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VOLUME 7, ISSUE 2, AUTUMN 2014

TABLE OF CONTENTS

PHD THESIS SUMMARIES

Erasmus Journal for Philosophy and Economics,
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Ricardo's discursive demarcations: a Foucauldian study of the formation of the economy as an object of knowledge

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Abstract: Set against previous attempts to grasp the work of British political economist David Ricardo on a theoretical and methodological level, this article explores the emergence of the 'economy' in Ricardo's *Principles of political economy and taxation* (1817) from a Foucault-inspired perspective on the formation of objects of knowledge. Several distinctions (or 'discursive demarcations') are brought to the fore with which Ricardo sought to determine the boundaries of political economy, such as that between natural economic processes and artificial interventions; between long-term and short-term trends; or between different kinds of conflict. Taken together, the discursive demarcations examined in this article contribute to the formation of the 'economy' as an object of knowledge, make specific theories possible, and enable the use of a particular method.

Keywords: David Ricardo, political economy, objects of knowledge, Michel Foucault, discursive demarcations, markets, governmentality

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The idea that all 'economic phenomena' are part of a distinct domain governed by its own laws and regularities only began to take shape in the second half of the 18th century. Before that time, there were numerous treatises devoted to phenomena we now consider to be economic—e.g., consumption and commerce, wages and wealth, and so on—yet, none of these were "grounded in the assumption of an autonomous social order" (Firth 2002, 40). In many economic discourses, the government's role is integral to maintaining order in economic life. For instance, the famous Italian penal reformer, Cesar Beccaria, proposed that a legislator ought to keep interest rates, as well

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as the price of labour and transportation costs, down. In a lecture on public economics delivered at the Palatine school in Milan in 1771, he predicted that chaos would ensue if such economic policies were not enforced by an enlightened despot (see Harcourt 2011, 65-68).

Although it is futile to look for the origin of economics in the text of a single author, we can trace the emergence of an overarching conception of the 'economy' in the work of 18th century French physiocrats, and subsequently, in that of Scottish moral philosophers. The physiocratic movement began as a writing workshop with François Quesnay and a small number of disciples at the Court of Louis XV in Versailles, and was later displaced to Paris where the new institution of the Salon provided the *économistes* with ample room for discussion (see Charles and Théré 2011). Both theoretical and applied works of the physiocratic movement were predicated on idea that a 'natural order' ruled the economic activity of a country. However, such a natural order still required government involvement in order to secure a nation's wealth and power—but not of the interventionist kind proposed by Beccaria. Quesnay claimed that an 'economic government' should provide the institutional structures to enable each individual to pursue his own interests, while at the same time protect agriculture as the eternal source of economic growth (see Steiner 2002, 100).[1] Amidst the intrigues of French court society, physiocratic doctrines began to spread to a wider audience. As a tutor to the Duke of Buccleuch, Adam Smith visited France in the mid-1760s where he became acquainted with members of the writing workshop. More than a decade later Smith published his misgivings with the teachings of the physiocrats in *The wealth of nations*; and subsequently this gave impetus to the British tradition of political economy (see Harcourt 2011, 79-85).

To provide a chronological account of the concept of the 'economy' as an autonomous entity would be beyond the scope of a single article. Therefore, this article ventures an alternative approach by commencing with the provisional end point of its development, which is to be found in David Ricardo's *Principles of political economy and taxation* (1817). In the history of economics, David Ricardo is regarded as one of the 'founding fathers' of the discipline. At a time when economics was still in its infancy as a science, Ricardo's *Principles* was instrumental

[1] The freedom to act upon your own interest on the market did require a very strict adherence to political authority. Quesnay's 'natural order' was bound up with a 'legal despotism' wherein a monarchy both hereditary and absolute was to punish all deviancy from the laws that made commercial society blossom, see Harcourt 2011, 94.

in giving political economy a distinctive profile, which led to fierce intellectual and political debates in the 1820s (see Blaug 1958, 44-45; Thompson 2002). By carefully examining Ricardo's 19th century classic in political economy, I will articulate a number of 'discursive demarcations' that are also found—to a greater or lesser extent—in 18th century physiocracy and political economy. I will show then how these elements together contributed to the formation of the 'economy' as an object of knowledge.

My epistemological focus on discursive demarcations is inspired by the French philosopher and historian Michel Foucault. The formation of objects of knowledge was central to Foucault's work in the history and philosophy of the human sciences. In *The archaeology of knowledge*, for instance, he said that "it is not enough for us to open our eyes, to pay attention, or to be aware, for new objects suddenly to light up and emerge out of the ground" (Foucault 1972, 44-45). In numerous studies he has shown how certain aspects of human action or 'being' were actively turned into an object up for grabs for further scientific inquiry, e.g., madness, illness, perverse behaviour, delinquency. Before elaborating upon the Foucauldian epistemological framework, however, I will take a closer look at two different ways in which Ricardo's work can be analysed. On the one hand, his writings can be examined on the contextual basis of particular economic theories; on the other hand, his work can be examined on the basis of contemporary interpretations of his scientific methods.

First, on the contextual basis of his theoretical work, David Ricardo has borrowed, developed, and refined a range of specific economic theories—these include: a theory of value; a theory of the relationship between the fast growth of the population and the slow increase of food; a theory about the equal rates of profits that spring from different capital investments; and a theory of the coming into being of rent (see Blaug 2003, 85-142). These theories, in turn, can be contrasted against those of his predecessors, contemporaries and successors. For instance, Ricardo's labour theory of value could be evaluated against the utility theory of value of Jean-Baptiste Say. It could then be shown that such theoretical differences are imbedded in their respective theories of the distribution of income (see Gehrke and Kurz 2010, 465-476). In similar vein, Keynes's criticism of Ricardo could be attributed to divergences in the classical theory of interest and a modern, Keynesian theory of interest (see Andrews 2000). When examined on this contextual basis, Ricardo's *Principles* is essentially a collection of theories that can be compared and contrasted against enumerable others.

Alternatively, on the basis of Ricardo's methodological approach, historians and philosophers of economics have called attention to other features of his work. Ricardo's scientific method combines different theoretical propositions to form a deductive framework thus articulating particular economic processes and tendencies (see Redman 1997). By systematically thinking through these principles and their consequences, Ricardo was able to sketch a grand narrative of the allocation of global productions among the three classes of community. As recent interpreters of Ricardo's method make clear, the stress on his deductive methodology must not be pushed too far. As an experienced and highly successful broker on the London stock exchange, a landlord with agricultural duties, and a keen observer of contemporary politics, Ricardo was also a man informed by empirical inquiry (see Morgan 2005). Being a financier and a Member of Parliament, Ricardo had access to different sources of information such as newsletters from 'the city', business associates, taxation reports and parliamentary reports. Given his heavy involvement in business and government, Ricardo was well informed about current economic events and could equally be seen as an "empirical economist" (Davis 2002).

Similar to his economic theories, Ricardo's scientific methodology may be compared to the methods of other political economists. Because of their friendship and correspondence, contemporary scholars focus on the potential differences between Ricardo's methodology and that of Thomas Malthus (see Cremaschi and Dascal 1996). With regard to empirically minded economists, including Malthus and the Cambridge inductivists, Ricardo put far more emphasis "on logical deduction as a means of validating theories" (Redman 1997, 284 46n.). The contrast between different economic methodologies subsequently led to the question of the influence of earlier economic writers on Ricardo's way of practicing political economy. The writings of Scottish philosopher Dugald Stewart, for instance, might have influenced Ricardo via his pupils: Francis Horner—an influential writer for the *Edinburgh Review*—and James Mill—a close friend of Ricardo's (see Depoortère 2008). Furthermore, there are differences in opinion about the relationship between Ricardo's method and his alleged religious convictions (see Cremaschi and Dascal 2002; Depoortère 2002). Finally, current scholars tend to focus on the critical reception of Ricardo's economic method—for instance by William Nassau Senior (see Depoortère 2013).

These theoretical and methodological approaches convey how Ricardo's *Principles* led to the development of an economic science; and this appears to fit nicely with contemporary categorizations of economics—after all, science can be taken as the combination of theory and method (see Zeuthen 1955; Ekelund and Hebert 2007; Hollander 2010; Schumpeter 2011). Nevertheless, an analysis based exclusively on theory and method would have serious drawbacks. While such analytic approaches may provide interesting insights into Ricardo's work, they would fail to address the formation of the concept of the 'economy' as an object of knowledge. This is problematic, for the development of theory and the application of method presupposes the existence of an 'object': the 'object' about which knowledge is learned.

The exclusive attention that has been paid to theory and method in the history and philosophy of science thus obscures the question as to the constitution of the economy as a distinct entity. In the words of Margaret Schabas, "hardly any scholars have asked how Ricardo conceived of an economy or even if he perceived such a construct" (2005, 104). According to Schabas, Ricardo did cultivate the concept of an 'economy' as an "integrated set of relations" (2005, 119-120). This would suggest that the economy is a domain where humans are inextricably bound together by market processes. Therefore, in such a "human economy" the ties linking economic phenomena with agriculture, which were prominent in the writings of Quesnay and the physiocrats, became less important (Schabas 2005, 120).

Although Schabas's analysis illuminates the broad transformation of economic science during the 18th and 19th centuries, her use of notions such as 'conceiving' and 'perceiving' seems to suggest that Ricardo somehow grasped the concept of the 'economy' prior to the actual formation of a discernable object of knowledge.[2] As a consequence,

[2] Another general feature of that transformation is Ricardo's downplaying of morality and religion by skirting the "broader question of human motivation and moral agency" (Schabas 2005, 103). Up until Ricardo's *Principles*, and even thereafter, political economy was part of a broader framework of moral philosophy and religious thought. In the scholarly debate on Adam Smith it is considered an affront to read *The wealth of nations* while totally ignoring his *Theory of moral sentiments* and *Lectures on jurisprudence*, see Winch 1996, 21-22, 95. For Smith, these projects were clearly related even in the absence of explicit cross-references. Other contributions to political economy had a profound religious take on economic issues, see Maas 2008. Malthus's doctrines of population and rent, for instance, were intertwined with the religious notion of divine providence; to draw a rigid distinction between his economic and his

it remains unclear exactly how the formation of that economy comes about in Ricardo's work. A Foucauldian analysis of the formation of objects of knowledge can be helpful in addressing this epistemological lacuna. In the following section, I will discuss Foucault's ideas on the formation of objects of knowledge in more depth and include a brief outline of his own attempts to come to terms with economics. In the main subsequent sections, I present a series of 'discursive demarcations' with which Ricardo tried to determine the boundaries of economic science. In the final section, I will show how these demarcations contributed to the formation of the economy as an object of knowledge, and then return to what the Foucauldian framework adds to the focus on Ricardo's theories and methods.

DISCURSIVE DEMARCATIONS

Foucault's reflections on economic science have recently received some attention from historians and philosophers of economics. Unfortunately, that attention has been restricted to clarifications of Foucault's own position vis-à-vis other authors from the philosophy of science—not in the continuation of his philosophical project. For instance, Kologlugil (2010) compares Foucault's archaeology of knowledge against the Western tradition of epistemology and modern strands of postmodern theorizing; and Vigo de Lima (2010) similarly analyses the archaeology of the human sciences in a book-length study of Foucault's analysis of economics. On the other hand Ryan Walter, in response to de Lima's analysis, concludes that Foucault's schemas and conceptual apparatus should not be taken as a set of rules or a definitive body of thought, but instead "need to be put to work, revised and developed" (2012, 110). In my view, this entails a Foucault-inspired approach by which specific concepts and themes are extracted from his work and applied to epistemological problems. It is this approach that I endorse in this paper. Therefore, I will first briefly recapitulate Foucault's twofold investigation of economic science and then give an outline of one such theme: the formation of objects of knowledge.

religious views would be inappropriate, see Winch 1996, 238, 349. For Richard Whately, political economist and Archbishop of Dublin, the ultimate explanation of economic misery was to be found in the vices inherent in human nature, not in the sound principles of political economy, see Vance 2000, 192. Set against this background, Ricardo's economic theories may strike the modern reader as particularly secular and free of moral connotations.

In Foucault's *The order of things*, economics is one of the three principal disciplines from the human sciences—together with linguistics and biology—that is analysed in an archaeological manner. The first feature of Foucault's archaeology of knowledge concerns humans' perspective to the outside world, that it is permeated by a "fundamental way in which it sees things connected to one another" (Gutting 1989, 139). This primordial experience of the order of things—or 'episteme', as Foucault called it—determined how the objects of these sciences could appear to those who performed scientific work. The second feature, which is directly related to the first, is that the experience of linguistic, biological, and economic phenomena, is subject to dramatic change from one historical era to the next.

Concerning the historical transformations in economic science, Foucault placed great emphasis on the differences between 18th century analysis of wealth and 19th century political economy. In this way, the work of Ricardo could only be understood against the background of the historical shift that took place around the year 1800 (Foucault 1994, 253-263). By being located at the beginning of the modern *episteme*, Ricardo could write about aspects of the economy that his predecessors were unable to. For example, he derived a characterization of labour as the ultimate determinant of the value of goods; he concluded that scarcity was a necessary feature of economic life; and he foresaw long-term developments in the production, consumption, and distribution of commodities. Thus, on this re-interpretation of Ricardo's theories of economic processes and events, Foucault determined that understanding the economy, as an object of knowledge, was dependent upon the particular *episteme* that ruled knowledge production in the modern era.

In 1978-1979, after a decade-long hiatus, Foucault's lectures at the Collège de France were fully devoted to economic science. In accordance with his increasingly political interests, economics re-emerged as crucial to a transformation of governmental reason or 'governmentality' (Foucault 2000b). In the lecture series preceding *The birth of biopolitics*, Foucault had started to sketch a history of the modes of governance, encompassing a variety of sources including political philosophies, religious treatises, and economic writings. From a governmental perspective, the intellectual tradition that ranged from physiocrats to David Ricardo implied a turning point in the history of political thought and action. The role of the 'economy' as an object of and as an alternative to political intervention was crucial in this regard. Foucault argued in much detail that "the market" had transformed from a "site of jurisdiction" into a "site of veridiction" (Foucault 2008, 32). First, by "site of jurisdiction" Foucault meant that politics were required to provide a verdict on economic events, for instance on the supposed unjustness of the price of certain commodities exchanged on the market. However, by "site of veridiction", he implied that the market was constituted as a natural realm with its own laws and regularities in terms of which one could evaluate the costs and effectiveness of political action. The transformation of Western political thought was thus entangled with a new conception of the economy.

In each case, Foucault's focus on the formation of the objects of economic science was to convey how fundamental change depends either on the means of production of knowledge, or alternatively on the nature of political intervention and non-intervention. However, neither of his accounts are satisfactory. In Foucault's archaeology of the human sciences all traces of human agency are eliminated from scientific inquiry. In this way, he depicts knowledge production as a process wherein scientists are mere mediators between stratified layers of *episteme* and the surface knowledge. This means, first of all, that there is no longer a place for the active role of scientists in shaping the object of inquiry. Moreover, it is difficult to conceive of a viable explanation of the whole process because the transformation is both sudden and beyond the grasp of any the participants. Finally, the rift between 18th and 19th century analyses of wealth seems to prohibit a more gradual emergence of the economy as an object of study.

In comparison with his archaeology of economics, Foucault puts more emphasis on the gradual emergence of the economy as an object of knowledge in the history of succeeding rationalizations of government (see 2008, 27-74; 2009, 333-362). This time, however, his interest in understanding the nature and transformation of modes of governance is stronger than his interest to explicate the formation of the object that provided leverage for that transformation. As a new conception of the economy played a pivotal role there, one might expect a detailed account of its formation. Unfortunately, it remains unclear how the 'economy' or the 'market' actually became a site of veridiction in the hands of 18th and 19th century economists. In fact, when it comes to this question Foucault merely points to "a number of economic problems being given a theoretical form" (2008, 33) as well as to the "discovery of the existence of spontaneous mechanisms of the economy" (2008, 61).[3]

Even though Foucault's own analysis of economic science is not entirely convincing, I do consider the formation of objects of knowledge an important epistemological theme. Moreover, I argue that Foucault's own work holds the key to a more satisfactory approach to the emergence of new epistemic objects. The most explicit and constructive account of the formation of objects of knowledge is found in *The archaeology of knowledge* (see Foucault 1972, 40-49). Although his reflections on archaeology were meant to elucidate the method used in previous historical and philosophical studies, they actually contain a far more dynamic picture of knowledge production than the analysis of economics in *The order of things*.

According to Foucault, the formation of an object of knowledge is entangled with the task of a scientific discipline, to find "a way of limiting its domain, of defining what it is talking about, of giving it the status of an object—and therefore of making it manifest, nameable, and describable" (1972, 41). What this means is that, on the one hand, scientists have to define the object in question. Only after a positive investigation could it become manifest as an object available for further analysis. However, on the other hand, an attempt to give something the status of an object has a negative corollary in that these scientists simultaneously have to limit the domain of inquiry. In doing so, certain elements are selected for scientific study, and others are, by necessity, ignored. So what are the criteria according to which "one may exclude certain statements as being irrelevant to the discourse, or as inessential and marginal, or as non-scientific" (Foucault 1972, 61)?

This dynamic endeavour of defining one's object and limiting one's domain can be defined as the problem of demarcation. This problem does not exist between science and non-science, but exists between the attributes that belong to an object and that do not. I introduce the term 'discursive demarcation' to refer to such attempts to determine the boundaries of the object within a particular scientific

[3] Again, this paper sides with Ryan Walter's (2008, 95) remark that "the emergence of the economy has never been specified" in the literature on governmentality. My paper, though, takes a different view on the way it should be specified. Whereas Walter stresses the constitutive role of notions of class interests and wealth, I doubt whether the mere introduction of these two notions is sufficient to constitute 'the economy' as Ricardo demarcated it.

discourse.[4] Each of these discursive demarcations is concerned with a particular aspect of what is either central or peripheral to the object of knowledge. In the economic discourse of David Ricardo we can discern five such demarcations: First, Ricardo sought to define the proper time span of economic analysis by distinguishing the *short term* chaos of fluctuating prices from the more stable developments in the *long run*. Second, he differentiated between elements that were *intrinsic* to economic processes and elements that were merely *contingent* upon them. Third, he distinguished between the *natural* course of events and *artificial* policy measures. The fourth demarcation is concerned with the separation of the *fluidity* of economic processes and the *force* of political interventions. And fifth, Ricardo contrasted an *economic* dimension of conflicts with a *socio-political* one. The elements that are of primary importance to Ricardo's political economy can be determined on the basis of these five discursive demarcations.[5]

ON SHORT TERM FLUCTUATIONS AND LONG TERM TENDENCIES

The first discursive demarcation that is found in Ricardo's *Principles* has to do with the time span that he considers appropriate for economic analysis. I will make this conception of time span explicit by focusing on his account of changes in prices, profits and rents.

Ricardo begins the discussion of economic change by distinguishing two different types of commodities. First, there are commodities "of which there exists a limited quantity, and which cannot be increased by competition". These commodities "are dependant for their value on the tastes, the caprice, and the power of purchasers" (Ricardo 1996 [1817], 135). For instance, the value of a bottle of wine "of a peculiar quality,

[4] Looking at the history of economics through a Foucauldian lens is not equivalent to spotting discursive demarcations in the work of key historical figures. At least it should be acknowledged that one crucial issue is omitted here, namely the complex relationship between the development of knowledge and the wielding of power that came to dominate Foucault's writings in the 1970s, see Foucault 1980. But even though discursive practices made way for non-discursive ones at that time, the detailed account of the 'delinquent' or 'dangerous individual' as new objects of knowledge in criminology and psychiatry shows that the earlier theme is still very much present, see Foucault 1995; Foucault 2000a.

[5] Of course, these five discursive demarcations do not exhaust the list of issues that a Foucault-inspired analysis of Ricardo could bring to the fore. They do, however, cover a number of important aspects of the latter's epistemological efforts. For each of these demarcations is linked to a fundamental feature of Ricardo's work: timeframe, causality, nature, forces, conflict. Taken together, moreover, they offer a fairly substantial account of his overall conception of an economy that additional demarcations could subsequently either extend upon or nuance.

which can be made only from grapes grown on a particular soil" depends solely on the wealth and willingness of those who desire to possess it (1996 [1817], 18). Opposite of rare and unique products are commodities that can be increased by production; however, manufactured commodities are also subject to influence by the tastes of consumers (p. 183). For instance, a change in fashion can cause an increase in the demand for a certain product—e.g., silks—and a decline in the demand for another—e.g., woollens (p. 63). On the common principle of supply and demand this can affect both commodities, precipitating a rise in the price of the former and a fall in the price of the latter. The difference between these two types of commodities is thus not a matter of capricious taste, but contingent upon the effect of demand on supply. Rare bottles of wine will continue to sell for the same high price as long as wealthy people are willing to pay for a particular *terroir*. The high price of silks, however, may return to the previous rate if capital drawn to this highly profitable sector precipitates a rise in its supply, thus equalizing its demand.

Whenever Ricardo discusses economic fluctuations of the latter kind, supply and demand effects, he states that their temporal scope is limited: price changes are due merely to "temporary effects" (1996 [1817], 20) or "temporary reverses" (pp. 82-83); they will remain with us for only "a limited period" (p. 118) or "a very limited time" (p. 183); they take place in "periods of comparatively short duration" (p. 202) or during "an interval of some little duration" (p. 268). In sum, when it comes to the demand for easily reproducible commodities, deviations from the average price are periodic but *short-lived*.

Over and against this mode of short term fluctuations, one finds a mode of economic change that takes place at longer intervals. Here, Ricardo speaks of the "natural progress of wealth and population" (1996 [1817], 53), of the "progress of society and wealth" (p. 83) and of the "progress of nations" (p. 185). This long-term narrative can be explained on the basis of more general laws and tendencies. For example, an increasing population cannot be sustained by only the most fertile lands; in due time it will become necessary to cultivate lands of inferior quality. As soon as the most fertile lands become scarce, landlords will be able to demand a higher price for it. Consequently, less fertile land will have a lower return on invested capital, meaning that additional labour and machinery will be necessary to cultivate the same amount of raw produce. Further, when additional labour is required to produce basic necessities their value will rise proportionally. As the value of basic necessities increases, the labourer's wage must also increase in order to afford these necessities for survival. In turn, the long-term increase of both rent and wage has consequences for the capitalists in the form of a decreased profit-margin. That is, if the dual tendency is not checked by improvements in machinery and discoveries in agricultural science, profits will gravitate towards the point where the investment of capital yields nothing in return (1996 [1817], 83).[6] By piecing together these processes Ricardo conceives of a *long-term* tendency, which predicts how society progresses over time—he concludes that,

> we have shown that in early stages of society, both the landlord's and the labourer's share of the value of the produce of the earth, would be but small; and that it would increase in proportion to the progress of wealth, and the difficulty of procuring food (Ricardo 1996 [1817], 77).[7]

By distinguishing economic phenomena according to these temporal indices, Ricardo declares that each can be studied without taking account of the other:

> Having fully acknowledged the temporary effects […] we will leave them entirely out of our considerations whilst we are treating of the laws which regulate natural prices, natural wages, and natural profits, effects totally independent of these accidental causes (Ricardo 1996 [1817], 64).

Ricardo thus singles out the laws regulating long term economic tendencies while leaving short term economic fluctuations out. Not only does this highlight the importance of law-like tendencies, the quote above signals two other discursive demarcations that are crucial to Ricardo's project of delimiting objects of economic inquiry. First, these temporary effects are linked to 'accidental causes' that political economy equally leaves out; this is the subject of the next section.

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[6] For an account of the knowledge Ricardo might have had of these checks, see Morgan 2005.

[7] This is also the temporal level were Ricardo can distinguish the different stages society can be subdivided in. References to the "early stages of society" like this one, serve a strategic purpose: they make it possible to open up a time frame spanning ages, if not millennia, turning the fluctuation in prices due to the caprice of taste into tiny deviations from a more constant price that can be brought under general laws, see Ricardo 1996 [1817], 18, 27, 38.

Second, it shows that the adjective 'natural' is important when it comes to the fluctuations in prices, wages and profits that are central to political economy; this will be the subject of the third section.

ON NECESSITY AND CONTINGENCY

With regard to causality, the main issue at stake is the distinction between causes that are deemed to be 'necessary' to economic processes and those that are considered merely 'contingent' or 'accidental'. In order to show the difference between them, I will first discuss several examples of both types of causes before turning to the general significance of this discursive demarcation.

The first cause that falls under the heading of 'contingency' is contained in the aforementioned taste and caprice of consumers. The sudden emergence of a preference for a certain product may have a significant effect on its price. The price of a manufactured commodity will rise when producers are unable to cope with the rising demand immediately (Ricardo 1996 [1817], 183). Furthermore, Ricardo also states that taxation may have effects of a contingent kind, destroying "the comparative advantage which a country before possessed in the manufacture of a particular commodity" (p. 183). A new duty will oblige producers to raise the price of a commodity beyond the ordinary, shifting the balance of trade between nations. Thirdly, war between nation states can be categorized as set of contingent influences with regard to economic fluctuations. The insecurity of war brings many difficulties along with it; manufacturers may be forced to refrain from exporting their products, or alternatively, may be forced to produce those products which are incapable of being imported. Finally, Ricardo states that he leaves "the accidental variations arising from bad and good seasons" out of consideration when discussing the price of corn (p. 79).

When Ricardo discusses his core economic principles and their consequences, the emphasis shifts from what is accidental and mere contingency to what is necessary, determined and inevitable. A few examples will give the reader an idea of what this category consists of. The first of these examples concerns the labour theory of value, i.e., the doctrine that "it is the comparative quantity of commodities which labour will produce, that determines their present or past relative value" (1996 [1817], 21). This principle has two implications: it means that for the cost of maintaining the means of production, along with the subsequent increase in value of basic necessities, the price of labour "necessarily rises" (1996 [1817], 37 1n.); but it also means that if the introduction of machinery enables the cultivator to obtain his product at a lesser production cost, this "will necessarily lower its exchangeable value" (p. 109). Similarly, if machinery used for processing raw cotton is rendered more efficient "the stockings would inevitably fall in value" (p. 27). Thus, the amount of labour required to produce some commodity determines its exchangeable value in a necessary and inevitable way.

Second, Ricardo's so-called "principle of rent" illustrates how economic production may be causally deterministic. He states:

> Is it not, then, as certain that it is the relative fertility of the land, which determines the portion of the produce, which shall be paid for the rent of land as it is that the relative fertility of mines determines the portion of their produce which shall be paid for the rent of mines? (Ricardo 1996 [1817], 229).

When it comes to mining, the poorest mine yields the usual profits of stock and all that the other mines produce more than this, "will necessarily be paid to the owners for rent" (Ricardo 1996 [1817], 58), Whereas in the case of agriculture, there is a determinate relationship between the unequal fertility of plots of land and the unequal amount of rent that must be paid to the landlord. The rent received by the landlord will decrease if the quality of different plots of land becomes homogeneous. However, a subsequent differentiation in the quality of these plots "necessarily produces an opposite effect" and tends to increase rent values (p. 56). Finally, Ricardo identifies deterministic relationships between income levels and social classes, as well as between capital and productivity. With regard to the former, he states that that "whatever increases wages, necessarily reduces profits" (p. 82); with regard to the latter, he claims that "in proportion as the capital of a country is diminished, its productions will be necessarily diminished" (p. 106).

On the basis of the examples given above it is shown that there is a tension between events and phenomena that are 'accidental' or 'contingent', and tendencies and laws that are 'necessary', 'determined' and 'inevitable'. The first three phenomena that Ricardo described as contingent or accidental—i.e., taste, taxation, and war—are entangled with human judgement and decision-making; for this reason they are difficult to categorize in terms of universal (deterministic) laws. Taste is dependent upon fashion, taxation is dependent upon the influence of political deliberation, and war is dependent upon international relationships and conflicts. With regard to the fourth phenomenon of accidental seasonal change, whether or not the seasons provide the farmer with an abundant crop depends upon the forces of nature—not upon economic forces. Again, this tension between contingency and necessity is hierarchically structured in that the contingencies are not properties of the object of economic science, whereas necessary relationships are part of the nature of economic processes.

By combining the first two discursive demarcations, the effects of long term economic processes can be seen as determined and inevitable, and can therefore be analysed independently of short term fluctuations, which are caused by a diverse range of accidental features.

ON NATURALNESS AND ARTIFICIALITY

In an earlier passage (located in the conclusion of the section on time-spans), Ricardo declared that laws that regulate the natural prices, natural wages and natural profits are the proper objects of economic inquiry. The adjective 'natural' indicates the third discursive demarcation: it distinguishes proper objects of economic inquiry from other objects that may be deemed 'artificial'. Below, I will describe how Ricardo conceives of the notion of artificiality before returning to the central theme of the formation of the economy as an object of knowledge.

The distinction between the natural and the artificial is first presented by Ricardo as a characterization of certain restrictions that operate in economic life. When he discusses international trade, for instance, he states that "the very best distribution of the capital of the whole world […] is never so well regulated, as when every commodity is freely allowed to settle at its natural price, unfettered by artificial restraints" (Ricardo 1996 [1817], 120). A closer look at these artificial restrictions reveals that taxation is to blame. A tax imposed on raw produce raises the price of commodities, thus preventing them to reach their 'natural' level; hence the imposed tax creates an 'artificial' price. However, the converse is also true. For instance, when the price of corn is diminished, it may have to do with an alteration in the 'natural' value of corn. In such a case, the change can be viewed as a consequence of some mitigating factor, e.g., that less labour is necessary for its production. Yet when price decreases are precipitated by a subsidy, Ricardo attributes this not to natural but to artificial conditions. That is, a fall in the price of corn due to the fact that its producer receives a bonus is conditioned by "artificial means" (1996 [1817], 224).

On an aggregated level, the adjective 'artificial' is subsequently used to characterize the overall effects of these policy measures. On that level, a whole country is said to be in an "artificial situation" as a result of a "mischievous policy of accumulating a large national debt" (Ricardo 1996 [1817], 168). Heavy taxation on luxuries, income and property is then necessary to pay off such debts. These taxes, in turn, may motivate the taxpayer to "withdraw his shoulder from the burthen" (p. 172). Finally, members of the capitalist class may even be tempted to move their capital to other countries as an ultimate consequence of this "artificial system" (p. 172).

Similar to the previous discursive demarcations, there is a clear tension between what Ricardo considered to be natural and artificial concerning economic processes. In one way or another, the examples above show that *artificiality* is always dependent upon political action. *Naturalness*, on the contrary, depends upon actions of market participants and therefore upon the effects of market processes. The state thereby becomes an actor that artificially intervenes in a domain with its own natural laws and tendencies. Thereby, the state is no longer an integral part of the economy but something that stands outside it. The third discursive demarcation thus distinguishes what is natural from what is artificial and excludes the latter from further economic inquiry.

ON FLUIDITY AND FORCE

The fourth discursive demarcation concerns the use of metaphor in economics in the 18th and 19th centuries. Initially biological, or more precisely, anatomical metaphors were used to explain economic systems. The French physiocrats spoke of the cycle of production, distribution and consumption in terms of the circulation of blood in the human body (Schabas, 2005, 46-48). With some reservations, Jean-Jacques Rousseau compared the internal coordination of a man's body to society and economy at large: what if we consider public finance the blood of the body politic, commerce, industry, and agriculture its mouth and stomach, and sovereign power its head (Rousseau 1987 [1755], 114)? Even Adam Smith incidentally made use of bodily metaphors to elucidate the potentially catastrophic effects of the monopoly of colonial trade in terms of overgrown vital parts and artificially swelled blood-vessels (Smith 2000 [1776], 653-654). Similarly, in Ricardo's work we come across the use of language akin to these anatomical metaphors; yet his are sufficiently distinct to deserve their own discussion.

In classical political economy, there is a strong tendency to treat the economy as a domain where goods and services move in a fluid manner.[8] In the *Principles*, metaphors of fluidity have become part and parcel of the depiction of economic mechanisms and processes. This can be understood in two distinct ways: first, the fluid movement of economic phenomena is depicted with such terms as "flow" and "flux". Ricardo speaks of the "natural flow of capital" (Ricardo 1996 [1817], 37); the "flow of gold" (p. 83); the risk of a "sudden influx of corn" for which farmers expect to be compensated (p. 89); the "influx of precious metals" (p. 107); and the lack of effect on the rate of profit from an "influx or efflux of money" (p. 88). Second, metaphorical fluidity also describes the courses phenomena take: the "channels" where the funds for the maintenance of labour have been diverted from (p. 177); the "stream of trade" which gives a certain impetus to money; the "current of money" (p. 91); the "tide of capital" that comes to a pause when rates of return on different employments of capital converge (p. 205). These metaphors are used to suggest that the market domain is free from any inherent friction. The elements of economic life (capital, money, commodities, and people) are able to circulate freely.[9] What is here at one instance can be there at another—hence the emphasis on the immediacy of effects following from fluid changes in production, consumption and distribution.

In accordance with the previous discursive demarcations, we must identify an antithesis to the fluid metaphor in order to determine its role in the process of demarcation. Now, it is only in terms of the essential fluidity of economic life that Ricardo speaks of the obstacles that hinder the flows of labour, capital and money. In this way, taxes are an "obstacle" to the increase of general income when they prevent a

[8] Smith explicitly uses the metaphor of fluidity when he discusses mercantilism in terms of an unsuccessful attempt to dam up a stream of water. Based on the description of the problems of water management, he uses the mercantilist quest for a high gold stock in order to describe the latter in terms of the former: the power of gold is just as irrepressible as the power of water and the policy of restricting its exportation is in the end doomed to fail, see Smith 2000 [1776], 547-548.

[9] There are some instances where the general emphasis on fluidity is temporarily subordinated to very concrete economic problems, as is the case when he speaks of the difficulty of subtracting capital from the soil once invested, see Ricardo 1996 [1817], 133, 187. But these instances never get to play a major role.

beneficial exchange of property (1996 [1817], 108); war is an "obstacle" to importation of corn (p. 186); and the mercantilist attempt to secure a high gold stock an "opposing obstacle" to the exportation of precious metals (p. 220).[10] Furthermore, he sets 'force' over and against the lack of resistance that characterizes the economic realm, as is the case when he mentions the mercantile system "forcing capital into channels where it would not otherwise flow" (p. 102); or the limitless variation in exchange between countries "whenever the current of money is forcibly stopped" by law (p. 218). Lastly, one can only speak of 'disturbance' if the economic domain is one of inherent harmony and equilibrium, as is the case when a tax "occasions a disturbance of the equilibrium of money" (p. 101).

Thus, the fourth discursive demarcation concerns the 'viscosity' of the economic domain. It distinguishes the solid obstacles, forces and disturbances found in politics from the natural flow of economic phenomena. In terms of the formation of objects of knowledge, the demarcations of fluidity and force broaden the divide between the economy (as a market) and the state (as an independent political entity). Not only are economic processes of a natural kind, they are also characterized by a lack of friction; and not only is the state an artificial agent, it is also an agent that imposes obstacles upon these natural economic processes, forcing trade in unnatural directions and causing disturbances that would otherwise not have happened.

ON SOCIO-POLITICAL AND ECONOMIC CONFLICT

The fifth and final discursive demarcation regards the nature of conflict in the economy and society. Concerning the "harmony of interests", Ricardo is sometimes either praised or blamed for the emphasis he places upon conflict as an essential part of society, one which explicitly separates the different classes and their interests from one another (see Winch 1996, 353). For instance, in the chapter on machinery—which was not included until the third edition of his *Principles*—he illustrates that opposing interests of the labour class and capitalist class can be demonstrated by capitalists' use of machines to replace human labour. Labourers were in fact right to observe that the introduction of machinery might diminish the demand for labour:

[10] In fact, two of these passages are quotes from Jean-Baptiste Say; the characterization of government as an external 'force' might thus be a more common one in the literature of political economy.

> […] the opinion entertained by the labouring classes, that the employment of machinery is frequently detrimental to their interests, is not founded on prejudice and error, but is conformable to the correct principles of political economy (Ricardo 1996 [1817], 273).

Furthermore, in stark contrast to Adam Smith and Thomas Malthus, Ricardo states that "the interest of the landlord is always opposed to that of the consumer and manufacturer" (1996 [1817], 232). In the long run the landlord will benefit from the rising price of goods due to the increasing difficulty of production, while capitalists and consumers suffer the consequences.

Without diminishing the importance of this distinction between Ricardo and his predecessors, the role of conflict in Ricardo's treatise must be specified. As it turns out, not all kinds of conflict are essential to understanding economic processes. Ricardo's description of the emergence of rent is crucial in this regard. In the most well-known description, the scarcity of fertile land is the decisive factor for the emergence of rent. However, a closer look into the text reveals a far more ambiguous depiction of its genesis. Rent is "that portion of the produce of the earth, which is paid to the landlord for the use of the original and indestructible powers of the soil" (1996 [1817], 49). There were times, however, when land was free of charge and no one thus had to pay for its use. To account for the initial availability of free land, Ricardo roughly distinguishes between two different stages in the development of society. On the initial settling of a country, when there is an abundance of fertile land, only a small proportion of the land will have to be cultivated in order to supply the population with the necessities required for its subsistence. On the common principle of supply and demand, the boundless supply of land ensures that it bears no price; it is at every man's disposal and there is no private ownership of the land. In the second stage, however, fertile land becomes scarce due to the expansion of population, and this demands price and rent come into being (1996 [1817], 34).

Albeit, the transition from the one to the other is not unproblematic. In between these two stages of development there emerges the class of landlords who collect the rent. It is only in a footnote that Ricardo suggests—parroting the words of Jean-Baptiste Say—that a unique distribution and ownership of the land underwrites the formal possibility of rent. This uniqueness is due to the fact that the earth is the only agent of nature that "one set of men take to themselves to the exclusion of other; and of which, consequently, they can appropriate the benefits" (Ricardo 1996 [1817],47 1n.). In other words, at the threshold of the second stage the most fertile land must fall into the possession of a small minority of landowners powerful enough to enforce others to pay for the services rendered. Looking into further detail regarding the creation of rent, we see that it requires more than just the condition of scarcity; it also presupposes a specific allocation of land and a specific allocation of the power to enforce the emerging division of property.[11] The well-known economic explanation of rent thus hides another, socio-political explanation from view.

The fifth discursive demarcation is intended to define which forms of conflict are necessary for proper economic inquiry and also which forms of conflict are not essential to it. Concerning the role of conflict in Ricardo's *Principles*, it is now evident that there is a function for the regular clash of interests between economic classes, but not a function for conflicts that have led to the formation of these classes themselves. With regard to the formation of the economy as an object of knowledge, this demarcation ensures that long term and natural economic processes can be studied without the problematic issue of the legitimacy of the current divisions of property. The picture of a stable class structure thus keeps difficult normative and socio-historical questions at bay—questions that would have transformed Ricardo's reputation as a controversial yet respected writer into a far greater intellectual threat to the existing social and political order.

THE FORMATION OF THE 'ECONOMY' AS AN OBJECT OF KNOWLEDGE

Now that these five discursive demarcations in Ricardo's *Principles* have been brought to the fore, we can address the central theme of this article: the formation of the 'economy' as an object of knowledge. In *The archaeology of knowledge*, Foucault depicted the formation of objects of knowledge as a dual endeavour, which consists of defining

[11] As Keith Tribe (1978, 129) remarks: "The analysis of distribution does not concern itself with the origin of the possessions of these agents: it is as irrelevant to consider the source of the capital held by the capitalist as it is to question the title of the landowner to his land". However, contrary to Tribe, it is far from self-evident that this consideration 'falls outside the bounds of an economy'. It is precisely Ricardo's boundary work that makes it do so. In fact, the exclusion of class formation from economic analysis is one of Marx's central reproaches levelled at classical political economy, see Marx 1993 [1939], 81-111.

the object of inquiry and of excluding what is considered irrelevant, inessential or marginal to the inquiry. Ricardo's discursive demarcations are clearly part of such an epistemological endeavour. In each of these demarcations some things are selected as central to political economy while others are excluded as being unworthy of further economic inquiry.

In this section, I will zoom out from the details of the previous analysis of Ricardo's discursive demarcations. First, I will show how these demarcations together contribute to the formation of the 'economy' as an object of knowledge. Second, I will return to theory and method as distinct fundamental concepts that are used to understand what (economic) science is about, and show how the focus on the formation of objects of knowledge contrasts with the conception of Ricardo as a theorist and methodologist. Third, I will elaborate upon the relationship between Foucault's accounts of economics and my own; that is, I will state how a more fine-grained analysis of the formation of the economy as an object of knowledge adds to the frameworks of archaeology and governmentality. Finally, I will briefly reflect on the formation of objects of knowledge beyond the present applied framework, by asking: how can the study of discursive demarcations be extended in new directions?

Discursive demarcations in Ricardo's *Principles*

(positive)	**(negative)**
1. Long term	Short term
2. Necessity	Contingency
3. Naturalness	Artificiality
4. Fluidity	Force
5. Economic conflict	Socio-political conflict

First and foremost, the focus on discursive demarcations makes it possible to trace the composition and constitution of a new object of knowledge. This is shown by the first two demarcations, which shield the economy from exorbitant chaos and fluctuation. Ricardo's concept of the 'economy' develops in the long run according to its own necessary laws, unimpeded by events and processes that might interrupt its steady course. Diverse phenomena including tastes (preferences), war, taxation, and weather are considered to be merely temporary, accidental, and contingent and are therefore regarded as causally indeterminate. What remains is the slow but inevitable course of events that influences the distribution of the earth's produce amongst different classes of society.

The next two demarcations contribute to the separation of the economic domain from the political domain (i.e., state and government). First, the demarcation between naturalness and artificiality isolates politics from the market. According to Ricardo, the state is not an integral part of the economy; it is an external entity that artificially intervenes in a natural domain that has its own internal rules and regularities. Second, the distinction between fluidity and force further illustrates the differences between the economy and the state by presenting each domain its own level of viscosity. Given Ricardo's fluid metaphor, politics thus creates obstacles, applies forces and causes disturbances in a domain that otherwise contains no such hindrances or frictions. The economy is thereby understood to be something dynamic and flexible in the sense that commodities, capital, and labourers move freely from one sector to another.

The last distinction identifies the forms of conflict that are misunderstood in Ricardo's text. As it turns out, the conflict between the different social classes is taken to be an essential part of economic processes. Nevertheless, the formation of such social classes and the socio-political conflict this entails is external to economic analysis. In a positive sense, these discursive demarcations contribute to the formation of the economy as a natural and fluid domain wherein the different social classes and their conflicts are subject to certain necessary factors (laws, tendencies, regularities) that operate over longer time spans. In a negative sense, these discursive demarcations exclude as unimportant or unnecessary contingent and artificial factors that are appropriated by other social sciences; the emergence and reproduction of class divisions, processes of state formation and violent conflicts between nation states, the formation of taste and fashion and so forth are phenomena not of interest to economic science.

As I made clear in the introduction, Ricardo is currently conceived of as both an economic theorist who embraced, developed and refined a number of specific theories about production, consumption and distribution; as a methodologist he is regarded for certain approaches to doing political economy. The focus on the formation of the economy as a new object of knowledge makes it possible to cast these theoretical and methodological perspectives on Ricardo's *Principles* in a new light. First of all, some of the discursive demarcations discussed above pertain to the core of Ricardo's theoretical principles. Without the metaphor of fluidity, for instance, one of his central principles of political economy would be incoherent: the principle of equal profit rates presupposes that capital and labour move freely from the sectors of lesser profit to the sectors of greater profit. This is because profit rates will only normalize in a quick and frictionless way if economic processes are presumed to be fluid in nature. Moreover, Ricardo's theory of rent would be far more ambiguous were it not for the distinction between two types of conflict. The laws that regulate the progression of rent would then have to include the far less regular power-politics that has led to the current class divisions. If Ricardo gave his side-remarks on the appropriation of the earth more weight, the human laws that made land into private property would be equally part of economic inquiry. At least in these cases, what was presented as two basic principles of Ricardian political economy now emerges as an end point of epistemological work.

In addition to their relevance to theoretical principles, the discursive demarcations above also pertain to methodological inquiries. Notwithstanding that Ricardo was a keen observer of contemporary political events, the deductive method of reasoning is the dominant approach in his *Principles*. Such a method of reasoning, however, is instrumental only if the (economic) premises are clear and unambiguous. That is, deduction ceases to be explanatory (and therefore useful) if the premises are only loosely defined or if additional factors are not accounted for. Given the foregoing analysis, it is evident that the discursive demarcations are part of an attempt to keep ambiguity and interference out. One can deduce long term economic processes based on a small number of basic theoretical premises provided that contingent factors, such as war and weather, and artificial factors, such as government intervention, do not influence these premises (this also would include intrusion by temporal market fluctuations due to taste and human caprice).[12] What goes for theory thus goes for method. Instead of considering the method of deduction a basic feature of Ricardian economic analysis, he has to make room for

[12] There might be another interesting connection between Ricardo's method and the temporal demarcation, namely that of his preference for comparative statics in allowing the "permanent effects of changes" to reveal itself, see Milgate and Stimson 1991, 59.

a deductive style of reasoning by way of the discursive demarcations analyzed in the previous sections.

Having elaborated upon Ricardo's discursive demarcations and their relationship with his theories and method, we can now assess the above account of the formation the economy as an object of knowledge in a manner familiar to *The order of things* and *The birth of biopolitics*. At the beginning of this article, I stated that Foucault's own analysis of economic science and its history was lacking. Ricardo played an ineffectual role in Foucault's archaeology of economic knowledge in the sense that his work was presented only as a symptom of a fundamental shift in the way knowledge was produced. In the present study, I focused on Ricardo's active role in demarcating the object of economic inquiry—even if these discursive demarcations are not explicitly presented as such by the author himself. Thus, I maintain that we should consider the formation of objects of knowledge as an important epistemological theme, but without the concept of a deep layer of discursive rules that determine the production of knowledge.

Foucault's analysis of economic science was subsequently mainly directed towards its political significance in the history of governmentality. The new conception of the market brought to the fore by (political) economists was therefore the focal point of the emergence of a distinct liberal rationalization of government. The critical problem was that Foucault did not elucidate how the market actually became a site of veridiction, that is, a pulpit from which to judge the effects and effectiveness of government policies. However, by making the discursive demarcations explicit in Ricardo, we can show how the market was conceived as something natural and autonomous. Moreover, the extrication of the state from economic processes—by highlighting its alleged obstructive and artificial effects—further illuminates the governmental transformation that was analysed by Foucault. Consequently, the economy is no longer regarded as a domain in need of interventionist economic policies; having demarcated the non-natural political influences, it has become a domain with its own laws and regulations that leaves little room for beneficial state interference.

Of course, Ricardo's attempt to demarcate the economy as an object of knowledge did not set a precedent for the succeeding two centuries of economic science. Foucault's own *Birth of biopolitics* reveals a divergence from classical political economy in German ordoliberalism and American neoliberalism in the post-war period. Thus, in light of the subsequent developments in economics, how can we extend this reconfiguration of Foucault's formation of objects of knowledge in terms of discursive demarcations in new directions? I propose that there are, at least, two separate but related ways to do so.

One new line of inquiry would involve selecting further cases from the history of economic science to show how the academic economists responded to Ricardo's overall demarcation of the economy as an object of knowledge. After the general establishment of new objects of knowledge—e.g., economy, culture, and society—in the 19th century, economics became recognized as a separate academic discipline. For that reason, discursive demarcations have held a distinct disciplinary ring, and thus stand in need of an historical investigation of the origin and rivalry between disciplinary interpretations of the object of social scientific knowledge (see Heilbron 2004). Indeed, there are already some fine case studies of such instances of disciplinary demarcation. Lionel Robbins's intricate attempt to shield economics from history and psychology comes to mind here (see Maas 2009); the same can be said of Talcott Parsons, who attempted to enact a disciplinary division of labour between economics—the study of the *economic value* of things—and sociology—the study of the *social values* held by individuals and groups (see Velthuis 1999; Stark 2009, 7-8)

A second line of inquiry would draw connections between case studies such as these in a full-scale historical study of discursive demarcations. This is what I have attempted to do for the notion of 'incentive' in *Governing by carrot and stick: a genealogy of the incentive* (Dix 2014). Herein, I traced the shifts in the formation of the incentive as an object of knowledge from the end of the 19th century until the beginning of the 21st century. From the 1880s onward, American engineers were the first professional authority to demarcate the incentive as something that could be studied in a circumscribed manner. For them, studying the 'incentivization' of employees was synonymous with the analysis of and experimentation with different variants of piece wages. From the 1920s onward, the authority of the engineers was challenged by management scientists with a background in psychology, sociology, and anthropology. They developed different explanations for human behaviour and developed a set of alternative techniques to steer that behaviour in desirable directions. It took until the 1970s for a third authority on the use of incentives to emerge. In this third approach, mathematically trained economists abstracted from the interwar problem of human motivation and behaviour, and forged a formal link between incentives and information. Such a genealogy of discursive demarcations—with regard to incentives, in this specific case—not only told us something about some particularities in the history of economic science, but revealed how economic science came to demarcate the object(s) of inquiry as they are studied today.

CONCLUSIONS

In this article I have presented an account of the formation of the 'economy' as an object of knowledge in the work of David Ricardo. In particular, I have argued that there are five distinct discursive demarcations at work in the *Principles of political economy and taxation* (1996 [1817]). I have shown that, taken together, these demarcations enable Ricardo to determine the boundaries of his object of inquiry. In a positive sense, he selects the natural and law-like processes that determine the long term distribution of agricultural and industrial products among the classes of the community and the way conflicts between these classes are played out in a realm where goods, people, and capital move in a fluid manner. In a negative sense, Ricardo excludes short term and contingent fluctuations, artificial and disturbing government action, and socio-political conflicts from economic inquiry. By highlighting this dual process of inclusion and exclusion in Ricardo's *Principles*, I have made use of a theme drawn from Michel Foucault's archaeology of knowledge. With some reservations, his focus on the formation of objects of knowledge in the human sciences proved an interesting addition to the scholarly interest in Ricardo's theories and methods. Finally, I have put forward that a Foucault-inspired analysis of economics is not necessarily restricted to one particular account of political economy, but can be used more generally to trace the development of (disciplinary) demarcations over time.

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The Vienna circles: cultivating economic knowledge outside academia

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Abstract: This article examines the intellectual scenery of interwar Vienna. It argues that its central institution was not academia, but rather the circles ('Kreise'). The prominence of these circles can partly account for the creative outburst in the social sciences in interwar Vienna. The article also helps to explain the peculiar character of the knowledge produced in interwar Vienna which is just as much concerned with social and political issues as it is with more traditional scientific issues. The lack of formal institutions and the marginal position of the University of Vienna also had downsides. It caused uncertainty in terms of career prospects and professional identities, although the informal interaction within the circles full of rituals and alternative institutions could partly make up for this. The uncertain future for scholars ultimately contributed to the enormous wave of migration from Vienna, frequently even before the political situation became an acute threat.

Keywords: Austrian school of economics, Vienna circle, interwar Vienna, economic practices

JEL Classification: B13, B25, B53, Z13

Fin-de-siècle Vienna has been widely studied for the creative outburst in both the arts and the sciences (see, e.g., Johnston 1972; Janik and Toulmin 1973; Schorske 1980). And understandably so, just think about the abundance of contributions across an enormous breadth: in physics (Mach and Boltzmann), in psychology (Freud and Adler), in the visual arts (Klimt, Kokoschka, and Schiele) in music (Mahler, Schönberg, and Berg), in architecture (Wagner and Loos), in literature (Hoffmanstahl, Roth, Musil, and Zweig) and in cultural criticism (Kraus). In some of the sciences, however the more important period was the interwar period which has attracted less attention. In philosophy, the Wiener Kreis and Karl Popper shaped the interwar scene. In economics, Othmar Spann a

German romantic competed with at least three alternative approaches to economics: Austro-Marxism, Austro-liberalism, and the emerging mathematical economists. Hans Kelsen developed his pure theory of law, Hermann Broch wrote his most important works, and some of the artists mentioned above continued to contribute (Leser 1981). Intellectually Vienna continued to flourish. An obvious question that emerges from that fact is whether there was something peculiar about Vienna during that period.

Schorske's explanation of the outburst of the fin-de-siècle period has attracted most attention, although his complex argument is not easily summarized. Schorske argues that political liberalism never gained a strong foothold in Vienna, and therefore the bourgeoisie turned to culture as an alternative outlet. He furthermore suggests that the collapse of the moral order and the failure of political liberalism generated a tension which allowed the Viennese intellectuals to foresee as it were, the twentieth century (Schorske 1980).

Other commentators have emphasized the Jewish background of many of the contributors to this Viennese culture (Beller 1989; Wistrich 1996). Additionally we should not neglect the fact that the Viennese society, especially pre-WWI, was extremely unequal. The cultural (and political) elite was formed by a couple of hundred families who were often related by blood or through recent marriages. To give just one example, economists Böhm-Bawerk and Wieser were life-long friends, who attended the same prestigious gymnasium, later they both served in various political functions. Böhm-Bawerk later became minister of finance, and Wieser was appointed minister of commerce. Böhm-Bawerk also married Wieser's sister.[1] Or take Hayek's description of the personal relations in Vienna:

> I began to go through the list [of famous people from Vienna], and I found I knew almost every one of them personally. And with most of them I was somehow connected by friendship or family relations and so on. I think the discussion began, 'Did you know Schrödinger?' 'Oh, yes, of course; Schrödinger was the son of a colleague of my father's and came as a young man in our house'. Or, '[Karl von] Frisch, the bee Frisch?' 'Oh yes, he was the youngest of a group of friends of my father's; so we knew the family quite well. 'Or, Lorenz?' 'Oh, yes, I know the whole family. I've seen Lorenz watching

[1] For a more general discussion of the importance of 'families' in Vienna, see Coen 2007.

ducks when he was three years old'. And so it went on (Hayek 1979, 7-8).

And then Hayek is not even mentioning his family relations to the Wittgenstein family. We are familiar with Ludwig the philosopher, but Maurice Ravel wrote his famous 'Piano Concerto for Left Hand' for Ludwig's brother Paul, an accomplished pianist, who lost his right hand during the War. The cultural world of pre-WWI Vienna in other words, is ill-described as cosmopolitan, it was a small village.

The situation, however, was different during the interwar period. Far from turned inward many intellectuals were politically motivated and active. Economic as well as social differences were diminishing and many migrants arrived, especially from the east following the break-up of the Habsburg Empire. During that period, the most important economic Viennese circles are to be found (although they sometimes had pre-WWI predecessors). This paper argues that to understand the outburst of the interwar period it is essential to study the Viennese circles ('Kreise'). We are well acquainted with the most famous of them, the Wiener Kreis: a circle of logical positivists around Moritz Schlick. Interwar Vienna, however, was filled with such circles. In a recent article, Timms has produced a visual representation of these scientific and artistic circles in Vienna in which he suggests that there were as many as fifty (Timms 2009, 25).[2] Perhaps even more striking than the sheer number of these circles is their overlap. Above we have already emphasized the importance of personal relationships, but these were further cultivated through the participation in a number of partly overlapping circles. If one did not know someone directly, he was never more than one or two circles away.[3] The historian and economist Engel-Janosi, for example, belonged to four of such circles (Engel-Janosi 1974, 108-128). It should hence come as no surprise that gossip was pervasive in Viennese society; social bonds were thick.

A proper understanding of these circles is crucial to understand the contribution of the economists from Vienna for three reasons. First,

[2] It is not precisely clear which time period Timms's picture represents, but at least some of the circles in his figure never existed simultaneously. An earlier version of the picture suggests that it shows the situation in the late 1920s, see Timms 1993.
[3] One exception should be mentioned, there was a more strict segregation between Jewish and non-Jewish circles. This is also emphasized by Hayek in the interview cited above. On the other hand assimilated Jews were regularly fully respected members of non-Jewish circles.

because their work was the outcome of the debates between 'members' of these circles,[4] the circles are the most important intellectual context. Secondly the character of the knowledge that emerged from these circles differed from that produced in strictly academic settings. While in many other European countries modern universities were coming to dominate the intellectual atmosphere, Viennese intellectual life took place within the social sphere. While knowledge and artistic production became organized along disciplinary lines in many other European countries (and the U.S.A.), intellectual life in Vienna remained both broad and relatively informal. While in many other countries theoretical concerns came to dominate scholarly discussions, in Vienna such these discussions were invariably tied to social and cultural concerns as has for example been shown by Janik and Toulmin for the work of Wittgenstein (Janik and Toulmin 1973). Third, the strong identities formed in these circles influenced the identity and prospective careers of these economists in significant ways when they migrated to the New World. The bi-weekly seminar was one such ritual which was identity-forming, but we will explore many more of them in section four.

The analysis of this paper of a number of intellectual communities ties in with a shift away from the study of individual scholars to creative communities. This shift occurred slowly when in physics historians of science realized that many of the great breakthroughs including quantum mechanics were achieved in small communities of about a dozen scholars (Heims 1991; Cushing 1994). A milestone was Collins's monumental study of *The sociology of philosophies* which showed that nearly every major philosopher had been part of a face-to-face community (Collins 1998). As Collins puts it in a later book: "the major thinkers are those most tightly connected to other important intellectuals […]. Successful intellectuals are the most socially penetrated of introverts" (Collins 2004, 358).

This trend is also reflected by in a recent issue of the journal *History of Political Economy* (Spring 2011) devoted to intellectual communities.

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[4] In the notes below I will present lists of members or rather regular participants to these circles. Membership to most of them was not a formal but an informal affair; nonetheless there was a degree of adherence to the shared perspective from some participants that others did speak of members. Such a distinction is nicely illustrated by what Alfred Schütz recounts about the involvement of his friend Felix Kaufmann with the Wiener Kreis: "Kaufmann was never a member and refused to be considered as such, yet attended their meetings regularly" (Schütz quoted in Helling 1984, 144). In the lists below you will find regular participants, rather than members.

Robert Leonard contributed an article on Vienna to this issue. He describes in great detail how Oskar Morgenstern established a community of mathematical economists during the early 1930s, and how this community was broken up by the rise of fascism and the consequent migration. Leonard mentions all the important factors that will be taken up in this article: “a pervasive feeling of anxiety; the close geographical confinement; the lack of anonymity; the presence of a cultivated elite; and the existence of a lively public sphere in which politics, science, and culture were objects of serious attention” (Leonard 2011, 84). He, however, does not develop any of these themes to explain the Viennese circles; instead they are the background to the story of Morgenstern. Consequently, Leonard does not reflect upon the nature of intellectual life in Vienna, and how practices in such circles differed from those in academia. This paper will, on the contrary, focus explicitly on the practices in such circles, and how they were situated more generally in Viennese cultural life.

In that sense this paper is in line with the efforts of Edward Timms who has sought to examine the practices and institutions which have stimulated and hampered intellectual life in interwar Vienna. For him the overlap between circles is especially important, to which, what he calls, the erotic subculture contributed further (Timms 1993; 2009). Timms, the biographer of Karl Kraus, does not pay much attention to economists, however. He instead studies more literary and artistic circles. He does observe that political factors play an increasingly important role during the Interwar period, which is true for economists as well as we will see below. So more than either Leonard or Timms we will study the alternative strategies pursued by Viennese intellectuals to establish legitimacy for their contributions and the rituals which sustained Viennese intellectual life.

In the first section, I will sketch the intellectual scene surrounding the most important of circles for our present purpose: the Mises Kreis (or Mises circle). The subsequent two sections will be devoted to the particular social space occupied by the Viennese circles; independent from the university but far from public. I will pay special attention to the alternative rituals developed outside of the official academia. Then, in the final section, I will analyze the legacy of this oral culture with its lack of formal institutions, and show how this influenced the character of Viennese economic knowledge.

WIENER KREISE, IN PLURAL

It is important to distinguish the intellectual circles that emerged in Vienna from intellectual networks. The intellectual scene of Vienna was a rather dense network with close ties, but the circles formed communities with a shared interest and a strong sense of belonging. If networks represent the ties between individuals these circles represent the smaller groups of intellectuals who shared similar interests and frequently a shared interest and who considered themselves to be *members* of the circle. The most important circle for scholars interested in the economy during the first half of the 1920s was undoubtedly the Mises Kreis. It was centred around, as the name suggests, Ludwig von Mises and was held biweekly in the years 1920-1934 from October to June. The subject matter would range from philosophy and problems of phenomenology, to methodology of the social sciences, and from economics to history. The members of this circle developed the Austrian criticism of central economic planning, also known as the socialist-calculation debate. Within this circle an attempt was made to forge the 'verstehende Soziologie' of Weber with economics (Craver 1986, 14-15). It was the place where the Austrian business cycle theory, as well as the more advanced theories of capital and money were developed, and one of the few places on the Continent where marginal analysis was still discussed. It also proved to be a fertile training ground for future economists. Mises mentored Hayek, Morgenstern, Haberler, Machlup, Rosenstein-Rodan, and Karl Menger in this circle. Building on the legacy of Menger, Böhm-Bawerk, and Wieser, it was in this circle that Austrian economics became the distinct approach to economics that it is still famous for. Mises liked to describe himself as 'primus inter pares' of this seminar, but he was clearly its intellectual leader. As Mises himself describes it, the participants: "came as pupils, but over the years became my friends" (Mises 1942/1978, 97). As such it was initially a kind of continuation of the famous seminar Böhm-Bawerk had held before the war for his advanced students such as Schumpeter, Rudolf Hilferding, and Otto Bauer. The seminar evolved into an intellectual community in which Mises truly was 'primus inter pares', but this was also the stage at which several of its participants decided to start their own (complementary or rival) seminars.

In Figure 1, I have collected the circles that were most relevant to economics, as it was practiced in Vienna. In the middle, we see the

Mises Kreis.[5] The circle which was intellectually closest to the Mises Kreis is the Geistkreis.[6] This circle was formed by a group of advanced students around 1921 led by Herbert Fürth and Friedrich von Hayek. The regular participants of this group overlapped to a large extent with that of the Mises Kreis, but its focus was quite different. Members were required to present on topics which were not their specialty and hence the conversations were (even) broader than in the Mises Kreis.

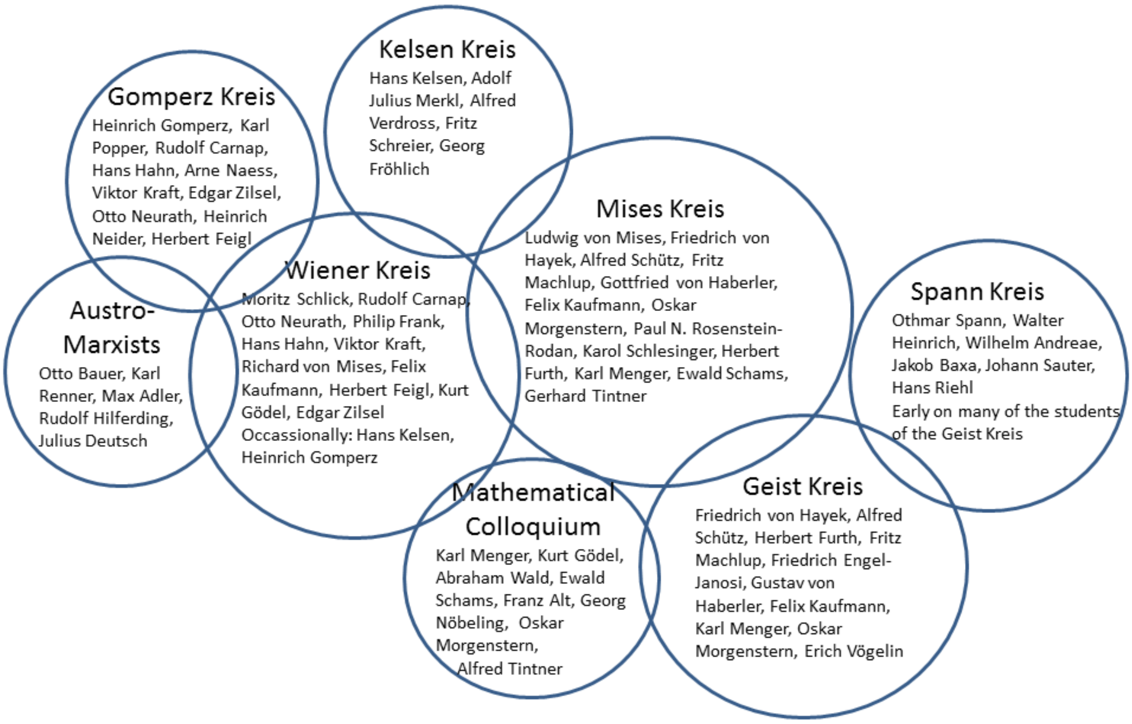

Figure 1: *The Wiener Kreise most concerned with economics around 1928. For the sake of clarity I have limited the visual overlap between the circles, which in reality is often greater.*

[5] An alphabetical full list of regular participants: Ludwig Bettelheim-Gabillon, Viktor Bloch, Karl Bode, Martha Stephanie Braun (later Steffy Browne), Walter Fröhlich (later Froehlich), Herbert Fürth, Gottfried von Haberler, Friedrich von Hayek, Marianne von Herzfeld, Felix Kaufmann, Fritz Kaufmann, Rudolf Klein, Helene Lieser-Berger, Rudolf Löbl, Getrud Lovasy, Fritz Machlup, Karl Menger, Ilse Mintz-Schüller, Ludwig von Mises, Oskar Morgenstern, Elly Offenheimer-Spiro, Paul N. Rosenstein-Rodan, Ewald Schams, Erich Schiff, Karol Schlesinger, Fritz Schreier, Alfred Schütz, Alfred Stonier, Richard von Strigl, Gerhard Tintner, Erich Vögelin (later Voegelin), Robert Wälder, Emmanuel Winternitz (list compiled from Kurrild-Klitgaard 2003, and from Craver 1986).

[6] An alphabetical full list of regular participants: Otto Benesch, Friedrich Engel von Janosi (later Engel-Janosi), Walter Fröhlich (later Froehlich), Herbert Fürth, Franz Gluck, Gottfried von Haberler, Friedrich von Hayek, Felix Kaufmann, Fritz Machlup, Karl Menger, Max Mintz, Oskar Morgenstern, Georg Schiff , Alfred Schütz, Erich Vögelin (later Voegelin), Robert Wälder, Johannes Wilde, Emmanuel Winternitz (list compiled from Craver 1986).

Rather than just science the Geistkreis also discussed contemporary developments in literature, music and art (for a list of subjects discussed, see Engel-Janosi 1974, 225-228). In fact some of its members who graduated in law later became well-established art historians. Since the members were all roughly from the same generation there was less hierarchy than in the Mises Kreis (Craver 1986, 16-17).

During the second half of the 1920s the third important community for (future) economists was founded by Karl Menger (Carl's son): the Mathematical Colloquium.[7] He and some of his friends grew dissatisfied with the anti-mathematical atmosphere in the Mises Kreis. Discussions in the mathematical colloquium were dominated by mathematical subjects, and were in fact frequented more by mathematicians than social scientists. Mises emphasized the unity of the social sciences under the banner of human action, while the members of the mathematical colloquium felt that mathematics could provide unity between the sciences. Karl Menger would end up writing a mathematical book about ethics, the Colloquium was also the place where the existence-problem of the economic general equilibrium model was first discussed and it was the place where Kurt Gödel first presented his famous impossibility theorems about logical systems. There was initially some overlap between this circle and the Geistkreis and the Mises Kreis, but this community increasingly distanced itself from the other two circles. While Hayek and Mises wrote in defence of a civilization they believed was in grave danger, Morgenstern and Menger were instead attempting to purify their economics, emptying it of any 'political' content (Leonard 1998; 2011).

To do so the participants of the Colloquium could draw inspiration from the discussions in what has become the most famous of the Wiener Kreise, *the* Wiener Kreis (or Vienna circle).[8] The Vienna circle was not a homogenous whole, as it has been portrayed in the past. There was at least a division between the left-wing of the circle, consisting of Neurath, Carnap, Feigl, and Waismann, and a more conservative wing.

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[7] An alphabetical (but perhaps slightly incomplete) list of regular participants: Franz Alt, Gustav Beer, Gustav Bergmann, Kurt Gödel, Hans Hahn, Bronisław Knaster, Karl Menger, Oskar Morgenstern, John von Neumann, Georg Nöbeling, Ewald Schams, Karl Schlesinger, Otto Schreier, Alfred Tarski, Olga Taussky-Todd, Alfred Tintner, Abraham Wald (compiled list based on Ingrao and Israel 1990, and on Leonard 2011).

[8] A more or less complete list of regular participants: Gustav Bergmann, Rudolf Carnap, Herbert Feigl, Philip Frank, Kurt Gödel, Heinrich Gomperz, Hans Hahn, Olga Hahn-Neurath, Béla Juhos, Felix Kaufmann, Hans Kelsen, Viktor Kraft, Karl Menger, Richard von Mises, Otto Neurath, Rose Rand, Josef Schächter, Moritz Schlick, Olga Taussky-Todd, Friedrich Waismann, Edgar Zilsel (Stadler 2003, 5n.).

Especially in the work of Otto Neurath, but also in the pamphlet published by the circle 'Wissenschaftliche Weltauffassung', there was a clear link between socialist and emancipatory ideals and scientific knowledge (Hahn, et al. 1929/1979). The conservative wing of the circle headed by professor Schlick, however, was more interested in pure science, free of values and metaphysics. The program for which the Wiener Kreis has become famous post WWII (Reisch 2005). At the same time there were links with the Mises Kreis via the phenomenologist Felix Kaufmann. One might expect links too via the Mises brothers Ludwig and Richard, but they refused to speak to one another and pursued very different intellectual goals. Karl Menger, at various points in time, frequented all four circles we have discussed so far. He was thus well informed on a very broad spectrum of intellectual discussions, and socially very well connected.

The left-wing of the Wiener Kreis was closely connected with the Austro-Marxists, who were part of the social-democratic party which governed Vienna during the 1920s. The community of Austro-Marxists however is not really a circle, since many of the people associated with it held official positions, and many of their organizations were far more institutionalized via the Social-Democratic party. Closely associated with that side of the Wiener Kreis was Heinrich Gomperz who, for several years, also organized a circle.[9] Gomperz was for a couple of years the most important teacher of Popper and his seminar was frequently attended by many of the younger members of the Wiener Kreis.

Two other circles deserve to be mentioned, as far as economics (considered broadly) is concerned. The first was formed around Hans Kelsen,[10] a prominent law scholar who developed 'A pure theory of law' along positivist lines. He is more widely known because he drafted the Austrian Constitution on behest of the Austro-Marxist chancellor Karl Renner. Kelsen was a good friend of Ludwig von Mises, although not a political ally. The other circle worthy of mention is that of Othmar

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[9] I compiled a somewhat tentative list of its frequent visitors: Rudolf Carnap, Herbert Feigl, Heinrich Gomperz, Hans Hahn, Arne Naess, Olga Hahn-Neurath, Viktor Kraft, Heinrich Neider, Otto Neurath, Karl Popper, Robert Reininger, Edgar Zilsel (from Heyt 1999, and Stadler 1994).

[10] I compiled a somewhat tentative list of frequent participants: Josef Dobretsberger, Georg Fröhlich, Walter Henrich, Felix Kaufmann, Hans Kelsen, Josef L. Kunz, Adolf Julius Merkl, Leonid Pitamic, Fritz Sander, Fritz-Schreier, Alfred Verdroß, Erich Voegelin. For an introduction to this circle, see Jabloner 1998, and the website of the Hans Kelsen Institute: www.univie.ac.at/staatsrecht-kelsen/kreis.php (accessed May 2014).

Spann,[11] who developed a universalist philosophy, and was a supporter of German nationalism (and consequently of the Anschluss). His romantic political-economic philosophies initially attracted many of the young economists such as Hayek and Morgenstern, but they soon left Spann's circle. Spann was able to exert this influence over these young students because he held one of the professorships in economics at the University of Vienna (Craver 1986).

These Kreise were not only important for the overlap between them and the mutual inspiration, but also for their mutually rivalry. The interwar work of Mises, Hayek, and Morgenstern can only be understood as part of the ongoing conversations and discussions between these circles. The famous socialist-calculation debate was waged between Otto Neurath and Ludwig von Mises, and Morgenstern carved out his position in relation and ultimately in opposition to the work of Mises. On a deeper level these communities were identity forming, one's membership to a Kreis formed one's intellectual identity. We will discover how different such identities could be from those formed along disciplinary line within academia.

BETWEEN COFFEEHOUSE AND UNIVERSITY

To understand the intellectual scenery in Vienna we need more than a description of the intellectual breadth of its circles, especially since we started this article with the purpose to explain why cultural and scholarly life was so vibrant in Vienna. The cliché about cultural life in Vienna is that it took place in the famous coffeehouses, where one could sit and chat all day while paying for only one cup of coffee. As with all clichés there's some truth to this: the entire Mises Kreis, to take one example, set off on their regular Fridays towards Café Kunstler. Contrary to the cliché, one might expect that they sometimes had more than one drink. In fact, for many Viennese these coffeehouses were much more than just a cafe, it was closer to a living room. It was where they read the newspapers, met their friends, and regularly received their mail and had their washed clothes delivered (Wechsberg 1966; Johnston 1972, 119-124; and for some additional visual material, see Brix 1998).

Like in regular living rooms, visitors were expected to observe specific rules. In certain cafés tables or even specific chairs belonged

[11] I have been unable to obtain more than a few of the regular participants: Walter Heinrich, Wilhelm Andreae, Jakob Baxa, Johann Sauter, Hans Riehl, and early on many of the later members of the Geistkreis.

to some of the intellectual hotshots, and in some of the literary coffeehouses each group of authors had their own table. Quarrels over such tables and the rights to them would not infrequently lead to physical disputes. As homage to this tradition one can find a life-size figure of the author Peter Altenberg sitting in his regular chair in café Central. The cliché is, however, also in need of correction. Private spaces were at least as important for the circles (Fuchs 1949, v-xvi). None of the circles we discussed above actually met for their discussions in one of these coffeehouses. These discussions instead took place in private salons or offices. The availability of which depended on private wealth and professional privileges. We should not forget that the various 'Von's' we have been talking about were (inherited) titles of nobility. There was also more recently acquired wealth, the prime example was the Wittgenstein family who had acquired its wealth through iron and steel, and was estimated to be the wealthiest family of Vienna. Despite these old or new inequalities social stratification became less during the 1920s in Red Vienna.

The social consequences of this diminishing stratification were felt in the circles. Take the Wiener Kreis, where Moritz Schlick was the most prominent individual. Not only was he the only one holding a professorship but he was also much wealthier than most its members. Schlick had always refused to admit Otto Neurath in his house. Neurath had grown up in a working class environment and he cultivated this background, frequently wearing a characteristic working man's cap and refusing to adjust his accent. This led Schlick to exclaim: "I cannot invite this man; I cannot bear his loud voice" (Schlick quoted in Neider 1973, 48). Neurath was undoubtedly offended by Schlick's refusal to receive him at his house, but at the same time he made fun of the 'aristocratzic' accent of Schlick. Such inequalities, however, had further consequences. Schlick could arrange certain jobs for his students, Feigl for example became librarian at the philosophy faculty, but this also meant that Feigl was 'merely' his assistant.[12]

Mises too was quite good at arranging jobs for his favourite students. In 1927 he even managed to set up a new institute under the umbrella of the Chamber of Commerce where he was secretary: the 'Institut for Konjunkturforschung' (Institute for business-cycle

[12] Stratification also took place along 'racial' lines. Tensions remained, sometimes hidden sometimes on the surface, between Germans, Austrians, assimilated Jews and recently migrated Ostjuden. For a nuanced account of these issues in the Mathematical Colloquium, see Leonard 2010, chapter 8.

research). The first director of this institute was Hayek who could hire Morgenstern as his assistant. On the one hand this can be interpreted as evidence that there were various opportunities for the Viennese scholars to get a job. On the other hand, it exemplifies the uncertainty in which they operated. The University of Vienna was marginalized *and* politicized, which made young intellectuals highly dependent on a few wealthy and powerful individuals. No wonder that the topic of migration frequently came up in the discussions of the Geistkreis. Even Mises was subject to these uncertainties and dependencies. When Böhm-Bawerk passed away and Wieser retired Mises was one of the candidates to succeed them, but the positions went to Mayer and Spann instead (Craver 1986). This decision in which Mises (and Schumpeter) were passed over reflected a general trend at the University of Vienna. It failed to hire and/or attract the most talented individuals, and hence became increasingly marginalized in Viennese intellectual life. This was further reinforced by a growing anti-Semitism in Vienna generally and at the university in particular. During the 1920s it became virtually impossible to obtain a university position as a Jew (which Mises was). Janik and Toulmin in their cultural history of Vienna even speak of an "authority gap", by which they mean the absence of any legitimating institutions in Viennese society and for intellectuals especially (Janik and Toulmin 1973, 248).

This authority gap was, however, not complete. For some of the Viennese intellectuals there was the opportunity of association with the social-democrats and their government. The social-democrats set up extensive social programs, most famously to solve the housing conditions and shortage in Vienna. This development did not improve matters, however, for the more neutral or liberal intellectuals. For them the changing political wind meant that political positions which many Viennese economists had occupied before WWI became unavailable. Schumpeter, as an exception, did obtain such position. And while he certainly tried to combine his position of the neutral expert with the goal of the socialization of the economy, his position was soon untenable (McCraw 2007, 96-103).

Another institution which was still standing strong was the gymnasium system, which provided a solid basis for many in the Viennese elite. Gymnasiums such as the Schottengymnasium, which Böhm-Bawerk, Wieser, and no less than three later Nobel Prize winners attended, were of a high quality.[13] On the other hand the gymnasium system also reflected and reinforced a big divide between the elite and the middle classes. In his memoirs, Karl Menger points to yet another factor which helped Viennese intellectual life flourish:

> The unusually large proportion of professional and business people interested in intellectual achievement. Many members of the legal, financial, and business world; publishers and journalists, physicians and engineers took intense interest in the work of scholars of various kinds. They created an intellectual atmosphere which, I have always felt, few cities enjoyed (Menger 1994, 9).

This interested group of professionals regularly participated in the Kreise. To give some examples from the participants of the Mises Kreis: Mises combined it with his work at the Chamber of Commerce, Karl Schlesinger was also a banker, Machlup worked in his parents' cardboard factory, and Schiff was a newspaper editor (Schulak and Unterköfler 2011, 133-135). It was from this professional class, also, that a more general audience could be drawn, for example for the public lecture series which various members of the Wiener Kreis organized.

Intellectual life as a consequence became separated from the official institutions. Famous is the artistic Viennese 'Sezession' (literally: separation) movement, which sought independence from the existing artistic styles and institutions. It is helpful to think of Viennese intellectual life as also separating itself from the official institutions. This is in line with Schorske's analysis of the failure of political liberalism in Vienna. This meant that intellectual life flourished, despite the lack of official institutions. For the scholar, however, it meant that, like the artists of the Sezession, he or she was in need of alternative institutions, alternative sources of finance, alternative sources of legitimacy, even an alternative identity.

THE RITUALS OF THE KREISE

Academic life is so full of rituals, that we sometimes hardly notice them: extensive rituals when (PhD) students graduate, or when a professor accepts a chair (or retires from one), and smaller rituals such as the celebration of centenaries of famous predecessors, or the opening of

[13] Julius Wagner-Jauregg won the Nobel Prize in Medicine in 1927, and Konrad Lorenz and Karl von Frisch shared the Nobel prize in Medicine with Nikolaas Tinbergen in 1973.

our academic year. Such rituals have a double function: they honour the people involved, the renowned scholar or the graduate, but they also legitimize the institutions that organize such rituals. Such legitimization was not self-evident in Viennese intellectual life. A position at the University of Vienna was the exception rather than the rule, and the continued conversation often depended on particular individuals within the Kreise, rather than on more formalized or official institutions. It should thus perhaps come as no surprise that Viennese intellectual life was filled with alternative rituals and strategies to establish legitimacy. Such rituals helped establish a scholarly identity for the intellectuals in Vienna, so that they could give an answer to some of those piercing everyday questions: who are you and what do you do?

Although no one has to my best knowledge ever paid particular attention to such rituals in the Wiener Kreise, we are fortunate to know quite a bit about them. The meetings of the Mises circle always started punctually at seven on a Friday evening. Mises would be sitting at his desk and usually he had a large box of chocolates—quite a luxury in years of hyperinflation—which he passed around. The meeting would last until half past nine or ten, after which the participants would have dinner at the Italian restaurant 'Anchora Verde', and those who had not yet had enough would continue to café 'Künstler' (Kurrild-Klitgaard 2003, 47). Undoubtedly the most striking ritual of the Mises Kreis is the songs which Felix Kaufmann wrote in honour of the seminars. The songs deal with the critical spirit of the circle ('Geschliffener Geist in Mises Kreis'), particular debates within the circle, and the Austrian tradition ('Der letzte Grenadier der Grenznutzenschule'). Other songs were written for special occasions: a song of celebration for the opening of the statistical institute and goodbye song to Mises when left Vienna to take up a post in Geneva.

Now it is easy to think of these songs as a kind of curiosity, but that would be too easy. Many years later Haberler was still able to sing these songs word for word, and he emphasizes that all regular participants could recite them (Haberler in Kaufmann 1992, 9-10). The songs were written to popular melodies and Haberler stresses that these songs were meant to be sung, not to be read (although even reading them is quite a delight). Such rituals established a certain rhythm to the meetings of the Mises Kreis, and provided a sense of belonging where the university could not do so. The songs also served to legitimize the Mises Kreis, take for example the following fragment:

An economist moved to Germany
A learned position to pursue
This should have been a certainty
For in Vienna he'd learned a thing or two
But the good man learned the tragic tale
Marginal Utility was deceased (Kaufmann 1992, 21-22).[14]

In the eponymous song of the Mises circle, the rituals discussed included the delicious chocolates that were consumed. In the final stanza Kaufmann wonders whether all these intellectual discussions lead anywhere, while life outside goes on as usual. Was it not easier to follow the stream, instead of attempting to change its course? Only to conclude affirmatively: "And yet there's no tradeoff at hand / Somehow we must take a stand" (Kaufmann 1992, 28).[15]

Such rituals established internal coherence and legitimacy, but the overlap between the circles meant that a strong internal identity would also become known in other circles. In fact there was a curious interdependence between all these Kreise. The identity of such circles was often defined in opposition to other circles. The Mises Kreis stood in opposition to the positivism of the Wiener Kreis and the universalism of the Spann Kreis. Meanwhile the Geistkreis was more informal and more cultural than the Mises Kreis. It was also only open to men and restricted to twelve members. In fact a degree of secrecy was not alien to these circles, Mises in his recollections written around 1940 explains: "Outsiders knew nothing of our meetings; they merely saw the works published by the participants" (Mises 1942/1978, 98). But one might critically ask who in the Viennese elite was really an outsider? The Mises Kreis was well known in intellectual circles in Vienna and abroad, from which visitors occasionally joined the seminar. The most prominent foreign visitor was perhaps Lionel Robbins, who would later offer Hayek a professorship at the LSE. Nonetheless access to particular circles could be a sensitive issue. This becomes particularly clear from the following passage from Popper's autobiography:

[14] From "Dei Grenznutzenschule", translated as "The Grenadier of the Marginal Utility School" by Arlene Oost-Zinner at http://mises.org/misestributes/misessongs.asp (accessed September 2014).
[15] From "Das Mises-Kreis-Lied", translated as "The Song of the Mises Circle" by Arlene Oost-Zinner at http://mises.org/misestributes/misessongs.asp (accessed September 2014).

> The Circle [Wiener Kreis] was so I understood, Schlick's private seminar, meeting on Thursday evenings. Members were simply those whom Schlick invited to join. I was never invited, and I never fished for an invitation. But there were many other groups, meeting in Victor Kraft's or Edgar Zilsel's apartments, and in other places; and there was also Karl Menger's famous 'Mathematische Colloquium'. Several of these groups, of whose existence I had not even heard, invited me to present my criticisms of the central doctrines of the Vienna Circle (Popper 1976, 84).

The quote not only highlights the opposition between the various circles, especially against the most prominent, but also the partly open and partly closed nature of the circles. Popper's labelling of Schlick's seminar as 'private' is especially telling, and revealing of the powerful position of Moritz Schlick. Popper's autobiography has become an archetypical example of how unreliable autobiographies can be, but it is beyond doubt that the tension between him and the Wiener Kreis was as much social as intellectual. Popper's biographer Cohen writes about the issue: "his personality made collaboration difficult. Even Popper's defenders, Carnap and Kraft [both members of the Wiener Kreis], admitted that he was a social problem" (Hacohen 2000, 209).[16]

The Wiener Kreis, too, is interesting to study for its search for legitimacy. Its most famous publication is a manifesto 'Wissenschaftliche Weltauffassung', which is usually translated somewhat awkwardly into 'scientific world-conception'. But let us pause for a moment, to realize what is happening here: a group of philosophers (!) who seek to purify science from metaphysics and values publish a manifesto. The manifesto is, and was, a rather revolutionary form: Marx and Engels published a manifesto pamphlet, and the Italian Futurists published one to declare a revolution in art. It is not, however, the form one would expect from a group of philosophers, let alone from one that is looking for the foundations of objective knowledge. In fact the most traditional of them, Moritz Schlick, was seriously taken aback

[16] The insider-outsider discussion is also interesting with respect to the very negative essays that both Schumpeter and Hayek have written about intellectuals; see Schumpeter 1943/1976, 145-155; and Hayek 1949. One is tempted to also think of the Viennese scholars of the interwar period as (public) intellectuals but in their search for legitimacy they had to distance themselves from outsiders. Their repeated arguments against intellectuals or men of science are perhaps best understood as an attempt to create a professional identity outside academia, read as such they are testimonies of a certain existential 'angst'.

by the publication (Mulder 1968).[17] The pamphlet as a scientific form is of course still far from accepted, but understood as an alternative strategy to seek legitimacy it makes sense. It also succeeded, in the sense that it gave the Wiener Kreis a very clear identity to the outside world, and the movement soon attracted followers, disciples and opponents in other countries (Gruen 1939; McGill 1936). It, furthermore, provided the stimulus for cooperation between members of the Wiener Kreis and the cultural avant-garde in Europe (Galison 1990). Membership of a circle as such became a mark of expertise, but also a lasting allegiance to a particular intellectual position and a certain style of doing science.

Looking back on the interwar situation in Vienna it becomes clear, however, that the situation was ultimately unstable. The uncertainty and the lack of official positions made it tempting to migrate. The more senior and successful scholars were the first to migrate, not uncommonly before the political situation in Vienna became an acute threat. Hayek already migrated in 1931. The domestic situation became particularly problematic in 1934 when Dollfuss rose to power. Between 1934 and 1938, the year of the Anschluss, Austria was ruled by the Austrofascists and public life became more restricted. Mises, who expected the worst for the future, left for Geneva in 1933, only to move to New York in 1940. The Wiener Kreis was particularly disturbed by the shooting of Moritz Schlick in 1936 by a former student. Although the murder was not motivated by anti-Semitic sentiments, the press described it as such. Migration was not easy for everyone; those with little international visibility depended on friends from Vienna who migrated earlier. Popper, for example, had to migrate to New Zealand in 1937 where he held a low-prestige job at a university. The adaptation to these foreign *and* academic cultures would require a separate article, but it is safe to say that this process occurred far from smoothly. Individuals with considerable prestige in the Kreise of Vienna frequently ended up at the bottom of the ladder, employed at marginal universities.

It is tempting to argue that first Austro-fascism and later the Anschluss with Nazi-Germany caused the migration, but that might also be too simple. The social situation for many of the intellectual

[17] Schlick was nonetheless very aware of the revolutionary nature of the philosophical project in which he and his fellow Wiener Kreis members were involved as is evident from his 'Die Wende der Philosophie' (1930).

talents was uncertain even apart from the political situation. On the one hand the Viennese intellectuals were, as Fürth wrote years later to Hayek, "spoiled" by the intellectual stimulation around them (Fürth quoted in Hennecke 2000, 25). On the other hand they could not obtain an official academic position, they were dependent on not more than a handful of powerful and wealthy individuals, and there were few signs of future improvement. So when Hayek was offered a position at the LSE he knew what he left behind, but also what he stood to gain. What helped in his particular case was that he was offered a full professorship. Overall it is doubtful how long Vienna would have been able to retain its greatest talents, even if the political situation would have remained stable.

THE LEGACY OF AN ORAL INTELLECTUAL CULTURE

The vibrancy of Viennese intellectual life tends to cause quite a bit of nostalgia. That nostalgia is wonderfully cultivated in some of the memoirs about the period (Zweig 1943; Spiel 1987). More than anything, however, we should ask why this intellectual culture disappeared. Reisch (2005) examines the disappearance in detail for the Wiener Kreis.

He argues that it never came to fruition because it was smothered before it could really flourish. He suggests that the central ideal from within the Viennese intellectual scene has been lost and forgotten: the ideal of the unity of science. Reisch shows that this was not as much a philosophical ideal as it was a practical program: "the unity of science program transformed from a practical, collaborative goal to a more narrow academic thesis, […] it became an empirical hypothesis about science […] after it was decoupled from the ideal of active collaboration" (Reisch 2005, 375-376). This is not the place to debate the merits of the unity of science thesis or these other social projects. But what is interesting for us is the shift Reisch describes away from these social goals, towards purely philosophical and academic goals. Reisch is not the only one with this sentiment. Janik and Toulmin (1973) in their study of Wittgenstein's Vienna also lament the professionalization which made the position of therapeutic philosophers in modern intellectual life increasingly difficult. Both Reisch and Janik and Toulmin recognize that within the Viennese tradition there is no clear separation between science and politics or philosophy and life. They argue that social, cultural and sometimes political goals went hand in hand with scholarly concerns for the Viennese.

This unique feature of the Viennese tradition combined with its breadth often puzzled outsiders and it made moving to another intellectual climate, another country, or rather into a university a difficult process.[18] When Schumpeter visited the U.S.A. in 1913 he was asked to deliver a lecture by Seligman, an economics professor at Columbia. Seligman's description of the lecture is a wonderful example of this confusion:

> [He did not only speak of economics] but the relation of economics to psychology and sociology. He was—what is very unusual—both brilliant and profound; his choice of novel illustrations taken from a great variety of different fields, shows a surprising breadth of culture, which is unusual in a specialist (Seligman, quoted in McCraw 2007, 81).

But Schumpeter was no specialist, and never became one; he was a student of civilization, schooled in wide cultured conversation not with just Wieser or Böhm-Bawerk, but with Marxists, Max Weber, and artists from Vienna. This is also exemplified by Hayek's tribute to his mentor Wieser. Hayek chose not to compare him to a great economist of the past, but to Goethe, the great symbol of German culture, who had: "[w]ide-ranging interests encompassing all fields of culture and art, worldly wisdom and the worldly tact of the minister of Old Austria combined with an aloofness from daily trivia" (Hayek 1926/1992, 125). It was a description that suits intellectual life in interwar Vienna just as well.

The reception of Hayek in the U.S.A. is another prime example of such confusion.[19] He is often associated with the Chicago school of economics, because he held a position in Chicago. But Hayek was never offered a position at the economics department in Chicago. There is still no absolute clarity regarding the reasons for this, but it is clear that there were concerns about the non-economic nature of his work. Friedman, the main figure within the Chicago school, explained why Hayek was not offered a job in an interview from 2000: "[m]y understanding is that this was because, at that stage, he [Hayek] really

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[18] Reisch study of the migration of the Wiener Kreis contains many examples of such difficulties; see Reisch 2005.

[19] For reasons of space it is not possible to discuss all major economic figures, but it is worth noting that one of Mises's students, Alfred Schütz, suggests that Mises was not hired because it was believed that he was too practically oriented and not academic enough; see Kurrild-Klitgaard 2003, 52.

wasn't doing any economics" (Friedman quoted in Cassidy 2000). In fact, it should not really surprise us that Hayek was not considered to be a professional economist in 1950. His book on capital theory from the 1930s was not very well received, and thus his main claim to fame was *The road to serfdom*, a political rather than an economic book.

Hayek was instead hired at the 'Committee of Social Thought' which was oriented much more broadly. In fact Hayek was happy with this position precisely because it was concerned with what he described as 'borderline problems in the social sciences', and in an interview he even claimed that he was bored with the purely economic atmosphere at the LSE. In that same interview he speaks very positively about especially the initial period on this committee:

> I announced a seminar on comparative scientific method, and the people who came included Sewall Wright, the great geneticist; Enrico Fermi, the physicist; and a crowd of people of that quality. It only happened once; we couldn't repeat this. But that first seminar I had in Chicago was one of the most interesting experiences I had (Hayek, interviewed by Buchanan 1979, 262).

Hayek was once again back in cultured conversation with scholars from many fields. And not just scholars, the committee on social thought also invited individuals from the literary world. Hayek was never happy in just one discipline, but thrived in an atmosphere like the one in which he came of age.

In fact, at one point there was the opportunity to restart in Vienna what had been lost during WWII. In the same interview with James Buchanan, Hayek explains that he could get money from the Ford Foundation, a lot of money, to start a new centre in Vienna. Then Buchanan asks whether this was to reestablish the University of Vienna, to which Hayek responds quite accurately: "[w]ell, to reestablish its tradition" (Hayek 1979, 253). Of course reestablishing the University of Vienna would have been nearly a contradiction in terms, for in many fields it had never really been established, and it certainly had never been the centre of scholarly life. What Hayek sought to do was to reestablish its tradition, and for this he needed to bring the people back: "to bring all the refugees who were still active back to Vienna—people like Schrödinger and Popper and—Oh, I had a marvelous list! I think we could have made an excellent center" (Hayek 1979, 253). This is Hayek's nostalgia for a tradition, for the Viennese conversation, which always took place on the borderlines between disciplines and between science and society. Needless to say this initiative remained a nostalgic dream and never materialized.

CONCLUSION

In this paper we have studied the *practice* of intellectual life in Vienna. Central in this practice were the circles in which intellectual conversations took place. The conversations were the *practice* par excellence of Viennese intellectual life; not experiments, not armchair observations, not statistical methods, not modelling, but talking. One of the downsides for the historian is that little remains of such conversations. In this chapter I have analyzed the setting in which these conversations took place, and by which rituals they were surrounded, but the conversations themselves are permanently lost. All we have left are some lists of topics discussed during the seminars. In fact if one looks back on the interwar period one notices a peculiar absence of written work. Hayek hardly published anything during the 1920s, and was hired at the LSE based on the *lectures* he delivered there. Mises *wrote* his most important books before and after the flourishing period of his seminar. And while I certainly do not want to claim that there was no output, it seems that the conversations were indeed more important than the written word. The written output was produced later when they migrated to an academic culture in which the written word was far more important than it had been in Vienna. If they did write it was just as often a contribution to some contemporary political debate as it was an academic paper. In fact a recent volume which collects the writings of Mises during the interwar period shows that his reflections on political and economic developments far outweigh the more traditional academic issues (Mises 2002).

In this paper I have demonstrated the unique structure of Viennese cultural world with special attention to economics. This institutional setting not only influenced the practices of economic thinkers, but also the content of their contributions. Except for the participants of the Mathematical Colloquium, the Viennese economists were involved with, and felt attached to the cultural and political context of Vienna and Europe. These circles shaped their intellectual identities, and when they migrated they kept looking for institutional settings which allowed them to transcend disciplinary boundaries, and contribute on theoretical, social, and political levels.

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Challenging the majority rule in matters of truth

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Abstract: The majority rule has caught much attention in recent debate about the aggregation of judgments. But its role in finding the truth is limited. A majority of expert judgments is not necessarily authoritative, even if all experts are equally competent, if they make their judgments independently of each other, and if all the judgments are based on the same source of (good) evidence. In this paper I demonstrate this limitation by presenting a simple counterexample and a related general result. I pave the way for this argument by introducing a Bayesian model of evidence and expert judgment in order to give a precise account of the basic problem.

Keywords: competence, evidence, social epistemology, testimony, trust in experts, two-expert problem

JEL Classification: C11, D71, D82, D83

> Surprises can be useful in epistemology. Epistemology is most helpful when it leads to normative recommendations that are surprising in that they are counterintuitive or in contradiction with established practice (Miriam Solomon 2006, 30).

Seeking and utilising the advice of experts is a very common and useful practice in a complex world; this is especially so given the ever-increasing stream of information, which no single individual can comprehend and process entirely on her own. We regularly ask experts for advice. And in many cases we ask different experts for their independent advice on one and the same issue. If we, for instance, fear a serious disease we may well ask several medical specialists for their diagnoses. But what if the diagnoses given are inconsistent?

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How should and how do we actually cope with disagreement among experts? In an explorative paper Alvin Goldman (2001) investigates what good reasons a novice might have for trusting one putative expert more than another. He first presents a (non-exhaustive) list of such reasons. According to the second entry in his list, Goldman's advice to the layman confronted with conflicting expert judgments is to check for "agreement from additional putative experts on one side of the subject in question" (Goldman 2001, 93).

As a matter of fact, this piece of advice is the one Goldman discusses most extensively in his paper. He gives the following formal argument:

> **Goldman's proposition**. *If expert judgments are sufficiently reliable and independent of each other, then additional experts confirming some proposition* φ *add positive credibility to* φ (Goldman 2001, 99-101).

The reader may be somewhat disappointed about this result. It does not seem to suit the needs of a layperson confronted with conflicting expert judgments very well. What we would rather have is something like this:

> **Proposition (*).** *If expert judgments are sufficiently reliable and independent of each other, then* φ *is more probably true than not, if the number of experts confirming* φ *exceeds the number of experts confirming* $\neg\varphi$.

The idea that the majority of expert judgments may play a decisive role as a basis of informed decisions appears very natural to us. This is somewhat reflected in the vast literature on the aggregation of opinions in psychology and management science (see Budescu 2006; Yaniv 2004 for overviews). There is much evidence to suggest that people predominantly use simple averaging rules to aggregate information from multiple sources. Averaging has recently also become salient in the debate about swarm intelligence and the wisdom of crowds.[1] In the case

[1] See, most prominently, Surowiecki 2004. Exploiting the wisdom of crowds presupposes some sort of aggregation of the information dispersed across the crowd. Most of the cases Surowiecki discusses are—like the classic Galton example—cases of simple averaging. The other aggregation procedure prominently mentioned is the market price (e.g., on the stock market) as a representation of a collective assessment of expected value or success (chapter 11). On one occasion Surowiecki refers to Bayesian updating (pp. xx-xxi), but the case discussed is a Bayesian search based on an aggregate expert opinion. How this aggregate opinion is produced is not specified (but it does not seem to be itself based on some form of Bayesian updating).

of a categorical binary judgment (e.g., whether or not a surgery is needed to cure a disease), assigning equal weight to the independent judgments of various experts and then averaging them amounts to following the advice of the majority (Yaniv 2004, 76).

In social epistemology, the rediscovery of Condorcet's jury theorem[2] eventually made the majority rule a standard reference point in the debate about the demands of epistemic rationality when merging the opinions of independent judges, and in other places such as the recent debate about the so-called discursive dilemma (Kornhauser and Sager 1986, see discussion below).

Thus, there seem to be good reasons to consider the majority rule not only a widely used, but also a well-founded guiding principle in forming an opinion on the basis of diverging expert judgments. However, a claim like (*) is not true in general and Goldman is well advised not to make it! There are important cases in which the majority just cannot decide on matters of truth, even though all judgments are made independently by equally competent jurors with exactly the same opportunity to obtain information on the issue. The object of this paper is to demonstrate the existence of these cases, mainly by presenting a simple counterexample.

Within the theory of judgment aggregation (see List 2012 for an overview) it has been acknowledged that simply following the majority of judgments is not necessarily optimal if truth is the object. The main reason for this is that a rational decision maker would consider that judgments are given by individuals with different degrees of competence. Consequently, the judgments of individuals should be differently weighted according to their competence and/or other qualities of the individual and her informational resources, which might influence the reliability of her judgment. In what follows, my argument presupposes that all individuals are equally competent and that all are in essentially the same position to obtain relevant information on the matter at hand; thus all judgments should be assigned the same weight. Nevertheless, following the majority of judgments will turn out to be suboptimal.

The argument in this paper is related to more general findings from the literature on 'opinion pooling' (Dietrich and List 2014). This literature is concerned with aggregating opinions, which are represented

[2] Condorcet 1785 did not explicitly formulate the theorem in his famous essay. The basic proposition was first explicitly stated by Duncan Black (1958).

by an assignment of probability to some proposition. 'Linear pooling' determines the average of the—possibly weighted—individual opinions as the correct collective opinion and corresponds to the majority rule in judgment aggregation. Franz Dietrich and Christian List (2014) argue that this procedure is suboptimal as a truth-tracking procedure. Their reasoning is that collective opinions generated by linear pooling may not adequately incorporate the whole body of information upon which individual opinions rest.

I will use Bayesian updating here as the benchmark of truth tracking. This corresponds to an opinion aggregation procedure known as 'supra-Bayesian opinion pooling' (Morris 1974), which Dietrich and List refer to at the end of their survey, but do not discuss in detail. Their reason for not engaging thoroughly with this procedure is that it rests on unrealistic assumptions that pertain to the information available to individuals. Rather than to formulate a consistent theory of judgment aggregation, my aim in this paper is to challenge the majority rule as it is used in epistemic discourse on judgment aggregation. My argument proceeds as follows:

I will begin with a short presentation of the discursive dilemma as presently discussed. The goal is to identify hidden assumptions and unquestioned premises that underwrite the majority rule as a guiding principle in opinion aggregation. In subsequent sections, I present a counterexample and the formal results—these serve to show that the assumptions of majority rule are by no means indisputable. However, before presenting a counterexample the underlying problem must be articulated. I will, therefore, motivate the primary argument by introducing a simple Bayesian model of evidence and expert judgment.[3] The paper concludes with some general remarks.

THE DISCURSIVE DILEMMA

In philosophical debates about epistemic rationality, the majority rule, as an aggregation procedure for judgment, has been a predominant topic in the context of the so-called discursive dilemma. Here is a quote and an example from a recent paper to illustrate the focus of this debate (Pigozzi 2006, 285fn.):[4]

[3] Goldman (1999, 103-109) introduces a similar model for the special case of testimonial evidence.

[4] The problem first became known as "the doctrinal paradox" in the discipline of jurisprudence. The example given here has the same logical structure as the original example by Lewis A. Kornhauser and Lawrence G. Sager (1986).

> A department of a prestigious university offers one career development fellowship to a candidate (proposition R) if and only if the candidate proposed a good research project (P) and if she has an excellent track record of publication (Q) [...] Suppose that there are three members in the departmental committee. Each of them consistently casts her vote on R (the conclusion) depending on her judgments on P and Q (the premises). The three members vote as shown in the table below.

	P: Good project?	*Q: Excellent publication?*	*R: Fellowship?*
Member 1	Yes	Yes	Yes
Member 2	Yes	No	No
Member 3	No	Yes	No
Majority	Yes	Yes	No

The problem is that the majority rule produces inconsistent results. While a majority certifies that the candidate has a good project and that publications of the candidate are excellent, there is no majority to accept the candidate as a fellow. Based on the above propositions, majority voting implies that premises P and Q are accepted whereas the conclusion R is rejected.

Two ways to escape the paradox suggest themselves and have come to dominate the debate. One can either confine aggregation to premises and derive the conclusion deductively according to these premises (the *premise-based procedure*); or an individual can start by drawing the conclusion according to her individual assessment of the premises and the votes on the conclusion are then aggregated (the *conclusion-based procedure*). Note that both procedures adhere to the majority rule as an essential standard of judgment aggregation; albeit, they restrict its use to a proper subset of the judgments to prevent inconsistencies.

List and Pettit (2002) introduce a general result that extends the discursive dilemma to a wider class of aggregation procedures. The defining property of this class is “systematicity” (List and Pettit 2002, 99), which requires that the collective acceptance of a claim depends solely on the pattern of individual acceptance and not, for example, on the content of the judgment or the evidence it is based on. To determine the collective judgment of some claim it suffices to consider individual votes (whatever the claim is).[5] This is a characteristic of all voting

[5] See List and Pettit 2003, 99; compare also the related but probably more transparent characterization “systematic responsiveness” in List 2006, 376.

procedures that are based solely on counting votes. Not surprisingly, the debate that follows from this—and other attempts to generalise the observation of inconsistency in the discursive dilemma—focuses on both the variations and strengths of the majority rule as a truth-tracking aggregation procedure.[6]

The root of the whole debate is the fundamental finding that using the majority rule in aggregating individual judgment may result in inconsistencies if several, logically connected judgments are at issue. Yet, those participants leading the debate never seem to seriously consider that something might be fundamentally wrong with applying the majority rule to matters of truth. Occasionally scholars recognise tension between democratic procedures and the fundamental quest for truth.[7] But the general reaction to the discursive dilemma is not a closer inspection of the relation between truth and majority voting in principle. Instead, alternative procedures are considered that constrain majority voting to suitable selections of judgments or by quota rules (Grossi and Pigozzi 2012). It is not the general truth-tracking capabilities of the majority rule that are called into question, but rather the proper pattern of its application.

To the outside observer this may appear somewhat strange. Two related observations from the history of this body of thought might help to better understand the approach taken in this debate.

First, one aspect of the debate (and a familiar domain for many of its participants) stems from social choice theory. From the very beginning the standard view of the problem has included elements of social choice theory—such as Arrow's impossibility theorem or the Condorcet paradox—which are formally similar and were perceived as substantially related. So, as an intuitive point of departure, the focus on voting with the majority rule was transferred from social choice theory to social epistemology.

Second, Condorcet's jury theorem has always been understood as a guiding background insight. Roughly, the theorem says that the majority judgment on some yes/no issue by a group of N sufficiently competent individuals is more probably true than a single judgment by one of its members, and the probability of its truth approaches 1 as N increases.[8]

[6] See, e.g., List 2005; Bovens and Rabinowicz 2006; Hartmann and Sprenger 2012.
[7] See the distinction between procedural and epistemic democracy in List and Goodin 2001.
[8] See, e.g., Bovens and Rabinowicz 2006 for an overview and generalizations of the theorem.

Looking at the theorem in this rough formulation, it may well seem that the supremacy of the majority rule as a truth-tracking procedure is not a matter of debate but a formally proven and indisputable result.

Notice, however, that Condorcet's jury theorem states that the majority rule outperforms individual judgment under certain conditions, it does not say anything about how it relates to other aggregation rules. Moreover, it is worthwhile to have a closer look at the conditions, particularly at the assumption of sufficient competence. This assumption could be specified as follows: There is a $p > 0.5$, such that the probability that an individual judgment is true is at least p for every individual in the group. The hidden assumption behind this condition is that for every individual *i* there is a constant probability p_i that one of his judgments is correct, independently of the specific content of the judgment. If his judgment is that there will be a thunderstorm tomorrow, this will come true with probability p_i; but if his judgment would have been 'no thunderstorm tomorrow', this would have indicated 'no thunderstorm' with probability p_i as well. Whatever an expert says about whatever issue has the same authority in terms of truth. As we will see, this is a very strong condition, which cannot be assumed to be satisfied in general—not even among individuals whom we unhesitatingly and rightly classify as competent experts.

One aim of this paper is to challenge the social choice perspective and to sow some uneasiness regarding the majority rule when truth is at stake. This will be done in a much simpler context than that of the discursive dilemma by way of illustrating a counterexample that focuses on a single statement. But before this subversive task is undertaken, I will introduce some formal specifications of evidence and expert knowledge to get a firm grip on the problem.

EVIDENCE

As is common in the debate about the discursive dilemma I will confine my analysis to simple yes/no problems. An individual has evidence for some proposition φ being true if he has information on some matter of fact that is suitably related to the truth of φ. A convenient example is evidence for prostate cancer. The PSA (Prostate-Specific Antigen) blood level is the most common and—although contested to some extent—the best prostate cancer marker available. Prostate cancer causes, as a rule, a high PSA blood level, which can easily be detected. An increased PSA blood level, however, can have other causes as well, e.g., prostatitis (prostate inflammation) or benign prostatic hyperplasia (a swelling of the prostate). A high PSA blood level is evidence for prostate cancer because the probability of a high PSA blood level is significantly greater in the event of prostate cancer than not.

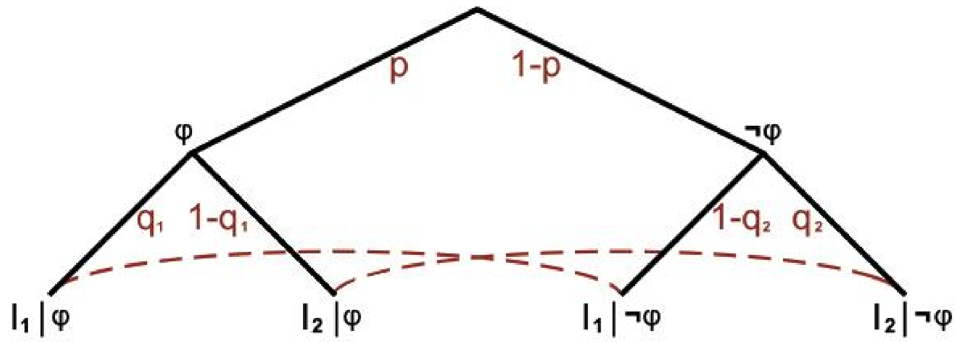

Figure 1: *An indicator model of evidence with two indicator states*

Figure 1 shows an abstract model of the basic interrelations. Call this an indicator model of evidence in a yes/no problem with two indicator states. An individual *A* is interested in knowing whether φ (*A* has prostate cancer) or $\neg\varphi$ (*A* does not have prostate cancer) is true. His prior estimation of φ is $p > 0$. *A* is observing an indicator I (the PSA blood level) with (as is assumed to simplify the problem) two indicator states I_1 (high PSA blood level) and I_2 (low PSA blood level). *A* can correctly judge the state of the indicator, but he does not know whether φ is the case, i.e., he cannot discriminate between I_1 on condition that φ and I_1 on condition that $\neg\varphi$ (indicated by the curved line connecting the nodes $I_1|\varphi$ and $I_1|\neg\varphi$). However, he does know the conditional probabilities $q_1 = prob(I_1|\varphi)$ and $q_2 = prob(I_2|\neg\varphi)$. It should be clear now how the graph in Figure 1 represents the situation.[9]

Observing the indicator may provide information about φ to *A*. Bayes' rule describes how the information about φ is conveyed by *A*'s observations. Given his basic information about the probabilistic interrelations, *A* can update his initial probability estimate for φ being true ($prob(\varphi) = p$) after observing I_1 or I_2:

[9] The representation follows the conventions of the theory of dynamic games. In fact, Figure 1 displays the game form of a very simple interaction in which nature is the only player choosing over alternatives.

$$\text{prob}(\varphi|I_1) = \frac{\text{prob}(I_1|\varphi)\text{prob}(\varphi)}{\text{prob}(I_1|\varphi)\text{prob}(\varphi) + \text{prob}(I_1|\neg\varphi)\text{prob}(\neg\varphi)} = \frac{q_1 p}{q_1 p + (1-p)(1-q_2)}$$

$$\text{prob}(\varphi|I_2) = \frac{\text{prob}(I_2|\varphi)\text{prob}(\varphi)}{\text{prob}(I_2|\varphi)\text{prob}(\varphi) + \text{prob}(I_2|\neg\varphi)\text{prob}(\neg\varphi)} = \frac{(1-q_1)p}{(1-q_1)p + (1-p)q_2}$$

If the probability that φ is true rises after observing indicator state I_i one may say that I_i *indicates* φ. Or, to give a more formal, general definition:

Definition 1. *An event* ω *is said to indicate that* ψ *or to* ***be evidence for*** ψ *iff* $\text{prob}(\psi|\omega) > \text{prob}(\psi)$.

There is a convenient characterisation of I_1 (i.e., the event of observing I_1) being evidence for φ in our indicator model of evidence:

Remark 1. *Consider an indicator model of evidence in a yes/no problem as represented in Figure 1.*
I_1 *is evidence for* $\varphi \Leftrightarrow I_2$ *is evidence for* $\neg\varphi \Leftrightarrow q_1 + q_2 > 1$.

The proof is by simple algebra (see Appendix).

We are especially interested in those indicators whose indicator states signal that the state of affairs they indicate is the most probable state:

Definition 2. *An event* ω *is said to be* ***decisive for*** ψ *iff* $\text{prob}(\psi|\omega) > 0.5$.

Definition 3. *In an indicator model of evidence in a yes/no problem as represented in Figure 1, the indicator* I *is said to be* ***decisive*** *iff* I_1 *is decisive for* φ *and* I_2 *is decisive for* $\neg\varphi$.

Here is a convenient characterisation of an indicator being decisive:

Remark 2. *Consider an indicator model of evidence as represented in Figure 1. With* $q^* := q_1 p + q_2(1-p)$, *the following equivalences hold:*

I_1 *is decisive for* $\varphi \Leftrightarrow \frac{1-q_2}{q_1} < \frac{p}{1-p} \Leftrightarrow q^* > 1-p$

I_2 *is decisive for* $\neg\varphi \Leftrightarrow \frac{p}{1-p} < \frac{q_2}{1-q_1} \Leftrightarrow q^* > p$.

I *is decisive* $\Leftrightarrow \frac{1-q_2}{q_1} < \frac{p}{1-p} < \frac{q_2}{1-q_1} \Leftrightarrow q^* > \max\{p; 1 - p\}$.

Again, the proof is by simple algebra (see Appendix).

Notice that q^* represents the overall probability that the state of the indicator I indicates the true state of affairs.

EXPERT JUDGMENTS

Expert judgments are fundamentally related to evidence in at least two ways.

First, an expert has privileged access to evidence. She is a person who is in a particularly favourable situation to obtain information about some subject and/or who is particularly competent to process such information. Call the circumstances that favour or disfavour the access of some person to information on some subject her *opportunity* and her capacity to process this information correctly her *competence* (see Goldman 1999, 109). The evidence available to an expert is a function of her opportunity and her competence. Whatever the particular opportunity and competence of an expert may be, the evidence available to her will ultimately be of the same basic form as any other evidence. Thus, if an expert has evidence pertinent a yes/no problem it may be provided by an indicator as shown in the model above (e.g., given her opportunity and competence, the expert may be in a position to observe the PSA blood-level).

Second, an expert judgment about some subject is itself evidence of or for the subject. If an expert is sufficiently competent and has the opportunity to obtain relevant information about some subject (i.e., if she has sufficient evidence), then her judgment may be understood as an indicator on this subject.

Consider an expert judgment on a yes/no problem with the expert choosing between two options only: I_1 = 'yes, φ is true' or I_2 = 'no, φ is not true'. In presuming that competence and opportunity are sufficient, this judgment is evidence for φ in exactly the same way as described in our model in Figure 1, with q_1 and q_2 jointly representing the competence and opportunity of the expert. Moreover, if the evidence for φ used by the expert is based on a *decisive* indicator I* with indicator states I_1^* and I_2^*, then the evidence given by the expert judgment is formally identical and equivalent to the evidence the judgment is based on, i.e., it is described by a formally identical indicator with formally identical indicator states and the same conditional probabilities.

Notice that the judgment of an expert may differ from his testimony. An expert may judge that such and such is the case, but testify something else for various reasons. Testimony may, of course, still be evidence of or for some subject. To give an account of such evidence, not only are competence and opportunity to be included in the analysis but also the properties of 'honesty' and 'integrity'.[10] I abstract away the related strategic problems here by assuming that the judgment of the expert is correctly communicated and can be correctly assessed. Thus, my analysis concentrates on the evidential character of pure judgment only.

If two or more experts have the same competence (with respect to some issue φ) and are in the same general position to obtain information on the issue (i.e., have the same 'opportunity'), then the evidence provided by a judgment of one expert will be characterized by the same conditional probabilities q_1 and q_2 as the evidence provided by a judgment of another expert. The experts *judge under equivalent epistemic conditions* relative to φ iff their judgment on φ can be described by the same indicator model with identical parameters q_1, q_2, and p.

If experts judge under equivalent epistemic conditions, their judgments that φ should be assigned the same weight. A judgment 'φ' of one expert represents exactly the same evidence for φ as a judgment 'φ' of another expert under equivalent epistemic conditions. But this does not mean that a judgment in favour of φ should also have the same weight as a judgment for $\neg\varphi$. In fact, as we will see in the next section, an expert judgment 'φ' may produce stronger support for φ than the opposite judgment for $\neg\varphi$ under equivalent epistemic conditions. This suggests that, in such cases, positive judgments should have more weight than negative ones. But this is exactly what is implicitly neglected by the assumptions behind Condorcet's theorem and explicitly ruled out by the systematicity condition.

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[10] See Lahno 2012. Goldman discriminates 'opportunity', 'competence', and 'sincerity' (or 'honesty') as basic determinants of testimonial information. He does not make a distinction between integrity and honesty. See Goldman 1999, 109.

ARE THERE SHARKS IN THE ISLAND WATERS?

We are now in a position to give a precise formulation of the majority rule problem of expert judgments in simple yes/no problems:

Suppose an uneven number of m experts[11] judge (independently) under equivalent epistemic conditions on a decisive indicator as given in Figure 1, with l experts judging 'φ is true' and $m–l$ experts judging '¬φ is true': *Is the indicator* I* *with indicator states* I_1^* = '$l > m–l$' and I_2^* = '$m–l > l$' decisive? The answer, as indicated above, is 'not necessarily!' A simple counterexample demonstrates this.

Imagine an island about which fishermen are interested to know whether there are sharks in the waters (φ) or not (¬φ) before they go out to work. Assume that the probability of sharks in the water is 10% (this knowledge is shared by all fisherman):

$$\text{prob}(\varphi) = p = 0.1.$$

Also assume that there are three individuals, each of which are located on top of one of the three hills on the island with excellent panoptic visibility. If sharks are in the waters, sometimes a shark fin cuts across the surface of the sea (I_1) and, therefore, may become visible to an observer on one of the hills. Assume that the three 'experts' on the hills share the same basic competence and opportunity in watching for shark fins. If there are sharks in the waters (φ), then for each of them, independently of the other two, there is a 5% chance to spot a shark fin:

$$\text{prob}(I_1|\varphi) = q_1 = 0.05.$$

If there are no sharks (¬φ), no fin will be observed (I_2):

$$\text{prob}(I_2|\neg\varphi) = q_2 = 1.$$

For each observer spotting a fin is evidence for sharks being in the water and spotting no fin is evidence for 'no sharks' ($q_1+q_2 = 1.05 > 1$). Moreover, the evidence given to each observer is decisive ($q^* > 0.9$).

So we have three experts independently (in the relevant sense) judging under equivalent epistemic conditions on whether φ or ¬φ is true. Each of the three judgments is evidence for φ described by an indicator model as in Figure 1 with identical parameters for all experts.

Suppose expert 1 spots a fin and the other two do not. Then the evidence suggests the presence of sharks:

[11] We assume an uneven number of experts to avoid problems with ties.

prob(φ| 1 observes I_1 AND 2 observes I_2 AND 3 observes I_2) = 1.

But the majority of the experts judge $\neg\varphi$: No sharks!

A GENERAL RESULT

The example illustrates that the aggregated judgment of a minority of experts may have more weight than the majority judgment even if all experts judge independently and under equivalent epistemic conditions. One may suspect that the result is an exception, the consequence of an extreme and exceptional parameter arrangement. A particularly striking feature of our example is the assumption that $q_2 = 1$: if there is no shark, no fins will be seen. This assumption, in fact, simplifies the problem dramatically. However, the force of the counterexample is not necessitated by this peculiar assumption. The example maintains its rebutting force if q_2 is changed to a value below but sufficiently near to 1.

There is a general mechanism behind the example: one indicator state may have more evidential force than another; if this is the case, *n* observations of the first indicator may outweigh *n*+1 observations of the second (for some integer *n*).

To be more precise, let us first specify what can plausibly be meant by saying that one indicator state has 'more weight' than another.

Definition 4. *Consider an indicator model of evidence in a yes/no problem as represented in Figure 1. Let* I_1I_2 *denote the event that two observations are independently made:* I_1 *is observed first and* I_2 *is observed second. The indicator state* I_1 *is said to* ***have more weight than*** *the indicator state* I_2 *iff the event* I_1I_2 *indicates* φ*, i.e., iff* prob(φ | I_1I_2) > p.

Here is a convenient characterisation:

Remark 3. *Consider an indicator model of evidence as given in Figure 1 with* I_1 *being evidence for* φ*. Then:*

I_1 *has more weight than* I_2 $\Leftrightarrow$ $q_1(1–q_1) > q_2(1–q_2)$ $\Leftrightarrow$ $q_1 < q_2$.

We can now state our general result (see the Appendix for proofs):

Main proposition. *Consider an indicator model of evidence in a yes/no problem as represented in Figure 1. Let* I *be decisive, let* I_1 *be evidence for* φ *and* I_2 *evidence for* $\neg\varphi$*. For any integer n, let* $I_1^nI_2^{n+1}$ *denote the event that 2n+1 observations are independently made,*

> I_1 *is observed n times and* I_2 *is observed n+1 times. Then the following holds:*
>
> *If* I_1 *has more weight than* I_2 *(i.e.,* $q_2 > q_1$*) and* $q_1 \neq 0$*, there is an integer n_0 such that for all $n \geq n_0$,* $\mathrm{I}_1^n \mathrm{I}_2^{n+1}$ *is decisive for* φ*.*

If the indicator states have different weights, then there is always a number *n* such that *n* expert judgments made with regard to independent observations of the weightier indicator state will outweigh a majority of *n*+1 judgments to the contrary by other experts.

CONCLUSION

The general question behind the problem discussed here is: How should we aggregate or accumulate the evidence obtained by different expert judgments? A seemingly natural suggestion seems to be: weigh the evidence of any single expert judgment in some suitable form representing her competence and her access to the relevant information; the total evidence then is determined by the weighted sum of the evidence given by the total multitude of individual judgments. If expert judgments come in the form of yes-or-no statements, and if all experts possess the same competence and have equivalent access to relevant information, then this amounts to accepting that the aggregated evidence favours the statement that is endorsed by a majority. The simple counterexample and the main proposition in the last section show that this is not a good way to aggregate the expert evidence in general.

The indicator model of evidence suggests a different way to aggregate expert evidence, namely by updating probability judgments according to Bayes' rule. In simple situations, similar to those analysed in this paper, this aggregation method will also amount to counting votes and forming judgment based on a simple number rule:

> *Let all experts independently judge under equivalent epistemic conditions and let expert judgments be aggregated according to Bayes' rule. If l denotes the number of experts that testify* φ*, then there is a number k such that 'l > k' is decisive for* φ*.*

In the example above $k = 0$. Notice that *k* not only depends on q_1 and q_2; it is also dependent on the prior probability $p = \mathrm{prob}(\varphi)$. So it will change in the process of learning about φ as more and more evidence is accumulated. The amount of confirmative expertise needed to believe a certain proposition φ is dependent upon the sort of claim that φ states and on its prior credibility. There is no general ratio of 'yes' to 'no' testimonies that can be used as a reliable decision rule when it comes to believing or disbelieving a statement.

How does all this relate to the fundamental role of the majority rule in theories of judgment aggregation? I will conclude with two short remarks to this question.

First, the alleged foundation rests on the assumption that *the amount of affirmation that is needed to justify a belief in a certain claim should not depend on the nature of the claim at stake*. This is the essence of the condition of systematicity in Pettit and List's (2002) impossibility result concerning the discursive dilemma. When spelled out and made explicit in this way it loses much of its initial plausibility. If I see a shark this is better evidence for the proposition 'there are sharks' than not seeing sharks is for 'no sharks'. And this is not a peculiarity of the example given here. The idea that the weight given to an affirmation should be the same for all claims independently of their specific content is also embodied by the common assumption (of most versions) of Condorcet's jury theorem that the competence of a juror is defined by one single and constant probability p of judging correctly.[12] Again, the example shows that this may not be appropriate. The probability that somebody who judges that there are no sharks on the basis that he did not observe one is wrong may well be larger than the probability of being wrong when judging the presence of sharks on the basis of observation. As statisticians would claim: the probability of an error of type 1 may not be identical to the probability of a type 2 error. And we know from many contexts such as medical diagnostics that type 1 and type 2 errors are, in fact, not equally probable. In our model the probability of type 1 and type 2 errors are represented by q_1 and q_2. The general result, thus, shows that whenever the type 1 and type 2 errors are not equally likely, we have a good reason to mistrust the majority rule in aggregating expert judgments.

Second, democracy is a normative standard in defining collective measures on the basis of individual interests. Individual opinions cannot have the same fundamental role in epistemology. Whereas interests are the ultimate, independent reference points in politics, opinions and judgments ought to be assessed in terms of a single more fundamental criterion: truth. Democracy (or the majority rule) and truth are not necessarily related. To be sure: the majority may (and often does)

[12] Kirchstein and Wangenheim 2010 is an exception to this rule.

indicate truth. But this is a contingent relationship that calls for critical investigation and cautious assessment on the basis of all relevant circumstances.

APPENDIX

Proof of Remark 1.

$$I_1 \text{ is evidence for } \varphi \Leftrightarrow \frac{q_1 p}{q_1 p + (1-p)(1-q_2)} > p$$
$$\Leftrightarrow q_1 > q_1 p + (1-p)(1-q_2)$$
$$\Leftrightarrow q_1 > 1 - q_2 \Leftrightarrow q_1 + q_2 > 1.$$

$$I_2 \text{ is evidence for } \neg\varphi \Leftrightarrow \frac{(1-p)q_2}{(1-q_1)p + (1-p)q_2} > 1-p$$
$$\Leftrightarrow q_2 > (1-q_1)p + (1-p)q_2$$
$$\Leftrightarrow q_2 > 1 - q_1 \Leftrightarrow q_1 + q_2 > 1.$$

Proof of Remark 2.

$$\text{prob}(\varphi|I_1) > 0.5 \Leftrightarrow \frac{q_1 p}{q_1 p + (1-p)(1-q_2)} > \frac{1}{2}$$
$$\Leftrightarrow 2q_1 p > q_1 p + (1-p)(1-q_2)$$
$$\Leftrightarrow q_1 p + q_2(1-p) > 1 - p \Leftrightarrow \frac{1-q_2}{q_1} < \frac{p}{1-p}.$$

$$\text{prob}(\neg\varphi|I_2) > 0.5 \Leftrightarrow \frac{(1-p)q_2}{(1-q_1)p + (1-p)q_2} > \frac{1}{2}$$
$$\Leftrightarrow 2(1-p)q_2 > (1-q_1)p + (1-p)q_2$$
$$\Leftrightarrow q_1 p + q_2(1-p) > p \Leftrightarrow \frac{p}{1-p} < \frac{q_2}{1-q_1}.$$

The third equivalence is a combination of the first two equivalences.

Proof of Remark 3.

From Bayes' rule we get:

$$\text{prob}(\varphi|I_1 I_2) = \frac{q_1(1-q_1)p}{q_1(1-q_1)p + q_2(1-q_2)(1-p)}.$$

Then:

$$\text{prob}(\varphi|\text{I}_1\text{I}_2) > \text{p} \quad \Leftrightarrow \frac{\text{q}_1(1-\text{q}_1)\text{p}}{\text{q}_1(1-\text{q}_1)\text{p} + \text{q}_2(1-\text{q}_2)(1-\text{p})} > \text{p}$$
$$\Leftrightarrow \text{q}_1(1-\text{q}_1) > \text{q}_1(1-\text{q}_1)\text{p} + \text{q}_2(1-\text{q}_2)(1-\text{p})$$
$$\Leftrightarrow \text{q}_1(1-\text{q}_1)(1-\text{p}) > \text{q}_2(1-\text{q}_2)(1-\text{p})$$
$$\Leftrightarrow \text{q}_1(1-\text{q}_1) > \text{q}_2(1-\text{q}_2).$$

Remember that I_1 is evidence for φ, and thus $\text{q}_1+\text{q}_2 > 1$. Therefore:

$$\text{prob}(\varphi|\text{I}_1\text{I}_2) > \text{p} \quad \Leftrightarrow \text{q}_1(1-\text{q}_1) > \text{q}_2(1-\text{q}_2)$$
$$\Leftrightarrow \text{q}_1 - \text{q}_2 > \text{q}_1^2 - \text{q}_2^2$$
$$\Leftrightarrow \text{q}_1 - \text{q}_2 > (\text{q}_1 - \text{q}_2)(\text{q}_1 + \text{q}_2)$$
$$\Leftrightarrow (\text{q}_1 - \text{q}_2 > 0 \wedge 1 > \text{q}_1 + \text{q}_2)$$
$$\vee\ (\text{q}_1 - \text{q}_2 < 0 \wedge 1 < \text{q}_1 + \text{q}_2)$$
$$\Leftrightarrow \text{q}_1 < \text{q}_2.$$

Proof of Main proposition.

From Bayes' rule we get:

$$\text{prob}(\varphi|\text{I}_1^n\text{I}_2^{n+1}) > 0.5 \quad \Leftrightarrow \frac{\text{q}_1^n(1-\text{q}_1)^{n+1}\text{p}}{\text{q}_1^n(1-\text{q}_1)^{n+1}\text{p} + \text{q}_2^{n+1}(1-\text{q}_2)^n(1-\text{p})} > .5$$
$$\Leftrightarrow \text{q}_1^n(1-\text{q}_1)^{n+1}\text{p} > \text{q}_2^{n+1}(1-\text{q}_2)^n(1-\text{p})$$
$$\Leftrightarrow \frac{\text{p}}{1-\text{p}} > \left(\frac{\text{q}_2(1-\text{q}_2)}{\text{q}_1(1-\text{q}_1)}\right)^n \cdot \frac{\text{q}_2}{1-\text{q}_1}$$

From Remark 3, we know that $\frac{\text{q}_2(1-\text{q}_2)}{\text{q}_1(1-\text{q}_1)} < 1$, so there is an n_0 such that for all $n > n_0$ the inequality holds.

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Measured, unmeasured, mismeasured, and unjustified pessimism: a review essay of Thomas Piketty's *Capital in the twenty-first century*

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Thomas Piketty has written a big book, 577 pages of text, 76 pages of notes, 115 charts, tables, and graphs, that has excited the left, worldwide. "Just as we said!" the leftists cry. "The problem is Capitalism and its inevitable tendency to inequality!" First published in French in 2013, an English edition was issued by Harvard University Press in 2014 to wide acclaim by columnists such as Paul Krugman, and a top position on the *New York Times* best-seller list. A German edition came out in late 2014, and Piketty—who must be exhausted by all this—worked overtime expositing his views to large German audiences. He plays poorly on TV, because he is lacking in humor, but he soldiers on, and the book sales pile up.

It has been a long time (how does "never" work for you?) since a technical treatise on economics has had such a market. An economist can only applaud. And an economic historian can only wax ecstatic. Piketty's great splash will undoubtedly bring many young economically interested scholars to devote their lives to the study of the past. That is good, because economic history is one of the few scientifically quantitative branches of economics. In economic history, as in experimental economics and a few other fields, the economists confront the evidence (as they do not for example in most macroeconomics or industrial organization or international trade theory nowadays). When you think about it, all evidence must be in the past, and some of the most interesting and scientifically relevant evidence is in the more or less remote past. And as the British economic historian John H. Clapham said in 1922—rather in the style of Austrian economists, though he was a Marshallian—"the economist is, willy-nilly, an historian. The world has moved on before his conclusions are ripe" (Clapham 1922, 313). True, economic historians are commonly concerned with the past also for its own sake (I am, for example), and not only as a way of extrapolating into the future, which is Piketty's purpose. His book after all is about capital in the *twenty-first* century, which has barely gotten under way. But if you are going to be a scientific economist, or a scientific geologist or astronomer or evolutionary biologist, the past should be your present.

Piketty gives a fine example of how to do it. He does not get entangled as so many economists do in the sole empirical tool they are taught, namely, regression analysis on someone else's "data" (one of the problems is the word *data*, meaning "things given": scientists should deal in *capta*, "things seized"). Therefore he does not commit one of the two sins of modern economics, the use of meaningless "tests" of statistical significance (he occasionally refers to "statistically insignificant" relations between, say, tax rates and growth rates, but I am hoping he does not suppose that a large coefficient is "insignificant" because R. A. Fisher in 1925 said it was). Piketty constructs or uses statistics of aggregate capital and of inequality and then plots them out for inspection, which is what physicists, for example, also do in dealing with their experiments and observations. Nor does he commit the other sin, which is to waste scientific time on existence theorems. Physicists, again, don't. If we economists are going to persist in physics envy let us at least learn what physicists actually do. Piketty stays close to the facts, and does not, for example, wander into the pointless worlds of non-cooperative game theory, long demolished by experimental economics. He also does not have recourse to non-computable general equilibrium, which never was of use for quantitative economic science, being a branch of philosophy, and a futile one at that. On both points, *bravissimo*.

His book furthermore is clearly and unpretentiously, if dourly, written, and I imagine is also in its original French (Piketty is to be commended for following the old rule, not so popular among *les français* nowadays, that *ce qui n'est pas clair n'est pas français*, "that which is not clear is not French"). True, the book is probably doomed to be one of those more purchased than read. Readers of a certain age will remember Douglas Hofstadter's massive *Gödel, Escher, Bach: an eternal golden braid* (1979), which sat admired but unread on many a coffee table in the 1980s, and rather younger readers will remember Stephen Hawking's *A brief history of time* (1988). The Kindle company from Amazon keeps track of the last page of your highlighting in a downloaded book (you didn't know that, did you?). Using this data, the mathematician Jordan Ellenberg (2014) reckons that the average reader of the 655 pages of text and footnotes of *Capital in the twenty-first century* stops somewhere a little past page 26, where the highlighting stops, about the end of the Introduction. To be fair to Piketty, a buyer of the hardback rather than the Kindle edition is probably a more serious reader, and would go further. Still, holding the attention of the average *New York Times* reader for a little over 26 pages of dense economic argument, after which the book takes an honored place on the coffee table, testifies to Piketty's rhetorical skill, which I do admire. The book is endlessly interesting, at any rate if you find intricate numerical arguments interesting.

It is an honest and massively researched book. Nothing I shall say—and I shall say some hard things, because they are true and important—is meant to impugn Piketty's integrity or his scientific effort. The book is the fruit of a big collaborative effort of the Paris School of Economics, which he founded, associated with some of the brightest lights in the techno-left of French economics. *Hélas*, I will show that Piketty is gravely mistaken in his science and in his social ethics. But so are many economists and calculators, some of them my dearest friends.

§

Reading the book is a good opportunity to understand the latest of the leftish worries about "capitalism", and to test its economic and philosophical strength. Piketty's worry about the rich getting richer is indeed merely "the latest" of a long series stretching back to Malthus and Ricardo and Marx. Since those founding geniuses of classical economics, a market-tested betterment (a locution to be preferred to "capitalism", with its erroneous implication that capital accumulation, not innovation, is what made us better off) has enormously enriched large parts of a humanity now seven times larger in population than in 1800, and bids fair in the next fifty years or so to enrich everyone on the planet. Look at China and India (and stop saying, "But not *everyone* there has become rich"; they will, as the European history shows, at any rate by the ethically relevant standard of basic comforts denied to most people in England and France before 1800, or in China before its new beginning in 1978, or in India before 1991). And yet the left in its worrying routinely forgets this most important secular event since the invention of agriculture—the Great Enrichment of the last two centuries—and goes on worrying and worrying.

Here is a partial list of the worrying pessimisms, which each has had its day of fashion since the time, as the historian of economic thought Anthony Waterman put it,

> Malthus's first [1798] *Essay* made land scarcity central. And so began a century-long mutation of 'political economy', the optimistic science of wealth, into 'economics', the pessimistic science of scarcity (Waterman 2012, 425, punctuation slightly modified).

Malthus worried that workers would proliferate and Ricardo worried that the owners of land would engorge the national product. Marx worried, or celebrated, depending on how one views historical materialism, that owners of capital would at least make a brave attempt to engorge it. (The classical economists are Piketty's masters, and his theory is self-described—before page 26—as the sum of Ricardo and Marx.) J. S. Mill worried, or celebrated, depending on how one views the sick hurry of modern life, that the stationary state was around the corner. Then economists, many on the left but some on the right, in quick succession from 1880 to the present—at the same time that market-tested betterment was driving real wages up and up and up—commenced worrying about, to name a few of the pessimisms concerning "capitalism" they discerned: greed, alienation, racial impurity, workers' lack of bargaining strength, workers' bad taste in consumption, immigration of lesser breeds, monopoly, unemployment, business cycles, increasing returns, externalities, under-consumption, monopolistic competition, separation of ownership from control, lack of planning, post-War stagnation, investment spillovers, unbalanced growth, dual labor markets, capital insufficiency (William Easterly calls it "capital fundamentalism"), peasant irrationality, capital-market imperfections, public choice, missing markets, informational asymmetry, third-world exploitation, advertising, regulatory capture, free riding, low-level traps, middle-level traps, path dependency, lack of competitiveness, consumerism, consumption externalities, irrationality, hyperbolic discounting, too big to fail, environmental degradation, underpaying of care, slower growth, and more.

One can line up the later items in the list, and some of the earlier ones revived à la Piketty or Krugman, with particular Nobel Memorial Prizes in Economic Science. I will not name here the men (all men, in sharp contrast to the method of Elinor Ostrom, Nobel 2009), but can reveal their formula. First, discover or rediscover a necessary condition for *perfect* competition or a *perfect* world (in Piketty's case, for example, a more perfect equality of income). Then assert without evidence (here Piketty does a great deal better than the usual practice) but with suitable mathematical ornamentation (thus Jean Tirole, Nobel 2014) that the condition might be imperfectly realized or the world might not develop in a perfect way. Then conclude with a flourish (here however Piketty falls in with the usual low scientific standard) that "capitalism" is doomed unless experts intervene with a sweet use of the monopoly of violence in government to implement anti-trust against malefactors of great wealth, or subsidies to diminishing-returns industries, or foreign aid to perfectly honest governments, or money for obviously infant industries, or the nudging of sadly childlike consumers, or, Piketty says, a tax on inequality-causing capital worldwide.

A feature of this odd history of fault-finding and the proposed statist corrections is that seldom does the economic thinker feel it necessary to offer evidence that his (mostly "his") proposed state intervention will work as it is supposed to, and almost never does he feel it necessary to offer evidence that the imperfectly attained necessary condition for perfection before intervention is large enough to have much reduced the performance of the economy in aggregate. (I repeat: Piketty exceeds the usual standard here.) Clapham made such a complaint in 1922 when the theorists were proposing on the basis of a diagram or two that government should subsidize allegedly increasing returns industries. The economists did not say how to attain the knowledge to do it, or how much their non-quantitative advice would actually help an imperfect government to get closer to the perfect society. The silence was discouraging, Clapham wrote sharply, to "the student not of categories but of things". It still is now, ninety years on. He chided Pigou thus: one looks into "*The economics of welfare* to find that, in nearly a thousand pages, there is not even one illustration of what industries are in which boxes [that is, in which theoretical categories], though many an argument begins, 'when conditions of diminishing returns prevail' or 'when conditions of increasing returns prevail', as if everyone knew when that was". He ventriloquizes the reply of the theorist imagining without quantitative oomph "those empty economic boxes", a reply heard still, with no improvement in its plausibility: "If those who know the facts cannot do the fitting, we [theorists finding grave faults in the economy] shall regret it. But our doctrine will retain its logical and, may we add, its pedagogic value. And then you know it goes so prettily into graphs and equations" (Clapham 1922, 311, 305, 312).

A rare exception to the record of not checking out what oomph an alleged imperfection might have was the Marxists Paul Baran's and Paul Sweezy's book *Monopoly capital* (1966), which actually tried (and honorably failed) to measure the extent of monopoly overall in the American economy. For most of the other worries on the list—such as that externalities require government intervention (as have declared in historical succession Pigou, Samuelson, and Stiglitz)—the economists so claiming that the economy is horribly malfunctioning and needs immediate, massive intervention from government advised by wise heads such as Pigou, Samuelson, and Stiglitz have not felt it was worth their scientific time to show that the malfunctioning matters much in aggregate. Piketty tries (and honorably fails). The sheer number of the briefly fashionable but never measured "imperfections" has taught young economists—they naïvely believe there *must* be facts behind the pretty theorems in their textbooks—to believe that market-tested betterment has worked disgracefully badly, when all the quantitative instruments agree that since 1800 it has worked spectacularly well.

By contrast, economists such as Arnold Harberger and Gordon Tullock claiming on the contrary that the economy works pretty well have done the factual inquiry, or have at least suggested how it might be done (see, e.g., Harberger 1954; Tullock 1967). The performance of Pigou, Samuelson, Stiglitz, and the rest on the left (admittedly in these three cases a pretty moderate "left") would be as though an astronomer proposed on some qualitative assumptions that the hydrogen in the sun would run out very, very soon, but did not bother to find out with serious observations and quantitative simulations *roughly how soon* the sad event was going to happen. Mostly in economic theory it has sufficed to show the mere direction of an "imperfection" on a blackboard (that is, it has sufficed to propound the "qualitative theorems" so disastrously recommended in Samuelson's *Foundations*) and then await the telephone call from the Swedish Academy early on an October morning.

One begins to suspect that the typical leftist—most of the graver worries have come from thereabouts, though not so very naturally, considering the great payoff of "capitalism" for the working class—starts with a root conviction that capitalism is seriously defective. The conviction is acquired at age 16 when he discovers poverty but has no intellectual tools to understand its source. I followed this pattern, and therefore became for a time a Joan-Baez socialist. Then the lifelong "good social democrat", as he describes himself (and as I for a while described myself), in order to support the now deep-rooted conviction, looks around when he has become a professional economist for any qualitative indication that in some imagined world the conviction would be true, without bothering to attach numbers drawn from our own world (of which, I say yet again, our Piketty can *not* be accused). It is the utopianism of good-hearted leftward folk who say, "Surely this wretched society, in which some people are richer and more powerful than others, can be greatly improved. We can do much, much better!" The utopianism springs from the logic of stage theories, conceived in the eighteenth century as a tool with which to fight traditional society, as in *The wealth of nations*, among lesser books.

True, the right can be accused of utopianism as well, when it asserts without evidence, as do some of the older-model Austrian economists, and as do some of the Chicago School who have lost their taste for engaging in serious testing of their truths, that we live already in the very best of all possible worlds. Yet admitting that there is a good deal of blame to spread around, the leftward refusal to quantify about the system as a whole seems to me more prevalent and more dangerous. I have a beloved and extremely intelligent Marxist friend who says to me, "I *hate* markets!" I reply, "But Jack, you delight in searching for antiques *in markets*". "I don't care. I *hate* markets!" The Marxists in particular have worried in sequence that the typical European worker would be immiserized, for which they had little evidence, then that he would be alienated, for which they had little evidence, then that the typical Third-World-periphery worker would be exploited, for which they had little evidence. Recently the Marxists and the rest of the left have commenced worrying about the environment, on what the late Eric Hobsbawm called with a certain distaste natural in an old Marxist "a much more middle-class basis" (Hobsbawm 2011, 416). We await their evidence, and their proposals for what to do about it, other than returning to Walden Pond and the life of 1800.

Long ago I had a nightmare. I am not much subject to them, and this one was vivid, an economist's nightmare, a Samuelsonian one. What if *every single* action had to be performed exactly optimally? Maximize Utility subject to Constraints. Max U s.t.C. Suppose, in other words, that you had to reach the *exact* peak of the hill of happiness subject to constraints with *every single* reaching for the coffee cup or *every single* step in the street. You would of course fail in the assignment repeatedly, frozen for fear of the slightest deviation from optimality. In the irrational way of nightmares, it was a chilling vision of what economists call rationality. A recognition of the impossibility of exact perfection lay, of course, behind Herbert Simon's satisficing, Ronald Coase's transaction costs, George Shackle's and Israel Kirzner's reaffirmation of Yogi Berra's wisdom: "It's tough to make predictions, especially about the future".

We young American economists and social engineers in the 1960s, innocent as babes, were sure we could attain predictable perfection. "Fine tuning" we called it. It failed, as perfection must. The political scientist John Mueller (1999) made the point that we should be seeking instead merely the "pretty good"—which would require some fact-based sense that we are not too terribly far from optimality in, say, Garrison Keillor's imagined Lake Wobegon, Minnesota in which Ralph's Pretty Good Grocery is in its advertising comically modest and Scandinavian ("If you can't find it at Ralph's, you probably don't need it anyway"). Mueller reckons that capitalism and democracy as they actually, imperfectly are in places like Europe or its offshoots are pretty good. The "failures" to reach perfection in, say, the behavior of Congress or the equality of the distribution of income in the U.S.A., Mueller reckons, are probably not large enough to matter all that much to the performance of the polity or the economy. They are good enough for Lake Wobegon. And driving across town to buy at the Exact Perfection Store, staffed by economic theorists specialized in finding failures in the economy without measuring them, often leads to consequences you probably do not need.

At least, then, Piketty is a serious quantitative scientist, unlike the other boys playing in the sandboxes of statistics of significance and theorems of existence and unmeasured imperfections in the economy and the setting of impossible tasks (unhappily in this last respect he joins the boys and their sandcastles) for an imperfect government. Indeed, Piketty declares that:

> it is important to note that […] the main source of divergence [of the incomes of the rich compared with the poor] in my theory has nothing to do with any market imperfection [note: possible governmental imperfections are off the Piketty table]. Quite the contrary: the more perfect the capital market (in the economist's sense) the more likely [the divergence] (p. 27; compare p. 573).

That is, like Ricardo and Marx and Keynes, he thinks he has discovered what the Marxists call a "contradiction" (p. 571), that is, an unhappy consequence of the very perfection of "capitalism". Yet all the worries from Malthus to Piketty, from 1798 to the present, share an underlying pessimism, whether about imperfection in the capital market or about the behavioral inadequacies of the individual consumer or about the Laws of Motion of a Capitalist Economy—this in the face of the largest enrichment that humans have ever witnessed. During the pretty good history of 1800 to the present the economic pessimists on the left have nonetheless been subject to nightmares of terrible, terrible failures

Admittedly, such pessimism sells. For reasons I have never understood, people like to hear that the world is going to hell, and become huffy and scornful when some idiotic optimist intrudes on their pleasure. Yet pessimism has consistently been a poor guide to the modern economic world. We are gigantically richer in body and spirit than we were two centuries ago. In the next half century—if we do not kill the goose that lays the golden eggs by implementing leftwing schemes of planning and redistribution or rightwing schemes of imperialism and warfare, as we did in many places, 1914-1989, following the advice of the clerisy that markets and democracy are terribly faulty—we can expect the entire world to match Sweden or France.

§

Piketty's central theme is the force of interest on inherited wealth, causing, he claims, inequality of income to increase. In 2014 he declared to the BBC's Evan Davis in an interview that "money tends to reproduce itself", a complaint about money and its interest repeatedly made in the

West since Aristotle. As the Philosopher said of some men, "the whole idea of their lives is that they ought either to increase their money without limit, or at any rate not to lose it […]. The most hated sort [of increasing their money], […] is usury, which makes a gain out of money itself" (Aristotle *Politics*, Book I).

Piketty's (and Aristotle's) theory is that the yield on capital usually exceeds the growth rate of the economy, and so the share of capital's returns in national income will steadily increase, simply because interest income—what the presumably rich capitalists get and supposedly manage to cling to and supposedly reinvest—grows faster than the income the whole society is getting. Aristotle and his followers, such as Aquinas and Marx and Piketty, were much concerned with such "unlimited" gain. The argument is, you see, very old and very simple. Piketty ornaments it a bit with some portentous accounting about capital-output ratios and the like, producing his central inequality about inequality: so long as $r > g$, where r is the return on capital and g is the growth rate of the economy, we are doomed to ever increasing rewards to rich capitalists while the rest of us poor suckers fall relatively behind. The merely verbal argument I just gave, however, is conclusive, so long as the factual assumptions are near-enough true: namely, only rich people have capital; human capital does not exist; the rich reinvest their returns—they never lose it to sloth or someone else's creative destruction; inheritance is the main mechanism, not creativity raising g for the rest of us just when it results in r shared by us all; and we care ethically only about the Gini coefficient, not the condition of the working class.

Notice one aspect of that last: in Piketty's tale the rest of us fall only *relatively* behind the ravenous capitalists. The focus on *relative* wealth or income or consumption is one serious problem in the book. Piketty's vision of a "Ricardian Apocalypse", as he calls it, leaves room for the rest of us to do very well indeed, most non-apocalyptically, as in fact since 1800 we have. What is worrying Piketty is that the rich might possibly get richer, even though the poor get richer too. His worry, in other words, is purely about difference, about the Gini coefficient, about a vague feeling of envy raised to a theoretical and ethical proposition.

Another serious problem is that r will almost always exceed g, as anyone can tell you who knows about the rough level of interest rates on invested capital and about the rate at which most economies have grown (excepting only China, recently, where contrary to Piketty's prediction, inequality has increased). If his simple logic is true, then the Ricardian Apocalypse looms, *always*. Let us therefore bring in the sweet and blameless and omni-competent government—or, even less plausibly, a world government—to implement "a progressive *global* tax on capital" (p. 27) to tax the rich. It is our only hope.

Yet in fact his own things ingeniously seized in his research, his *capta*, as he candidly admits without allowing the admission to relieve his pessimism, suggest that only in Canada, the U.S.A., and the U.K. has the inequality of income increased much, and only recently. "In continental Europe and Japan, income inequality today remains far lower than it was at the beginning of the twentieth century and in fact has not changed much since 1945" (p. 321, and Figure 9.6). Look, for example, at page 323, Figure 9.7, the top decile's share of income, 1900-2010 for the U.S.A., the U.K., Germany, France, and Sweden. In all those countries $\boldsymbol{r} > \boldsymbol{g}$. Indeed, it has been so, with rare exceptions, very occasionally, since the beginning of time. Yet after the redistributions of the welfare state were accomplished, by 1970, inequality of income did *not* much rise, Piketty admits, in Germany, France, and Sweden. In other words, Piketty's fears were not confirmed anywhere 1910 to 1980, nor anywhere in the long run at any time before 1800, nor anywhere in continental Europe and Japan since World War II, and only recently, a little, in the United States, the United Kingdom, and Canada (Canada, by the way, is never brought into his tests).

That is a very great puzzle if money tends to reproduce itself, always, evermore, as a general law governed by the Ricardo-plus-Marx inequality at the rates actually observed in world history. Yet inequality in fact goes up and down in great waves, for which we have evidence from many centuries ago down to the present which also does not figure in his tale (Piketty barely mentions the work of the economic historians Jeffrey Williamson and Peter Lindert (1980) documenting that inconvenient fact). According to his logic, once a Piketty-wave starts—as it would at any time you care to mention if an economy satisfied the almost-always-satisfied condition of the interest rate exceeding the growth rate of income—it would never stop. Such an inexorable logic means we should have been overwhelmed by an inequality-tsunami in 1800 CE or in 1000 CE or for that matter in 2000 BCE. At one point Piketty says just that: "$\boldsymbol{r} > \boldsymbol{g}$ will *again* become the norm in the twenty-first century, *as it had been throughout history until the eve of World War I*" (p. 572, italics added; one wonders what he does with historically low interest rates right now, or the negative real interest rates in the inflation of the 1970s and 1980s). Why then did the share of the rich not rise anciently to 100 percent? At the least, how could the share be stable at, say, the 50 percent that in medieval times typified unproductive economies with land and landlords dominant? Sometimes Piketty describes his machinery as a "potentially explosive process" (p. 444), at other times he admits that random shocks to a family fortune mean that "it is unlikely that inequality of wealth will grow indefinitely, […] rather, the wealth distribution will converge toward a certain equilibrium" (p. 451). On the basis of the *Forbes* lists of the very rich, Piketty notes, for example, "several hundred new fortunes appear in [the $1 billion to $10 billion] range somewhere in the world almost every year" (p. 441). Which is it, Professor Piketty? 'Apocalypse' as you put it, or (what is in fact observed, roughly, with minor ups and downs) a steady share of rich people constantly dropping out of riches or coming into them, in evolutionary fashion? His machinery seems to explain nothing alarming, and at the same time does too much alarming.

The science writer Matt Ridley has offered a persuasive reason for the (slight) rise in inequality recently in Britain. "Knock me down with a feather", Ridley writes,

> You mean to say that during three decades when the government encouraged asset bubbles in house prices; gave tax breaks to pensions; lightly taxed wealthy non-doms [that is, "non-domiciled", the citizens of other countries such as Saudi Arabia living in the U.K.]; poured money into farm subsidies [owned by landlords mainly rich]; and severely restricted the supply of land for housing, pushing up the premium earned by planning permission for development, the wealthy owners of capital saw their relative wealth increase slightly? Well, I'll be damned […] [Seriously, now] a good part of any increase in wealth concentration since 1980 has been driven by government policy, which has systematically redirected earning opportunities to the rich rather than the poor (Ridley 2014).

In the United States, with its pervasive welfare payments and tax breaks for our good friends the very rich, such as the treatment of "carried interest" which made Mitt Romney a lot richer, one can make a similar case that the government, which Piketty expects to solve the alleged problem, is the cause. It was not "capitalism" that caused the recent and restricted blip, and certainly not market-tested betterment at the extraordinary rates of the past two centuries.

The inconsequence of Piketty's argument, in truth, is to be expected from the frailties of its declared sources. Start by adopting a theory by a great economist, Ricardo, which failed entirely as a prediction. Landlords did not engorge the national product, contrary to what Ricardo confidently predicted. Indeed the share of land rents in national (and world) income fell heavily nearly from the moment Ricardo claimed it would steadily rise. The outcome resembles that for Malthus, whose prediction of population overwhelming the food supply was falsified nearly from the moment he claimed it would happen.

All right. Then combine Ricardo's with another theory by a less-great *economist*, Marx (yet the greatest *social scientist* of the nineteenth century; without compare though mistaken on almost every substantive point, and especially in his predictions). Marx supposed that wages would fall and yet profits would also fall and yet technological betterments would also happen. Such an accounting, as the Marxist economist Joan Robinson frequently pointed out, is impossible. At least one, the wages or the profits, has to rise if technological betterment is happening, as it so plainly was. With a bigger pie, someone has to get more. In the event what rose were wages on raw labor and especially a great accumulation of *human* capital, but capital owned by the laborers, not by the truly rich. The return to physical capital was higher than a riskless return on British or American government bonds, in order to compensate for the risk in holding capital (such as being made obsolete by betterment—think of your computer, obsolete in four years). But the return on physical capital, and on human capital, was anyway held down to its level of very roughly 5 to 10 percent by competition among the proliferating capitalists. Imagine our immiserization if the income of workers, because they did *not* accumulate human capital, and their societies had *not* adopted the accumulation of ingenuities since 1800, had experienced the history of stagnation since 1800 that the per-unit return to capital has. It is not hard to imagine, because workers earn such miserable incomes even now in places like Somalia and North Korea. Instead, since 1800 in the average rich country the income of the workers per person increased by a factor of about 30 (2,900 percent, if you please) and even in the world as a whole, including the still poor countries, by a factor of 10 (900 percent), while the rate of return to physical capital stagnated (McCloskey 2015, chapter 2).

Piketty does not acknowledge that each wave of inventors, entrepreneurs, and even routine capitalists find their rewards taken from them by entry, which is an economic concept he does not appear to grasp. His lack of grasp is a piece with his failure to understand supply responses, that is, how increased scarcity leads to the entry of new firms Look at the history of fortunes in department stores. The income from department stores in the late nineteenth century, in Le Bon Marché, Marshall Fields, and Selfridge's, was entrepreneurial. The model was then copied all over the rich world, and was the basis for little fortunes in Cedar Rapids, Iowa and Benton Harbor, Michigan. Then in the late twentieth century the model was challenged by a wave of discounters, and they then in turn by the Internet. The original accumulation slowly or quickly dissipates. In other words, the profit going to the profiteers is more or less quickly undermined by outward-shifting supply, if governmental monopolies and protectionisms of the sort Ridley noted in Britain do not intervene. The economist William Nordhaus (2004) has calculated that inventors and entrepreneurs nowadays earn in profit only 2 percent of the social value of their inventions. If you are Sam Walton that 2 percent earns you personally a great deal of money for introducing bar codes into stocking supermarket shelves. But 98 percent at the cost of 2 percent is nonetheless a pretty good deal for the rest of us. The gain from macadamized roads or vulcanized rubber, then modern universities, structural concrete, and the airplane, has enriched even the poorest among us.

Piketty, who does not believe in supply responses, focuses instead on the great evil of very rich people having seven Rolex watches by mere inheritance. Liliane Bettencourt, heiress to the L'Oréal fortune (p. 440), the third richest woman in the world, who "has never worked a day in her life, saw her fortune grow exactly as fast as that of [the admittedly bettering] Bill Gates". That is bad, Piketty says, which is his ethical philosophy in full.

The Australian economists Geoffrey Brennan, Gordon Menzies, and Michael Munger make a similar argument in a recent paper, written in advance of Piketty's book, that inheritance *inter vivos* of human capital is bound to exacerbate Gini-coefficient inequality because "for the first time in human history richer parents are having fewer children [...]. Even if the increased opulence continues, it will be concentrated in fewer and fewer hands" (Brennan, et al. 2014). The rich will send their one boy, intensively tutored in French and mathematics, to Sydney Grammar

School and on to Harvard. The poor will dissipate what little they have among their supposedly numerous children.

But if on account of Adam Smith's hoped-for "universal opulence which extends itself to the lowest ranks of the people" all have access to excellent education—which is an ethically sensible object of social policy, unlike Gini-coefficient inequality, and has the additional merit of being achievable—and if the poor are so rich (because the Great Enrichment has been unleashed) that they, too, have fewer children, which is the case in, say, Italy, then the tendency to rising variance will be attenuated (see Smith 1776, book I, ch. 1, para. 10). The economist Tyler Cowen reminds me, further, that "low" birth rates also include "zero children", which would make lines die out—as indeed they often did even in well nourished royal families. Non-existent children, such as those of Grand Duke of Florence Gian Gastone de' Medici in 1737, cannot inherit, *inter vivos* or not. Instead their very numerous second- and third-cousins do.

And the effect of inherited wealth on children is commonly to remove their ambition, as one can witness daily on Rodeo Drive. Laziness—or for that matter regression to the mean of ability—is a powerful equalizer. "There always comes a time", Piketty writes against his own argument, "when a prodigal child squanders the family fortune" (p. 451), which was the point of the centuries-long struggle in English law for and against entailed estates. Imagine if you had access to ten million dollars at age 18, before your character was fully formed. It would have been an ethical disaster for you, as it regularly is for the children of the very rich. We prosperous parents of the Great Enrichment can properly worry about our children's and especially our grandchildren's incentives to such efforts as a Ph.D. in economics, or serious entrepreneurship, or indeed serious charity. However many diamond bracelets they have, most rich children, and maybe all our children in the riches that the Great Enrichment is extending to the lowest ranks of the people, will not suffer through a Ph.D. in economics. Why bother? David Rockefeller did (University of Chicago, 1940; and he *did* understand supply responses), but his grandfather was unusually lucky in transmitting born-poor values to his son John Jr. and then to his five John-Junior-begotten grandsons (though not to his one granddaughter in that line, Abby, who never worked a day in her life).

Because Piketty is obsessed with inheritance, moreover, he wants to downplay entrepreneurial profit, the market-tested betterment that has made the poor rich. It is again Aristotle's claim that money is sterile and interest is therefore unnatural. Aristotle was on this matter mistaken. It is commonly the case, contrary to Piketty, and setting aside the cheapening of our goods produced by the investments of their wealth by the rich, that the people with more money got their more by being more ingeniously productive, for the benefit of us all—getting that Ph.D., for example, or being excellent makers of automobiles or excellent writers of horror novels or excellent throwers of touchdown passes or excellent providers of cell phones, such as Carlos Slim of Mexico, the richest man in the world (with a little boost, it may be, from corrupting the Mexican parliament). That Frank Sinatra became richer than most of his fans was not an ethical scandal. The "Wilt Chamberlain" example devised by the philosopher Robert Nozick (Piketty mentions John Rawls, but not Nozick, who was Rawls's nemesis) says that if we pay voluntarily to get the benefit of clever CEOs or gifted athletes there is no further ethical issue. The unusually high rewards to the Frank Sinatras and Jamie Dimons and Wilt Chamberlains come from the much wider markets of the age of globalization and mechanical reproduction, not from theft. Wage inequality in the rich countries experiencing an enlarging gap of rich vs. poor, few though they are (Piketty's finding, remember), is mainly, he reports, caused by "the emergence of extremely high remunerations at the summit of the wage hierarchy, particularly among top managers of large firms". The emergence, note, has nothing to do with $\boldsymbol{r} > \boldsymbol{g}$.

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The technical flaws in the argument are pervasive. When you dig, you find them. Let me list two that I myself spotted. Other economists, I have heard, have spotted many more: google "Piketty". (I have not done the googling, since I do not want merely to pile on. I respect what he tried to accomplish, and he therefore deserves from me an independent evaluation.)

For example—a big flaw, this one—Piketty's definition of wealth does not include human capital, owned by the workers, which has grown in rich countries to be the main source of income, when it is combined with the immense accumulation since 1800 of capital in knowledge and social habits, owned by everyone with access to them. Therefore his laboriously assembled charts of the (merely physical and private) capital/output ratio are erroneous. They have excluded one of the main forms of capital in the modern world. More to the point, by insisting on defining capital as something owned nearly always by rich people, Piketty mistakes the source of income, which is chiefly embodied human ingenuity, not accumulated machines or appropriated land. He asserts somewhat mysteriously on page 46 that there are "many reasons for excluding human capital from our definition of capital". But he offers only one: "human capital cannot be owned by any other person". Yet human capital is owned precisely by the worker herself. Piketty does not explain why self-ownership without alienation permitted (à la Locke) is not ownership. If I own improved land, and the law prevents its alienation (as some collectivist laws do), why is it not capital? Certainly, human capital is "capital": it accumulates through abstention from consumption, it depreciates, it earns a market-determined rate of return, it can be made obsolete by creative destruction.

Once upon a time, to be sure, Piketty's world without human capital was approximately our world, that of Ricardo and Marx, with workers owning only their hands and backs, and the bosses and landlords owning all the other means of production. But since 1848 the world has been transformed by what is between the workers' ears. The result of excluding human capital from capital is to artificially force the conclusion Piketty wants to achieve, that inequality has increased, or will, or might, or is to be feared. One of the headings in chapter 7 declares that "capital [is] always more unequally distributed than labor". No, it is not. If human capital is included—the ordinary factory worker's literacy, the nurse's educated skill, the professional manager's command of complex systems, the economist's understanding of supply responses—the workers themselves in the correct accounting own most of the nation's capital, and Piketty's drama from 1848 falls to the ground.

The neglect of human capital on the Problems side of the book is doubly strange because on the Solutions side Piketty recommends education and other investments in human capital. Yet in his focus on raising the marginal product of unemployed workers by government program rather than by correcting the distortions that created the unemployment in the first place he joins most of the left, especially those with university jobs. Thus in South Africa the left proposes to carry on with high minimum wages and oppressive regulation, solving the unemployment problem governmentally generated by improving through the same government the education of unemployed South Africans. No one, left or right or libertarian, would want to complain about better education, especially if it falls from the sky at no opportunity cost—though we bleeding heart libertarians would suggest achieving it by some other means than by pouring more money into a badly functioning nationalized industry providing elementary education or into a higher education system grossly favoring the rich over the poor, as it does strikingly in France, by giving the rich student, better prepared, a tuition-free ride into the ruling class. In any case the "we-love-education" ploy exempts the left from facing the obvious cause of unemployment in South Africa, namely, a sclerotic system of labor-market and other regulations in aid of the Congress of South African Trade Unions and against the wretchedly poor black South African sitting jobless with a small income subsidy in a hut in the back country of KwaZulu-Natal.

Piketty's book is by no means without good and interesting and technical economics. He offers an interesting theory (chapter 14), for example, that the very high CEO salaries we have nowadays in the U.K. and especially the U.S.A. are a result of the fall in marginal tax rates from their high levels during 1930-1970. In those halcyon days it was not so bright of the managers to pay themselves huge salaries which after all the government would take away on March 15. Once this disincentive was removed, Piketty plausibly argues, the managers could take advantage of the clubby character of executive-remuneration committees to go to town. And so Piketty recommends returning to 80 percent marginal income tax rates (p. 513). But wait. Technically speaking, if on ethical grounds we do not like high CEO salaries, why not legislate against them directly, using some more targeted tool than a massive intrusion into the economy? Or why not shame the executive-remuneration committees? Piketty does not say.

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The fundamental technical problem in the book, however, as I have hinted, is that Piketty the economist does not understand supply responses. Because he doesn't understand supply responses he thinks that any tightness in supply is permanent, which is how he gets his Ricardian Apocalypse and all our woe. In keeping with his position as a man of the left, he has a vague and confused idea about how markets work, and especially about how supply responds to higher prices. If he wants to offer pessimistic conclusions concerning "a market economy based on private property, if left to itself" (p. 571), he had better know what elementary economics, agreed to by all who have studied it enough to understand what it is saying, does in fact say about how a market economy based on private property behaves when left to itself.

Startling evidence of Piketty's misunderstanding occurs as early as page 6. He begins by seeming to concede to his neoclassical opponents (he is I repeat a proud Classicist: Ricardo plus Marx).

> To be sure, there exists in principle a quite simple economic mechanism that should restore equilibrium to the process [in this case the process of rising prices of oil or urban land leading to a Ricardian Apocalypse]: the mechanism of supply and demand. If the supply of any good is insufficient, and its price is too high, *then demand for that good should decrease, which would lead to a decline in its price* (p. 6, italics added).

The (English) words I italicize clearly mix up movement along a demand curve with movement of the entire curve, a first-term error at university. The correct analysis (we tell our first-year, first-term students at about week four) is that if the price is "too high" it is not the whole demand curve that "restores equilibrium" (though the high price in the short run does give people a reason to conserve on oil or urban land with smaller cars and smaller apartments, moving as they in fact do up along their otherwise stationary demand curves), but an eventually *outward-moving supply curve*. The supply curve moves out because entry is induced by the smell of super-normal profits in the medium and long run (which is the Marshallian definition of the terms). New oil deposits are discovered, new refineries are built, new suburbs are settled, new high-rises saving urban land are constructed, as has in fact happened massively since, say, 1973, unless government has restricted oil exploitation (usually on environmental grounds) or the building of high-rises (usually on corrupt grounds).

Piketty goes on—remember: it does not occur to him that high prices cause after a while the *supply* curve to move out; he thinks the high price will cause the demand *curve* to move *in*, leading to "a decline in price" (of the scarce item, oil or urban land)—"such adjustments might be unpleasant or complicated". To show his contempt for the ordinary working of the price system he imagines comically that "people should [...] take to traveling about by bicycle". The substitutions along a given demand curve, or one mysteriously moving in, "might also take decades, during which landlords and oil well owners might well accumulate claims on the rest of the population" (now he has the demand curve moving *out*, for some reason faster than the supply curve moves out) "so extensive that they could easily [on grounds not argued] come to own everything that can be owned, including" in one more use of the comical alternative, "bicycles, once and for all". Having butchered the elementary analysis of entry and of substitute supplies, which after all is the economic history of the world, he speaks of "the emir of Qatar" as a future owner of those bicycles, once and for all. The phrase must have been written before the recent and gigantic expansion of oil and gas exploitation in Canada and the United States. In short, he concludes triumphantly, having seen through the obvious silliness found among those rich-friendly neoclassical economists, "the interplay of supply and demand in no way rules out the possibility of a large and lasting divergence in the distribution of wealth linked to extreme changes in certain relative prices […]. Ricardo's scarcity principle" (pp. 6-7).

I was so startled by the passage that I went to the French original and called on my shamefully poor French to make sure it was not a mistranslation. A charitable reading might say it was—very charitable indeed because after all the preparatory senselessness remains: "then demand [the whole demand curve?] for that good should decrease" (*alors la demande pour ce bien doit baisser*). Yet Piketty's English is much better than my French—he taught for a couple of years at MIT, and speaks educated English when interviewed. If he let stand the senselessness in the translation by Arthur Goldhammer (a mathematics Ph.D. who has since 1979 done fully 75 translations of books from the French—though admittedly this is his first translation of technical economics), especially in such an important passage, one has to assume that he thought it was fine economics, a penetrating, nay decisive, criticism of those silly native-English-or-German-speaking economists who think that supply curves move out in response to increased scarcity. (Yet again I urge a bit of charity: she who has never left a little senselessness in her texts, and especially in translations out of her native language, is invited to cast the first stone.) In the French version one finds, instead of the obviously erroneous English, "which should lead to a decline in its price", typical of the confused first-term student, the clause *qui permettra de calmer le jeu*, "which should permit some calming down", or more literally, "which would permit some calming of the play [of, in this case, supply and demand]". *Calmer le jeu*, though, is in fact sometimes used in economic contexts in French to mean heading off a price bubble. And what "calming down" could mean in the passage other than an economics-and-common-sense-denying *fall* in price without a supply response having taken place is hard to see. The rest of the passage does not support the charitable reading. The rest is uncontroversially translated, and spins out the conviction Piketty evidently has that supply responses do not figure in the story of supply and demand, which anyway is unpleasant and complicated—so much less so than, say, the state taking a radically larger share of national income in taxes, with its attendant inefficiencies, or the state encouraging the spurning of capitalist ownership in favor of "new forms of governance and shared ownership intermediate between public and private" (p. 573), with its attendant corruptions and lack of skin in the game.

Piketty, it would seem, has not read with understanding the theory of supply and demand that he disparages, such as in Smith (one sneering remark on p. 9), Say (ditto, mentioned in a footnote with Smith as optimistic), Bastiat (no mention), Walras (no mention), Menger (no mention), Marshall (no mention), Mises (no mention), Hayek (one footnote citation on another matter), Friedman (pp. 548-549, but only on monetarism, not the price system). He does not have the scientific standing to sneer at self-regulating markets (for example on p. 572), because he shows in this and many other passages that does not understand how they work, even in principle. It would be like someone attacking the theory of evolution (which is identical to the theory economics uses of entry and exit in self-regulating markets—the supply response—an early version of which inspired Darwin) without understanding natural selection or the Galton-Watson process or modern genetics.

In a way, it is not his fault. He was educated in France, and the French-style teaching of economics, against which the insensitively-named Post-Autistic Economics (PAE) movement by economics students in France was directed, is abstract and Cartesian, and never teaches the ordinary price theory that one might use to understand the oil market, 1973 to the present.[1] Because of supply responses, never considered in books by non-economists such as Paul Ehrlich's *The population bomb*

[1] On the other hand, the French economist Bernard Guerrien who inspired the movement has his own problems, see McCloskey 2006b.

(1968) or by economists who do not understand elementary economics, the real price of oil, for example, has *fallen* since 1980.

More deeply, Piketty's "structural" thinking characterizes the left, and characterizes too the economic thinking of physical and biological scientists when they venture into economic issues. It is why the magazine *Scientific American* half a century ago loved input-output analysis (which was the love also of my own youth) and regularly publishes fixed-coefficient arguments about the environment by physical and biological scientists. The non-economic scientists declare: "We have such-and-such a structure in existence, which is to say the accounting magnitudes presently existing, for example the presently known reserves of oil". Then, ignoring that search for new reserves is in fact an economic activity, they calculate the result of rising "demand" (that is, quantity demanded, not distinguished from the whole demand curve), assuming no substitutions, no along-the-demand curve reaction to price, no supply reaction to price, no second or third act, no seen and unseen, such as an entrepreneurial response to greater scarcity. In the mid-nineteenth century it was Marx's scientific procedure, too, and Piketty follows it.

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Beyond technical matters in economics, the fundamental *ethical* problem in the book is that Piketty has not reflected on why inequality by itself would be bad. The Liberal Lady Glencora Palliser (née M'Cluskie) in Anthony Trollope's political novel *Phineas Finn* (1867-1868) declares that "Making men and women all equal. That I take to be the gist of our political theory", as against the Conservative delight in rank and privilege. But one of the novel's radicals in the Cobden-Bright-Mill mold ("Joshua Monk") sees the ethical point clearer: "Equality is an ugly word, and frightens", as indeed it had long frightened the political class in Britain, traumatized by wild French declarations for *égalité*, and by the example of American egalitarianism (well… egalitarianism for male, straight, white, Anglo, middle-aged, non-immigrant, New-England, mainline Protestants). The motive of the true Liberal, Monk continues, should not be equality but "the wish of every honest [that is, honorable] man […] to assist in lifting up those below him" (Trollope 1867-1869, vol. I, 126, 128). Such an ethical goal was to be achieved, says Monk the libertarian liberal (as Richard Cobden and John Bright and John Stuart

Mill were, and Bastiat in France at the time, and in our times Hayek and Friedman, or for that matter M'Cluskie), not by direct programs of redistribution, nor by regulation, nor by trade unions, but by free trade and tax-supported compulsory education and property rights for women—and in the event by the Great Enrichment, which finally in the late nineteenth century started sending real wages sharply up, Europe-wide, and then worldwide.

The absolute condition of the poor has been raised overwhelmingly more by the Great Enrichment than by redistribution. The economic historians Ian Gazeley and Andrew Newell noted in 2010 "the reduction, almost to elimination, of absolute poverty among working households in Britain between 1904 and 1937". "The elimination of grinding poverty among working families", they show, "was almost complete by the late thirties, well before the Welfare State". Their Chart 2 exhibits weekly income distributions in 1886 prices at 1886, 1906, 1938, and 1960, showing the disappearance of the classic line of misery for British workers, "round about a pound a week" (Gazeley and Newell 2010, Abstract, p. 19, and Chart 2 on p. 17).

To be sure, it is irritating if a super rich woman buys a $40,000 watch. The purchase is ethically objectionable. She should be ashamed. She should be giving her income in excess of an ample level of comfort—two cars, say, not twenty, two houses, not seven, one yacht, not five—to effective charities. Andrew Carnegie (1889) enunciated the principle that "a man who dies thus rich dies disgraced" Carnegie gave away his entire fortune (well, at death, after enjoying a castle in his native Scotland and a few other baubles). But that many rich people act in a disgraceful fashion does not automatically imply that the government should intervene to stop it. People act disgracefully in all sorts of ways. If our rulers were assigned the task in a fallen world of keeping us all wholly ethical, the government would bring all our lives under its fatherly tutelage, a real nightmare approximately achieved before 1989 in East Germany and now in North Korea.

One could argue, again, as Piketty does, that growth depends on capital accumulation—not on a new ideology and the bettering ideas that such an ideology encouraged, and certainly not on an ethics supporting the ideology. Piketty, like many American High Liberals, European Marxists, and conservatives everywhere, is annoyed precisely by the ethical pretension of the modern CEOs. The bosses, he writes, justify their economic success by placing "primary emphasis on their personal merit and moral qualities, which they described [in surveys] using term such as rigor, patience, work, effort, and so on (but also tolerance, kindness, etc.)" (p. 418). As the economist Donald Boudreaux puts it,

> Piketty prefers what he takes to be the more honest justifications for super-wealth offered by the elites of the novels of [the conservatives] Austen and Balzac, namely, that such wealth is required to live a comfortable lifestyle, period. No self-praise and psychologically comforting rationalizations by those early-nineteenth century squires and their ladies! (Boudreaux, personal correspondence, 2014)

Piketty sneers from a conservative-progressive height that "the heroes and heroines in the novels of Austen and Balzac would never have seen the need to compare their personal qualities to those of their servants". To which Boudreaux replies,

> Yes, well, bourgeois virtues were not in the early nineteenth century as widely celebrated and admired as they later came to be celebrated and admired. We should be pleased that today's [very] high-salaried workers brag about their bourgeois habits and virtues, and that workers—finally!—understand that having such virtues and acting on them is dignified (Boudreaux 2014).

The theory of great wealth espoused by the peasants and proletariat and their *soi-disant* champions among the leftish clerisy is non-desert by luck or theft. The theory of great wealth espoused by the aristocracy and their champions among the rightish clerisy is desert by inheritance, itself to be justified by ancient luck or theft, an inheritance we *aristoi* of course should collect without psychologically comforting rationalizations. The theory of great wealth espoused by the bourgeoisie and by its friends the liberal economists, on the contrary, is desert by virtue of supplying ethically, without violence, what people are willing to buy.

The bourgeois virtues are doubtless exaggerated, especially by the bourgeoisie, and sometimes even by its friends. But for the rest of us the results of virtue-bragging have not been so bad. Think of the later plays of Ibsen, the pioneering dramatist of the bourgeois life. The bank manager, Helmer, in *A doll house* (1878) describes a clerk caught in forgery as "morally lost", having a "moral breakdown" (Ibsen 1879, 132). Helmer's speech throughout the play is saturated with an ethical rhetoric we are accustomed to calling "Victorian". But Helmer's wife

Nora, whose rhetoric is also ethically saturated, commits the same crime as the clerk's. She commits it, though, in order to save her husband's life, not as the clerk does for amoral profit. By the end of the play she leaves Helmer, a shocking move among the Norwegian bourgeoisie of 1878, because she suddenly realizes that if he knew of her crime he would not have exercised the loving ethics of protecting her from the consequences of a forgery committed for love, not for profit. An ethical bourgeoisie—which is what all of Ibsen's plays after 1871 explore, as did later the plays of Arthur Miller—has complicated duties. The bourgeoisie goes on talking and talking about virtue, and sometimes achieves it.

The original and sustaining causes of the modern world, I would argue contrary to Piketty's sneers at the bourgeois virtues, were indeed ethical, not material (see McCloskey forthcoming). They were the widening adoption of two mere ideas, the new and liberal economic idea of liberty for ordinary people and the new and democratic social idea of dignity for them. The two linked and preposterous ethical ideas—the single word for them is "equality", of respect and before the law—led to a paroxysm of betterment. The word "equality", understand, is not to be taken, in the style of some in the French Enlightenment, as equality of material outcome. The French definition is the one the left and the right unreflectively assume nowadays in their disputes: "You didn't build that without social help, so there's no justification for unequal incomes"; "You poor folk just aren't virtuous enough, so there's no justification for your claim of equalizing subsidies". The more fundamental definition of equality, though, praised in the Scottish Enlightenment after the Scots awoke from their dogmatic slumber, is the egalitarian opinion people have of each other, whether street porter or moral philosopher (see Peart and Levy 2008).[2] The moral philosopher Smith, a pioneering egalitarian in this sense, described the Scottish idea as "allowing every man to pursue his own interest his own way, upon the liberal plan of equality, liberty and justice" (Smith 1776, book IV, ch. 9, p. 664).

Forcing in an illiberal way the French style of equality of outcome, cutting down the tall poppies, envying the silly baubles of the rich, imagining that sharing income is as efficacious for the good of the poor

[2] Kim Priemel of Humboldt University of Berlin suggests to me that "equity" would be a better word for the Scottish concept. But I do not want to surrender so easily an essentially contested concept such as French *égalité*, which indeed in its original revolutionary meaning was more Scottish than what I am calling "French".

as are equal shares in a pizza, treating poor people as sad children to be nudged or compelled by the experts of the clerisy, we have found, has often had a high cost in damaging liberty and slowing betterment. Not always, but often.

It would be a good thing, of course, if a free and rich society following Smithian liberalism produced a French and Pikettyan equality. In fact—old news, surprising to some, and to Piketty—it largely has, by the only ethically relevant standard, that of basic human rights and basic comforts in antibiotics and housing and education, compliments of the liberal and Scottish plan. Introducing the Scottish plan, as in Hong Kong and Norway and France itself, has regularly led to an astounding betterment and to a real equality of outcome—with the poor acquiring automobiles and hot-and-cold water at the tap that were denied in earlier times even to the rich, and acquiring political rights and social dignity that were denied in earlier times to everyone except the rich.

Even in the already-advanced countries in recent decades there has been no complete stagnation of real incomes for ordinary people. You will have heard that "wages are flat" or that "the middle class is shrinking". But you also know that you should not believe everything you read in the papers. This is not to say that no one in rich countries such as the United States is unskilled, addicted, badly parented, discriminated against, or simply horribly unlucky. George Packer's recent *The unwinding: an inner history of the new America* (2013) and Barbara Ehrenreich's earlier *Nickel and dimed: on (not) getting by in America* (2001) carry on a long and distinguished tradition of telling the bourgeoisie about the poor that goes back to James Agee and Walker Evans, *Let us now praise famous men* (1944), George Orwell, *The road to Wigan Pier* (1937), Jack London, *The people of the abyss* (1903), Jacob Riis, *How the other half lives: studies among the tenements of New York* (1890), and the fount, Friedrich Engels, *The condition of the working class in England* (1845). They are not making it up. Anyone who reads such books is wrenched out of a comfortable ignorance about the other half. In fictional form one is wrenched by Steinbeck's *The grapes of wrath* (1939) or Farrell's *Studs Lonigan* (1932-1935) or Wright's *Native son* (1940), or in Europe, among many observers of the Two Nations, Zola's *Germinal* (1885), which made many of us into socialists. The wrenching is salutary. It is said that Winston Churchill, scion of the aristocracy, believed that most English poor people lived in rose-covered cottages. He could not imagine back-to-backs in Salford, with the outhouse at the end of the row. Wake up, Winston.

But waking up does not imply despairing, or introducing faux policies that do not actually help the poor, or proposing the overthrow of the System, if the System is in fact enriching the poor over the long run, or at any rate enriching the poor better than those other systems that have been tried from time to time. Righteous, if inexpensive, indignation inspired by survivor's guilt about alleged "victims" of something called "capitalism" and by envious anger at the silly consumption by the rich do not invariably yield betterment for the poor. Remarks such as "there are still poor people" or "some people have more power than others", though claiming the moral high-ground for the speaker, are not deep or clever. Repeating them, or nodding wisely at their repetition, or buying Piketty's book to display on your coffee table, does not make you a good person. You are a good person if you actually help the poor. Open a business. Arrange mortgages that poor people can afford. Invent a new battery. Vote for better schools. Adopt a Pakistani orphan. Volunteer to feed people at Grace Church on Saturday mornings. The offering of faux, counterproductive policies that in their actual effects reduce opportunities for employment, or the making of indignant declarations to your husband after finishing the *Sunday New York Times Magazine*, does not help the poor.

The economy and society of the United States are not in fact unwinding, and people are in fact getting by better than they did before. The children of the sharecropping families in Hale County, Alabama whom Agee and Evans objectified, to the lasting resentment of the older members of the families, are doing pretty well, holding jobs, many of their children going to college (Whitford 2005). That even over the long run there remain some poor people does not mean that the system is not working for the poor, so long as their condition is continuing to improve, as it is, contrary to the newspaper stories and the pessimistic books, and so long as the percentage of the desperately poor is heading towards zero, as it is. That people still sometimes die in hospitals does not mean that medicine is to be replaced by witch doctors, so long as death rates are falling and so long as the death rate would not fall under the care of the witch doctors.

And poverty is indeed falling, even recently, even in already rich countries. If income is correctly measured to include better working conditions, more years of education, better health care, longer retirement years, larger poverty-program subsidies, and above all the rising quality of the larger number of goods, the real income of the poor has risen, if at a slower pace than in the 1950s—which followed the calamitous time-outs of the Great Depression and the War (Boudreaux and Perry 2013). The economist Angus Deaton notes that "once the rebuilding is done [as it was in, say, 1970], new growth relies on inventing new ways of doing things and putting them into practice, and this turning over of virgin soil is harder than re-plowing an old furrow" Deaton 2013, 231). Nor are the world's poor paying for the growth. The economists Xavier Sala-i-Martin and Maxim Pinkovsky report on the basis of detailed study of the individual distribution of income—as against comparing distributions nation-by-nation—that:

> World poverty is falling. Between 1970 and 2006, the global poverty rate [defined in absolute, not relative, terms] has been cut by nearly three quarters. The percentage of the world population living on less than $1 a day (in PPP-adjusted 2000 dollars) went from 26.8% in 1970 to 5.4% in 2006 (Sala-i-Martin and Pinovsky 2010, see also Sala-i-Martin 2006).

It is important in thinking about the issues Piketty so energetically raises to keep straight what exactly is unequal. Physical capital and the paper claims to it are unequally owned, of course, although pension funds and the like do compensate to some degree. The yield on such portions of the nation's capital stock is the income of the rich, especially the rich-by-inheritance whom Piketty worries most about. But if capital is more comprehensively measured, to include increasingly important human capital such as engineering degrees and increasingly important commonly-owned capital such as public parks and modern knowledge (think: the Internet), the income yield on capital is less unequally owned, I have noted, than are paper claims to physical capital.

Further, consumption is much less unequally enjoyed than income is measured. A rich person owning seven houses might be thought to be seven times better off than a poor person with barely one. But of course she is not, since she can consume by occupying only one house at a time, and can consume only one pair of shoes at a time, and so forth. The diamond bracelet sitting un-worn at the bottom of her ample jewelry box is a scandal, since she could have paid the school fees of a thousand families in Mozambique with what she foolishly spent on the bauble last season in Cannes. She ought indeed to be ashamed to indulge such foolish expenditure. It is an important ethical issue, if not a public issue. But anyway the expenditure has not increased her actual, point-of-use consumption.

Further, and crucially, the consumption of basic capabilities or necessities is very much more equally enjoyed nowadays than the rest of consumption, or income, or capital, or physical wealth, and has become more and more equally so as the history of enriching countries proceeds. Therefore economic growth, however unequally it is accumulated as wealth or earned as income, is more egalitarian in its consumption, and by now is quite equal in consumption of necessities. As the American economist John Bates Clark predicted in 1901:

> The typical laborer will increase his wages from one dollar a day to two, from two to four and from four to eight [which was accurate in real terms of per-person income down to 2012, though such a calculation does not allow for the radically improved quality of goods and services since 1901]. Such gains will mean infinitely more to him than any possible increase of capital can mean to the rich [...]. This very change will bring with it a continual approach to equality of genuine comfort (Clark 1901).

In 2013, the economists Donald Boudreaux and Mark Perry noted that:

> [A]ccording to the Bureau of Economic Analysis, spending by households on many of modern life's 'basics'—food at home, automobiles, clothing and footwear, household furnishings and equipment, and housing and utilities—fell from 53 percent of disposable income in 1950 to 44 percent in 1970 to 32 percent today (Boudreaux and Perry 2013).

It is a point which the economic historian Robert Fogel (1999) had made for a longer span. The economist Steven Horwitz summarizes the facts on labor hours required to buy a color TV or an automobile, and notes that:

> [T]hese data do not capture [...] the change in quality [...]. The 1973 TV was at most 25 inches, with poor resolution, probably no remote control, weak sound, and generally nothing like its 2013 descendant [...]. Getting 100,000 miles out of a car in the 1970s was cause for celebration. Not getting 100,000 miles out of a car today is cause to think you bought a lemon (Horwitz 2013, 11).

Nor in the United States are the poor getting poorer. Horwitz observes that:

> [L]ooking at various data on consumption, from Census Bureau surveys of what the poor have in their homes to the labor time required to purchase a variety of consumer goods, makes clear that poor Americans are living better now than ever before. In fact, poor Americans today live better, by these measures, than did their middle class counterparts in the 1970s (Horwitz 2013, 2).

In the summer of 1976 an associate professor of economics at the University of Chicago had no air conditioning in his apartment.[3] Nowadays many quite poor Chicagoans have it. The terrible heat wave in Chicago of July 1995 killed over 700 people, mainly low-income (Klinenberg 2003).[4] Yet earlier heat waves in 1936 and 1948, before air-conditioning was at all common, had probably killed many more.[5]

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The political scientist and public intellectual Robert Reich argues that we must nonetheless be alarmed by inequality, Gini-coefficient style, rather than devoting all our energies to raising the absolute condition of the poor. "Widening inequality", he declares, "challenges the nation's core ideal of equal opportunity".

> Widening inequality still hampers upward mobility. That's simply because the ladder is far longer now. The distance between its bottom and top rungs, and between every rung along the way, is far greater. Anyone ascending it at the same speed as before will necessarily make less progress upward (Reich 2014).

Reich is mistaken. Horwitz summarizes the results of a study by Julia Isaacs on individual mobility 1969-2005: "82% of children of the bottom 20% in 1969 had [real] incomes in 2000 that were higher than what their parents had in 1969. The median [real] income of those children of the poor of 1969 was double that of their parents" (Isaacs

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[3] Horwitz 2013's Table 4 reports the percentage of poor households with various appliances: in 1971, 32 percent of such household had air conditioners; in 2005, 86 percent did.

[4] The 2003 heat wave in non-air-conditioned France killed 14,800 people, and 70,000 Europe-wide.

[5] Barreca and collaborators (2013) show the very large effect in the United States of air conditioning in reducing excess mortality during heat waves.

2007, quoted in Horwitz 2013, 7). There is no doubt that the children and grandchildren of the English coal miners of 1937, whom Orwell describes "traveling" underground, bent over double walking a mile or more to get to the coal face, at which point they started to get paid, are much better off than their fathers or grandfathers. There is no doubt that the children and grandchildren of the Dust Bowl refugees in California are. Steinbeck chronicled in *The grapes of wrath* their worst and terrible times. A few years later many of the Okies got jobs in the war industries, and many of their children later went to university. Some went on to become university professors who think that the poor are getting poorer.

The usual way, especially on the left, of talking about poverty relies on the percentage distribution of income, staring fixedly for example at a *relative* "poverty line". As the progressive Australian economist Peter Saunders notes, however, such a definition of poverty "automatically shift upwards whenever the real incomes (and hence the poverty line) are rising" (Saunders 2013, 214). The poor are always with us, but merely by definition, the opposite of the Lake Wobegon effect—it is not that all the children are above average, but that there is always a bottom fifth or tenth or whatever in any distribution whatsoever. Of course.

The philosopher Harry Frankfurt noted long ago that "calculating the size of an equal share [of income in the style of poverty lines or Gini coefficients] is plainly much easier than determining how much a person needs in order to have enough"—"much easier" as in dividing GDP by population and reporting with irritation that some people earn, or anyway get, more (Frankfurt 1987, 23-24). It is the simplified ethics of the schoolyard, or dividing the pizza: "That's unfair". But as Frankfurt also noted, inequality is in itself ethically irrelevant: "economic equality is not, as such, of particular moral importance" (Frankfurt 1987, 21). In ethical truth we wish to raise up the poor, Joshua-Monk style, to "enough" for them to function in a democratic society and to have full human lives. It does not matter ethically whether the poor have the same number of diamond bracelets and Porsche automobiles as do owners of hedge funds. But it does indeed matter whether they have the same opportunities to vote or to learn to read or to have a roof over their heads. The Illinois state constitution of 1970 embodies the confusion between the condition of the working class on the one hand and the gap between rich and poor on the other, claiming in its preamble that it seeks to "eliminate poverty *and inequality*".[6] We had better focus directly on what we actually want to achieve, which is equal sustenance and dignity, eliminating poverty, or what the economist Amartya Sen and the philosopher Martha Nussbaum call ensuring adequate capabilities. The size of the Gini coefficient or the share of the bottom 10 percent is irrelevant to the noble and ethically relevant purpose of raising the poor to a condition of dignity, Frankfurt's "enough".

Much of the research on the economics of inequality stumbles on this simple ethical point, focusing on measures of relative inequality such as the Gini coefficient or the share of the top 1 percent rather than on measures of the absolute welfare of the poor, on inequality rather than poverty, having elided the two. Speaking of the legal philosopher Ronald Dworkin's egalitarianism, Frankfurt observed that Dworkin in fact, and ethically, "cares principally about the [absolute] value of people's lives, but he mistakenly represents himself as caring principally about the relative magnitudes of their economic assets" (Frankfurt 1987, 34; italics added). Piketty himself barely gets around to caring about "the least well off" (p. 577; the last phrase in the last sentence of the book, though he does occasionally mention the issue in the body of the book, as on p. 480).

Dworkin and Piketty and much of the left commonly, in other words, miss the ethical point, which is the liberal, Joshua-Monk one of lifting up the poor. By redistribution? By equality in diamond bracelets? No: by the dramatic increase in the size of the pie, which has historically brought the poor to 90 or 95 percent of "enough", as against the 10 or 5 percent attainable by redistribution without enlarging the pie. The economic historian Robert Margo noted in 1993 that before the U.S.A. Civil Rights Act of 1964 "blacks could not aspire to high-paying white collar jobs" because of discrimination. Yet African Americans had prepared themselves, by their own efforts, up from slavery, to perform in such jobs if given a chance. "Middle-class blacks owe their success in large part to themselves", and to the increasingly educated and productive society they lived in. "What if the black labor force, poised on the eve of the Civil Rights Movement, was just as illiterate, impoverished, rural, and Southern as when Lincoln freed the slaves? […] Would we have as large a black middle class as we do today? Plainly not" (Margo 1993, 68, 65, 69).

[6] http://www.ilga.gov/commission/lrb/conent.htm

Yet the left works overtime, out of the best of motives—and Piketty has worked very hard indeed—to rescue its ethically irrelevant focus on Gini coefficients and especially the disgraceful consumption of the very rich.

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For the poor in the countries that have allowed the ethical change to happen, then, Frankfurt's "enough" has largely come to pass. "Largely", I say, and much more than alternative systems have allowed. I do not say "completely", or "as much as every honorable person would wish". But the contrast between the condition of the working class in the proudly "capitalist" United States and in the avowedly social-democratic countries such as the Netherlands or Sweden is not in fact very large, despite what you have heard from journalists and politicians who have not looked into the actual statistics, or have not lived in more than one country, and think that half of the American population consists of poor urban African-Americans. The social safety net is in practice rather similar among rich countries.

But the safety net, with or without holes, is not the main lift for the poor in the United States, the Netherlands, Japan, Sweden, or the others. The main lift is the Great Enrichment. Boudreaux noted that a literal billionaire who participated in a seminar of his did not look much different from an "impoverished" graduate student giving a paper about Gini coefficients.

> In many of the basic elements of life, nearly every American is as well off as Mr. Bucks [his pseudonym for the billionaire]. If wealth differences between billionaires and ordinary Americans are barely visible in the most routine aspects of daily life, then to suffer distress over a Gini coefficient is to unwisely elevate ethereal abstraction over palpable reality (Boudreaux 2004).

Mr. Bucks undoubtedly had more houses and more Rolls-Royces than the graduate student. One may ask, though, the cheeky but always relevant question: So what?

The most fundamental problem in Piketty's book, then, is that the main event of the past two centuries was not the second moment, the distribution of income on which he focuses, but its first moment, the Great Enrichment of the average individual on the planet by a factor of 10 and in rich countries by a factor of 30 or more. The greatly enriched world cannot be explained by the accumulation of capital—as to the contrary economists have argued from Adam Smith through Karl Marx to Thomas Piketty, and as the very name "capitalism" implies. Our riches were not made by piling brick upon brick, bachelor's degree upon bachelor's degree, bank balance upon bank balance, but by piling idea upon idea. The bricks, BAs, and bank balances—the capital accumulations—were of course necessary, as was a labor force and the existence of liquid water. Oxygen is necessary for a fire. But it would be unenlightening to explain the Chicago Fire of October 8-10, 1871 by the presence of oxygen in the earth's atmosphere. Better: a long dry spell, the city's wooden buildings, a strong wind from the southwest, and Mrs. O'Leary's cow.

The modern world cannot be explained by routine brick-piling, such as the Indian Ocean trade, English banking, the British savings rate, the Atlantic slave trade, the enclosure movement, the exploitation of workers in satanic mills, or the original accumulation of capital in European cities, whether of physical or of human capital (see McCloskey 2010). Such routines are too common in world history and too feeble in quantitative oomph to explain the ten- or thirty- or one hundred-fold enrichment per person unique to the past two centuries. It was ideas, not bricks. The ideas were released for the first time by a new liberty and dignity, the ideology known to Europeans as "liberalism". The modern world was not caused by "capitalism", which is ancient and ubiquitous—quite unlike liberalism, which was in 1776 revolutionary. The Great Enrichment, 1800 to the present, the most surprising secular event in history, is explained instead by bettering ideas, sprung from liberalism.

Consider in light of the Great Enrichment one of Piketty's and the left's favorite suggestions for policy. Taxing the rich to help the poor seems in the first act a fine idea. When a bourgeois child first realizes how very poor people are in other neighborhoods she naturally wishes to open her purse to them, or still better Daddy's wallet. It is at such an age—16 or so—that we form our political identities, which like loyalties to football teams we seldom then revise in the face of later evidence. Our families, after all, are little socialist economies, with Mother as central planner. Let us remake society, the generous adolescent proposes, as one big family of 315 million people. Surely the remaking will solve the problem of poverty, raising up the poor by big amounts, such as the 20 or 30 percent of income stolen by the bosses. In an ancient society of slaves the slave-owning child had no such guilt, because the poor were very different from herself. But once the naturalness of hierarchy was questioned, as it was in the eighteenth century in northwestern Europe, and in the nineteenth century more generally, it seems obvious to adopt socialism. Ye cannot serve God and mammon ("mammon" is the Aramaic word for "money").

The equality of a home is natural, with one source of income—the father or, lately, the mother—and a task of "distributing" the proceeds. Papa might get more food if he is a hewer in a mine and needs the extra calories to get through a ten-hour shift at the coal face, but otherwise the distribution is naturally, and ethically, equal. Equality is natural to a home. The Swedish political motto from the 1920s on, *folkhemmet*, was "the national home". But a nation is not a home. In the Great Society—as, in advance of President Johnson, Hayek called it, meaning a big society as contrasted with a little band or a family—the source of income is not the father's pay packet but the myriad specialized exchanges with strangers we make every day. Equality of "distribution" is not natural to such a society, of 9 million in Sweden, and certainly not one of 315 million in the United States.

And in some important ways even French-style equality is improved by an ethic of markets. Free entry erodes monopolies that in traditional societies keep one tribe rich and the other poor. A market in labor erodes differentials among equally productive workers in cotton textiles, or indeed between on the one hand a professor who teaches with the same scant equipment that Socrates used—a place to draw diagrams, a stretch of sand in Athens, Greece or a whiteboard in Athens, Georgia, and a crowd of students—and on the other an airline pilot working with the finest fruits of a technological civilization. The pilot produces thousands of times more value of travel services per hour than a Greek steersman in 400 BCE. The professor produces if she is exceptionally lucky the same insight per student-hour as Socrates. But equality of physical productivity does not matter in a free, great society, a trading and mobile one. Entry and exit to occupations are what matter. The professor could in the long run have become an airline pilot, and the pilot a professor, which is enough to give even workers like the professor who have not increased in productivity in the past 2,500 years an equal share of the finest fruits.

Having noted this highly egalitarian result of a society of market-tested betterment, though, what about subsequent "distribution" of the fruits? Why shouldn't we—one might ask, who "we"?—seize the high incomes of the professor and the airline pilot and the heiress to the L'Oréal fortune and distribute them to dustmen and cleaners? The reply is that what people earn is not merely an arbitrary tax imposed on the rest of us. That is what an inequality within the little socialism of a household would be, Cinderella getting less to eat than her ugly sisters out of mere spite. Earnings, however, support an astonishingly complicated, if largely unplanned and spontaneous, division of labor, whose next move is determined by the differentials—the profit in trade or in occupation. If medical doctors make ten times more than cleaners, the rest of the society, which pays voluntarily for the doctors and cleaners is saying, "If cleaners could become doctors, viewing the matter in the long run, shift more of them into doctoring". If we reduce the Great Society to a family by taxing the rich we destroy the signaling. People wander between cleaning and doctoring without such signals about the value people put on the next hour of their services. Neither doctoring nor cleaning gets done well. The Great Society becomes the unspecialized society of a household, and if consisting of 315 million people it becomes miserably equal, and loses the massive gain from specialization and the accumulated ingenuity that are transmitted by education to a trade and by the steadily bettering robots (all tools, note, are robots) applied to each, the nail guns and computers that make master carpenters and master school teachers better and better at providing houses and educations to others.

Redistribution, although assuaging bourgeois guilt, has not been the chief sustenance of the poor. The social arithmetic shows why. If all profits in the American economy were forthwith handed over to the workers, the workers (including some amazingly highly paid "workers", such as sports and singing stars, and big-company CEOs) would be 20 percent or so better off, right now. But one time only. The expropriation is not a 20 percent gain every year forever, but merely this one time, since you cannot expropriate the same people year after year and expect them to come forward with the same sums ready to be expropriated again and again. A one-time expropriation raises the income of the workers by 20 percent, and then their income reverts to the previous level—or at best (if the profits can simply be taken over by the state without damage to their level, miraculously, and then are distributed to the rest of us by saintly bureaucrats without sticky fingers or favored friends) continues with whatever rate of growth the economy was experiencing (supposing, unnaturally and contrary to the evidence of communist experiments from New Harmony, Indiana to Stalinist Russia, that the expropriation of the income of capital will not reduce the rate of growth of the pie).

Or, to speak of expropriation by regulation, the imposing by act of Congress a ten-hour pay for eight hours of work would, again, raise the incomes of the portion of the working class that got it, one time, by 25 percent. It would do so in the first act, under the same, unnatural supposition that the pie was not thereby reduced, when the managers and entrepreneurs desert the now unprofitable activity. The redistribution sounds like a good idea, unless you reflect that at such rates the bosses would be less willing to employ people in the first place, and anyway those who did not get it (agricultural workers, for example) would find their real incomes reduced, not raised.

Here is another idea for income transfers, then: If we took away the alarmingly high share of U.S.A. income earned by the top 1 percent, which was in 2010 about 22 percent of national income, and gave it to the rest of us, we as The Rest would be 22/99, or a tiny bit under 22 percent better off. Or put it still another way. Suppose the profits were allowed to be earned by the people directing the economy, by the owner of the little convenience store in your neighborhood as much as by the malefactors of great wealth. But suppose the profit earners, out of a Gospel of Wealth, and following Catholic social teaching, decided that they themselves should live modestly and then give all their surplus to the poor. The economist David Colander declares that "a world in which all rich individuals [...] [believed] that it is the duty of all to give away the majority of their wealth before they die would be quite different from [...] our world" (Colander 2013, xi). But wait. The entire 20 percent would raise the incomes of the rest—many of them university professors getting Guggenheim fellowships or sweetly left-wing folk getting Macarthur "genius" awards—but by a magnitude nothing like the size of the fruits of modern economic growth. And even that calculation supposes that all profits go to "rich individuals".

The point is that 20 and 22 and 25 percent are not of the same order of magnitude as the Great Enrichment, which in turn had nothing in historical fact to do with such redistributions or charitable contributions. The point is that the one-time redistributions are two orders of magnitude smaller in helping the poor than the 2,900 percent Enrichment from greater productivity since 1800. Historically speaking 25 percent is to be compared with a rise in real wages 1800 to the present by a factor of 10 or 30, which is to say 900 or 2,900 percent. The very poor, in other words, are made a little better off by expropriating the expropriators, or persuading them to give all their money to the poor and follow Me, but much better off by coming to live in a radically more productive economy.

If we want to make the non-bosses or the poor better off by a significant amount, 2,900 percent beats a range from 20 to 25 percent every time. Chairman Mao's emphasis on class warfare spoiled what gains his Chinese Revolution had achieved. When his heirs shifted in 1978 to "socialist modernization" they (inadvertently) adopted market-tested betterment, and achieved in thirty years a rise of Chinese per-person real income by a factor of 20—not a mere 20 percent but 1,900 percent.[7] Deng Xiaoping's anti-equalizing motto was, "Let some people get rich first". It is the Bourgeois Deal: "You accord to me, a bourgeois projector, the liberty and dignity to try out my schemes in a voluntary market, and let me keep the profits, if I get any, in the first act—though I accept, reluctantly, that others will compete with me in the second. In exchange, in the third act of a new, positive-sum drama, the bourgeois betterment provided by me (and by those pesky, low-quality, price-spoiling competitors) will make *you* all rich". And it did.

Unlike China growing at 10 percent per year and India at 7 percent, the other BRIICS of Brazil, Russia, Indonesia, and South Africa have stuck with anti-neo-liberal ideas such as Argentinian self-sufficiency and 1960s British unionism and 1990s German labor laws and a misunderstanding of Korea's "export-led" growth. Indeed, the literature of the "middle-income trap", which speaks in particular of Brazil and South Africa, depends on a mercantilist idea that growth depends on exports, which are alleged to have a harder time growing when wages rise (see McCloskey 2006c). Policies to encourage this or that export depend, that is, on denying comparative advantage, and anyway focus on externals when what mainly matters to the income of the poor is domestic efficiency. Therefore the middle-income countries with market-denying laws, such as slowing entry to new business and onerously regulating old business, drag along at less than 3 percent growth per year per person—at which a mere doubling takes a quarter

[7] On 1978, see Coase and Wang 2013, 37.

of a century and a quadrupling takes fifty years. Slow growth yields envy, as the economist Benjamin Friedman (2005) has argued, and envy yields populism, which in turn yields slow growth. That is the real "middle income trap". Getting out of it requires accepting, as Holland did in the sixteenth century and Britain in the eighteenth, and as China and India did in the late twentieth, the Bourgeois Deal.

Supposing our common purpose on the left and on the right, then, is to help the poor, as in ethics it certainly should be, the advocacy by the learned cadres of the left of equalizing restrictions and redistributions and regulations can be viewed at best as thoughtless. Perhaps, considering what economic historians now know about the Great Enrichment, but which the left clerisy, and many of the right, stoutly refuse to learn, it can even be considered unethical. The left clerisy such as Tony Judt or Paul Krugman or Thomas Piketty, who are quite sure that they themselves are taking the ethical high road against the wicked selfishness of Tories or Republicans or *l'Union pour un Mouvement Populaire*, might on such evidence be considered dubiously ethical. They are obsessed with first-act changes that cannot much help the poor, and often can be shown to damage them, and are obsessed with angry envy at the consumption of the uncharitable rich, of which they personally are often examples, and the ending of which would do very little to improve the position of the poor. They are very willing to stifle through taxing the rich the market-tested betterments which in the long run have gigantically helped the rest of us.

The productivity of the economy in 1900 was very, very low, and in 1800 even lower. The only way that the bulk of the people, and the poorest among them, were going to be made seriously better off was by making the economy much, much more productive. The share going to the workers was roughly constant (in one respect during the nineteenth and early twentieth century labor's share was rising, because land rent, once a third of national income even in Britain, fell in its share). The share was determined, as the economists such as the American J. B. Clark and the Swede Knut Wicksell put it in the late nineteenth century, by the marginal productivity of workers. And so according to the economists' argument even the poorest workers could be expected to share in the rising productivity—by those factors or 10 or 30 or 100. And they did. The descendants of the horribly poor of the 1930s, for instance, are doing much better than their ancestors. Radically creative destruction piled up ideas, such as the railways creatively destroying walking and the stage coaches, or electricity creatively destroying kerosene lighting and the hand washing of clothes, or universities creatively destroying literary ignorance and low productivity in agriculture. The Great Enrichment—in the third act—requires not the accumulation of capital or the exploitation of workers but the Bourgeois Deal.

The left explains the inability of workers themselves to grasp the hard-left dogma that all employment is exploitation by saying that the workers are in the grip of false consciousness (see Lemert 2012, 21). If the Bourgeois Deal is sound, though, the falsity in consciousness is attributable not to the sadly misled workers but rather to the leftish clerisy themselves, and the politics is reversed. Workers of the world unite: demand market-tested progress under a régime of private property and profit-making. Still better, become bourgeois, as large groups of workers in rich countries do believe they have become, approaching 100 percent in the United States, measured by self-identification as "middle class". It would then seem at least odd to call "false" a consciousness that has raised the income of poor workers in real terms by a factor of 30, as from 1800 to the present conservatively measured it has. If workers have been "fooled" by accepting the Deal, then for such a way of being fooled let us give two-and-a-half cheers—the deduction of half a cheer being because it's not dignified to be "fooled" by anything. Two-and-a-half cheers for the new dominance since 1800 of a bourgeois ideology and the spreading acceptance of the Bourgeois Deal.

On the next to last page of his book Piketty writes: "It is possible, and even indispensable, to have an approach that is at once economic and political, social and cultural, and concerned with wages and wealth". One can only agree. But he has not achieved it. His gestures to cultural matters consist chiefly of a few naively used references to novels he has read superficially, for which on the left he has been embarrassingly praised (Skwire and Horowitz 2014). His social theme is a narrow ethic of envy. His politics assumes that governments can do anything they propose to do. And his economics is flawed from start to finish.

It is a brave book. But it is mistaken.

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Rational choice as a toolbox for the economist: an interview with Itzhak Gilboa

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Itzhak Gilboa (Tel Aviv, 1963) is currently professor of economics at the Eitan Berglas School of Economics at Tel-Aviv University and professor of economics and decision sciences at the Hautes Études Commerciales (HEC) in Paris. He earned undergraduate degrees in mathematics and in economics at Tel Aviv University, where he also obtained his MA and PhD in economics under the supervision of David Schmeidler. Before joining Tel Aviv University in 2004 and the HEC in 2008, Gilboa taught at the J. L. Kellogg Graduate School of Management at Northwestern University, the University of Pennsylvania, and Boston University.

Gilboa's main area of interest is decision-making under uncertainty, focusing on the definition of probability, notions of rationality, non-Bayesian decision models, and related issues. He has published broadly in areas such as decision and game theory, microeconomics, philosophy, social choice theory, and applied mathematics. He has written over 90 articles in these fields. Gilboa has furthermore written a textbook entitled *Rational choice* (Gilboa 2010a), in which he lays out what he takes to be the main toolbox for studying and improving human decision-making. Additional books include *A theory of case-based decisions* (Gilboa and Schmeidler 2001), *Theory of decision under uncertainty* (Gilboa 2009), *Making better decisions* (Gilboa 2010b), and *Case-based predictions* (Gilboa and Schmeidler 2012).

Professor Gilboa was interviewed by Catherine Herfeld at the department of economics of the University of Mainz (Germany) on July 13, 2013. In this interview, Gilboa lays out his perspective on the nature and purpose of the rational choice paradigm, discussing it in the context of recent philosophical questions about the advantages of axiomatization and its relation to empirical research, the usefulness

NOTE: Catherine Herfeld is currently a postdoctoral fellow at the Munich Center for Mathematical Philosophy, Germany. Before coming to Munich, she was a research fellow at the Center for the History of Political Economy (Duke University), while finishing her PhD at Witten/Herdecke University in the history and philosophy of economics. This interview is part of a larger project entitled 'Conversations on rational choice theory', which aims at critically discussing the different manifestations of rational choice theory and the ways in which they have been used in philosophy and economics. Contact email: <Catherine.Herfeld@lrz.uni-muenchen.de>

of unrealistic assumptions, the future of neuroeconomics, the status of economics as a science, and his view of truth.

CATHERINE HERFELD: ***Professor Gilboa, you are currently professor of economics and decision sciences at Hautes Études Commerciales de Paris. What, broadly speaking, are the decision sciences?***

ITZHAK GILBOA: 'Decision sciences' is a general term. As I understand it, 'decision sciences' refers to the field of decision in general. 'Decision sciences' encompasses decision theory, applied work, and experimental work. But the field of decision theory today is starting to undergo a process of 'disintegration'. I do not want this to sound bad. This happened to game theory about 15 years ago. Both in game theory and in decision theory there is a general paradigm that is extremely beautiful and extremely insightful, and which has a lot to say about almost everything. But this general paradigm is at some point exhausted, and you start having to commit yourself to a specific type of theory you work with. And then you might find out that the theory you work with is not as general as the paradigm.

Could you illustrate this view with an example?

For instance, consider game theory, which is not too far away from the field of decision sciences, where you have a general approach to human interaction. You can model a wide variety of situations: you identify players and strategies to begin with, and then you have some things to say about the interaction. For example, the concept of Nash equilibrium, or maybe even that of a perfect equilibrium, allows you to say something insightful about everything that can be modelled as interaction among decision makers. It could be the interaction between couples, like battle of the sexes; it could be the interaction between two countries; it could be the interaction between buyers and sellers in a market; or it could be the interaction between species, such as in hawk-dove games. Surprisingly, game theory can capture those different situations and can say something meaningful about each of them, which is fantastic. But at some point, when you start looking at refinements of, for example, Nash equilibrium, or dynamics that would or would not lead to Nash equilibrium, you would say: 'Wait a minute. The dynamics that would capture evolution are not the same sort of dynamics that are applicable to the market'.

So game theory constitutes a general paradigm, offering a set of theories and concepts that become modified for particular applications within this paradigm. In what way does this happen with decision theory?

I think that in decision theory we see something similar. The general approach, taking Leonard Savage's work as being the main achievement along those lines, could be used to think about any kind of problem under uncertainty (see Savage 1972 [1954]). We should identify states of the world, acts, and outcomes, and can use concepts such as probability and utility. As such, this general approach always has something meaningful to say about decision-making. But sometimes what it says is not enough. And when you start going beyond that, you might have to decide what exactly you want to apply this paradigm to, that is, whether what you understand as an 'application' is, for example, the situation of sitting down with a patient who has to decide whether to undergo surgery; or whether an 'application' is a purely theoretical model in applied theory. When you are sitting there with a patient who has to decide whether to undergo surgery or not, you have to estimate actual parameters; you have to take this general approach seriously. You also have to take it seriously when an application for you is pointing out to a colleague who is doing a search model in labour economics: 'Wait a minute. Maybe you do not get this sigma, let us think about uncertainty instead of risk'. Both are called applications, but they are both very different things. And it is not at all obvious that the same paradigm, or conceptual framework, provided by decision theory is going to be relevant for both kinds of application to the same degree. In short, there is something common about the paradigm that is relevant to everything, but if you actually want to do something concrete with it, then you might have to commit to the kind of application you have in mind in order to capture the specifics of this particular application. This is what decision sciences and game theory have in common.

As such, 'decision sciences' encompass many things. They include theoretical work and experimental work. Even within theory, one probably has to decide whether one is developing a theory to be used by theoretical economists, by empirical economists, or to actually make decisions about whether we should build nuclear plants, take a specific drug, or whatever.

Can we say that you understand the relation between the general paradigm of decision sciences and the theories formulated and applied within this paradigm along Lakatosian lines: we have a research program consisting of a hardcore (what you call the paradigm) that remains untouched, but allows for formulating specific, falsifiable theories to address a variety of concrete problems?

Yes, that is right. To a large extent I see such a process along Lakatosian lines. Yet this process is not always accompanied by sufficient self-reflection in the field. I think that decision theorists tend to think of decision sciences as providing a particular theory, not a general paradigm. As such, the distinction between the general paradigm and the theory is not always sufficiently clear.

This sounds similar to the warning you voiced in your article entitled "Questions in decision theory" (Gilboa 2010c), where you also talk about the recent "soul searching" occurring in the decision sciences. Could you say a bit more about how the paradigm and the theories exactly relate to each other and why you think the distinction between a paradigm and an application is crucial?

The theories are obtained from the paradigm by two main processes. First, there is a specification of terms. For example, when I think of a 'player' in a game, it can correspond to a person or to a nation in a given reality. I can, for instance, decide to model an interaction in which the U.S.A. is a player, or to take the same interaction and think of the President of the U.S.A. as one player, Congress as another, and so on. We are confronted by similar modelling choices when we think of terms such as 'state of the world', 'time period', 'strategy', 'outcome'. Hence, the same paradigm allows for a host of different theories, all compatible with it, for the very same real-life application. Second, there is a process of tweaking and generalizing a theory within the same general paradigm. For example, expected utility theory suggests that payoffs are aggregated by mathematical expectation, and someone, say Kahneman and Tversky (1979), may propose that probabilities that are close to either 0 or 1 are 'distorted'. In and of itself, this generalization can be viewed as a newer theory within the very same paradigm. Note, however, that other ideas of these two scholars broke from the standard theory in more dramatic ways.

Keeping the difference between the paradigm and the theory is then crucial in appraisal for example when we discuss 'theories', whether they 'work' or 'fail', and so forth. We should be careful to distinguish between different interpretations of the same mathematical models. Often the models of rational choice can be interpreted both as theories (coupled with auxiliary assumptions) and as paradigms, and often the empirical failure of the former does not imply that of the latter. This is why we should keep them strictly separate from each other.

Your own work in decision theory mainly focuses on applications of the paradigm in epistemology—questions about belief revision, statistical decision-making, etc.—and in philosophy of science—taking the main goal-directed activity to be scientific inquiry. What are the potential uses of decision theory for the social sciences?

I think that decision and game theorists are often interested in one of the most fundamental problems in the social sciences: How do people think, and how should they be thinking? As this is also a major concern for philosophers, it is why I also feel that parts of philosophy are a social science, especially if we focus on the normative question of how people should be thinking. When we take a more descriptive interpretation, we are closer to psychology and to its applications in behavioural economics. In these applications, there is a focus on ways of thinking that might be simply erroneous and that are not very useful for philosophy of science or statistics. But when we take a normative approach, asking ourselves how rational agents should be thinking in social set-ups, we are basically asking the question that a statistician asks when she wonders what can be inferred from the data, or that a philosopher of science asks if he takes a normative approach. For example, the preference for simple theories is considered to be an important criterion for the selection of theories (ever since Ockham), and it is correspondingly an important criterion for 'model selection' in statistics. The two strands of the literature differ in many ways, but they are asking the same fundamental question. Therefore, it is not too surprising that similar ideas have been developed in these fields.

Decision sciences have experienced an enormous expansion in the last decades, especially in the social sciences. Why do you think the representation of individual decision-making became so important, and how much does a mathematical theory of decision-making really matter in the social sciences? Economics, for example, seems to be at

least equally concerned with understanding macro-phenomena as with explaining individual behaviour.

First, I am not sure what economics is concerned with, nor what it should be concerned with. For instance, if you are interested in predicting how many kids seek college education, or what it might take to change that number, you need decision theory more than macroeconomic theories. Second, if you are dealing with questions on the macro-level such as central bank policy or bank runs, you are interested in game theory and its decision theoretic foundations. This would certainly be the case if you are interested in, for example, the possibility of a war because of its effect on macro variables and on financial markets: whether a certain country will wage war on another is not a classical economic question, and it is one that decision sciences can help to analyze. In fact, it is quite difficult to talk about economics (or political science, for that matter), without the shadow of decision sciences hovering above your head. Consumers and firms, governments and politicians, traders and bankers do just that: they make decisions.

But let me draw the link between the issue of the importance of a decision theory and axiomatization in economics. I find that the choice of the word 'representation' in your question is quite revealing, and this is where decision theory might be more important than one would expect: when one writes a model in macroeconomics, finance, or labour economics, whether the work is theoretical or empirical, there is often a need to model decision makers. Actual parameters may be assumed or estimated, but one needs a general framework into which parameters can be plugged. And this is why representation becomes important. It is a bit of a paradigm, as the specific parameters, defining a theory, are not yet specified. For that reason, one cannot yet test the representation, at least not in its intended use. One can test something similar to it in an experiment, but this gives rise to external validity issues. As a result, axiomatic work becomes more important: it is a way to convince scientists, who have not yet developed their economic theories, that a particular paradigm may be more useful to adopt and plug into their models.

Your view on separating paradigms from theories has several implications, not only for the question of how we appraise decision theory, but also for the assessment of new fields such as neuroeconomics and experimental economics. Those new fields

provide new ways of addressing decision theoretic questions. But you also observe that the question emerges: "what would be the right mix of axioms and theorems, questionnaires and experimental, electrodes and fMRIs?" (Gilboa 2010c, 2). What do you think those new branches can contribute to the field of decision sciences?

We can observe that many more people are now interested in neurological research, which is a good thing. At least if we ignore the moral dilemmas posed by neuroeconomics (both in terms of animal studies and in terms of alternative medical uses of equipment), it is a wonderful thing that we can know more about the human brain while making decisions. There is much more opportunity to connect between psychologists, neuroscientists, economists, decision theorists, game theorists, etc. And so far, neuroeconomics has been very exciting and I think that some of the questions that neuroeconomists pose are worthwhile.

However, I suppose that at some point the communities of economics and neuroeconomics will disintegrate, or separate. Neuroeconomics as a community is probably going to flourish, not necessarily within economics, but maybe in psychology. It is just not obvious that neuroeconomics is the best use of resources if we want to solve economic problems. And I do not think that neuroeconomics makes economics any more promising than it was before there was neuroeconomics. For the time being, there seems to be a very large gap between what we know, what we can possibly measure, and what we need to know about the brain in order to deal with economic questions. I also think that more and more people are very sceptical of the reductionist idea underlying neuroeconomics. In short, neuroeconomics may be a very respectable field within psychology or neuroscience, but it is not necessarily changing the way we do economics. And I thus, in all likelihood, I think that these two fields of research—economics and neuroeconomics—will remain separate for decades to come.

But you also seem to think that new fields, such as neuroeconomics, provoke novel questions for theoretical decision theory and that we can try to use decision theory to formulate those problems in a better, more precise, way. How fruitful do you consider attempts of behavioural economists or neuroeconomists to axiomatize their findings to reach a higher level of precision in formulating those new problems?

I make a living out of axiomatization, so I should not say anything bad about it. But people in the field are often not sufficiently self-reflective. Let us take an extreme view against axiomatizing scientific theories: some people would ask why one would axiomatize a theory at all. A theory should primarily match the data and thus we should first see if it in fact does so. Proving a characterization theorem that shows the equivalence of one formulation of a theory to another formulation does not, by definition, prove anything about the data. The two formulations will be just as close to, or far away from, the data. So you have to ask yourself: 'Why am I doing this and what purpose does it serve?'

But from my point of view, it is more important to axiomatize paradigms than theories. With respect to the axiomatization of a paradigm, I can tell a coherent story of scientific development where axiomatization would play a major role: scientists are dealing with various problems and we could help them. For example, many economists are developing models. And the questions that we can address are: 'What models and theories should they be using?' and 'In which language should they be formulating their models, when they develop them?' Here, an axiomatization can help. And in such a way it could also help in behavioural economics and neuroeconomics. But I do not think that these fields have yet developed such a paradigm. To the extent the behavioural economics has a paradigm, it seems to be the same rational-choice paradigm of economic theory.

So, what exactly is the purpose of axiomatization in economics? And what is the role of the decision scientist in this context?

Imagine that, within their own discourse, some economic theorists cannot answer the question of which language they should use to develop a theory, or which paradigm they should use. This problem arises because they cannot yet compare the theory or the paradigm in question to the data because they have not gathered them yet. Post hoc economists could say: 'Ok, this paradigm was great; it has allowed us to develop all these theories and explained all those phenomena'. But before that, they cannot resort to the data to help them convince each other. What decision theorists can do is use an axiomatization basically as a rhetorical device that says: 'If you find these axioms reasonable, you should find the implication derived from those axioms reasonable'. In other words, they can try to convince the economist to use the mathematical results, which are sometimes useful.

Here comes fancy math, or at least surprising math, which shows you things that are not obvious. If you can imagine a very convincing but complicated proof that takes you from this set of axioms to this theorem, as for example Savage (1972 [1954]) or John von Neumann and Oskar Morgenstern (1947) did, then axiomatization is more powerful because it is not saying something that is obvious to see, but that is mathematically correct. But as mentioned before, I think that sometimes people in the field are not sufficiently self-reflective. While we should develop axiomatic theories, we should be more sensitive to the question, why we are doing that; why we play the game; whether we are really trying to be logical positivists; whether what we are trying to do is descriptive or normative; and what the role of axiomatic work is for realizing what we are trying to do.

What you seem to say is that axiomatization might be useful in economics when we do not yet have a theory, when we want to derive specific, maybe surprising, conclusions from a set of axioms, because these conclusions might be hard to reach without axiomatization. But we can then subject the conclusions to empirical testing. You also seem to suggest that once we have a theory, we should take seriously the idea that it should explain the data. In order to fulfil those two roles, to what extent should axioms be inspired by reality?

Yes, that is right! And I think that the important thing here is what we call 'intuitive' or 'natural', something that informs our axioms but is not necessarily related to a particular example or to a concrete empirical observation about how human beings in fact behave.

So, is it sufficient for a good theory of decision-making that the axioms appear intuitively or naturally plausible?

When you think about axiomatizing a paradigm and not a specific theory, a good decision 'model'—let us avoid here the word 'theory'—is one that will be relatively abstract and sufficiently general, so that I can use it to think about many specific theories. For instance, we can think in a rather general way that there is an outcome. I can specify what that means by thinking in terms of a particular example: you give me 100 Euros. That is an outcome. But I can think of other definitions of 'outcomes' that would include psychological and social payoffs as well, such as 'getting 100 Euros when my friend got 1000' or 'accepting 100 Euros when my friend was exploited by strategic weakness', and so on.

Clearly, this re-definition of an outcome can result in theories that belong to the same paradigm, but have different predictions. The same would be true of other conceptual aspects of a general paradigm, such as ‘player’ and ‘strategy’, ‘state of the world’ or ‘time horizon’. Good models will typically need to be both abstract and convincing, where they can be convincing either because they are intuitive, or because they are mathematically derived from intuitive conditions (axioms). A model needs to be abstract to allow for a range of applications, for many specific theories, for us to feel that we have a general tool that can address many issues. It has to be intuitive for us to believe that these applications, often not yet developed, have a chance of making sense, explaining data well, and generating good predictions.

But maybe some sort of abstraction is sometimes to be trusted more than focusing on a particular experiment that I just observed. Sometimes the examples that a group of scientists starts off with, especially if they have a particular experiment in mind, may be a somewhat biased basis for generalization. There are situations in which, when we think about them in the abstract, we might get a better global view of what is going on in such situations rather than if we get into the details. If you focus on one particular example in greater detail to subsequently generalize, that might affect your generalization; things present in the example might look bigger and more important to you than they really are, and sometimes you might get a better idea if you zoom out. The standard examples are availability heuristics: you estimate the probability of an event and you split it down to a couple of events and get a bigger estimate. That is a case where thinking more is not necessarily thinking better, because you end up with something that might be a worse example. For instance, we can talk about the probability of your car being stolen and elaborating on various scenarios. And, when you give me an estimate at the end, this estimate might be worse than what you would have given me based upon your overall intuition. I think something similar can happen when we think about an abstract problem. Of course this has to do with the external validity of the experiment, and if I am interested in something that is extremely close to that experiment, then it might be fine. But if I start by looking at an experiment and then I switch over to talk about how people behave in markets, even when this experiment was conducted in social psychology, I might get this problem.

To make a long story short, sometimes we can overly generalize. So I would not insist so much on the idea that axiomatizations should be inspired by actual experiments, and on a very close relationship between empirical observation and axiomatization. Rather, as I said, I think axioms should be mostly generalizable and acceptable to us in an intuitive way.

So what role should examples play then and which examples do you consider useful?

We should not ignore examples, as it is fine to be motivated by an example. But I think that one must be able to understand the example as a sort of paradigmatic example, something that can be easily generalized so that one sees exactly what general point it makes.

Could you give a particular instance of such a paradigmatic example?

Sure! When David Schmeidler (1989) began with his research on probability and expected utility, he was not motivated by Daniel Ellsberg's (1961) experiments. He looked at the Bayesian theory and found that it is too limited to capture uncertainty, especially when one is in a condition of ignorance. Schmeidler gives the example of the coin that comes out of his pocket that he has tested many times and the coin that comes from someone else's pocket that he knows nothing about. I think the example is sort of paradigmatic. It is intuitively also quite convincing. It turned out to be very similar to Ellsberg's two-urn experiment, but it was a mere example of a very general difficulty. By contrast, when people looked at Ellsberg's experiment itself, I think that they had a tendency to develop theories that were much less generalizable.

In your own work, empirical observation and axiomatization are closely related. Even when using a theory for prescriptive purposes, you consider the descriptive dimension relevant. For example, in your article "Rationality of belief or: why Savage's axioms are neither necessary nor sufficient for rationality" (Gilboa, et al. 2012), you praise the flexibility of the rational choice paradigm compared to previous attempts in economics to conceptualize human behaviour. The notion of rationality that you refer to is basically defined as consistent choice. And obviously you use the axiomatic method to formulate this concept. But your definition of rationality is also

inspired by observed deviations from the rationality axioms. You stress that we should not think about rationality as detached from reality. Even when used as a normative concept, observing actual decision-making matters. Can you expand on your view about the usefulness of empirically inadequate axioms in both cases, i.e., for empirical and normative purposes?

I stress the practicality of a decision theory in my work. Axioms or axiomatic theories in the social sciences have double lives. You try to use the theories that rest upon a set of axioms as positive theories and if they do not work, you try to sell those theories as normative theories. But even for their normative use, theories should be practical. As Reinhart Selten once informally said, a normative theory that says you should run 100 meter in 4 seconds is not very useful, because you simply cannot do it. As such, the theory does not allow for a practical prescription. When you think about a normative decision theory, it is important to think about the practical behaviours that could be selected by decision makers. That is what I wanted to capture by the notion of rationality that I have articulated.

Could you give an example to introduce the idea of regret that characterizes your definition of rationality?

For example, is it rational to make calculation mistakes? To answer this question, I ask: 'Would you be embarrassed if I were to show you that you do not calculate correctly?' Well, if it were the case that the calculation was too complicated to be performed correctly, you would probably not be embarrassed. If it is impossible for any human being to calculate correctly, you could respond: 'How could anybody have known?' In that case, your behaviour is rational according to my definition. It is consistent, or robust to our analysis, in the sense that preaching to you to behave differently would be useless. For the sake of usefulness, we need to somehow place practicality into the picture when endorsing normative theories.

In your article entitled "Is it always rational to satisfy Savage's axioms?" (Gilboa, et al. 2009, 289) you write: "The question we should ask is not whether a particular decision is rational or not, but rather, whether a particular decision is more rational than another. And we should be prepared to have conflicts between the different demands of rationality. When such conflicts arise, compromises are called for.

Sometimes we may relax our demands of internal consistency; at other times we may lower our standards of justifications for choices. But the quest for a single set of rules that will universally define* the *rational choice is misguided". So you formulated a definition of rational choice that weakens the idea of a unique standard of rationality...

My definition of rationality started with this: asking what do people have in mind when they refer to something as 'rational'. But the best definition I came up with is in terms of what most people would be willing to accept as their decision making modes, as opposed to what they would like and could change.

Is this definition of a rational choice pragmatically useful for improving one's decision-making?

Yes. I think that we can play around with definitions to our heart's content, and judge them for elegance and beauty of results as we do in mathematics. But to the extent that we care about a particular definition—say, what is and what is not called 'rational', or, for that matter, 'scientific', and the like—we should ask what it is exactly that we care about in specific definitions and then choose them accordingly. Why do we bother to dub some modes of behaviour as 'rational' or 'irrational'? If it is only a matter of name calling, then it is not so clear that it is worth the effort. Rather, we need to think about what kind of discourse we have in mind that might be facilitated by a specific definition. Indeed, this boils down to a pragmatic position.

How does the mathematics enter this picture?

Part of what happens is that the way people choose is an issue of 'either-or'. Either a person makes decisions in a completely intuitive way, or she makes decisions in a supposedly rational, but at the same time highly mathematical way. People tend to view these two things, mathematics and rational choice, often as going hand in hand. People who are scared of mathematics often tend to not even listen to what insights there are behind the mathematical apparatus. Rather, they tend towards the other extreme, that is, they fully reject mathematical theories of decision-making. But one does not have to be scared of the mathematics. Good decisions, be that for individuals or for society as a whole, should involve some kind of dialogue between the theory and the subjective input, be this an input that originates in emotions, intuitions, or something else.

And what do you take 'rational choice theory' to be in this context?

Well, it is a bit of everything. It is a toolbox and not everything in it is tightly related. It consists of a couple of useful things we find in decision and game theory, microeconomics, and so forth. Yet, they come from the same way of thinking, and they describe the state of the art in a way that is not too biased. I do think that these are very beautiful ideas that should be more publicly available.

You mention at some point in your textbook* Rational choice *(Gilboa 2010a) that you would like to live in a society where everybody knows about the tools in this book contains. Why?

Yes, I do indeed think that. People vote. People make decisions for themselves and for us, and they do it based on various pieces of information that they get. This information can be highly manipulable. For example, you hear that a certain percentage of inmates belong to a certain ethnic group and people in their minds begin to think that people belonging to this ethnic group must be criminals. In this example, people confuse the probability of A given B with the probability of B given A, a psychological phenomenon we understand very well. But we could teach people to become aware of this confusion and learn to avoid it.

Is improving decision-making your pedagogical aim when teaching with this textbook?

Yes, I think it is valuable to teach the tools in this book to everyone, including people who have no background in mathematics and no willingness to get into that. Ultimately, mathematics itself is not important for the public debate. It is often rather a sort of barrier to entry to many people involved in practical decision-making. What is important is to convey the basic messages and to have people participate in such public debates in a more educated way and especially to address the basic questions about what is feasible in achieving the desirable.

So, we should use the toolbox of rational choice as a normative instrument to make people behave rationally...

Yes, that is right. I think it could teach us to improve our reasoning and judgment, to think more critically about so-called experts, be it in politics, medicine, or whatever. Judging whether politicians are more or less successful could be done in a more rational way. I think I could even convince some of them that their way of making decisions can be improved (in their own eyes). I think it is essential to understand how to be rational in the context of economic and political questions, and to understand the nature of democracy in the context of the limitations of preference aggregation.

Another thing that often bothers me in the political domain is that people tend not to think in terms of what is feasible when they think about what is desirable. For example, in political debates, people sometimes reason by assuming that what they want is also possible. This would be considered flawed reasoning according to many rational choice models. Indeed, most people would not make this type of mistake in the context of, say, a financial investment. But when it comes to ideological questions, it is often a big no-no even to pose the question of feasibility.

In your account of rationality, you fuse several different dimensions of a theory of rational choice. You repeatedly talk about the trade-off between having a mathematically beautiful theory—one that people might not conform to—and a more descriptively accurate account of human behaviour. And, you use your toolbox of rational choice for prescribing the rational course of action. To make the mathematical theory more descriptively accurate, you can either change the theory—like behavioural economists do—or you can make people conform to the theory. You define rational choice as a choice where a decision maker does not want to change anything when confronted with a mathematical analysis of his or her behaviour; the decision maker might regret the choice in light of new information, but not as a result of a theoretical argument. This account of rational choice allows the study of how people in fact deviate from the prescribed rational choice and assumes that they would regret it. How exactly do you bring those different dimensions under one roof? How does this definition relate to what rationality is usually considered to be,

i.e., independent from subjective elements, arrived at by reason and rational calculation and serving as an ideal standard?

First of all, I do not have any bit of understanding of metaphysics, so I do not know whether anything exists, unless I know how to measure it. And that is partly why I appreciate axiomatic work, because it gives concrete meaning to things that can be very abstract. I can only understand metaphysical concepts when there are psychological manifestations thereof. For example, I can talk about free will, but I only refer to the psychological phenomenon that people seem to experience making choices and exercising free will. Whether they really make free choices, and what that would mean, are questions I do not fully understand. Taking this non-metaphysical stance, I do not know what exists out there that is 'independent from subjective elements', and I am not even trying to grasp it.

Second, I think of myself as a generally democratic, liberal person, and I do not think that I have the right to decide for people what to do with their lives, their children, their money, etc. I am in this sense, somewhat anti-elitist. If you ask me what we should do with taxpayers' money, what we should teach in schools, and so forth, I would give answers that I believe I can support based on these people's future well-being as perceived by them. For example, should we expose kids to Mahler's music at school? I would say 'Yes'. But I would say that not because I think Mahler's music is great, although it is, but because I think that, if you were to run experiments, many people would acquire the taste for it and think that it is the greatest music that exists. I am not sure if I can make the same case for Karlheinz Stockhausen. If we have a kind of music that remains something extraordinary only for a very small group of people, an elite, it is not clear why we should use taxpayers' money because it is not the case that most people would benefit from it.

My non-metaphysical stance and my democratic criterion form the background to how I address the question of rationality: I do not believe we have access to anything out there that determines 'true' rationality independently of human beings' judgment. And I do not think that a bunch of smart people should define what rationality is for the rest of humanity, whether the latter does or does not agree with it. I believe that, eventually, judgments of rational choice should go back to the people about whom we are talking, for whom we are making decisions, whose money we are spending. This is why my notion of rationality is about explaining and convincing, and will eventually depend on the majority's view.

Yet, does your definition of a rational choice not presuppose that reasoning according to logical rules is of greater value than reasoning ignoring logic and that people would therefore regret violating the basic rules of logic?

Not necessarily: I am willing to subject logic to the same test. True, I think that most people would be convinced by logic, and, for example, be able to understand modus ponens, and feel bad about violating it. But this claim of mine is an empirical claim. If you show me ample evidence to the contrary, I will have to give up my faith in logic as a widely accepted form of reasoning. I will have to admit that the structures I like in reasoning are not necessarily shared by people in the society I live in. I hope I will not be caught saying that these structures have 'greater value' than other structures. Just as I hope not to be supercilious about the kind of music or literature I consume. At present, I do believe that many of the principles we preach will, given the exposure, be adopted by a large majority of people. But I should be ready to admit that I might be wrong about that.

Why, do you think, would people feel bad about violating the principles of rational choice?

I believe that we have immediate, affective responses to cognitive inputs such as logical reasoning. It is akin to, or maybe just a special case of, aesthetic judgments. Just as we can have positive or negative affective responses to a painting or a piece of music, we can have such responses to reasoning. I conjecture that, as an empirical claim, we are hardwired, by and large, to enjoy coherent reasoning and to be aversive to contradictions. I suppose that we will have to go to evolutionary psychology to answer this question. We can explain much of our aesthetic and even ethical judgments by evolutionary stories and I think the same applies to reasoning and even to decisions. One can argue, for example, that because cyclical preferences were dysfunctional, humans evolved to dislike them, or to feel uneasy about them.

How then are psychology and decision sciences exactly related?

As most social sciences, decision sciences have a descriptive and a normative side: they are about what reality is, but also about how we can change it. Psychology feeds decision sciences with facts about how people actually behave, which is the reality that has to be taken into account. At times one has the chance to try to change this reality (say, if someone asks you for advice). But then again you need to know something about reality—that is, what can possibly be done, what we can expect humans to do.

And what are the implications of such a view for the potentials and limitations of rational choice theory?

I indeed do not think it is a theory. In most real life situations, it does not commit to any specific prediction. Rather it is a way of thinking that may, at least post hoc, explain a remarkable array of observations and phenomena. When viewing rational choice as a paradigm rather than a theory, it offers ways of thinking about decision problems, but it does not commit us to produce a single, well-defined answer in all cases. For descriptive and normative purposes alike, a paradigm may offer more than one prediction or recommendation, and one may need to use common sense or ad hoc considerations to choose among them. In short, while the rational choice approach is indeed limited as a theory, I think it is quite successful as a paradigm, as a way of organizing our thoughts, and as a way of testing and critiquing reasoning.

This relates to another prominent debate in philosophy of economics about the empirical limitations of economics as a discipline. There was a time in which philosophers like Alex Rosenberg did even call into question its status as a science (e.g., Rosenberg 1992). Critics of economics often referred to the axiomatic theory of rational choice as the main weakness of economic theory and believed that behaviourally or psychologically more accurate theories of individual agents would rescue economics from all its troubles. Is this still a legitimate criticism of economics and rational choice theory specifically?

Economics certainly has limitations as a science. However, we should not take this criticism too seriously for two reasons. First, one should not expect to be able to predict the behaviour of large, complex, and causally interrelated systems such as economies, polities, or societies. Even in the case of weather prediction, where the basic physical laws are well understood, prediction for more than ten days ahead is rather limited. In the social sciences we have two additional problems:

a) we do not have the basic laws, the counterpart of the flow equations; b) we are dealing with systems that respond to the predictions made about them. Thus, there are some fundamental limitations to the possibility of predicting the behaviour of economies and we should have realistic expectations about these limitations. Indeed, when we are dealing with smaller, isolated systems, that are causally independent of each other and can be experimented with, prediction is much easier.

Second, the failures of basic choice theory in psychological experiments are often exaggerated. Surely every axiom and every principle has counter-examples. The question is not whether a theory is perfectly correct, but whether it is incorrect in an important way for economic applications. Psychological experiments are selected by their ability to shed new light on the working of the human mind. They need not be a representative sample of economic decisions. I do not think we should be entrenched in defending our classical theories (as economists were some 20 years ago), but we should not get carried away to the other side, decide that the theory is completely wrong, and make predictions only on the basis of vague similarities between experimental situations and real-life economic decisions.

Furthermore, it remains unclear whether the empirical shortcomings of economic models are always to be sought in the rational choice foundations. Even if we are unhappy with a model's predictions, I would argue that the problem rests only sometimes in the foundations of rational choice theory and very seldom in the rational choice paradigm. Let us start with the rational choice paradigm: behavioural economics, by and large, retains the paradigm. In fact, it has been criticized precisely on these grounds, namely, that it does not do much beyond incorporating one more variable in the utility function. As for rational choice theory, there are many problems in the very assumption that we observe equilibria, that all agents share the same beliefs, or that beliefs can be represented by probabilities. All these assumptions are highly questionable and have little or nothing to do with rationality, as I understand it. But even if you think that the agents should be rational à la Savage, and care only about monetary payoffs, the assumptions that they all have the same prior probabilities, or that they play an equilibrium of the game cannot easily be derived from each agent's rationality, even when the terms are very broadly understood.

So, do economic models based upon rational choice foundations have epistemic value?

Yes! Let me stress two more points. First, the rational choice paradigm can also be used for making predictions by drawing analogies to models, and not only by applying general rules. This is a different view of scientific reasoning than the classical, Popperian one, and it is a way in which economic models can be useful without providing general rules that are empirically validated.

Second, an important role of economics is to criticize reasoning. Just as logic is a basic tool for such criticism, so is equilibrium analysis. According to this view, economics is not about making predictions, but only about finding flaws in reasoning by others (say, politicians). I think that there is little doubt that the rational choice paradigm has been very useful as an aid to such criticism.

But would economics not lose its empirical value if we took its role to be criticizing reasoning?

It is not the only way to understand or do economics. But suppose we do follow this line—economics can be very useful without being an empirical science. History is an example of a discipline that is broadly considered to be very useful. Yet, very few historians would venture to make empirical predictions as if they were scientifically based. Similarly, the standard view of philosophy is that it is very far from being an empirical science, yet that it is a good idea to study philosophy, and that, in some ways, the world will become a better place with philosophers. I believe that economists could justify the existence of their discipline in a similar way: focusing on criticism and helping society avoid major mistakes would be enough to justify the field and its costs to society.

Taking this issue one step further, the status of economics as a science has frequently been addressed in discussions about using rational choice theory in economic models, asking the question whether a model based on descriptively unrealistic assumptions can have any epistemic value and, if so, what kind of knowledge it generates. In your book review (Gilboa, forthcoming) of Mary Morgan's **The world in the model** ***(Morgan 2013) you highlight that one frequent defence of abstract economic models is that what matters for them to have epistemic value is not the realisticness of***

assumptions, but the consistency of assumptions with reality. How much 'inaccuracy' can economists accept without jeopardizing the little if any empirical value that is still granted to economics, especially after the recent economic crisis?

First, I think that the economic crisis of 2008 is not a good example. As mentioned earlier, economists are no better equipped to predict financial crises than are physicists to predict tsunamis. This goes back to the issues of complexity of the system, the inability to test the system in isolation, and so on. There are problems that the last crisis highlighted—whether it is a matter of incentives or the belief in free markets (which might involve a major component of betting)—but it should be born in mind that unpredicted crises do not cast doubt on economics as such any more than unpredicted tsunamis cast doubt on physics.

Second, the easiest way to defend the position that economics has some value is to emphasize models as tools for criticism: models, even if they make assumptions that are generally implausible, can be very useful in testing the logic of claims being made in the public domain. And such criticism can be very useful and save us a lot of unnecessary suffering. Relatedly, assumptions that are implausible as general rules may still be very useful in constructing models that may be, to some degree, similar to reality.

However, I do believe that the lack of realisticness should be kept in mind. And when we see economists who truly believe the predictions of their models, we should be wary. It is wonderful to have models, as long as we acknowledge their limitations. Here starts one important task of the philosopher. Philosophers should not just endorse the use of unrealistic assumptions. They should ask: 'When and how do and should scientists use such assumptions despite their unrealism?', 'Why do scientists find unrealistic assumptions still useful?', 'When should we, philosophers, warn them that they have been going too far with the implications of these assumptions?'

So, when should philosophers warn economists?

This question has a theoretical and an empirical side. On the theoretical side, I could say that the answer depends on the model of philosophy of science that you apply to economics: do you think of it as a Popperian science, as a practice of reasoning by analogies, as a field of criticism, and the like. On the empirical side, I fear that I do not have a good answer. My approach to philosophy of science as a social science requires that I restrict myself to its theories and keep silent on empirical questions. Just as I would not make empirical comments in, say, labour economics, I should not make them in philosophy of science. Hopefully there are empirical researchers who can give much better founded answers to these questions than I possibly could.

Philosophers and psychologists are often ignored by economists and decision theorists. Although behavioural economics has gained prominence in economics, psychologically informed decision theories, such as the research program defended by Gerd Gigerenzer (Gigerenzer, et al. 2011) have not had a considerable impact on economics. Why?

There may be several, perhaps related reasons for that. First, economists and decision theorists tend to be people who like beauty and elegance. Given a wonderful construction such as the Bayesian paradigm, it is just not fun to use other methods, which are less elegant, and whose inclusion would make the entire theory even messier. That is, one has to have a meta-theory, describing when one should use a Bayesian approach and when one should use other approaches. The whole thing may look rather ad hoc. Second, for many questions that are interesting to economists, the origin of beliefs as requested by the Bayesian paradigm may not matter that much. Thus, some economists ask: 'Why should I care? If I can capture the relevant aspects of behaviour by a model using probabilities, why should I bother to specify the process of generation of such probabilities? If I need to know the probabilities for empirical work, I will anyway have to measure them directly'. This line of reasoning also conforms to the 'black box' interpretation of choice theory—the revealed preference paradigm, on which most economists have been educated. I should mention that, while I personally take issue with this line of reasoning, it is not easy to make the case for the importance of the process, and it is particularly difficult to make an argument to convince economists that these foundational choices might lead to different predictions; partly because we are comparing paradigms, or languages, rather than specific theories.

Could you nevertheless try to sketch such an argument?

With pleasure! Suppose that we wish to predict economic behaviour after a financial crisis such as that of 2007-2008. Past examples are very few, and it is hard to argue that we have observed behaviour under many similar circumstances. Worse still, these are causally intertwined: just because governments did little in 1929, they were prodded to do more in 2008. That is, we cannot use available data to make predictions as in standard statistics; the very fact that certain things happened changes the likelihood that they will happen again. So we cannot rely on the behaviour of the black box in the past, and pretend that we know enough about behaviour so as not to worry about cognition. And we have to ask: How do people think? Will they make predictions here by analogies or by rules? Will they end up using a probability measure, and if so, how will they find one? And, if not, what will they use instead? In short, when we have sufficient data on past cases that are similar and causally independent, we can say that how people think is a problem for psychologists, and we only care about their (economic) behaviour. But when we do not have enough such data, we have to roll up our sleeves and delve deeper into the decision making process.

A similar observation of ignoring new approaches can be made in the research on decision-making under uncertainty. There are several kinds of axiom systems and more generally rational choice theories that attempted to capture the idea of uncertainty. There is the Bayesian approach, J. M. Keynes's approach (Keynes 1921), and Isaac Levi's work. Together with David Schmeidler (Gilboa and Schmeidler 1989), you suggested an axiomatic foundation of the maxmin expected utility decision rule to address the problem of a non-unique prior for example, and thereby made an important contribution to decision making under uncertainty that takes the Knightian concept of uncertainty seriously (and also opposed many accounts that reduce uncertainty to risk for operational purposes). While those approaches have profoundly influenced each other, why do you think some of them failed to be influential in economics while others, like the Bayesian approach, were widely taken up?

Isaac Levi's work is mostly unknown to economists, but it also does not provide the axiomatization that is needed to convince economists that a particular paradigm is the one to use. So we are mostly left with the Bayesian paradigm and the alternatives proposed by the uncertainty theories, starting with Schmeidler's version of Choquet expected utility (Schmeidler 1989), the maxmin rule, and others.

Independently of whether we think of these new theories as part of a 'protective belt' of the same traditional research program or as a new research program, I think they do get sufficient attention in economics. When economists see phenomena that are difficult to reconcile with the Bayesian approach, at some point they are willing to look a bit beyond—and then it becomes advantageous to have a model that is a slight generalization, as opposed to a whole new approach, using a different language.

Surely, 'paradigms' or 'conceptual frameworks' such as Savage's are adhered to longer than are specific theories, precisely because these are paradigms within which new theories can be developed. But there comes a point where people are willing to look beyond the paradigm as well.

This process differs from Thomas Kuhn's in that no one expects a theory—or a paradigm—to be universal (see Kuhn 1962). So that the fact that we need to go beyond a certain paradigm to explain some phenomena does not mean that the paradigm should be discarded. If you will, you can also argue that this is the case in classical examples such as physics. Just as Newtonian physics is still the basic working tool for engineering, the Bayesian approach may well remain the basic workhorse of economic theory.

What are the most important unresolved questions in decision sciences today?

Maybe we should start with resolved ones. I fear I do not know of any. We still do not know how people make, and should make decisions, under risk as well as under uncertainty, in lab situations and in real life, in economic set-ups, or in others. We have some wonderful ideas constituting a fantastic paradigm, but we have very few concrete answers.

Yet, I think we gained a much better understanding of the questions. We learned to distinguish between, say, risk and uncertainty, groups and individuals, and so forth. But, as mentioned, I think we also need to distinguish between types of applications—say, a theoretical application where an economist plugs a representation into a formal model, or a practical one, where a patient decides whether to undergo an operation. Also, it is not clear that the same model would apply to people's decisions when they trade stocks as when they get married, purchase products or wage wars, when they consciously make decisions, or find out that a certain decision has simply occurred.

In a sense, I think that we have not quite resolved the question of what decision sciences are about, or what their questions are precisely. In my view, we need to realize that there are many different questions that need not share an answer. Once we realize that, we can start asking which of these questions have been resolved.

Do you expect decision sciences to progress?

It is possible that we suddenly see less axiomatic models, just like we saw tons of refinements of the Nash equilibrium in the 1980's and then, at some point, people lost interest in them. The research moved forward, or backward, or sideward. It is hard to tell whether it moved forward in a progressive way. There are always fads in the different disciplines. And although we now see a lot of general decision-theoretic models, it is possible that after a while people would still keep asking: 'What has decision theory done for us lately?' And if the answer is negative, then we might be seeing less of these models.

Are you after truth?

Not in a metaphysical sense of truth that exists outside—I do not understand what it means. So, I am willing to do only psychological metaphysics, which is along the lines of 'let us take a metaphysical question and consider its psychological manifestations'. Let me then reread 'truth', or translate the term to mean, a warm feeling of understanding, or the warm feeling that comes from understanding, coupled with the belief that I am not going to change my mind so soon. If that is truth, then yes, I am after that. I like to understand things, and mathematics allows me to do that because, once you check the proof, you rarely change your mind about it. You might change your mind more about things that cannot be mathematically proven or have not been proven yet. So in short, if the meaning of truth is psychological subjective truth, then yes, I am after it.

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Review of Don Ross's *Philosophy of economics*. Palgrave Macmillan, 2014, 341 pp.

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Don Ross begins his recent book by explaining why he regards analytical philosophy of science as "a barren enterprise" (p. 1). Analytical philosophy of science (derived from the twentieth century analytical philosophy tradition) accords the philosopher special expertise in the 'logic of concepts' and thus a unique responsibility for determining universal norms of thought for the sciences. This entails that technical concepts in science should be translated by the philosopher into more general, non-technical concepts—a kind of semantic reduction—purportedly in order to make the concepts of science more clear and precise, and thus more fully illuminate the achievements of science. However, Ross points out that this aim is at odds with the scientist's expectation that it is "the course of empirical discovery and theoretical refinement [that] will make her technical concepts more coherent and consistent" (p. 5). It is not pure conceptual analysis that advances philosophical understanding in science, Ross argues, but rather what scientists learn about their concepts from their investigation of the world. He accordingly takes up this vantage point in his book, and since the book is on the philosophy of science of economics, his starting point is "the course of empirical discovery and theoretical refinement" in economics itself that would make technical concepts in economics "more coherent and consistent".

Analytical philosophers of science have another ambition, Ross goes on, closely connected to their self-identification as experts in concept analysis. This is to promote the unity of science as an ideal and a particular way in which they believe the sciences 'fit together'. Here parallel to the semantic reduction idea is a strategy of 'boiling down' (p. 8) how different sciences explain into some single set of relationships—what Ross characterizes as ontological reduction and the simplest form of unification. For example: "This program encourages [scientists] to treat psychological structures and processes as just equivalent to neurophysiological structures and processes", such that neuroscience would displace or eliminate psychology (p. 9). Ross does not reject the ideal of unifying the sciences, but he does reject eliminativist unification strategies, and follows Philip Kitcher's more flexible view that scientists seek common 'argument patterns' without recourse to explicit methodological reflection and engage in a kind of "mutual disciplinary adaptation" (p. 10). He defends this view on pragmatic grounds and from his understanding of the history of science. The development of science is just too rich and complicated to bundle into simple conceptual packages and overarching schemas. Nonetheless, Ross still thinks philosophers of science and philosophers of economics have a role to play. Specifically, the philosopher of science/economics needs to be a "*speculative, forward-looking historian of science with a special focus on interdisciplinary unification*" (p. 13).

I will discuss the basis for Ross's understanding of interdisciplinary unification below. Here I flag Ross's position that what the philosophy of economics is principally about is interdisciplinary unification, and in particular economics' relation to psychology and sociology, because I think many interested in the philosophy of economics will find this view novel, counter-intuitive, and perhaps disagree with it. Indeed, many might rather say that the philosophy of economics is only about philosophical concepts and issues that are specific to economics, especially the concept of economic rationality. But for Ross preoccupation with the concept of economic rationality in the philosophy of economics is a "deep distraction and a red herring" (p. 24) which has perhaps done as much damage to the philosophy of economics as analytical philosophy has done to the philosophy of science. What we ought to do, he argues, is put aside our endless conceptual analysis of rationality and focus our attention on economics' scientific development in relation to its near neighbors.

This is what Ross himself does in this book after chapter one's discussion of the philosophy of economics and philosophy of science, devoting the second chapter to the evolution of the economics of markets in relation to its neighbors—mostly psychology—before 1980, and the last two chapters of the book, four and five, respectively to economics and individualistic psychology and economics and aggregative forms of social science, which includes macroeconomics and sociology (or social psychology). The third, middle chapter of the book presents his understanding of economic science around which his overall argument is built. His position on economics' relation to its neighbors follows from this understanding; it is that debates in the philosophy of economics "have been distorted by undue emphasis on the integration of *psychology* with economics by comparison with attention to the unification of economics with *sociology*" (p. 23). Thus, the most important question for the philosophy of economics is:

> Are the principles of normative decision theory, or at least those principles most relevant to identification of relative opportunity costs and opportunity values, more closely approximated by individual people making choices in relative isolation, or by groups of people making choices in certain sorts of institutional contexts? (pp. 36, 186).

His answer is the latter, and thus he emphasizes that one of the 'main themes' of his book is that we should reject the idea "that all important properties [economics studies] 'boil down to' properties of individual people" (p. 2)—the standard microfoundations project. Indeed he rejects methodological individualism as a 'dogma' that economists would be better off abandoning (p. 20, see 114ff.). So what is his conception of economic science that underlies these views?

Ross calls it neo-Samuelsonianism. Following Paul Samuelson's development of revealed preference theory that completed twentieth century economics' long move away from psychology, economics in the latter half of the twentieth century became a science that operates at a level of aggregation above individuals.

> Choice behavior, for a neo-Samuelsonian, is simply any behavior that is systematically (but typically stochastically) related to changes in incentives. The causal basis of choice behavior, at the individual scale but also at the aggregate scales that economists mainly study, includes channeling structures in the social and institutional environment that are often not explicitly represented in choosers' nervous systems, let alone in conscious awareness (pp. 251-252).

That is, economists study the ways in which markets themselves work in specific environments. The misconception that economics is about the individuals who participate in markets, then, stems from what Ross sees as a mis-reading of Leonard Savage's decision theory. Savage produced an idealized, 'small worlds' understanding of decision-making in which "institutionalized constraints tightly limit agents' goals and narrow the domains of the beliefs and conjectures that matter to their actions" (p. 239). It is still 'bedrock' theory for economics, and underlies the expansion of economics' toolkit to include game theory (discussed at length in chapter three), which Ross regards as a revolutionary advance in economic science. But Savage's 'small worlds' domain is not really the domain that economists investigate. Rather—here Ross follows Ken Binmore's cue—economists investigate 'large worlds' with:

> Macro-scale labor markets, coalition-formation markets driven by politics and regulation (the main source of determinants for international trade), markets for innovation and entrepreneurship, financial markets, insurance and risk management markets—all of these abound with uncertainty (p. 239).

Savage's decision theory and Samuelson's revealed preference theory transfer well enough to these more complicated environments, but at the price of giving up the individualist orientation of the 'small world' frame for a more socially oriented approach. The economist, that is, needs to know a lot about the world that social sciences other than individualist psychology investigate in order to make good use of the modern achievements of economic science. A paradigmatic example for Ross in this regard is the "neo-Samuelsonian Nobel laureate Vernon Smith" whose concept of 'ecological rationality' provides a broader, more flexible framework of economic analysis (p. 239) than what Ross believes one will find in much of recent behavioral economics, and even neuroeconomics (about which he is quite critical—the main purpose of most of chapter four).

This, then, gives us a quick overview of Ross's understanding of economic science and his grounds for saying that the philosophy of economics should be concerned with economics' relationship to its near neighbors. However, the last section of chapter four (three quarters of the way through the book) suddenly opens up a new line of argument—though it has been implicit earlier in the book, and will not be new to those familiar with Ross's earlier works. The title of the section is: "Ecological rationality, externalism, and the intentional stance". When I reached this discussion, my first impulse as a reviewer was to say to readers that they ought to begin their reading of the book here rather than on page one. That is not really very practical advice, but I think the point is still basically fair since Ross's understanding of science and economics, and thus his philosophy of economics, cannot be easily separated from his long-standing commitment to a version of Daniel Dennett's philosophy.

Dennett (e.g., 1991) rejected internalist philosophies of mind that accounted for intentional behavior in terms of peoples' internal mental states on the grounds that 'looking inside' people is an incoherent exercise and one of the biggest dead-ends in the history of philosophy. In its place, Ross says, Dennett argued we should be concerned with "*real patterns* [of intentional behavior] at the scale of *social* organization, as opposed to approximate descriptions of states or events at the scale of individual psychology" (p. 245). In Ross's earlier book (2005) he labeled this view 'intentional-stance functionalism'.

> It begins from a hypothesis about the *function* of mental concepts that caused them to evolve as a part of every normal person's behavioral repertoire. In order to coordinate their expectations, people must model one another as goal-directed systems. Furthermore, they must do so by reference to goals and means of achieving goals that they can share (p. 245; emphasis added).

The emphasis on function is important. Mental concepts develop from and are functional to people's interactions with one another, and thus one learns little about behavior by asking what people's motivations in isolation are. Rather, to understand behavior we need to look at whole populations of interacting individuals since it is at the aggregate level that we can observe patterns of behavior. This, Ross claims, is what sociologists are concerned with.

Thus the last chapter of the book turns to the issue of whether economics and sociology are converging. Its premise is that both are aggregative social sciences, but the hard work in developing the case for convergence lies in reconciling the different batteries of concepts employed in the two disciplines, concepts often seen in each discipline as radically opposed to one another. Ross's discussion at this point is accordingly prescriptive and programmatic, though he does examine arguments against unification from both sides. I will leave it to readers to review the details, and instead note how for Ross the matter ultimately depends on what he regards as the great failing of contemporary economics, namely many economists' continuing attachment to methodological individualism.

The problem, he argues, is that economists tend to be confused between normative individualism, which he supports, and descriptive individualism, which he believes is false—Ross calls himself a descriptive anti-individualist (p. 304). Normative individualism, the ethical promotion of individuality, is an achievement of modern societies and market economies derived from widespread recognition of the intrinsic and instrumental value of individuality. But descriptively speaking, people are not single, independent individuals in the sense most economists believe because, though

> an economic agent is identified with a utility function […] people's preferences are dynamically sculpted by socialization processes [and] an economic model of any relatively long stretch of a person's biography must depict the person as a *succession* of economic agents (p. 305).

This passage exhibits both the basic argument Ross wants to make and the tensions inherent in that argument. His vision of economics' achievement as a science is neo-Samuelsonian economics with individual utility functions and incentives. But those individual utility functions are not single individuals' utility functions in the sense most economists believe them to be, and yet they are still single individuals' utility functions. I am sympathetic to the idea that "people's preferences are dynamically sculpted by socialization" but do not see why people should still be thought to have individual utility functions. The only basis for this seems to be that this is the standard position in economics science, though that is hard to separate from the fact that most economists, Ross allows, are still methodological individualists.

Ross does recognize that it can be thought tautological to say that individuals "maximize 'their own' utility functions" since when "an economic agent is individuated in the first place by a utility function […] there is no logical room for a utility function to 'belong' to any entity but the agent defined by reference to it" (p. 201)—a circularity argument I have made (Davis 2011, 6ff.). But he takes this to be a critique of egoism in standard theory rather than a problem about the arbitrariness involved in assigning individual utility functions to *any* kinds of economic agents as their 'own' utility functions. This is hardly an unimportant issue, moreover, since one of the principal achievements of any established science is getting causality right, and the assignment of utility functions to agents assigns them the status of individual causal agents. Why, then, should agents with preferences that are "dynamically sculpted by socialization" be assigned individual utility functions and the status of independent causal agents?

Ross brings a well-motivated philosophy of science critique of analytic philosophy to the philosophy of economics, and he uses Dennett persuasively to undermine individualist explanations in economics and to cast doubt on what psychology offers to economics. Less clear is how neo-Samuelsonian economics survives.

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Review of Lisa Herzog's *Inventing the market: Smith, Hegel, and political theory*. Oxford University Press, 2013, 208 pp.

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This short but insightful book is based on the author's Oxford DPhil thesis. As its title indicates the book analyses and compares the constructions of the market by Adam Smith and G. W. F. Hegel, and puts special emphasis on their relevance for contemporary philosophical issues. Two challenges are central to this project. First, the obvious interdisciplinary angle of this study means that it takes its subject matter, the market, from the field of economics, but the treatment it receives "has little in common with economic theory as it is [generally] practised today" (p. 11). The intention of this study is rather to show how Smith's and Hegel's understanding of the market can benefit and deepen certain sterile (from the Hegelian point of view) contemporary philosophical debates such as the one on liberalism/ individualism versus communitarianism. Here Herzog's approach is certainly legitimate and deserves to be encouraged. Second, the author's sustained (and at times strained) efforts to make these historical thinkers "fruitful" for "contemporary problems" or at least current "debates" in political philosophy, lead her to advance a bold methodological programme which she calls "a post-Skinnerian approach" (pp. 11-14). The merits of Quentin Skinner and in general of the 'Cambridge school' of the history of ideas are not denied or minimized, but the author does argue that such "contextual" readings may lead to neglecting "systematic questions" (p. 12). This may be more contentious.

Chapter 1 provides a general introduction that highlights the meaning and, indeed, the power of the market over our lives, while insisting that the market "has not figured prominently in the political theory of the last decades [...] Often, the market seems to be the ghostly 'other' of the institutions political theorists focus on, something that needs to be tamed and restricted, but not itself made an issue" (p. 3). By way of this diagnosis (and the criticism it implies) the general direction of this study becomes evident. The introduction also provides some basic information on Smith and Hegel, as well as their impact on later thinkers (p. 5ff.), before arriving at the methodological statement already mentioned.

Chapters 2 and 3 give brief competent accounts of the respective "constructions of the market" by Smith and Hegel. On Smith, Herzog begins by attacking the superficial readings ("clichés") of *The wealth of nations* (WN) that were long common and survive in some economics textbooks and other odd corners. To deepen his image and to make Smith emerge more clearly as a philosopher, Herzog briefly describes his context and gives an idea of Smith's overall system—drawing not only upon WN, but also *The theory of moral sentiments*, *Lectures on Jurisprudence*, and even his essay on the history of astronomy—and of his subtle notion of nature. The chapter culminates with Smith's account of the market society, presented in three steps: (a) the institutional framework with its classical tasks of protecting the members of society from foreign invasion as well as from internal oppression and of establishing an exact administration of justice; (b) the mechanisms and functions of the free market, including the metaphor of the "invisible hand"; (c) a consideration of the possible failures and insufficiencies of the market, and various remedies. The author might have paid more attention to the last point (dealt with in only about one page: 36-37), to counter-balance the optimistic conclusion of the preceding section on the general opulence that might spring from the proper functioning of the market it (she does however come back to this in chapter 6).

The chapter on Hegel begins with general overviews of Hegel's reception and the place that is (or is *not*, in more piece-meal revivals of certain aspects only of Hegel's practical philosophy) attributed to Hegel's system. While it is of course indispensable to sum up the relevant material I would have wished for a more decided stand on the issues, rather than playing the detached observer on points like this "some see [Hegel] as the forerunner of Marx and critical theory, others as a right-wing defender of the Prussian state […] [who may have] paved the way to fascism" (p. 41).

The crucial section on the market begins with a summary of the historical evidence for Hegel's reading of Smith (and of political economy in general). Deciphering, and 'translating' as it were, the philosopher's difficult language into modern terms, Herzog finds the market economy in Hegel's "system of needs" and she succeeds in explaining it clearly—no slight achievement. She seems however to exaggerate the differences between Hegel and Smith, insisting that Hegel perceived the market as irrational—she uses not only the terms "unstable and unpredictable" (fair enough), but also refers to a "Dionysian, chaotic process" (p. 54f.). To be sure, Hegel underlined the undesirable and even dramatic impact that accidents, caprices, and far-off foreign circumstances may have on the market (was he not realistic and even far-sighted in this?) but, given the kind of philosopher he was, he could never be satisfied with the appearance of chaos. What strikes him at first glance as arbitrary, messy and even chaotic, always spurs him on to seek deeper understanding of the phenomenon under consideration. It is precisely for that reason that Hegel turns to the "new science" of political economy and even refers to its achievements as an "honour for thought"! What Herzog says of Smith, that "the task of science is to uncover the 'hidden chains' behind phenomena and to unite them into a coherent system" (p. 28), Hegel too would have accepted wholeheartedly. Indeed he says the same in his own words (*Philosophy of right*, § 189ff.).

The remaining four chapters address key issues in political philosophy for which Smith and Hegel can be seen as providing lasting inspiration. Chapter 4 "The self in the market" deals with the different and at times conflicting ways in which relations between the individual and society are seen both in recent political philosophy and by Smith and Hegel. Herzog is concerned to correct superficial readings according to which Smith, constructing "economic man" as an "atomistic" self, might be supposed totally opposed to Hegel, who as a devoted follower of Aristotle could hardly conceive an individual severed from the social whole. Reading Smith's economics more appropriately as coming from *The theory of moral sentiments*, with the strong emphasis given there to fellow-feeling and sympathy, Herzog insists that for Smith too, "men *qua* men cannot exist without society" (p. 63). In the wake of John Rawls's *A theory of justice*, this issue has sprung up again in Michael Sandel's criticism of Rawls for depending on an implausibly "unencumbered self" (Sandel 1984). Defenders of Rawls and Sandel have kept the controversy going, with some peacemakers (like Charles Taylor) trying to mediate. Herzog not only shows the significance of Smith and Hegel for this debate, she even renders their rich views on the social "embeddedness" of the self so attractive as to make the recent controversies seem old hat.

Chapter 5 on "Justice in the market" is for me the heart of the book, especially the short but dense section "What about the poor?" (pp. 101-111). Earlier within the same chapter, in a section on "Are market outcomes deserved?", Herzog returns to contemporary debates around Rawls's *A theory of justice*. When markets are seen as a consequence of just institutional structures, they may appear rationally "justified", but this falls short of any stronger sense of "just". Going further than Rawls, markets may also be evaluated in terms of whether their outcomes provide people with "what they deserve". As Herzog points out, for Hegel it would be wrong to ask such a question. Since the market or the "system of needs" is *per se* the realm of radical subjectivity, an expression of modern subjective freedom, arbitrariness and accident can never be banned from it. In this sense, I might add, ultra-liberal thinkers, like Hayek, may be described as following Hegel (pp. 86-89)—though they might not like that idea; nor would many Hegelians have appreciated such a following! Along the same lines, Herzog is right in saying that "contemporary theory has largely followed a Hegelian strategy: it has given up the idea of realizing justice *in* markets, and has concentrated on the institutions that surround it" (p. 115), though contemporary thinkers may not always appreciate, or be aware of, the founding father of this strategy. It needs to be emphasized, however, for this does not emerge clearly enough from Herzog's presentation, that the "system of needs" is not Hegel's last or even sole word on the matter. The realm of egoism that Hegel associates with the market society is balanced by other parts of his system, coming before or after the relevant section in the *Philosophy of right*—and this separates Hegel definitively from the ultra-liberals. But rather than taking this aspect of the question further, let us proceed to the heart of the matter, the question of the poor.

Herzog continues her analysis by arguing that Smith was probably as preoccupied with the poor as Hegel—witness his many keen observations and comments on beggars, on charity, and so on. Herzog's point that the problem of poverty was a starting-point for Smith's thinking on economic matters may be strengthened by recalling the late Istvan Hont's brilliant analysis (1983) of the economic basis of the debates about Scotland's relation to England. But, as Herzog notes, in their theories about poverty there are also differences between Smith and Hegel. *Under a condition* that can neither be overestimated nor overstressed in presenting his views, Smith was convinced that a properly functioning, free market society may contribute to overcoming poverty, in the sense that it will help the poor to provide for themselves (see pp. 104-109). The condition *sine qua non* is economic growth; as Herzog expresses it, “the economic growth of commercial society is a tide that lifts all boats” (p. 103). What Smith thought might happen in the absence of constant growth is less clear, nor does he seem to be particularly worried about the costs that such growth might impose upon society (such as ecological problems) nor anticipate “alternative ways of securing economic prosperity” (p. 35).

Hegel, on the other hand, has a darker or, shall we say, more realistic view of poverty. The biblical aphorism, ‘the poor will always be with us’, never seems far from him. In more strictly economic terms, for Hegel poverty is a structural problem of market society, occurring just when “civil society” is in full swing (*Philosophy of right*, § 243). Hence the market cannot by itself provide a solution to the problem and “civil society is pushed beyond itself” (§ 246). While Hegel may be and has often been accused of failing to provide a perfect solution to the problem of poverty, he does (pace Herzog, p. 110) provide a rather good discussion of possible remedies: the intervention of the public sphere, what he calls the “police”, colonization, the corporations, and so on. Thus, this apparent weakness may dialectically be turned into a strength: Hegel is not trying to impose a dogmatic, ready-made answer that is bound to fail, but pragmatically exploring several ways out. However that may be, Herzog is good at reminding her readers of the brilliant things Hegel has to say about the non-material (in Hegel’s terminology, ‘ethical’) dimensions and consequences of poverty (p. 107ff.).

The study ends with two related chapters, on the market’s relation to freedom and the history of the market. The two topics are particularly related from the Hegelian point of view, for the philosopher gave the well-known definition of world history “as the progress of the consciousness of freedom”. Chapter 6, on freedom and the market, begins with Isaiah Berlin’s distinction between freedom from and freedom to, or between ‘negative’ and ‘positive’ liberty, and arrives at the conclusion that “these different notions of freedom should not be viewed as rivalling concepts”, but rather as “a number of intrinsically related aspects or dimensions of freedom” (p. 15). While she documents a number of differences between Smith and Hegel, Herzog insists upon a fundamental agreement: “Smith and Hegel share the same idea:

where the economic structures prevent the development of the citizen's capacity to act autonomously, the state has to take action" (p. 129). The study does touch upon such larger questions as the transition from the realm of the "objective" to that of "absolute spirit" in Hegel's system—for Hegel it is "clear that there are higher aims in life than pursuing commercial interests" (p. 131)—but such complex questions could have deserved a more detailed treatment. (Indeed several of the concluding chapters might have been developed at book length.) This is even more so in the brief concluding chapter on the market and history. Crucial issues are certainly raised, but it is utterly impossible to treat a difficult question like Hegel's "cunning of reason" satisfactorily in just one paragraph (see p. 152). A full consideration of Hegel's *Lectures on the philosophy of world history* would have been necessary for this.

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Review of Ricardo F. Crespo's *Philosophy of the economy: an Aristotelian approach*, Springer, 2013, 154 pp.; and of *A re-assessment of Aristotle's economic thought*, Routledge, 2014, 145 pp.

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Ricardo Crespo's two short books are welcomed contributions to the small but growing literature on the relationship between Aristotle's work and contemporary economic theory and society. There is considerable overlap between the two books, both having basically the same goal: to demonstrate Aristotle's "contribution to present day economics" (2014, 6). Despite having been published earlier in 2013, Crespo's *Philosophy of the economy* seems to have been written after *A re-assessment of Aristotle's economic thought*. It is also a smoother read, and contains some provocative material in the later chapters not found in the other book, especially what he considers to be Aristotelian approaches to economic model building, business, and human labor. Hence, since the two books are such close substitutes, I recommend *Philosophy of the economy: an Aristotelian approach* as the better and more important book.

Crespo has a PhD in both philosophy and economics, so he is a well-trained and sure guide to the subject. His main contribution—found in both books—is his articulation of what he thinks a properly constructed science of economics should be, based upon Aristotelian lines. By Crespo's interpretation of Aristotle, the term "economics" may denote an action, a capacity, a habit, as well as scientific knowledge associated with the use of the material things required to live a good life. Economics is a practical science, which should explicitly consider various values; hence it is also an essentially moral or evaluative science. Ideally, people ought to only acquire the goods needed to live a virtuous life for human fulfillment. Humans are also by nature political animals, so to live virtuously, people need to live in a *polis*. Hence, virtues are always developed and consolidated within a community. A *polis* is an association of families with the common goal of living the good life. Therefore, economics as a practical science should be subordinated to politics. Moreover, market exchange is natural, but, Crespo argues, the market itself should also be subordinated to the ends of both individuals and the *polis*. Hence, from an Aristotelian perspective, the proper education and development of economists should have a very broad curriculum, including instruction in political science, ethics, other branches of philosophy, cultural anthropology, history, economic history, and the history of economic thought. Crespo also argues for methodological pluralism in the study and practice of economics, including for example, various case studies which aim to develop the necessary practical wisdom and interdisciplinary abilities needed for skilled economists.

Crespo claims that economics deals with general facts, which occur most times in the same way. Although he emphasizes that explanation (as opposed to prediction) is the main aim of economics and other sciences, he also insists that "values must be placed on the table" (2014, 123). Since economics is based upon generalizations which occur most of the time in the same way, its explanations and predictions will necessarily be inexact. So, to summarize, for Crespo, economics should be explicitly normative, concerned with the promotion of personal virtues, and taught as part of a virtue-based education, embedded in ethics and politics. It will help people deliberate with reason, to make good, proper choices to satisfy human needs and to live the good life. Along the way, Crespo—as to be expected—criticizes twentieth century mainstream economics from an Aristotelian perspective. These criticisms include, among other things, being too narrowly focused, overly concerned with mere technique, imperialistic forays into other social sciences with its instrumental maximizing rationality (epitomized by the research program of the late Gary Becker which is denigrated as being economics in an improper sense), its putative dichotomy between facts and values, and its claims to value neutrality.

Although I am largely in agreement with Crespo, there are some parts of Aristotle's corpus which most contemporary philosophers and economists will want to deeply consider before fully embracing his approach. Firstly, I think most modern and post-modern philosophers and economists will have a difficult time accepting Aristotle's epistemological and ontological claims that humans can grasp reality, and that we mortals can acquire absolute knowledge of the true essences and causes which lie behind empirical observation. Crespo approvingly quotes Aristotle from *De Anima* that our "actual knowledge is identical with its objects" (III, 7, 431a: 1; in 2014, 105). So, for Aristotle and Crespo, things exist, they are knowable, and our intellectual intuition can grasp this knowledge and sometimes even become one with them. I think the dubiousness of this position is evident when Crespo discusses economic model building (2013, chapter 6, 67-80). Crespo holds that the building of a model assumes the ability to grasp what is essential through processes that require imagination, intellectual intuition, well-trained practical reason, and essential knowledge about reality. For Crespo "the described model should bring the knower to the real connections involved in a way that allows him to understand them directly" (2013, 71) so that knowledge of these real relations will pass through models to the modeler. Hence, Crespo claims that though Aristotle does not talk about models, models fit with the Aristotelian theory of knowledge.

I think Crespo is overly optimistic about the ability of models to describe and grasp real causes, essences, and reality; a bit of wishful thinking. Moreover, I am not persuaded that model building fits in with Aristotle's theory. If we humans can truly appropriate reality directly, why would there be a need for models at all? More likely, economists use models of reality precisely because they cannot understand reality itself. Reality is much too complicated, complex, and unfathomable for economists to fully comprehend. Hence, economists create models, and manipulate and explore their properties in the hopes of shedding light on economic reality. Yet, the precise relationship between the models and the reality that they purport to illuminate is always problematic. Moreover, economists tend to confuse their little toy models with reality itself (see Morgan 2012, especially the concluding chapter 10, "From the world in the model to the model in the world"). This is a continuing vexing problem for contemporary economists.

Also problematic and worth serious, deep reflection is Crespo's interpretation of Aristotle's concept of chrematistics. For Crespo, there are good and bad types of chrematistics. Good chrematistics is the technique of wealth acquisition that can be positively used to moderately and liberally support the acquisition of goods needed for the good life. The bad type of chrematistics occurs when acquisition goes beyond satisfying human needs, and money is obsessively pursued as an end in itself, due to unlimited appetites and desires. While the good life, the life of virtues, that leads people to fulfillment and flourishing lives depends upon the good type of chrematistics, the bad life is one that pursues unlimited money for its own sake, knows no limits, is wicked and unnatural (2014, 51).

The key question, what causes bad chrematistics, was starkly posed in an exchange between William Kern and Spencer Pack in *History of Political Economy* (1985). Crespo follows Kern in arguing that for Aristotle, the cause is relatively superficial. Our passions tend to dominate our reason, and we need to control these passions for unlimited wants, desires, and greed with reason, backed by good habits and excellent education. Pack argued that the cause is much deeper and more systemic. It is the mode of acquisition itself, the use of money to acquire more money, the final goal of capitalist enterprises, and of what we would now call capitalism itself, which, for Aristotle, necessarily generates the destructive bad side of chrematistic acquisition. On chrematistics itself, the scholar of medieval economic thought Odd Langholm explained that,

> the word is not used consistently […] sometimes it is used broadly to mean acquisition in general, elsewhere it indicates acquisition by trade, and this is the kind which Aristotle condemns. The root of the word is "chrema", thing needed or used; in plural it means goods, property. But chrematistics in its narrow sense is one of the Aristotelian words which have found their ways into modern languages untranslated; it is hard to convey with precision its particular sense of disdain for the slightly unsavory skills of the commercial classes (Langholm 1983, 51).

There is a tradition (that includes Marx) which views Aristotle as providing the basis for a successful critique of capitalist society and of mainstream modern economic theory (see Pack 2010, especially pp. 109-111). For recall that for Aristotle, retail trade, the use of money to acquire more money, knew no limit, was unnatural, and was bad. Even worse, and more unnatural for Aristotle, was the lending out of money for interest, for more money; its goal was also simply to acquire more money and also knew no limit. For Aristotle, even wage labor itself was unnatural and bad (Pack 2010, 15-32). This tradition offers a much more radical, critical reading of Aristotle than is explicitly proffered by Crespo.

By this reading, the use of money to acquire or accumulate more money is for Aristotle a corruption of money's positive state and form which should merely be used to circulate goods, that is, to facilitate the exchange process. However, when the acquisition and accumulation of money, wealth and riches becomes an end in itself, the natural or proper function or excellence of money is corrupted and becomes unnatural. Thus, it is our mode of acquisition, money which is used to acquire more money, which causes people to become ruled by their desires and passions. Capitalism, our socioeconomic system, is ruining our characters.

Yet, notice also what this value-laden discourse does to our own discourse, should we whole-heartedly adopt, or go back to an Aristotelian approach. This reading of Aristotle, suggests that we may want to especially view our business, our corporate leaders, and their hired representatives, lackeys, and spokespeople, as unnatural, corrupt, wicked and morally bad characters obsessed by the desire to acquire more money. Yet, do we really want to go down this road? Characterizing our opponents as unnatural, corrupt, bad people is for me indeed tempting. Yet, I ultimately think this ratcheting up the stakes and heat of our discourses to this level is probably not a good idea; I also think it is where a fully Aristotelian approach tends to lead us.

In discussing the bad type of chrematistics, Crespo writes that "the point is not eliminating capitalism, as Marx claimed, but rather living up to the virtues associated with economic prosperity" (2013, 111). Nonetheless, I think Crespo's own Aristotelian analysis calls for changes that are much more radical than he seems to realize. Crespo writes in *A re-assessment of Aristotle's economic thought* that "Society should not be a market subject to competition, but rather, a community of cooperating human beings" (2014, 71-72). He does not follow up the implications of this, but I think this really is a call for a post-capitalist society. This call becomes more evident in chapters 10, "Capital and entrepreneurship", and 9 "Human labor" in *Philosophy of the economy*. For Crespo, following Aristotle, thinks profits should be a condition, not an end in itself (2013, 131). Firms should contribute to the common good and "a firm's commitment must be to society, not to profit" (2013, 136). Crespo concludes that "firms' operations should take place in the context of their service to the common good of civil society and the business community. Profits and salaries thus remain limited to being conditions of these activities and cannot have maximizing goals, but rather sufficient and limited ones" (2013, 136). Yet, is any of this possible in a competitive, capitalist society? Will not firms in our society that try to follow this be run out of business?

Similar, but even more severe problems occur with his handling of human labor. In Crespo's view, labor should be fulfilling to workers, and contribute to their self-realization. Hence "understanding labor as a personal human action […] bears profound consequences. First, in terms of human action, work should perfect workers" (2013, 124). Again, is this possible under competitive capitalist conditions? This seems rather utopian in our current society. For the most part, capitalists and their managers generally care not a whit about the perfection or moral development of their workers. Nor can they, and remain in business for long. Also, in these chapters there is a tendency for Crespo to think that he is criticizing the science of economics, when he is really criticizing the socioeconomic system itself. So, for example, he concludes that "[…] given the particularly personal nature of labor, current economics, so concerned with work as a factor of production, neglects its most valuable elements almost entirely" (2013, 126). Yet economics, as the study of the economy, seems to be doing a pretty good job reflecting this aspect of reality, since it is the underlying economy itself which does indeed generally consider the worker only as a factor of production. The problem here is not the science of economics; Crespo, I think, is actually calling for the replacement of capitalism, of rule by the market, of competition. Ultimately, I think, even by Crespo's reading, an Aristotelian philosophy of the economy is not a philosophy of a capitalist economy at all. Were it to be really put in practice, it would need, or call for, a post-capitalist economy.

Crespo's work is careful, knowledgeable, scholarly, and thought-provoking. All interested in the complex relationship between Aristotle's work and contemporary economic theory and society should read it and contemplate its implications.

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Review of Jack Russell Weinstein's *Adam Smith's pluralism: rationality, education, and the moral sentiments.* New Haven & London: Yale University Press, 2013, 360 pp.

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Jack Russell Weinstein's new book sets itself two major tasks: to argue that Adam Smith offers "a coherent philosophy of education that permeates his system" (p. 216) and that Smith's thinking is interestingly attuned to the very modern problem of pluralism. The first strand of argument seeks to provide a way of reading Smith that demystifies some of the remaining ambiguity across his oeuvre. The second sees in Smith an anticipation of current debates about cultural diversity and pluralism.

In seeking to apply Smithian ideas in a contemporary setting Weinstein rejects the limiting over-emphasis on contextualism. His approach accepts the importance of getting Smith 'right' through historically informed readings, but denies that this is where the inquiry must cease. As Weinstein himself admits this is a difficult task (p. 9), but it is a potentially profitable approach and one which is proving increasingly attractive. Weinstein aims to examine Smith's potential contribution to contemporary debates on pluralism by offering "the first full-length investigation of Smith's philosophy of education and his theory of rationality" (p. 15).

Weinstein provides an interpretation of Smith that sees his writings as characterised by the desire to provide an expansive account of human rationality. He points out Smith's attempts to distance himself from a dependence on formal logic and stresses Smith's interest in rhetoric and narrative notions of learning and rationality. The early chapters trace Smith's interaction with Mandeville, Shaftesbury, and Hutcheson, suggesting that his dissatisfaction with elements of the thought of each is combined with a facility for absorbing what is of use. There is a particularly interesting comparison of Smithian spectatorship and Shaftesbury's soliloquy (p. 44), which opens the way into the idea of rationality that runs through Weinstein's reading. Both Smith's impartial spectator and Shaftesbury's soliloquy involve "dialogical self-division" that results in the need for "rational adjudication" between competing mental features in a given context (p. 44).

The book has three significant contemporary interlocutors: Alasdair MacIntyre, Michel Foucault, and James Otteson. The account of rationality provided is clearly influenced by MacIntyre; in the closing chapters Foucault is considered in terms of the notion of progress in history and the extent to which he may have failed to grasp Smith's views; Otteson on the other hand appears as a foil in the initial part of the book. A great deal of time is spent establishing that Weinstein is offering an alternative to Otteson's (2002) use of the metaphor of the 'Marketplace' in his account of Smith's theory. The objection seems more than a little manufactured. As Weinstein admits, the two agree on a large range of issues; the disagreement as he sees it is that by using the metaphor of the marketplace Otteson might mislead readers into prioritising market-like interactions in reading *The theory of moral sentiments* [TMS] (p. 50) or into viewing the market as Smith's sole organisational principle (p. 65). But the disagreement seems to be less about their very similar accounts of unintended consequences and more about potential misreading and extensions of the market into other areas (via *homo economicus*). This strikes me as a manufactured disagreement in that the underlying similarities of the two authors are ignored in favour of a rhetorical disagreement. Otteson's account of Smith does not depend on *homo economicus*, nor does it invite any but the most superficial reader to see that as Smith's view. He uses the market as a metaphor for more general spontaneous order accounts—including Smith's argument in TMS—while accepting that the general model has important nuances in its application. Weinstein appears to accept this point (p. 66). But he then goes on to say that the metaphor is misleading because Smith's moral theory does not include 'exchange'. This is a very strange claim from someone who will later dwell at length on how humans react to the judgments of others and learn from the exchange of emotional cues before making an explicit comparison between Smith's views on price and the impartial spectator (pp. 152-153).

More troubling still is Weinstein's argument that for Smith the rules of morality are not spontaneous but rather are progressive and the product of rational inquiry (p. 66). This bodes ill for what follows as it suggests confusion about one of Smith's central points: that there is a distinction between the abstract reasoning of a philosopher, who explains morality and accounts for it, and the everyday individual who experiences and reflects upon moral issues. The idea that morality is subject to *deliberate* refinement through philosophical interaction is at odds with the entire spirit of Smith's mode of inquiry where the philosopher seeks to reveal the already existent chains that link ordinary moral thinking. The source of this strange reading becomes clear when Weinstein introduces his central organisational concept.

Weinstein's exploration of Smith's views on rationality focuses on what he sees as the sophisticated rumination on the nature of rationality in TMS. The aim is to explain how Smith moves away from calculative notions of reasoning to develop an understanding of the human mind that links sentimental motivation with rational reflection through the idea of the impartial spectator. The impartial spectator demonstrates "Smith's commitment to the rational adjudication of multiple motivations of an act" (p. 57). It is "an anthropomorphization of the rational process and incorporates the sentimental foundations into the reasoned analysis" (p. 76).

Weinstein provides an interesting account of Smith as moral psychologist, along the way demonstrating that there is no crude bifurcation of the accounts of rationality between TMS and *The wealth of nations*, but the crux of his reading lies in the view that Smith's account of the operation of sympathy and spectatorship as a form of reflection is helpfully understood as part of more general account of reasoning. Here Weinstein's account proves slightly less convincing. It rests on showing that Smith thought that calculative reasoning and deductive logic were distinct from more sentimental, rhetorical and narrative forms of understanding, and then bringing these back together in an expanded account of rationality. Leaving aside the question of whether the discussion of motivation from sentiment as a prompt to reasoning is as fully fleshed out as it might be in the light of Smith's place in the development of sentimentalist moral psychology, I am still left with the sense that Weinstein is overegging the 'reasoned' nature of the reflection involved in the operation of the impartial spectator. In explaining the detail of the operation of spectatorship Weinstein accurately describes the sophisticated moral thought process attributed to it by Smith. Judgment is certainly brought to bear, information is certainly assessed in an imaginative manner and decisions are reached—but the process as Smith described it is emotional and imaginative rather than calculative. One wonders then what is to be gained by reading the imaginative and emotional reflection of Smithian agents as part of a theory of 'rationality' rather than a more general anatomy of moral psychology that combines rational, sub-rational, and emotional elements. It is not clear what interpretative gains are made by using 'reason' derived from the impartial spectator as grounds for bringing them together.

Moreover, Smith describes a process that is oftentimes near instantaneous, often sub-conscious, and is recognised as a feeling more akin to the direct drive of a passion rather than a deliberative calculation. Weinstein is right to say that Smith did not think that all of our mental processes were akin to rational calculation, and that he had a deep interest in narrative, context, and rhetoric, but Smith was also describing in great detail a process that more often than not occurs instantaneously. Most of what we do, most of the time, is not the result of conscious reflection: it is the result of sub-conscious assessment or the following of socialised or habitual rules. To incorporate these into 'rationality' seems less than helpful if we are seeking to understand or anatomise the various elements of moral psychology. In other words Smith might have had good analytical reasons not to include these elements of moral experience under the heading of rationality.

The main thrust of Weinstein's view here is that there is a "rationality implicit in the moral sentiments" (p. 109), which in turn leads to the view that: "Sympathy is a rational process; it is not a form of intuition" (p. 111). So the heart of the rationality reading of Smith's moral psychology lies in the fact that "A person's self-awareness derives from the socially constructed rational self-reflection informed by the judgments of others" (p. 70), or "the conscious use of rationality to adjudicate between competing positions" (p. 109) concerning the moral sentiments. But applying a reading of the moral sentiments and sympathy as deliberative elides a whole range of other possibilities. Part of the problem here is the division between the intuitive and the deliberative that frames the discussion.

Weinstein admits that socialisation and learning from social norms and the judgments of others is a key part of Smith's account, but the intuitive/deliberative division leads him to ignore the central role of a third vital category in Smith's account of moral psychology: sub-rational learning, habit formation, and emotional—indeed aesthetic—decision-making (p. 112). Weinstein, for all of his attention to socialisation, views this apart from the account of deliberation, and only offers a much attenuated discussion of habit at the end of the section in question (p. 127).

When Weinstein returns to the theme of the sub-conscious he rightly notes that intuition and deliberation are not either/or for Smith—but one might also want to argue that nor are they exhaustive of mental experience. Weinstein accepts that "what appears as intuition may very well be the product of long-standing deliberation, education, and acculturation" (p. 158), but he then insists that the fact that the impartial spectator is invoked in adjudicating between competing sources of motivation and information means that this aspect of Smith's account, where 'reasoned' reflection takes place, is the central element of moral experience. As Weinstein himself notes (p. 162), Smith accepted that most 'reasoned' justification is post-hoc—so moral agents do not in most cases engage in the sort of moral reflection about their actions that Weinstein sees as part of rationality, until afterwards when we try to understand our actions. The impartial spectator does indeed 'speak' to us, but it is only on reflection that we come to see what he has based his advice upon.

In a similar vein Weinstein makes a clear connection between Smith's account of language and his 'reasoned' approach to moral psychology (pp. 176-177), but in so doing he pays insufficient attention to the fact that according to Smith we acquire language in a non-deliberative fashion and our day to day use of it in communication is habitual and 'un-thinking' rather than deliberative and analytical. People express themselves through the learned habits of their language; they do not reflect on these as they speak. Weinstein is right that Smith is interested in illustration and rhetoric and less concerned with formal logic, and he does make interesting points about the relationship between the sentiments and reason and about Smith's use of system. But his focus on bending all of Smith's account of moral experience into an overarching account of rationality leaves him stressing an overly deliberative interpretation of the impartial spectator's place in the totality of moral experience. The impartial spectator is but one part of Smith's account. It is a very interesting and innovative part, but privileging it to the extent that we see here provides us with a somewhat skewed reading of TMS.

Bringing this approach to the theme of pluralism leads Weinstein to make two interesting claims about the impartial spectator. The first is that: "Smith's goal is for the moral agent to become as impartial as possible while still understanding that it is not immoral to prefer oneself to others [...]" (p. 141). It is certainly true that Smith describes the operation of a psychological process whereby we develop an impartiality check that ensures our actions match the standards of propriety, but the impartial spectator—as the quotation above notes—is more than willing to approve of self-regarding behaviour that passes the test of what an impartial spectator would regard as appropriate. Smith is providing us with a *descriptive* account of conscience. Whether it makes sense to then say that Smith advocates increased impartiality or increased attention to the voice of an already impartial feature of our psychology is left open in the discussion that follows, as Weinstein moves on to the idea of educating our impartial spectators.

The second interesting claim is that Smith's approach leaves him uniquely placed to provide an account that addresses the modern pre-occupation with the problems of social pluralism arising from ethnically and culturally diverse populations co-existing within the same political system. Weinstein rightly observes that Smith accepts difference as a "fact" (p. 25), and that important aspects of his systematic thinking, such as stadialism, represent attempts to understand diversity (p. 29). The discussion of Foucault in the penultimate chapter trades on the differences between Smith and Foucault on the extent of universals in human nature and human knowledge, or, to be more accurate, on the extent of our possible knowledge of universals. But Weinstein's account here seems over optimistic in its view of Smith's attitude to diversity. Certainly Smith accepted diversity, but far from seeing it as valuable for moral education to the extent that it aids sympathy by extending experience (p. 83), it seems rather to be something that he sought to 'deal' with as best as he was able.

Overplaying Smith's acceptance of diversity into a celebration of it fails to account for the role of socialisation within a group which is a key stage in Smith's account of moral 'education' (the very point raised in Weinstein's chapter about socialisation as education for social unity). Smith clearly struggled with the fact of the diversity of moral practices, perhaps most famously in the infanticide discussion at the end of Part 5, chapter II, of TMS, but the account offered here of a more rationalistic Smith committed to the growth of knowledge through the extension of moral judgment among diverse people seems to read too much of a twenty-first century sensibility back onto Smith. Compared to the account of similar aspects of Smith offered by Fonna Forman-Barzilai (2010) the account here seems overly enthusiastic in reading as a strength what Smith more probably saw as a problem for his approach.

It is true, as Weinstein argues, that Smith saw competition between religious sects as useful in preventing domination. But it seems too much to say that this shows Smith's desire to institutionalise and support diversity (p. 23), rather than him noticing a useful unintended consequence of the fact of a diversity of religious beliefs. These sects are still dangerous in Smith's view and they may still hate each other, but they have come to a *modus vivendi*. There is no moralised "fusion of horizons" or recognition by sect members of the value of other sects. It is only as the philosopher with an overview that we can see the unintended benefits of religious competition. Smith seems to believe in a universal ethic, but provides a theory that sees moral belief as generated in context. There is no easy way of squaring that conflict by a progressive and rationalistic reading of the impartial spectator.

In the second part of the book Weinstein moves on to consider improvements in rational judgment. Here the first couple of chapters focus on education. Again the same strategy is at play. Weinstein wants to use an over-arching notion of education to capture all of what Smith has to say about socialisation, formal teaching, and the improvement of humans' decision-making capacity. Once again the detail here is clearly depicted and for the most part convincing. The worry lies with the more general aim of conflating all forms of human 'learning' under one heading. Weinstein acknowledges the distinctions between socialisation, informal knowledge and education while seeking to bring them together under a broad organisational principle. Formal education and socialisation are forms of knowledge acquisition vital for Smith, but they are not the same, and so it is not clear that it makes sense to place them under the same heading. Moreover, as with 'rationality' it is not clear that 'education' proves a useful analytic to get at the heart of what Smith thought about such matters.

One is left with a series of interesting discussions of aspects of Smith's view of psychology and learning, but the over-capacious conceptions of reason and education, while revealing in some respects, obscure important aspects of Smith's analysis—most obviously his deep interest in sub-rational, emotional, and habitual processes. Moreover, one gets the sense that Weinstein's initial desire to show that Smith could be of use in dealing with contemporary issues of pluralism seems to be driving the use of the over-arching conceptions of rationality and education, leading to a reading which seems in places strained as it seeks to make Smith relevant in a social and political debate whose terms he could not have foreseen.

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Review of Cyril Hédoin's *L'institutionnalisme historique et la relation entre théorie et histoire en économie* [*Historical institutionalism and the relation between theory and history in economics*]. Paris: Classiques Garnier, 2013, 435 pp.

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This book originates from Cyril Hédoin's doctoral work on institutionalism in economic thought. It supplies extensive knowledge on the founding figures of institutional economics (Gustav von Schmoller, Max Weber, Thorstein Veblen, John R. Commons, and Karl Polanyi), which makes it a potential textbook on the subject, and it provides a detailed analysis of what defines an "authentically institutionalist approach" to economics (p. 9). Because economists can say "*we're all Institutionalists now*" (p. 7, emphasis in the original), the author reflects on the identity of what he and other French economists call *historical institutionalism*—a research programme that is distinct from (and critical of) mainstream economics and its method of analysing institutions. The adjective 'historical' not only refers to an approach that emphasizes the history of economic thought, but also suggests the specific criterion of demarcation used by Hédoin to identify the essence of this heterodox institutionalism. This approach recognizes the importance of the historicity of social phenomena and of social knowledge, and therefore investigates the relation between *theory* and *history*. This book also contributes to the history of ideas, the philosophy of economics, and the economics of institutions.

By using two categories of Imre Lakatos's methodology of science, Hédoin proposes a rational reconstruction of the thought of Schmoller, Weber, Veblen, Commons, and Polanyi in order to delineate the hard core of the research programme of historical institutionalism. The choice to concentrate on these five authors (thus avoiding the reduction of historical institutionalism to American institutionalism) is based on their importance for this research programme: they are representative of its identity and offer significant insights that relate theory and history which, for the main part, are convergent and often complementary. Hédoin interprets these authors by analysing the logical connection between theory and history: specifically, this refers to the epistemological issue of *historicization of theory* (i.e., the method of social knowledge) and the substantive issue of *theorization of history* (i.e., the explanation of historical dynamics). The book is structured according to these mirror issues in order to analyse the primary methodological and theoretical principles that characterize historical institutionalism.

Part one of the book develops what Hédoin identifies as the three principles underlying the historicization of theory: (1) consideration and treatment of the problem of historical specificity (this addresses the tension between the general and the particular); (2) adoption of methodological institutionalism (this addresses the tension between action and structure); and (3) appeal to abduction and ideal types as methods of knowledge (this addresses the tension between concept and reality). Hédoin begins with an exploration of the philosophical foundations of these principles which have their basis, according to him, in German neo-Kantian philosophy (Wilhelm Dilthey, Heinrich Rickert) and American pragmatist philosophy (Charles Sanders Peirce, John Dewey). Although there are differences between these philosophical traditions, they share a common emphasis on the historicity of science, which opposes the positivist epistemology that has dominated economics. Neo-Kantianism is, Hédoin states, the first philosophy to deal with the relation between theory and history; it addresses the specificity of the cultural sciences and aims to establish their scientific legitimacy. Pragmatism makes a "decisive contribution" (p. 63) by articulating connections between its theory of knowledge and its theory of action; these connections underlie all three principles for historicizing theory, especially in the social sciences. These philosophical positions legitimize the historical institutionalist research programme whose main characteristics are to recognize the historical specificity of social phenomena (that is, the uniqueness of historical events), and, consequently, to stress that social sciences are not nomological sciences, that they rather produce theories that are contextual means of understanding a global historical process.

Part one of the book continues with a study of how the German historical school (under Schmoller and Weber) and the American institutional school (under Veblen, Commons and, by extension, Polanyi) have developed these principles of the social sciences. Each author is scrutinized in order to identify his contributions and his weaknesses concerning the historicization of theory (Commons is regarded as having originally married the two philosophical foundations of institutionalism). In so doing, Hédoin demonstrates both the differences and commonalities among these authors. He argues that their epistemological contributions converge in a historical conception of knowledge and theory, which deals with the tension between the general and the particular. This conception identifies the scientific character of the social sciences by taking into account their specificities. For the authors examined, these specificities pertain to the absence of natural laws and to the role of values in studying social facts and human phenomena.

Part two of the book develops what Hédoin identifies as the three principles underlying the theorization of history: (1) a substantive conception of the subject-matter of economics; (2) an evolutionary approach to institutions; (3) and the concept of capitalism as a historical system specific to Western economies. This part begins with a critical evaluation of how Schmoller, Weber, Commons, and Polanyi analyse the substantive dimension of economics. Borrowing from Polanyi (pp. 174-178), Hédoin defines substantive economics in opposition to the formal meaning of 'economic' in terms of maximizing rationality. In contrast, the substantive economy is "the set of institutions aiming at allowing individuals to fulfil their needs" (p. 19). The study of the economy in this sense requires the continuous articulation of actions and institutions (methodological institutionalism).

For Hédoin, Schmoller developed the first systematic study of institutions (though his contribution is often overlooked); but it is Commons' theory that completely satisfies the principle of substantive economics. Additionally, the contributions of Weber and Polanyi are very important, but the author indicates that their variants of methodological institutionalism are incomplete: Weber considers institutions to be essentially constraints on actions, whereas Polanyi fails to develop a theory of action—this, in part, explains the ambiguity surrounding his notion of "embeddedness" as discussed by Hédoin (pp. 255-256). Furthermore, their analyses (especially Polanyi's) are not evolutionary. It is for this reason that the author focuses on the contributions of Veblen and Commons when investigating the second principle of the theorization of history—this pertains to the evolutionary approach to institutions. Building on the work of Geoffrey Hodgson, Hédoin analyses the ontological and methodological lessons derived from the Darwinian revolution, before examining the contributions of Veblen and Commons. He maintains that historical institutionalism should combine Veblen's analysis in terms of natural selection and Commons's analysis in terms of artificial selection. Hédoin argues that these two types of evolutionary processes refer to two dimensions and two timescales of social evolution, and that natural selection includes cultural selection from a Darwinian ontological point of view.

Part two of the book concludes with a discussion of the essential object of study for all the authors considered: capitalism as a contingent historical system produced by a non-teleological evolutionary process. According to Hédoin, these authors develop a theorization of history in order to understand the process of emergence of capitalism, and each author focuses on different aspects of this process. Hédoin brings together Veblen and Weber because they both (although differently) analyse this process as part of a more general process of institutional rationalization of the Western world. He also draws connections between Commons and Polanyi because they both stress the social construction of the institutional foundations of capitalism, which is characterized by the generalization of market processes. Through specific systems of concepts, their contributions converge, argues Hédoin, in the understanding of the historical dynamics of Western capitalism as an empowerment of the economic order that brings tensions in the human world.

This book offers a very clear, comprehensive and stimulating analysis of the theories of the founding figures of historical institutionalism. By comparison with the existing literature on heterodox institutionalism, it has two specific merits. The first is that it integrates both the German and the American foundations of historical institutionalism; it builds many dialogues and relevant links between the authors examined without neglecting their differences and weaknesses. The second merit lies in its detailed analysis of the relation between theory and history. Indeed, the choice to focus on this relation to delineate this research programme directs one's attention to its main characteristics: its epistemological and methodological claims about the process of theorization in the social sciences, and further, its substantive and theoretical claims about the analysis of the evolution of our modern economic system. However, I want to highlight two limitations of this erudite book.

First, because of its thorough presentation of the work of five 'big authors', the book has little room to question the present status and stake of this research programme. The concluding chapter broaches the discussion of this subject by listing some research in economics that can be included in this tradition (e.g., French theories of regulation and conventions, and the work of Masahiko Aoki and Douglass C. North). Given the ongoing transformations of mainstream economics, Hédoin endorses an avenue of research that consists in combining historical institutional economics with modern techniques of modelling; this is such that the combination does not reduce the preoccupation for historical specificity and all its methodological implications. I agree with Hédoin that the (relative) decline of positivism in economics, along with the resurgence of pragmatism in philosophy, create opportunities for development in historical institutionalism today. I also agree that this development could definitively break from the now obsolete *Methodenstreit*. But I maintain—in accordance with an authentic institutionalist approach—that the conditions for a balanced mix between historicization and formalization must be properly identified. Crucially, it must be stated that historical institutional economics encompasses formalized institutional economics given it deals with social complexity in historical dynamics. Furthermore, the issue of the type of formalization consistent with methodological institutionalism and its evolutionary content must be clarified. Finally, this discussion cannot escape inquiring what should be rightly considered 'empirical' in economics—this is an issue that has always been a point of divergence between historical institutionalism and mainstream economics.

The second issue, in connection to this last remark, is not so much a criticism than it is an extension of the author's study of historical institutionalism. Through Hédoin's analysis of the methodological positions of Weber and Commons, one can sense a tension between the (epistemological) nominalism of the neo-Kantian tradition and the (ontological) realism of the pragmatist tradition. If both traditions state that concepts are not a 'copy of reality', pragmatism defends a mediated link between concepts and reality and is therefore pressed with the issue of the validity of theories in reality—that is, in social life and ordinary experience. Here the connection between knowledge and action—science and praxis—becomes essential and distinctive for historical institutionalism. Dewey and Commons have best shown that social theories and philosophies have to be tested in action, that is, according to their empirical consequences; this is especially so for implementations of public policy and institutional design which are regarded as experimentations for the social sciences. From this perspective, history is crucial not only as 'past' (i.e., as it concerns the study of the specific historical path for understanding the present state of a system) but also as 'future' (i.e., as it concerns experimentation of specific institutional policies to test the changes deemed necessary). Thus, when one takes into account the relation between theory and reality, the normative and practical consequences of theories become a critical part of the research programme of historical institutionalism. On this point, although all the authors here considered relate economics, politics and ethics, there remain significant differences between them. On the one side, Schmoller, Commons, and Polanyi explicitly applied economic analysis to social reform and were engaged in issues concerning democracy and social control over the economy. On the other side, Weber and Veblen stressed the issue of objectivity in the social sciences and were somehow reluctant to apply science to policy. Surely, an adequate discussion of the positions of the authors with respect to this issue would have been lengthy—that might be the reason why Hédoin left it out. But this issue of the relations between positive and normative judgments, and between theory and practice, is highly relevant when the goal is to characterize historical institutionalism within economics. In any case, it sheds light on the institutionalist conception of the specificities and the stakes of the historical sciences.

In this book, Cyril Hédoin investigates the founding fathers of institutionalism with the aim of analysing the specific principles that distinguish this research programme. The originality of this book lies in its attempt to identify a single criterion of demarcation that could be both epistemological and substantive: the institutionalist's attention to the relation between theory and history. One might argue that this criterion is not the only marker of the identity of heterodox institutionalism; but surely it is the most structuring and the most consensual one. Hédoin's book demonstrates this point very convincingly.

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PHD THESIS SUMMARY: The J-PAL's experimental approach in development economics: an epistemological turn?

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The rise of experimental economics has changed the research agenda of economic science. Today economics is undergoing an empirical turn, which entails an epistemological change in economics. The fact that contemporary economists consider empirical tools as "more reliable" than theoretical ones reflects this turn. Based on the observation that empirical works tend to take over most of the research activity of the discipline, authors like Joshua D. Angrist and Jon-Steffen Pischke have described this tendency as an "empirical revolution". This revolution privileges questions that can be answered using an experimental approach, while relegating other questions to a secondary place. The rise of randomization in development economics offers the perfect illustration of this tendency. Abhijit Banerjee and Esther Duflo institutionalized the use of randomized experiments in development economics. Together with the Massachusetts Institute of Technology (MIT), they created in 2003 the 'Abdul Latif Jameel Poverty Action Lab' (J-PAL) with the aim of conducting experimental work that would give scientific insight to our understanding of poverty. These kinds of experiments, that randomly assign subjects to two groups, remove many statistical biases and produce results with a strong internal validity, which has led some economists to consider such methodology as a "gold standard" for empirical research.

The aim of my doctoral dissertation is to conduct an epistemological analysis of the J-PAL's approach within development economics from two dimensions: methodological and theoretical. The methodological dimension examines the randomization method promoted by J-PAL's researchers; two main interrogations guide this analysis: (1) the "gold-standard" character of randomization, and (2) the possible transposition of J-PAL's results in the political sphere. The theoretical dimension of the thesis investigates J-PAL's contributions to the theoretical debates of development economics during this last decade. Focusing on these two dimensions allows me examine the J-PAL's approach as a whole and establish the extent to which it has led to "a turn" in economics. Thus, my thesis shows that the J-PAL's randomized experiments do not help producing precise (clear) policy recommendations aiming at the eradication of poverty. In fact, the focus on the internal validity of the experiments jeopardizes their external validity. Hence, I show that the two J-PAL's objectives, to produce evidence and guide decision makers, are antagonistic.

The first part of my thesis seeks to define the method of randomization by focusing on one specific aspect: that of internal validity. For that reason, I redraw the history of randomization. I show that Charles Sanders Peirce first used this method in para-psychology to thwart Fechner's law. This method was widely used, after Peirce's work, to test the existence of telepathy. The statistician Ronald Fisher was the first to define precisely the experimental protocol of randomization. At first, Fisher designed this protocol for agriculture, but the method turned out to be most successful in medicine through clinical trials. The J-PAL's randomization borrows from medicine its experimental design. Furthermore, the J-PAL borrows another key dimension from another discipline, political science, where experiments are used to evaluate large-scale public policies. It is this dimension (policy evaluation) that the J-PAL borrows from political science. These two disciplinary borrowings define the J-PAL's approach and its objectives: (1) producing evidence through well-defined experimental design in order to (2) assess development policies. This first part of my thesis expresses the twofold J-PAL's objectives through the history of randomization and around the notion of internal validity.

The second part of my thesis further analyzes the method of randomization, but focuses on the notion of external validity, which turns out to be weak with respect to the randomization method. I show that there is an important tension between internal and external validity within J-PAL's randomization. This tension makes it necessary to make a trade-off between both kinds of validity. The J-PAL, however, seems to refuse this methodological trade-off. In order to make that explicit, I focus on criticisms from development and experimental economists, as well as on Nancy Cartwright's analysis. I seek to unify these criticisms by emphasizing one of them: what I term the a-theoretical dimension of the J-PAL's approach. In order to guarantee the reliability of its results, this approach refuses all upstream theories, but aims to define a "new development economic theory" based on its reliable results. Hence, I distinguish two theory levels: the *ex-ante theory refusal* and the *wish of ex-post theory building*. I show that the ex-ante theory refusal makes building of an ex-post theory difficult, weakening the external validity of randomization. Consequently, this prevents the approach to provide clear (precise) policy recommendations, thus weakening one of the J-PAL's objectives.

In the last part of my thesis, I seek to question the J-PAL's theoretical contributions to development economics. From that perspective, I focus on one specific debate to which the J-PAL aims to contribute: the development aid debate. This debate is characterized by two main positions: the advocates of massive international aid to fight poverty and their detractors. The J-PAL seeks to offer an alternative, through the results of its experiments. I analyze one of the main themes of this debate: the bed nets heavies in the fight against malaria. I redraw all the experiments that the J-PAL has implemented in order to evaluate the effectiveness of such heavies. I show that these experiments highlight a puzzle: even if the bed nets are completely free, they are not sufficiently use in order to eradicate malaria. The further experiments do not seek to understand this puzzle, but aim to test nudging devices in order to increase the bed nets used in poor countries. Recently, Esther Duflo (one of the J-PAL's leaders) appealed to a strong paternalism to fight poverty. This proposition is in total contradiction with the initial J-PAL's position of evidence-based policy recommendations. Duflo based her paternalism on the notion of freedom defined by Amartya Sen within the capabilities approach, intending to improve the freedom of poor people. I question the philosophical foundations of this paternalism and show that it has two main problems. Firstly, from a Senian perspective, paternalism cannot be a tool for more freedom; since freedom is both instrumental and substantial according to Sen. Secondly, Duflo suggests removing some of the poor's choices in order to improve their capabilities. I show that, actually, Duflo confuses the notion of functioning and capability in Sen's approach (capability is a process while functioning is a fixed element). I explain these two confusions through the idea that the J-PAL's experimental approach cannot properly account for the processes of development or poverty. And it cannot do so, because of the strong focus on internal validity and the avoidance of ex ante theories suggested by its proponents. Furthermore, this penalizes its external validity and impends the providing of clear (precise) policy recommendations from experimental results. Hence, Duflo is compelled to invoke a policy recommendation independent from her method and her results. Thus, the J-PAL's experimental approach definitely produces a new way to apprehend poverty in development economics by looking to for concreteness; therefore J-PAL's results offer a very precise picture of the life of the poor. From that perspective, J-PAL's experiments surely constitute an "empirical revolution". However, it remains an open question whether they contribute to any more substantial revolution in the fight against poverty.

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PHD THESIS SUMMARY: Learning from ignorance: agnotology's challenge to philosophy of science

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PhD in history and philosophy of science, May 2014
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The recent commercialization and privatization of scientific research has reconfigured the organization of science worldwide, fostering new scientific practices and new political tools to manage scientific research. Focusing on the mechanisms of ignorance production, the recent literature in *agnotology* has been a fruitful approach for understanding the social and epistemological consequences that emerge in commercialized science today. Strictly speaking, agnotology is the study of ignorance broadly conceived. Agnotology's innovative contribution to the studies of science stems from its treatment of ignorance as a social construction, one that differs from the traditional conception of ignorance as a natural vacuum (Proctor 2008). Agnotology has uncovered different ways in which the commercialization of scientific research has encouraged the production of ignorance, thus challenging the epistemic adequacy of the current social organization of science. Consequently, agnotology has made evident the need for a well-articulated normative approach capable of identifying and evaluating the epistemic concerns raised by the private funding and performance of science. Although philosophers of science have dealt with some of the social aspects of scientific knowledge production, they have yet to articulate an appropriate social epistemology that addresses these pressing issues. In my dissertation I take up this task.

The aim of my dissertation is twofold. First, I examine the epistemic and social problems emerging from cases of ignorance production to argue that agnotology poses a serious challenge for philosophy of science. Second, I draw a path for philosophers of science to address this challenge.

The dissertation is divided in five chapters. The introductory chapter describes some of the main challenges that philosophy of science has encountered in the past half century, i.e., the historical challenge posed by scholars such as Thomas Kuhn and Paul Feyerabend, and the constructivist challenge posed by the Strong Program in the sociology of scientific knowledge and the social studies of science more generally, and then introduces agnotology as a new terrain, posing important questions for the philosopher of science. In the second chapter, I present the works of Philip Kitcher (2011) and Helen Longino (2002) as representative of a philosophy of science concerned with the social dimensions of scientific knowledge. I also give a historical account of the major changes that the organization of science has undergone in the past three decades with the move towards the commercialization and privatization of research. I then argue that Kitcher's and Longino's accounts of scientific knowledge have important limitations when evaluating the process of knowledge production in the commercialized framework in which scientific research develops today.

The third chapter examines the recent literature on agnotology, focusing on four cases: the tobacco industry's support of cancer research (Proctor 2011), the ongoing debate over global warming (Oreskes and Conway 2010), the pharmaceutical industry's design of clinical trials (Michaels 2008; Nik-Khah 2014), and economists' assessment of the financial crisis of 2008 (Mirowski 2013). I argue that scholars working on agnotology seem to hold implicit normative commitments that are in tension with their descriptive accounts of ignorance-constructive practices. Accordingly, and despite uncovering the limitations of the current organization of science, agnotology does not provide an appropriate normative account of the current production of scientific knowledge either. Further exploration into the normative aspects of agnotology is still needed.

In order to start addressing the challenge of agnotology, I build upon the contributions of philosophers of science to the science and values debate. Thus, the fourth chapter presents the science and values framework, describing the lines of argument that philosophers of science have used to understand the role of social and political values in scientific inquiry (e.g., Douglas 2009), as well as some of the main approaches in feminist philosophy of science that have used such a framework to understand the role of sexist and androcentric values in scientific research (e.g., Anderson 2004; Kourany 2010). I then argue that feminist philosophers of science faced challenges that are similar to agnotology's challenge, making feminist philosophy of science a particularly promising approach for our purposes.

In the fifth chapter, I analyze the challenge of agnotology in terms of the conceptual tools that the values approach have contributed to the discipline, emphasizing the importance of identifying the political values behind the current organization of science, as well as the resources available to the philosopher for theorizing the influence of such values in the production of scientific knowledge. My aim is to provide a sketch of a normative account capable of evaluating the ignorance-constructive practices previously identified, without dismissing the empirical facts regarding the organization of scientific research today. Accordingly, I argue for a naturalized social epistemology that endorses a contextualist view of scientific knowledge, understands the bi-directional influence of facts and values, and is explicit about its value commitments. This preliminary sketch opens the door for a broader philosophical project, the project of a politically informed philosophy of science. In the closing remarks, I present future directions in which this research should be further developed.

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PHD THESIS SUMMARY: Governing by carrot and stick: a genealogy of the incentive

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Managers, politicians, and scientists frequently use the term 'incentive' in their explanations of human action. At the same time, individuals in the public and private sectors are now governed with the help of incentives: a bonus is an incentive for the banker to perform in an optimal way; the introduction of market forces in healthcare is an incentive for healthcare providers to use tax payers' money efficiently; the public availability of information about school performance is an incentive for school administrators and teachers to work hard. In this dissertation, I study the incentive from a theoretical and normative perspective inspired by the work of the French philosopher and historian Michel Foucault. To challenge the current self-evidence of the incentive as an explanatory term and instrument of power, I focus on the contingency that permeates the transformations in nineteenth- and twentieth-century thoughts about and uses of the carrot and the stick.

The relationship between knowledge and power is a key theme in Foucault's work. Sometimes, power is a brute phenomenon, but more often, it is cloaked in discourses that try to rationalize its exercise and justify its existence. At the end of the 1970s, Foucault (2008; 2009) began to investigate the history of these rationalizations of government in detail. In particular, he focused on two interrelated aspects of different 'governmentalities' that were developed by Western thinkers from the Middle Ages to the twentieth century. First, he studied the way the objects and objectives of political action were demarcated by different groups of (scientific) experts (see Dix 2014a). Second, he studied the development of techniques with which the behaviour of individuals and groups could be steered in a different direction. Similarly, in this thesis, I study three successive attempts to demarcate the 'incentivizable subject' as an object of knowledge and to design the techniques of power with which that subject could be governed.

American engineers were the first professional authority in matters related to the implementation of incentives. They held a unique position in the late nineteenth century because they worked closely with the workers and foremen and, at the same time, had access to the higher echelons of management. For the engineers, the incentivization of employees was synonymous with the introduction of a variant of piece wages. Their proposals for industrial government became more refined as their understandings of wage incentives progressed. Frederick Taylor's principles of scientific management and Henry Gantt's system of charting each worker's performance are exemplary of this development.

From the 1920s onward, the authority of the engineers was challenged by social scientists from a variety of disciplines. British and American economists criticized their one-sided focus on the material motives of workers. That criticism was not lost on a group of management scientists who, in the 1930s, moved industrial research in a new direction. With backgrounds in psychology, sociology and anthropology, these management scientists developed different explanations for employee behaviour and developed a set of alternative techniques to bring the individual in harmony with him- or herself, with the working group and with managers and foremen. The proper mental and social adjustment to factory conditions came to be seen as the major incentive for people to apply themselves.

It took until the 1970s for a new rationalization of governing with the help of incentives to come into being. This time, mathematically trained economists broadened the theoretical debate on socialism versus capitalism as rival allocation mechanisms, to include a number of problems that were faced by all who governed. In their models, these economists forged a link between central economic planning and the information that was available to economic actors on the local level. The optimal allocation of goods and services, therefore, required that economic actors reported their private information to the planner honestly. But what if they were not inclined to do so? Indeed, if individuals were acting out of self-interest, it would be rational for them to withhold information from the planner. To minimize such instances of information asymmetry, the planner would have to give each individual an incentive to speak truthfully.

The relationship between information and incentives was developed further in the economic theory of principal and agent. The principal is someone who can only achieve his or her goals if a set of agents either honestly provides the necessary information or adequately performs certain actions. According to economists, the world is inhabited by principals and agents; thereby, the idea took hold that governing with carrot and stick was not a local matter—as the engineers still thought—but that the information-incentive nexus could be located in a wide range of relationships between governors and governed, in both the public and the private sectors. A study of the introduction of performance pay in Dutch primary and secondary education shows that principal-agent theory played a vital role in the articulation of alleged problems in the educational system and in the proposal of suitable solutions (Dix 2014b).

In the concluding chapter I analyze a number of recurrent themes in the three incentive-infused governmentalities. First, each rationalization of government comes with a specific delimitation of the incentivizable subject and a particular role for the governor. Second, I show how a new modality of power can be extracted from my genealogy of incentivization. For despite the contingent historical shifts, the incentive is also a novel and quite coherent device that contrasts sharply with discipline as a rival technique of power (see Foucault 1995). Finally, I focus on the things that are taken for granted in the nineteenth- and twentieth-century views on and uses of the carrot and the stick as twin elements in a comprehensive program for wielding power over people.

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