics and that it accounts for the properties, limitations, and success of macroeconomics and, even, that microeconomics may be adequate on its own—that is, macroeconomics may be dispensable or even illegitimate. This is the microfoundational impulse.

A second alternative is to assert than macroeconomics is autonomous from microeconomics—though whether this is a conceptual, ontological, epistemological, explanatory, or pragmatic autonomy is a point of contention. Some hold that the macroeconomic may influence the microeconomic, which amounts to a denial or at least to a tempering of the microfoundational impulse.

A third possibility is that micro- and macroeconomics are relatively independent and that, while the connection between them is interesting, it is not scientifically essential.

King is clearly opposed to the first position and somewhat straddles the second and third. There is no pose of historical neutrality. His book is a systematic attack on the first view:

> the microfoundations dogma has had, and continues to have, a large and pernicious effect: directly on academic economics, and indirectly on economic policy and public discourse on economic issues (p. 10).

I am sympathetic. A large part of my professional life has been devoted to considering the history and the methodological issues surrounding the microfoundations of macroeconomics, and I typically come down close to King's position. But although I doubt that historians of economics can or should maintain intellectual neutrality, it is a virtue to strive for interpretive charity: try to grasp the best case for views that we oppose and let normative judgments arise from the interaction of our inevitably non-neutral perspective with a fair-minded and even-handed presentation of the facts and arguments, rather than dispensing labels (good guys/bad guys, insightful/benighted, consistent/inconsistent) according to whether or not historical characters conform to our own views. We cannot help making *ex post* evaluations, but the best history is not about those evaluations but

about understanding how and why people believed and acted as they did and how we got to where are now.

II

Although King's survey reaches back into the 1930s, he concludes that microfoundations is a product of the 1970s, first reaching its full expression in the real-business-cycle models of the early 1980s. He identifies microfoundations with the representative-agent rational-expectations model. The core of his book is an historical—or, at least, chronological—account of how that approach came to dominate macroeconomics and a case for why the approach itself is wrong.

King's argument begins with a methodological or philosophical treatment of the issue. He argues that advocates of microfoundations maintain a *reductionist thesis* that ignores the fallacy of composition—i.e., it ignores that the whole is greater than the sum of its parts. Second, he maintains that macroeconomics conditions microeconomics—that is, there is *downward causation*. "Downward" is to King really a misnomer, since (note the subtitle of the book) he sees history as guided by a poor metaphor: *foundations* imply microeconomics is more basic than macroeconomics. In contrast, King sees the relationship more as a bridge, a buttress, stepping stones, or some other horizontal, rather than vertical, relationship.

King is an historian, not a philosopher. His admirable canvass of a variety of relevant philosophical issues (e.g., reductionism, methodological individualism, the role of metaphor in science) does not amount to a compelling analytical account. Rather he simply endorses the views that support his starting position. He maintains that anyone who acknowledges emergence or the fallacy of composition, for example, opposes—or should oppose, if consistent—methodological individualism and microfoundations.

Treating the fallacy of composition, as King does, as equivalent to the whole being greater than the sum of its parts leaves unanalyzed just what the operation of summing amounts to. No reductionist denies (a) that a car is not just the mereological sum of its parts, but must be assembled correctly to be a car, and therefore (b) that distinctive car properties are fully explained, and ontologically depend on, the parts *and* their relationships. As a result, (i) the emergence of car properties is not mysterious or ineffable but predictable, and (ii) the car's

characteristics, as opposed to the parts and their relationships, are at least in principle dispensable.

There are varieties of anti-reductionists. None needs to deny that it is always legitimate to ask what explains a macro-phenomenon nor that *sometimes* wholes can be explained by analysis into parts and the relationships among them. If reductionists are too enthusiastic in elevating and projecting successful reductive explanations into fundamental ontological and epistemological claims that outstrip their actual practical explanatory achievements, the anti-reductionist need not fear that any successful reductive explanation is a step on the slippery slope that entails commitment to reduction in principle to individual agents (or subatomic particles!).

The anti-reductionist wants at least to keep the door open for unpredictable emergence. King's all or nothing approach—to admit a fallacy of composition is to reject reductionism—is hard to sustain with his own concrete examples. Keynes's income multiplier is held up as the paradigm of the fallacy of composition. Yet, nominal GDP is, as an accounting fact, the sum of individual incomes, and the multiplier process can be explained step by step completely in terms of the behavior of individuals, only adding up the incomes in the end to get aggregate GDP. The fallacy of composition is that the attempt of an individual to save, which *ceteris paribus* would increase his own savings, will not result in net aggregate savings if *all* individuals try to save simultaneously. There is no anti-reductionist mystery here: we can trace out the process individual by individual, and the aggregate result is predictable on that basis.

III

King's emphasis on the metaphor of foundations (e.g., the foundations of a building on which the higher floors are built) and on the importance of metaphor in science (more asserted than demonstrated or analyzed) forces him into a narrow historiographical box. The problem is that he adopts—if the term is not too oxymoronic—a "metaphorical literalism". Only those who view microeconomics as foundational in the sense that macroeconomics is supposed to be fully derivable from microeconomics and that macroeconomics is completely dispensable count as endorsing the metaphor of foundations and thus as supporting a microfoundational program. It is for this reason that King sees true microfoundations as entering macroeconomics only in the mid-1970s.

But metaphors are figures of speech, not rigid logical or conceptual templates. They are often taken loosely, incompletely, and inconsistently—that is the nature of all figures of speech. Even technical terms and concepts do not have absolutely precise meanings that remain stable among individuals or over time. Inconsistency mongers never want for trade.

My own preference is to define microfoundations—as I believe the economics profession broadly does—as involved whenever microeconomics is held to be ontologically, epistemologically, or explanatorily more basic than macroeconomics, such that persuasive, sound, reliable, robust macroeconomics must make explicit reference to microeconomics. Taken this way, microfoundations has existed since the 1930s, in practice if not in name.

To see the difference, note that King rejects the view that Lawrence Klein was a true microfoundationalist in the 1940s, because he wanted to treat both microeconomics and macroeconomics as valid approaches and to establish a consistent connection between them. King sees this as a horizontal relationship as opposed to the vertical one implied in the foundationalist metaphor. But I see Klein as having an explicit microfoundational program, since in his *Keynesian revolution* (1947) he expresses grave doubts as to whether Keynes's macroeconomics can be secure or empirically useful without establishing its connection to microeconomics. Later he adopts the position that we should *never* stop looking for reductionist explanations of aggregates. His preferred methodology is thus one of disaggregating and adding to the complexity of models as far as data allow and of getting better data as soon as possible.

On my view, microfoundations has been a central issue in macroeconomics from an early date, and there are multiple microfoundational programs distinguished by differing conceptions of the micro/macro relationship and various pragmatic and theoretical goals (Hoover 2012). My position resolves some puzzles thrown up by King's account. For example, he questions whether there really ever was a general-equilibrium microfoundational program (p. 94). The term "microfoundations" received an enormous boost from E. Roy Weintraub's article "The microfoundations of macroeconomics" (1977) and book *Microfoundations* (1979), which were devoted almost exclusively to such a program. King can question its existence only

because of the narrow way in which he has framed what can count as a microfoundational program.

IV

King's historiographic method does not serve him well. It amounts to a comprehensive survey in which schools and individuals are scored for their degree of conformity with the maintained right answer—namely, that microfoundations is a wrong and pernicious doctrine. For example, he notes that the new Keynesian macroeconomics was initially resistant to the representative-agent approach (score one for them), but inexplicably abandoned it (score one against). It would have been better to try to work out why they abandoned their initial position than to simply note their fall in the league table. Some post-Keynesians (King regards himself as one) are credited as anti-microfoundationalists; others are vilified for drinking the Kool Aid, maintaining that microfoundations are the right idea, but asserting that "ours are better than yours" (p. 150). It would yield greater insight to try, charitably, to grasp why different economists thought that their preferred answers made sense: What was their problem situation—both substantively and sociologically? How did their thinking develop? Why were they persuaded? The key here is not to seek out inconsistency and root out deviance, but to exercise interpretive charity. Don Patinkin used to say that historical interpretation should follow a regression model in which a thinker is credited with the views that constitute a line of best fit, recognizing that the observations (the actual published positions) will generally fall a little—and sometimes a lot—off the line. Outliers may be explained, but they should be understood as outliers.

Since King devotes an entire section ("10.7 Kevin Hoover: a special case") to my views, I trust that using it to illustrate my point will not seem unduly self-indulgent. King reviews virtually everything that I have written about microfoundations since 1988 and somewhat sorrowfully concludes that Hoover

> has sharpened his criticism over the years [b]ut has also shifted his ground and has been less than entirely consistent in his opposition to [microfoundations]. His is an instructive case study in how the question of microfoundations can generate some confusion even among the best of its adversaries (p. 218).

My 1988 book was not concerned with the validity of microfoundations at all, but touched on the subject only incidentally in the attempt to understand the doctrines of the new classical macroeconomics. It was not a work of methodology or of history, but rather what my dissertation adviser referred to as "higher journalism". In later years my interest in microfoundations took both an historical and a philosophical turn. While I do not wish to pretend that my views have not changed and developed, there has, I believe, been a clear consistency in them since the early 1990s. The reason that King is disappointed is that he collapses a variety of texts, spanning a quarter century into a virtual and timeless volume in which each appears as a chapter to be scrutinized for inconsistency with no serious attention to the way in which the succession of views relate to one another, to the different audiences for whom they were written, to the conversational gambits that they employ, to the views of the critics to whom they react, to the gaps I felt necessary to fill in the face of sharp philosophical interlocutors, to my willingness to explore how far views that I disagreed with might in fact be defensible, or to the fact that the issues of microfoundations interact in complex ways with other philosophical projects such as the nature of causation, the role of models in science, the nature of scientific idealization, and the role of intentionality. I would not profess to being perfectly consistent over time—after all, to paraphrase Keynes, when I get new information I reconsider my views ("what do you do?" Keynes added). Nor would I profess to being perfectly consistent at any time—who is? But I do think, to use Patinkin's metaphor, that either a cross-sectional or time-series regression line for my views would show a relatively low standard error.

In applying his defective historiographic method to my views, King does no real damage to his overall case: after all, King and I more or less play for the same side. But in applying his method to the microfoundationalists he weakens his case as only an uncharitable and unsympathetic critic can. Good history, like a good novel, requires an author to make an imaginative transposition, to try to occupy the point of view of the historical actors. King never really gets inside the head of people like Robert Lucas or Edward Prescott to see why the microfoundations dogma appeals to them. He is content, for example, to observe that Lucas "made little or nothing of microfoundations in his major articles of the early 1970s to mid-1970s" (p. 103). As history, this is bizarre. Lucas and Leonard Rapping's work on labor markets in

the late 1960s was, on their own telling, part of the program of Klein and the econometricians to provide microeconomic underpinnings to the main aggregate functions in the Keynesian macromodels. It was in that context that Lucas introduced rational expectations into mainstream macroeconomics, which in turn, partly for technical reasons, encouraged the adoption of market-clearing, general-equilibrium models as the framework for the analysis. His most famous paper, published in the *Journal of Economic Theory* in 1972, was an attempt to provide a microeconomic model of aggregate supply or the Phillips-curve phenomenon. It used Phelps's search-theoretic "island model", a model that was a centerpiece of the famous "Phelps volume", *Microeconomic foundations of employment and inflation theory* (1970). That King sees Lucas as having "made little or nothing of microfoundations" is testament to his having been boxed in by his own approach to view only representative-agent, rational expectations models as a true expression of microfoundations.

Not only does King not mention Lucas's paper in the *Journal of Economic Theory*, he does not discuss the most central paper in the new classical microfoundations literature, Lucas's "Econometric policy evaluation: a critique" (1976). It is that paper that provides the fundamental rationale for microfoundations after 1970. Lucas argues that Keynesian econometric models are bound to show structural breaks when used to inform policy because they are aggregative, and that only a model that is grounded in the invariants of taste and technology—that is, in the behavior and constraints of individual agents—will be stable in the face of shocks or will be capable of supporting counterfactual policy analysis. Every new classical or new Keynesian microfoundational model—at first, explicitly but eventually only implicitly—is justified in the minds of its advocates as an attempt to avoid Lucas's criticism. This is the linchpin of the history of microfoundations, yet King mentions the "Lucas critique" only obliquely, in a discussion of Mark Blaug. One might attack the Lucas critique or its relevance in various ways, but to ignore it in an account of microfoundations is to misunderstand the principal appeal of microfoundations to contemporary economists.

V

Perhaps I have been overly negative. King has done a monumental job of absorbing vast and diverse literatures. There is much to be learned in his book. I just cannot help thinking that a more sympathetic and

flexible approach would ultimately have produced a more valuable book and supported a more compelling argument against the microfoundations dogma.

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Review of Shahzavar Karimzadi's *Money and its origins.* London: Routledge, 2012, 268 pp.

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A fascinating subject can make a great book, and money is one of the topics that have captured the public mind and the intellectual interest of philosophers and scientists alike. The recent financial collapse (circa 2008), and particularly its reverberations in the European Union, has energized the debate around money and led to a new wave of publications of books and articles on the subject. The question of the origin of money may not seem directly relevant to the troubles of the Eurozone but it remains important and relatively neglected. The detailed analysis of Shahzavar Karimzadi's book certainly fills the gap, and also has something to contribute to contemporary debate about monetary policy.

The book is an exposition and a critique of the different accounts of the origin of money, and the definitions of money that are related to them. Karimzadi examines a long list of candidate explanations for the passage from a moneyless to a moneyed economy, usually based on a single causal mechanism associated with barter exchange and its limitations (the division of labor, surplus production, exchange, degrees of marketability), and finds them all wanting. Karimzadi argues, rightly I think, that a system of pure barter is a convenient fiction invented to support an account that has remained relatively unchanged since the time of Aristotle, and that in any case the limitations of barter alone cannot explain the origin of money. He goes on to argue that each and every candidate explanation is illuminating but partial, and cannot alone account for the origin of money. Karimzadi is also skeptical of the definitions that support the different accounts, which privilege a specific function or 'form' of money, because he finds them too restrictive to describe money in all its complexity and mutability. If one declares that money is defined by its function as a means of exchange and proceeds to offer an analysis of its origin in those terms, such an

analysis will necessarily be incomplete, since it leaves out other 'forms' of money.

The book itself does not come to an explicit conclusion about the proper definition of money, though the author seems sympathetic, up to a point, to the Marxist description of money as universal equivalent. Nor does Karimzadi privilege any one explanation of the origins of money, but prefers to write a more complex story that aggregates different instances of the emergence of money, which are connected to the various forms and functions that money has served in different historical contexts. The author prefers, in lieu of a conclusion, to compile an aggregating explanation of the origin of money and of its definition, where a variety of causes for its origin are enumerated and connected with different descriptions of what money is and does.

The mainstream commodity theory features extensively, with all its minor variations and particularities. The book traces the intellectual history of this account from the time of Aristotle to more recent incarnations relating to barter exchange, including its representation in economics textbooks since Alfred Marshall. Yet if one looks at Karimzadi's bibliography, the relative neglect of contemporary scientific articles on the subject is striking. Recent formal work by commodity theorists on the emergence of money, and their methodology, is not really considered. Karimzadi may assume, justifiably I think, that such recent accounts suffer from some of the same fundamental flaws that he ascribes to all the mainstream accounts of the origin of money: a commitment to the fiction of barter and to a clear-cut distinction between barter and monetary exchange, as well as a one-dimensional—functionalist—definition of money as a means of exchange. Still, work in economics (e.g., Alchian 1977; Jones 1976; Kiyotaki and Wright 1989), and more recently in philosophy (e.g., Aydinonat 2008; Tieffenbach 2010; Smit, et al. 2011), has provided new insights into the mechanisms behind the emergence of money by offering rational reconstructions of its origins in individual attitudes and behavior that are comparable to what Karimzadi does when he presents his own account in the final chapter of his book. Discussing such research, and perhaps juxtaposing it with the author's own account of the origin of money, could have been very useful and informative to the reader.

Karimzadi's proclaimed methodology is an even greater problem than the lack of contemporary references. In the part of the introduction entitled "Method of Inquiry", Karimzadi introduces Hume's account

of the problem of induction as an obstacle to an empirical resolution of the debate about the origin of money. But the problem of induction is irrelevant to the accounts the author evaluates. There may be some references to historical, anthropological, or even anecdotal evidence, but these are mere illustrations. The main burden of the argument is carried by verbal or formal reasoning. From Carl Menger (1892) to the recent reformulations of equilibrium explanations by Nabuhiro Kiyotaki and Randall Wright (1989; 1991; 1993) or Dan Kovenock and Caspar De Vries (2002) the issue of empirical evidence has never been part of the analysis of the emergence of money in mainstream economics. This point is of some historical significance. The nineteenth century academic debate over the origins of money between commodity theorists and state theorists from the historical school led to the *Methodenstreit* that established the deductive method combined with methodological individualism as the methodology of choice for mainstream economics. It is therefore a big distortion of the history of economic thought and of the theories of money discussed to introduce the problem of induction. Moreover, Hume's problem of induction does not seem very relevant to the question of the origin of money anyway—Hume was more concerned with law-like universal generalizations and their underdetermination by evidence than with the emergence of social facts, like money.

I am very sympathetic to Karimzadi's criticisms of the mainstream 'commodity' accounts of money, but I think that they fall short of a convincing argument for discarding such accounts altogether. Obviously, Karimzadi goes into some detail to explain the flaws in the accounts he criticizes, and the exposition that unfolds in the book is informative. But I believe that what economic theory has been concerned with in relation to money is not to provide an account of its actual historical origin, but rather to provide the logical structure of the emergence and the persistence of money in a market setting based on its function or functions.

There is thus no contradiction in relating the function of money to its emergence and it is not clear what the author means when he argues against functionalist accounts of the origin of money on the grounds that it is methodologically flawed to derive the origin of money from its form. Functional explanation is a legitimate, and probably the most common, type of explanation in the social sciences. Economists' functional analysis of the emergence of money is supported by invisible

hand arguments that add a positive feedback effect between the function of money as a means of exchange and its establishment. Both the commodity and the state theories of money attempt a rational reconstruction of the origin of money that is supportive of their definitions. These definitions are underlined by a 'natural selection' argument in which money is supposed to be selected to fulfill the specific economic functions that define it and are considered to be the reason for its existence.

The book's critique of the state theory of money is also problematic. Karimzadi is obviously correct when he argues that money precedes the state,[1] and in that sense it is wrong to argue that the state is the origin of money. But, as the author himself admits, the state theory refers to the emergence of modern fiat money (p. 210). The core of this theory is the dependence of money on a sovereign authority that represents the community and enforces a standard of value and taxation, and the reliance of money on power is equally true for modern and primitive societies. The organization of social relations in terms of indebtedness suggests that the very act of valuation and the concept of value predate the market. The primordial measures of compensation for damages, such as *Wergeld* or 'honorable payment', are the predecessors of economic value, and the first incarnation of money (Ingham 2004, 92). The origin of economic valuation in *Wergeld* is not just an historical fact, but also a mechanism that can explain the origin of money. The organization of a system of economic compensation for injuries constitutes a shared system of social valuations that anticipates the system of prices. The important difference with the commodity theory is that the system of valuation is not the outcome of bilateral exchanges, but the imposition of a cardinal taxonomy by authority. The significance of *Wergeld* and its dependence on authority provides a foundation for the state theory of money and its narrative about the origin of money that goes beyond the limitations of barter.

The book offers an intellectual history of the origin of money, which is interesting and detailed, accompanied by a critique and an alternative pluralistic conception of money and its origin Nevertheless, the author fails to incorporate the most recent work on the subject by philosophers and economists. In addition, the critique often misses the mark, partly

[1] Still Karimzadi's chronology of the different types of "debt-economies" (p. 204) is not accurate. Individual debt to the community is part of the social bond and predates the emergence of money (Graeber 2011).

because the author works from the wrong methodological premises and partly because he fails to recognize that what is really at stake in mainstream economics is to explain the emergence, the persistence, and the acceptability—or the value—of money in the context of a market economy. Strictly speaking, the question of the origin of money falls outside the subject matter of economics or of philosophy. These two disciplines can illuminate mechanisms that underlie the emergence of money, but they require the support of anthropology and history to establish the truth of their stories (Pryor 1977).

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Review of Christopher Nobbs's *Economics, sustainability, and democracy: economics in the era of climate change.* New York: Routledge, 2013, 280 pp.

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This book advances a threefold argument. First, it claims that what the author calls the "libertarian" approach to economics, which flourished in the last part of the 20th century, is "practically and ethically inadequate for the needs of the 21st century". Second, it asserts that although the welfarist economics approach that preceded it has some merits, it has "serious limitations" too. Third, it advances the alternative ideal of "an overarching economics based on ecological principles" (p. xx). This alternative, the author claims, offers an appropriate response to the challenges of the 21st century. In brief, the book is a presentation and criticism of established theories and doctrines, combined with an attempt to sketch the contours of an alternative to them.

Given these objectives, the book has to cover a wide range of literatures, from microeconomics and macroeconomics to institutional economics, and from political economy to ethical (meta-) theory and cutting-edge debates in political philosophy. Its 12 chapters are organized into four parts, each dealing with a set of themes: part I is an overview and criticism of 20th century microeconomics and macroeconomics; part II discusses the relationships between economics, ethics, and ideology; part III deals with the relationships between the economy, society, and the natural world; and part IV discusses economics and the political theory of social justice, democracy, and social deliberation. The breadth of the author's familiarity with the relevant debates and academic literatures is truly impressive. In this respect, the book is an excellent introduction to the state of an entire cluster of disciplines, themes and research programs. Excepting minor slips, the presentations and discussions manage to maintain a neutral and objective stance. (One such slip occurs in chapter 5, on economics and ideology, where the author falls in with the typical routine of identifying the usual ideological suspect, the "Chicago School", while the

"Keynesian School"—presumably a non-ideological, value-free endeavor that is above suspicion—goes unmentioned.) Overall, the book leaves the reader with a clear and fair map of the thematic and conceptual terrain, and this accomplishment alone is enough to make it both interesting and useful.

As one might expect, in the criticism of theories and schools of thought in the social sciences there is always room for multiple controversial interpretations. Readers trained in different traditions may find some of the critical claims advanced by Nobbs debatable, or they may feel the need for caveats and nuances. Regarding his big-picture critical narrative of the nature and evolution of economic thinking in the 20st century, some readers may consider his interpretation in need of amendments or missing significant elements. For instance, one might wonder whether the approach to economics that flourished in the last part of the 20th century truly deserves the label "libertarian" that Nobbs gives it, and indeed whether anything deserving that label actually flourished in the 20th century. Or one might wonder about the relevance (if any) of the 'public choice' revolution and its contributions to the theory of "government failure". Do the theoretical, empirical and normative arguments advanced by Public Choice scholars have any bearing on (a) our assessment of various competing contemporary schools of political economy and (b) the way we may imagine and construct a new economics in the 21st century?

But these, and similar questions that may be raised from different intellectual and ideological perspectives, are secondary to the assessment of Nobbs's basic thesis. One may quibble over details, one may disagree with this or that interpretation, but the bottom line remains that, whether we like it or not, a change in how we think about the economy is needed. A reorientation of economic theory and practice is necessary because our understanding, concerns, and problems have changed. Global climate change is only one of those changes. The climate of ideas also changes: the other natural and social sciences produce new ways of understanding the world; our social and environmental circumstances change; new challenges emerge; even our ethical and normative beliefs and sensibilities change. So, in the end, economics has to change too.

The question is, in what ways? We have now reached the most interesting question addressed by the book. How should economics be

conducted in liberal democracies in the 21st century? On what lines should the economics of the future be reconstructed?

Nobbs suggests two dimensions that this reconstruction should incorporate: the ecological and the ethical. The former requires rebuilding economics around the positive scientific observation that human societies are part of the natural world. To analyze and manage the "economy" as one aspect of socio-ecological systems requires understanding the interplay between physical laws and social processes. One has to take that basic reality into account when dealing with economic systems, institutions and policies. The latter requires adding a strong normative dimension to that expanded positive analysis. Ethical questions are deeply embedded in issues of economic governance and sustainability. In the end economics is about human action and decision-making, subjects with a strong ethical dimension. Sooner or later, economics will be forced to engage with this ethical dimension in ways that go beyond the concept of "efficiency". In conjunction, these two dimensions (the *positive* naturalization of economic systems and the *normative* endogenization of moral meaning) define the framework within which the reorientation of economic theory and practice for the 21st century should proceed.

What does this foundational reorientation mean in more concrete terms? Nobbs points first to what has been known for some time as "ecological economics", in which the economy is seen as part of natural systems and natural systems are seen as a foundation of economic processes and systems (Sagoff 2012; Constanza 1989). The focus shifts to social-ecological systems, and to complex adaptive systems in general. The central concern is with trying to capture the evolutionary forces that generate adaptive equilibria and the systemic processes associated with the ways the human economy is embedded in ecosystems (Levin 1999). This approach may rightly be seen as a response to the present "fixation on economic efficiency", ignoring "the physical characteristics of material objects" (p. 150).

However, Nobbs also points to another perspective, related to but quite different from ecological economics. Over the last three decades scholars have developed the field of "environmental economics", which takes a perspective firmly based on mainstream micro-economics and welfare theory. Environmental problems are diagnosed as market externalities using a theoretical apparatus centered on "welfare", "utility", and "willingness to pay" as key analytical and policy variables

(Sagoff 2012; Stavins 2008). In this view, the main focus is not the evolutionary equilibrium of the systems emerging as a result of interactions between humans and nature (as in ecological economics), but calculating the full costs of human activities and supporting decision-making regarding the environment, for example about trade-offs. Hence the methodological focus on shadow prices and cost-benefit analysis, and the general policy practice of using economics as a science of valuation to estimate and put prices on alternatives, situations and things, including ecosystems.

Both approaches are legitimate. But, unsurprisingly, their basic philosophies, epistemologies, and methodologies differ in nontrivial ways. Predictably, tensions emerge. However, both are alike in offering a view in which the voice of the technocratic, scientific community is not just salient but preeminent when it comes to the policies and interventions deemed desirable. The definitions and solutions of our problems come primarily from the technocratic elite, be they experts in social-ecological systems or in economics. That is to say, those who articulate the basic parameters of the correct or desirable solutions scientifically have a privileged position in the policy process.

But there is yet another perspective in the range of alternatives that have emerged in the last couple of decades, a perspective that departs in substantial ways from those described above, especially when it comes to the policy process. Interestingly enough, Nobbs seems to gravitate toward it, since he ends his book by exploring some of the building blocks of this position.

Let us call this perspective "institutionalist". It is an institutional approach based not on systems ecology or cost-benefit economics but on a theory of values implying an important role for a variety of ethical and aesthetic arguments, besides the economic-efficiency ones. In brief, it is a theory of institutions and governance under conditions of heterogeneity in individuals' values, beliefs, and preferences. The idea is that there are different ways of judging the value of things, whether natural or social. Some things may be seen in pure economic terms; other things should be seen in ethical, aesthetic, or even religious terms. A social-ecological system is not just a complex adaptive system or a welfare or utility generator, but may have many other intrinsic values in the eyes of the members of a community or society. The challenge is how to make collective decisions in such heterogeneous circumstances in which diversity of values, principles and preferences is

the norm: for instance, how to use ethical frameworks of responsibility in addition to (or as an alternative to) the frameworks of cost-benefit analysis. The institutional approach deals with this challenge by focusing on rulemaking and regulation based on deliberation, public discussion, and negotiation, conducted via democratic processes. Its distinctive feature is the idea of "second-order institutions" (Knight and Johnson 2011) that give voice to stakeholders and create a collective space for deliberation and negotiation. Such "democratic" arrangements offer a chance to incorporate alternative principles quite different from the "willingness to pay" principle or what the technocratic elite may suggest based on more or less "scientific" conclusions.

This "institutionalist" approach contrasts with the "technocratic" one in many respects. It is primarily about the institutional procedures and governance of collective decision making, about democratic deliberation and negotiation, and about ethical commitments; and only secondarily about the analytical and computational activities conducted by natural scientists and economists. In this view, Mark Sagoff argues, economics still has a role: "It may assist society by suggesting new institutional arrangements through which people may make the bargains that may now elude them" (Sagoff 2008, 26). That is to say, it may "show society how to redesign institutions"; how to facilitate communication, deliberation and search processes; and how "to lower transaction costs that burden voluntary exchange".

Once the alternatives suggested by Nobbs are de-homogenized, the picture becomes clearer. It is important to note the undeniable tensions, gaps and incongruities between these approaches. Integrating all three into an economics of the 21st century seems to be a tall order, a genuine challenge. Nobbs's book itself, with its vacillation between them, is an excellent illustration and warning of the difficulties involved. Yet Nobbs ends his book in a rather confident tone, heralding the rise of "ecological economics" as "part of the nascent science of sustainability", a compounded discipline that manages to both "couple the economy to society" and acknowledge human society "as an essential component of the natural ecosystem", thus coupling the economy to nature (p. 223). But, as indicated, reconciling such different approaches under an overarching conceptual and programmatic framework is a project that has to overcome serious philosophical, epistemological and methodological obstacles. As far as one can see, the track record of

similar attempts in the history of the sciences is rather discouraging, indicating slim chances of success.

And thus we are left with a rather interesting alternative. What if, in looking toward the economics of the 21st century, we try to think more in terms of diversity than in terms of unity? Instead of a paradigm of convergence (be it based on "efficiency", "evolutionary equilibria", or "sustainability") we might try to imagine a heterogeneous field of competing and complementary approaches. Instead of one economics for the 21st century, we might think of multiple schools of thought operating in an environment defined by nested, overlapping, epistemic communities and institutional infrastructures: an institutionalized social knowledge process based on a search strategy capitalizing on the strengths coming from the combined diversity of the perspectives, approaches and methods involved.

Nobbs's book helps us better understand the current landscape of contemporary economics and its intellectual vicinity, while outlining a thought-provoking proposal about how we should be thinking and doing economics in the 21st century. Yet at the same time the book sets the stage for an informed discussion about the variety of possible alternatives that may not take the direction Nobbs advocates.

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Review of Tomáš Sedláček's *Economics of good and evil: the quest for economic meaning from Gilgamesh to Wall Street.* New York: Oxford University Press, 2011, 384 pp.

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This book is well written and timely. The moralistic response of many politicians and commentators in the public debate about the economic crisis has made economists more interested in the ethical dimensions of economic activity and economic policy-making. With the aid of ancient literature Sedláček explains why ethics has always been important in economics, and why it should be. The old stories show that humans' material desires tend to be boundless, and thus the need for self-command and control over material longings if we want to develop a sustainable economy.

By starting from humanity's oldest writings, the author is able to put current economic thought in perspective. In Sedláček's view, knowledge of myths and stories is indispensable, even for economists. Narrow minded economists will never be good economists, because understanding the economy requires going beyond the specifically economic domain. John Maynard Keynes (1924, 322) already argued that master-economists "must be mathematician, historian, statesman, philosopher—in some degree". Mathematical models and statistics are merely the tip of the iceberg. Sedláček introduces what he calls 'meta-economics', which includes the historical, cultural, psychological, theological, and philosophical underpinnings of economics. He argues that modern economic theories are new (mathematical) forms of the meta-economic stories found in ancient myths. It is significant that all these stories are basically about good and evil: contemporary economic debate is more about competing stories of good and evil than technical discussions. Economists should be aware of this and stop denying that economics is inherently normative.

The book is composed of two parts. In the first part Sedláček looks for the economics in myths, religion, theology, philosophy, and science. In this part he tries to "tell the story of economics" by analyzing important milestones in the historical development of economic

thinking, or more specifically, economic ethics. In the second part, he looks for the myths, religion, theology, philosophy, and science still present under the surface of modern economics. In what follows I first provide a critical summary of parts I and II, and then make some more general comments.

Sedláček starts the book with an analysis of the Epic of Gilgamesh, the oldest surviving piece of world literature (dating from around 2000 BCE), and its implicit economics. Since there is no secondary literature on the economic meaning of the Gilgamesh epic, Sedláček provides a first explorative analysis. The epic illustrates, among other things, that economic effectiveness often demands suppressing the humanity in labor relations. People should not spend their time and energy on 'unproductive' labor, such as love, friendship, and the like. Still, and partly because of this, the productive economy cannot fully satisfy human desires.

In Chapter 2 Sedláček analyses the Jewish sacred texts retained in the Old Testament of the Christian bible. In contrast to the cyclical perspective found in the Epic of Gilgamesh, the Hebrews believed in historical progress in this world. Paradise is conceived as a place on earth rather than connected to a heavenly afterlife. Wealth is seldom condemned, nor is poverty valorized. Nature is not sacred, although humanity has responsibility to look after the earth and is viewed as a co-creator. In contrast to the Epic of Gilgamesh, good and evil are perceived as integral parts of human life, rather than as exogenous entities. Moral (I would rather say, spiritual) issues are decisive for human history. According to Sedláček, the Old Testament's moral philosophy strikes a compromise between Stoical philosophy (that we should not aim at pleasure, but live according to moral rules) and Epicurean philosophy (that we should maximize utility without need of respecting rules).

Sedláček gives very nice illustrations of the economic relevance of Old Testament stories, like Joseph's grain storage program to prevent famine in Egypt, an early example of economic stabilization policy. That story also illustrates that correct economic predictions of bad outcomes will normally not materialize, because of the very policy measures taken to prevent them. Other economic examples include religiously mandated laws to prevent the concentration of economic wealth and the resulting social inequality (debt bondage); the close connection between charity and responsibility; the holy Sabbath rest as

an "ontological break"—for the enjoyment of the fruits of our work—rather than 'productive' rest that allows one to work more efficiently the rest of the week.

Chapter 3 takes up classical Greek thought. An interesting example is Xenophon, who had already argued in favor of trading relationships with foreigners (instead of war) as a means of progress, stressing the positive sum gains for all involved. He was also aware of the economic benefits of the division of labor, the notion that Adam Smith famously described in his example of the pin factory in the *Wealth of nations*. Sedláček also discusses important differences between, for example, Plato and Aristotle, and the Stoics and the hedonists.

In chapter 4 Sedláček turns to the New Testament and Christian theology. Generally, I found Sedláček's reading of the Bible very well-informed and his application of it to economic issues very original and refreshing. Sedláček cites authors who argue that socio-economic issues rank as the most important topic in the Bible after idolatry. A major topic is the remission of debts. Often this has a spiritual meaning (forgiveness of sin), but the Greek word for sin (as used, for example, in Matthew 6:12) can also be translated as debt (as in Romans 4:4). Thus, the famous prayer in Matthew 6:12 ("Forgive us the wrongs we have done…") has a surprisingly topical ethical precept if one interprets it in an economic sense, as stating "Forgive us our debts, as we also have forgiven our debtors". Although the cancellation of debts seems unfair, in a modern economy just as in a premodern economy, it is sometimes necessary for the survival of society as a whole. Good bankruptcy laws prevent the immiseration of individuals, and also provide incentives to creditors to assess creditworthiness and reduce 'predatory' lending practices.

Sedláček explains how Jesus's command to love each other is also of great value for economic trust and cooperation. For a long time game theorists held that 'an eye for an eye' ("tit for tat") strategy was the most efficient cooperation strategy. But recent research has shown that the 'grace' strategy that Sedláček attributes to Jesus is more promising because it prevents a vicious negative spiral of mutual punishing and hence is more effective in promoting cooperation. Sedláček also derives an important lesson from the parable of the seed (Matthew 13:24-30): good can only grow to fruition if the evil mixed in with it is also allowed to grow.

In chapter 5 Sedláček highlights the links between the rational philosophy of Descartes and the mathematical and rational method in economics, including the account of rational man (*homo economicus*).

Chapter 6 provides an interesting analysis of Bernard de Mandeville's (in)famous book *The fable of the bees: or, private vices, public benefits*. Sedláček portrays Mandeville as criticizing the hypocrisy in society's condemnation of vice. People say they want to rid society of vice, but they also want to live in a great and rich society, for which, Mandeville argues, vices are indispensable as the source of demand for goods or services. That does not mean that Mandeville gives a moral defense of the vices. Mandeville does not judge which society is preferable: the virtuous or the vicious. He only wants to show that vice is inherent to a materially flourishing human society, and so we cannot have both virtues and material prosperity. Interestingly, this point can also be found in the Epic of Gilgamesh and Jesus's parable of the seed, discussed above. Evil cannot be uprooted without destroying the good as well, and thus social planners should instead try to redirect its energy towards good social outcomes.

The way Sedláček links the role of evil to the creation of good is fascinating. It should make us prudent in the way we approach evil. On the other hand, I believe that Sedláček could have been more critical of Mandeville's ideas. He does not pay much attention to recent empirical studies that show that virtues have a directly positive effect on economic development. For example, there is much evidence that trust (social capital) is an important determinant of economic growth (Knack and Keefer 1997; Beugelsdijk, et al. 2004), and it is difficult to see how trust can develop if virtues such as honesty and justice are lacking. Honesty, loyalty, truthfulness, and justice facilitate efficient coordination if individual and common goals are not perfectly aligned and if information is imperfect (Frank 2004, chapter 4). Furthermore, although Sedláček's interpretation of Mandeville is interesting, it is also disputable. Most scholars see Mandeville's charge of hypocrisy as concerning society's implausible *definitions* of virtue and vice, in which the merest hint of selfishness in one's motivations means one is behaving viciously. Such impossible standards make anyone who claims to be virtuous into a hypocrite.

Chapter 7 on Adam Smith closes the first part of the book. Sedláček discusses Smith's virtue ethics, and the concepts of the impartial spectator and the invisible hand. He compares Smith with Mandeville.

Although Smith is usually taken to strongly disagree with Mandeville, accusing him of abolishing the distinction between virtue and vice, according to Sedláček Smith's own position is not substantially different. That is because, while Smith turned the vice of self-love into a more neutral concept of self-interest, he nonetheless believed, like Mandeville, that it was such self-interested behavior that generated material prosperity. And although Smith did not perceive self-interest as the most important principle in all human relations, he considered it very important in the economic domain. By reframing Mandeville's moral analysis—by recasting the vice of self-love into the more neutral self-interest—Smith can be seen as making use of parts of Mandeville's economic analysis while avoiding the criticism Mandeville had received. Sedláček does not mean to say, however, that virtues were not important for Smith. He discusses the famous 'Adam Smith problem' and argues that Smith believed that humans are driven by several other motives even stronger than (rational) self-interest.

One can question whether Smith is really so close to Mandeville as Sedláček believes. In Smith's analysis of self-interest in the *Theory of moral sentiments*, the virtues of prudence and self-command play an important role (e.g., IV.i.17). Smith describes prudence as the exercise of superior reasoning and understanding, by which one discerns the remote consequences of one's actions for one's own happiness. A prudent person will have a clear understanding of their self-interest, and will take account of the interests of others at least insofar as that is instrumental to achieving their goals. Prudence thus prevents clear-eyed self-interest degenerating into delusional self-love. Self-command likewise moderates self-interest and prevents it from degenerating into short-term hedonism. If guided by prudence and self-command, self-interest therefore cannot be equated to the vice of (pure) egoism that aims to maximize one's own happiness without consideration of the interests of others.

Turning now to the second part of the book, in which the myths in economics are investigated, the first theme is greed. According to the story of Genesis 2, evil entered the world through greed. Although the supply of food in paradise was abundant, Adam and Eve were not satisfied and wanted more. According to the Epic of Gilgamesh, becoming aware of unmet needs stimulates culture. The dissatisfaction caused by discovering new needs stimulated the savage Enkidu to enter civilization. In his natural state, a human being hardly has more needs

than a wild animal, but when he develops civilization his wants multiply together with his means of meeting them. As Sedláček quotes Frank Knight, "[I]t is human nature to be more dissatisfied the better off one is". The questions that Sedláček then raises are to what extent we must accept this human craving for more, and how we can put limits on our desires.

Chapter 9 is about progress and whether that requires continuous economic growth or the economics of enough. Sedláček describes the hopes of J. S. Mill (in his well-known *Principles of political economy*, section 4.6) and J. M. Keynes (in his essay *Economic possibilities for our grandchildren*) that economic progress would eventually solve the economic problem and lead to a stationary zero-growth economy in which everyone would be able to live a life that is good, in both material and moral terms. Historically, Western society has never been as rich as today. But Keynes's prediction has not yet come true. Moreover, beyond a certain level national opulence does not seem to substantially increase (average) individual happiness, a finding that is also found at the micro level. According to Sedláček growth then becomes meaningless. It seems that we carry with us a persistent dissatisfaction with what we have that drives us to keep moving and striving for more. But this continuous pursuit of more material prosperity by individuals comes at the cost of their true peacefulness and even their enjoyment of the satisfaction of their desires. It is difficult to follow the Stoics' advice to be content with what we have. But if we did, we would probably have much more leisure and less working stress.

Chapters 10 to 12 explore Smith's invisible hand, homo economicus, and Keynes's animal spirits. Sedláček shows that, long before Smith, other authors had already expressed ideas very similar to his invisible hand mechanism. In contrast to Sedláček, I believe that the use of the notion of the invisible hand by Smith in the *Theory of moral sentiments* comes very close to his use of it in the *Wealth of nations*. The underlying idea is that divine Providence has implanted in human nature such sentiments as tend to bring about the happiness and welfare of mankind. In his discussion of the invisible hand, Sedláček also refers to Paul (Romans 7:21-25). Sedláček interprets this text as evidence that Paul was aware that good intentions may have evil consequences (i.e., the opposite of Mandeville's private vices, public benefits thesis). I wonder, however, whether Paul means to say this. This text is not

about the distinction between good intentions and good consequences, but about the inner conflict between the spirit and the flesh.

Chapter 11 ends by discussing Robert Nelson's claim that while self-interest contributes to economic prosperity, excessive self-interest undermines the proper functioning of the market economy. This suggests a curvilinear relationship between self-interest and social welfare. Chapter 12 deals with Keynes's notion that irrational animal spirits (described as spontaneous impulses to act not guided by quantitative deliberations) are necessary for entrepreneurial activity. Given irremediable uncertainty about the future, the rational self-interest attributed to homo economicus is insufficient to justify business initiatives. Human beings need both.

In chapters 13 and 14, Sedláček criticizes the large role of mathematics in economics. Because of their mathematical methodology, economists often lack a broader social vision of the economy. Economists should also be more modest in their claims, given the low predictive power of mathematical economics. The future is radically indeterminate. Only the static, non-living part of reality is predictable. When economists discuss actual economic policy, the mathematical models should be put aside. In the last chapter, Sedláček therefore concludes by returning to his central message: for the study of economic problems meta-economics—including philosophy, theology, anthropology, history, psychology, sociology, and other disciplines—is indispensable.

I agree with Sedláček that economics should not be limited to mathematical models. However, I believe that in the practice of economic policy mathematical economic models do not actually play such a large role. I know from my own experience as a model builder at the CPB (the Netherlands Bureau for Economic Policy Analysis, an independent government agency founded by Jan Tinbergen) that practical (non-abstract) and qualitative information about the economy is at least as important. Moreover, the limitations of mathematical models are already well known and economists have come to pay much more attention to empirical analysis (and the development of good data sources) and the qualitative analysis of institutions during the last decades. The boundedness of human rationality is also now widely recognized in economics (behavioral economics) and already influences policy advice, for example regarding pension systems. However, it is

true that the translation of such realistic theories into policy analysis is still too limited.

Sedláček delves into ancient writings to provide us a positive view on life, while also recognizing the dark side of human nature and evil. The result is very inspiring. Nevertheless, the book leaves me with a number of questions. First, it is not entirely clear whether or how Sedláček thinks humans can escape the restless search for more. If we do not know what we want, how can we limit our wants? What kind of economic behavior does Sedláček want to encourage? How can we stimulate a change in mindset, to learn to be content and enjoy what we already have rather than 'maximize'? On the macroeconomic level, this question leads me to wonder whether a stationary economy is feasible or desirable. From a Christian ethical perspective, I would rather stress the need for selective growth that really serves human needs and raises the quality of life, rather than an economics of mere sufficiency. Making progress on the aspects of human life that matter is a good goal to have, and it calls upon part of humanity's creative nature (Graafland 2010, 49-54). In the Bible, work is seen as a calling and as a service to others. One should develop one's talents and use the income that they generate not only for oneself, but also for those in need. In a modern society this Christian calling to serve others implies support for institutions that aim to guarantee a reasonable quality of life for everyone in society.

Second, although I fully agree that ethics is, and should be recognized as, an integral part of the economy and hence of economics, the influence of ethics may nonetheless be limited. The market seems to have its own pernicious logic of greed, capable of surviving the social and political condemnation associated with the current economic crisis and even the moralizing of economists themselves. For example, Joseph Stiglitz (2012) has argued that concentrations of wealth lead to concentrations of political power which give the rich substantial control over economic institutions and government policies and allow them to become rentier capitalists at the expense of both aggregate economic growth and its equitable distribution. It would be interesting to analyze the failure of such ethical arguments throughout history to overcome the corruption they condemn.

Sedláček rightly stresses the importance of stories, and it is very interesting to read about the economic implications of different ancient myths and stories. Stories are indeed still important in today's business

and politics. But obviously the inspiration that stories give can go in all kinds of direction. Alan Greenspan, for example, has stated that he was inspired by stories in making economic policy as chairman of the Federal Reserve System, namely by the books and philosophy of Ayn Rand. We cannot escape the problem of how to make best use of the stories we have inherited and their ethical or economic lessons. Although Sedláček helps the reader to recognize that economics is filled with value-laden stories and thus to overcome the "self-inflicted blindness" of contemporary economics, he does not deal with the moral question of which stories should be guiding us.

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PHD THESIS SUMMARY: The many faces of rational choice theory

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In recent decades the epistemic potential of 'rational choice theory' has been profoundly questioned. Skepticism towards economic man and his 'imperialistic attitude' has been advocated by behavioral economists, psychologists, and philosophers for several decades now. Not only does the rejection of rational choice theory appear to follow from our common sense. With the breakthrough of behavioral economics, rational choice theory has also become challenged on empirical grounds. Although these critiques may appear to be *prima facie* justified, it is significant that many appraisals of rational choice theory are conducted independently from the actual context in which the theory is applied. This often results in an underestimation of the pragmatic usefulness of rational choice theory in the context of different scientific practices. My dissertation points to the weaknesses inherent in context-independent appraisal. The overarching goal is to seek a comprehensive understanding of 'rational choice theory' against the backdrop of actual economic practices. This, I believe, will serve as strong basis for more nuanced appraisal of the theory.

In chapter 1, I show that there exists fundamental confusion in the philosophical and social scientific literature about what rational choice theory is. This originates in the fact that 'rational choice theory' has many faces. These faces are conceptually and methodologically distinct in contemporary economics and have been applied to strikingly disparate problems. They have, however, never been distinguished from one another. Furthermore, many appraisals rest upon the unsupported premise that rational choice theory is used as a psychological theory of human behavior (see Hausman 1995; Satz and Ferejohn 1994). My dissertation offers an account of rational choice theory that is more sensitive towards actual practices and runs contrary to this 'received view'. I suggest that the various manifestations of rational choice theory could be better understood as a family of theoretical approaches, which

can be subsumed under the heading of 'rational choice analysis'. They each constitute an account of human agency that is based upon some version of individual rationality, but they differ fundamentally in how rationality is interpreted and conceptualized. So far, this diversity of rational choice analysis has not been well understood. Yet, failure to recognize the existence and nature of these many faces has ensured that they are appraised in isolation from the problems for which they were designed in the first place to solve. I suggest that such isolation can often result in misdirected critique.

I argue further that developing a framework for appraisal that takes the various pragmatic contexts of rational choice explanations into account requires tracing the historical emergence of rational choice analysis. This is because contemporary rational choice explanations are rooted in different intellectual traditions and have emerged from earlier attempts to conceptualize the behavior of human agents within specific problem-contexts. To capture the variety of different historical backgrounds for rational choice explanations, I take an approach that is used in historical epistemology, namely 'case study analysis'. Case study analysis suggests itself when what I call the 'method of local critique' is used as a method of appraisal. This is the approach that implicitly underlies Philip Kitcher's work in philosophy of biology (see, e.g., Kitcher 2003). In my dissertation, I use both approaches to appraise the different faces of rational choice against the backdrop of their history.

In chapters 2 to 4, I show that the history of rational choice analysis reveals a four-fold shift that is related to economists' changing conceptualizations of individual behavior and the problems they address. First, the concept of rationality has become considerably narrower in comparison with its intellectual precursors, such as the theories of practical reason and rational behavior variously developed by Aristotle, Daniel Bernoulli, Thomas Hobbes, and David Hume. Second, following the Marginalist revolution, the focus of economic analysis shifted from understanding the sources of wealth and the functioning of markets towards extracting the logic of choice that underlies individual behavior. This shift is reflected in the changes of the methodological status that rational choice approaches occupied, away from W. S. Jevons's crude principles of utility towards Max Weber's ideal types and Ludwig von Mises's praxeology. It went hand in hand with the different justifications for re-conceptualizing human agency, namely allowing economists to better address the distinct problems

economists were respectively concerned with. Third, from the 1940s onwards, rationality became formulated axiomatically, allowing rational choice analysis to be employed as a highly flexible 'toolbox', applicable, with appropriate specifications, to a range of problems beyond the traditional scope of economics. The long-standing separation of human rationality and human psychology in much economic thought reached its apogee in Gérard Debreu's *Theory of value* published in 1959. Finally, in parallel with the introduction of the axiomatic choice method to economics, a fourth shift occurred that constituted an attempt to unify the social sciences by addressing social scientific problems beyond the traditional realm of the market. This shift is particularly pronounced in Gary Becker's *Economic account of human behavior* (1976) and the tradition of Chicago price theory. These four shifts reveal that none of the conceptually distinct manifestations of rational choice analysis was ever primarily intended as a psychological theory of human behavior.

In chapter 5, I outline some implications of these findings with respect to the potential and limitations of rational choice analysis, and for the often-voiced claim that economics requires a descriptively adequate (psychological or even neural) theory of human behavior. Given that economists address problems characterized by complexity, they can frequently provide only what F. A. von Hayek (1955) called 'explanations of the principle'. In those cases, explaining individual behavior on the neurological, psychological or behavioral level does not necessarily facilitate better comprehension of phenomena occurring at the institutional or macro-level.

Furthermore, I argue that on the three predominant interpretations of rationality in economics (i.e., consistency, maximization, self-interest), rational choice analysis cannot accommodate what I call the 'normative dimension of agency' and what Amartya Sen (1977) has termed 'acting from commitment'. On all three interpretations, rationality is understood as 'instrumental rationality' in the Humean tradition. As such, rational choice analysis finds itself in stark contrast with the Kantian tradition and the idea of moral agency as being essential and constitutive of rationality. It is the Kantian understanding of rationality, however, that characterizes Sen's concept of commitment. The difficulty in accounting for committed behavior—understood in a Kantian sense as acting from duty—with a theoretical framework that relies upon an instrumental understanding of rational action reveals a fundamental weakness of rational choice analysis, but only when it is

depicted as a universal theory of human behavior and appraised from a realist perspective. By analyzing Cristina Bicchieri's (2006) account for norm-conformity, I conclude that recent attempts by behavioral economists to accommodate pro-social behavior within an axiomatic choice framework are themselves questionable.

In a final discussion of the main findings of my thesis, I argue that conventional meta-narratives formulated in Lakatosian or Kuhnian terms are inadequate either as an interpretation of the conceptual history developed in chapters 2 to 4, or as a fruitful assessment of rational choice analysis. This is because the interpretation of what is meant by 'rational action' has drastically changed throughout history, as have the problems economists address. This diversity of meaning hinders easy comparison of the different faces of rational choice. However, what this historical reconstruction reveals is a lasting commitment by economists to what I call 'methodological rationalism', i.e., the doctrine that individual behavior can be conceptualized with recourse to some notion of rationality. I conclude with an epilogue on the methodology of philosophy of economics, defending case-study analysis and the method of local critique as a fruitful alternative to traditional methods of appraisal.

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PHD THESIS SUMMARY: Causal reasoning in economics: a selective exploration of semantic, epistemic and dynamical aspects

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Many broad questions of high philosophical interest about causal reasoning in economics remain poorly answered. First, what are the meanings of causal claims? This is a semantic question. Second, how can a causal claim be adequately supported by evidence? This is an epistemological question. Third, how are causal beliefs affected by new information? This is a question about belief dynamics.

This thesis uses a combination of case-based research and conceptual analysis to address these questions[1]. The case study used throughout the thesis is economic research on the causes of unemployment. It is mainly by studying this scientific practice that I come to formulate and defend answers to the three questions stated above. I do not claim that these answers are universal—they most probably do not apply to all instances of causal reasoning. But they do contribute to a better understanding of causal reasoning in economics and beyond.

In part I, the semantic part (co-written with Luis Mireles-Flores), we investigate the meaning of causal generalizations in the economics of unemployment. We argue that the standard approach to meaning is misguided in identifying the referential relation as being what constitutes meaning. To make sense of the widespread practice of demanding and supplying causal generalizations in disciplines like economics, we need an approach to meaning which prioritizes the *inferential* relation over the *referential* relation. We contribute to the development of this alternative approach to meaning by distinguishing different types of inferential relations which together constitute the meaning of an expression.

[1] The thesis can be accessed online at: http://repub.eur.nl/pub/38242.

In part II, the epistemological part, I argue that justification in sciences like economics often relies, and ought to rely, on evidential variety—i.e., the combination of evidence from multiple sources. Recognizing the importance of evidential variety is crucial to move the methodological debate away from single-source assessment. This part, the lengthiest of my thesis, is made up of three chapters. In chapter 2 (also published as Claveau 2011), I argue that a lively debate in contemporary econometrics between the design-based and the structural approaches suffers from a bias toward single-source assessment. In chapter 3 (also published as Claveau 2012), I turn to a debate in philosophy of science surrounding what is known as the Russo-Williamson thesis. I maintain that Russo and Williamson (2007) are wrong to read the quest by scientific researchers for both difference-making and mechanistic evidence as being incompatible with standard monist accounts of causality. I argue instead that this quest is simply an epistemic strategy for generating evidential variety, with no implications for the semantics or metaphysics of causality. In chapter 4 (separately published as Claveau 2013), I use a Bayesian model to investigate the truth of the variety-of-evidence thesis. The variety-of-evidence thesis states that, ceteris paribus, the strength of the confirmation of a hypothesis by an evidential set increases with the diversity of the evidential elements in that set. Modifying a model by Bovens and Hartmann (2002; 2003), I find that, although the variety-of-evidence thesis is a good guide in typical circumstances, it is false in extreme circumstances (i.e., when evidential sources are most likely unreliable).

In part III, the part on belief dynamics, I study deviant-case research. A case is deviant when it does not behave as expected. The behaviour of the German unemployment rate following the 2008 financial crisis is such a deviant case. Deviant cases have received various labels in post-positivist philosophy of science—e.g., 'falsifiers' and 'anomalies'. In chapter 5, I argue that an influential view of science called by Cartwright (1999, 184) the "'vending machine' view" gives an unhelpful picture of deviant-case research in sciences like economics. The core of the problem is that expectations in these sciences are not the result of deductions from believed premises. I flesh out an alternative picture of deviant-case research in sciences that I call 'eclectic'. These sciences are characterized by variety and combination; they are not structured around a monolithic theory.

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PHD THESIS SUMMARY: Value and prices in Russian economic thought (1890-1920)

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The subject of this dissertation should evoke several names and debates in the reader's mind. For a long time, Western scholars have been aware that the Russian economists Tugan-Baranovsky and Bortkiewicz were active participants in the Marxian transformation problem, that the mathematical models of Dmitriev prefigured forthcoming neo-Ricardian based models, and that many Russian economists were either supporting the Marxian labour theory of value or were revisionists. These ideas were preparing the ground for Soviet planning. Russian scholars knew that the turn of the 20th century was characterized by the introduction of marginalism in Russia, and that during this period economists were active in thinking about the relation between ethics and economic theory. Although these issues were well covered in the existing literature, there was also a big gap filled by this dissertation. The existing literature handles these pieces separately, although they are part of a single, more general, history: the *Russian synthesis*, i.e., the various attempts to coalesce classical political economy and marginalism, between labour theory of value and marginal utility, and between value and prices, that occurred in Russian economic thought between 1890 and 1920.

This dissertation is the first comprehensive history of the Russian synthesis. To accomplish this task, it has seldom been sufficient to gather together the various existing studies on aspects of this story. It has been necessary to return to the primary sources in the Russian language. The most important part of the primary literature has never been translated, and in recent years only some of it has been republished in Russian. Therefore, most translations from Russian have been made by the author of this dissertation. The secondary literature has been surveyed in the languages that are familiar to the author (Russian, English, French, and German), and which are hopefully the most pertinent to the present investigation. Additionally, some archival

sources were used to increase the acquaintance with the text. The analysis consists of careful chronological studies of the relevant writings and their evolution in their historical and intellectual context.

As a consequence, the dissertation brings new authors to the foreground—Shaposhnikov and Yurovsky—who were traditionally confined to the sidelines, because they only superficially touched the domains quoted above. In the Russian synthesis, however, they played an important role. As a side effect, some authors that used to play in the foreground—Dmitriev and Bortkiewicz—are relegated to the background, but are not forgotten. In addition, the dissertation refreshes the views on authors already known, such as Ziber and, especially, Tugan-Baranovsky. Ultimately, the objective of this dissertation is to change the reader's opinion of "value and prices in Russian economic thought".

The Russian synthesis was the result of multiple conditions: a specific intellectual context, specific developments within the discipline of economics, together with the authors' own intentions. The first part of this dissertation intends to give an overview of the most relevant theoretical elements of that background. It is essential to capture the ingredients of the synthesis—classical political economy and marginalist theory—as they were understood in Russia by the protagonists of the synthesis. Therefore, chapter 1 (Russian economic thought) provides a short account of Russian economic thought before the 1890s by way of an introduction to the Russian economy, its actors, and those who studied it. This enables an adequate description of the protagonists of the synthesis within the landscape provided by a review of the troop of Russian economists. Chapter 2 (Classical political economy in Russia) focuses on the reception of the labour theory of value by Russian economists prior to Tugan-Baranovsky. This section dwells on the order of reading (Ricardo *after* Marx), and on the articulation between the notions of labour value and costs of production, notably through Ziber's influential interpretation. Chapter 3 (Marginalism in Russia) draws up a map of the reception of marginalism from the 1890s onwards. It examines the relative influence of English, Austrian, and Walrasian marginalist theories, and their theories of exchange and production, as far as they were, or were not, involved. Taken together, these three chapters provide theoretical explanations of the genesis of the Russian synthesis, by pointing out, in its Russian context, where the protagonists of the synthesis took the various parts of their theories of value and of prices.

The second part analyses the most relevant attempts at synthesis, with a substantial interest in Tugan-Baranovsky's initial impetus. In order to understand the latter, his system of political economy is reconstructed, at the heart of which his synthesis takes a central meaning. For this purpose, chapter 4 (Tugan-Baranovsky on capitalism and socialism) first retraces Tugan-Baranovsky's analysis of the capitalist mode of production from his theory of crises and cycles to his analysis of Russian industry. In parallel, his reconsideration of Marxist political economy, to which he first subscribed, is retraced up to his rejection of Marx's notion of value. Then, starting with the background supplied by his reflections on utopia and science in his historical study of socialism, it evaluates Tugan-Baranovsky's positive theory of socialism, in which economic planning takes place according to his synthetic theory of value and prices. Chapter 5 (Tugan-Baranovsky's synthesis) retraces the development of Tugan-Baranovsky's synthesis and shows that his analysis of the gap between value and prices provides the key notion of his economic typology between capitalism and socialism. Chapter 6 (The mathematicians' syntheses) analyses the evolution of Tugan-Baranovsky's initial synthesis at the hands of the first generation of Russian mathematical economists (Dmitriev, Bortkiewicz, Shaposhnikov, and Yurovsky). Particular attention is given to Shaposhnikov and Yurovsky's attempts, offering the opportunity to conduct the story of the Russian synthesis up to its very end. The conclusion evaluates this whole episode.

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