ERASMUS JOURNAL FOR PHILOSOPHY AND ECONOMICS
VOLUME 4, ISSUE 1, SPRING 2011

The Erasmus Journal for Philosophy and Economics (EJPE) is a peer-reviewed bi-annual academic journal supported by the Erasmus Institute for Philosophy and Economics, Faculty of Philosophy, Erasmus University Rotterdam. EJPE publishes research on methodology of economics, history of economic thought, ethics and economics, and the conceptual analysis of inter-disciplinary work relating economics to other fields. EJPE is an open-access journal. For additional information, see our website: <http://ejpe.org>. All submissions should be sent via e-mail to: <editors@ejpe.org>

ACKNOWLEDGMENTS

EJPE WOULD LIKE TO THANK ALL THOSE WHO HAVE ASSISTED IN PREPARING THE PRESENT ISSUE:
Christian Arnsperger, Roger Backhouse, Tom Boylan, John B. Davis, Rogier De Langhe, Till Düppe, Andrea Finkelstein, D. Wade Hands, Clemens Hirsch, Duncan Hodge, Alessandro Lanteri, Aki Lehtinen, Caterina Marchionni, Julian Reiss, Ingrid Robeyns, Menno Rol, Eric Schliesser, Johanna Thoma, James Ullmer, Alex Voorhoeve, Jack Vromen.

http://ejpe.org/pdf/4-1-2011.pdf ISSN: 1876-9098

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VOLUME 4, ISSUE 1, SPRING 2011

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Erasmus Journal for Philosophy and Economics,
Volume 4, Issue 1,
Spring 2011, pp. 1-22.
http://ejpe.org/pdf/4-1-art-1.pdf

Strength and riches: Nicholas Barbon's new politics of commerce

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Abstract: Nicholas Barbon's *A discourse of trade* presents, in its construction, substance, and rhetoric, an early outline of a new science of the legislator for the new politics of commerce. Barbon drew together economic and political arguments, applying insights from the latter to a new understanding of the political potential of the former. His accounts of the aspect of infinity in economic growth, his attack on analogical theorizing, and his endorsement of prodigality all served a larger political purpose. While he is primarily remembered for these individual economic contributions, it is the larger project, the envisioning of a new politics of commerce and commercial empires that marks out his *A discourse of trade* as groundbreaking. Almost a century before Adam Smith's famous definition of economics as a branch in the larger science of the legislator, Barbon offered an early account of the vital connection between economic thought, political philosophy, and statecraft.

Keywords: Nicholas Barbon, Machiavelli, balance of trade, commercial empire

JEL Classification: A11, B11, B31

Nicholas Barbon (ca. 1640-1699) had good reason to avoid politics. As the son of Praise-God Barbon (1596-1679) the long-imprisoned namesake of the 'Barebones' parliament, Nicholas keenly understood the personal perils of public life. Living through the Wars of the Three Kingdoms, the Restoration, the Succession Crisis and ultimately the Glorious Revolution, Barbon experienced political life as a series of challenges, obstacles and opportunities to cope with, accommodate, and adjust to. Barbon managed by turning to trade. Trained at Leyden as a physician, he quickly adjusted his ambitions, turning from medicine to trade upon returning to England. He made his fortune first as a builder

AUTHOR'S NOTE: I am grateful for comments, insights, and suggestions on earlier versions of this text provided by Kelly L. Walker, Michael Bird, and the two anonymous referees.

in the aftermath of the Great Fire of London. Later, Barbon organized England's first fire insurance plan. Indeed, it is perhaps because Barbon was so intellectually, professionally, and personally adept at coping with challenge and change that his contributions to political and economic thought stand diminished today. His ideas concerning the relationship between commerce and politics were never fully detached from his own attempts to cope with the constant flux of seventeenth century English politics.

Admittedly, Barbon's early pamphlets, including "An apology for the builder; or a discourse shewing the cause and effects of the increase of building" (1859 [1685]), were little more than special pleadings for particular causes in which he was quite literally invested. Even his later, more substantial works, *A discourse of trade* (1905 [1690]) and *A discourse concerning the coining of new money lighter: in answer to Mr. Lock's* [sic] *considerations about raising the value of money* (1971 [1696]) contain transparently self-interested arguments. Unlike Machiavelli, who began *The prince* (1998 [1532]) with an explicit appeal to the Medici, Barbon's apparent self-interest, surely informed by the uncertainty of his times, profoundly diminished his standing in the history of political thought.[1]

Most modern evaluations of Nicholas Barbon tend to pry out of his larger arguments those elements that presaged future economic ideas. Today Barbon is remembered for his contributions to theories of currency, consumption and as an early analyst of what would much later become known as the Veblen effect (Barbon 1905 [1690], 34; Ullmer 2007, 110). These modern assessments of Barbon share a critical shortcoming, a failing that further explains his diminished status. They all fail to connect Barbon's economic insights to his political concerns. Indeed, in a poignant irony that is all too common in modern economic

[1] In terms of the simple availability of Barbon's *A discourse of trade*, its recent inclusion in Henry C. Clark's excellent collection *Commerce, culture, and liberty* (2003) represents at least a partial remedy. In the modern scholarly treatment of Barbon a number of authors have treated economic aspects of Barbon's argument, most notably Schumpeter's *History of economic analysis* (1954), and Vickers's *Studies in the theory of money, 1690-1776* (1959). More recently still, a number of scholars have considered Barbon's *A discourse of trade* against a larger backdrop of political, cultural, and ideological change, noteworthy among these are Christopher Berry's *The idea of luxury* (1994), and Joyce Appleby's *Economic thought and ideology in seventeenth century England* (1978).

readings of the early texts of political economy, Barbon's writing succumbed to the intellectual and disciplinary parochialism it explicitly cautions against. In the preface to *A discourse of trade*, Barbon warned against this modern propensity to consider commercial questions outside of their larger economic, social and political context. Barbon believed that these intellectual blinders, worn most prominently by the rising merchant class, threatened the political and economic order that facilitated their rise in the first place.

> The Merchant, and other Traders who should understand the true interest of TRADE, do either not understand it, or else, lest it might hinder their private Gain, will not Discover it (Barbon 1905 [1690], 7).

Barbon was among the first to recognize the propensity of economics, in its aspect as a science concerned with self-interest, to recognize and respond to only the immediate and tangible. Barbon makes this claim against Thomas Mun in particular, noting in the preface to *A discourse of trade* that Mun "doth better set forth the Rule to make an Accomplished Merchant, than how it may be most Profitable to the Nation" (Barbon 1905 [1690], 6).

Despite being concerned centrally with disciplining the new science of trade, Barbon's own thought represents perhaps the earliest casualty of the modern eclipse of political philosophy by economics. This eclipse, unwittingly wrought by Adam Smith and other figures of the Scottish Enlightenment (Cropsey 1975, 132), continues to conceal the early modern identification of economics with politics. Indeed, especially in the Anglo-American context, politics and economics constituted the central modern pairing in the science of the legislator (Smith 1979 [1776], IV. i). This eclipse, first for *A discourse of trade* and ultimately for major Enlightenment works, not least among them *The wealth of nations*, concealed the degree to which early modern economic arguments and analysis were profoundly informed, even bracketed, by larger political concerns.

In Barbon's case, the centrality of that larger political context to economic concerns initially revealed itself in the citational structure of his *A discourse of trade.* Barbon's treatment of trade began with a pointed appraisal of Machiavelli and Livy and concluded with a quotation from Campanella. In each instance, the authoritative voice of political philosophy provided the larger legitimating context for the upstart ideas of economics. By placing his economic ideas within the larger tradition of Livy, Machiavelli, and Campanella, Nicholas Barbon presented economics as a component and completion of political theory, but never an alternative to that discipline.

> Livy, and those Antient Writers, whose elevated Genius set them upon the Inquiries into the Causes of the Rise and Fall of Governments, have been very exact in describing several Forms of Military Discipline, but take no Notice of TRADE; and Machiavel [sic], a Modern Writer, and the best though he lived in a Government, where the Family of MEDICIS had advanced themselves to the Soveraignty by their Riches, acquired by Merchandizing, doth not mention TRADE, as any way interested in Affairs of State (Barbon 1905 [1690], 7).

This is an argument for evolution not revolution. Understanding the political significance of trade represents the new task, the last element of the still incomplete modern project of political theory. Barbon's economics, as a field of both enquiry and action, represented either a sub-set within politics and political theory or a previously unrecognized form of politics. Either way, Barbon's enterprise aimed at completing the study and practice of politics. It did not seek to replace it with a new and novel discipline and agenda.

Far from participating in the eclipse of politics by economics, Barbon pursued economic ideas and phenomena in order to better understand and act upon the politics of an increasingly commercial world system. Most significantly, Barbon approached economics from the perspective of politics. Barbon's political economy drew on and expanded the perspective of the "best" of political theorists, Machiavelli. At its most primordial, Barbon's *A discourse of trade* starts from the wisdom of Machiavelli's Romans, who understood that "time sweeps everything before it and can bring with it good as well as evil, evil as well as good" (1998 [1532], 13). Barbon considered Machiavelli and Livy's work incomplete. At the time of their composition, history had yet to sweep before it that most revolutionary and modern of things, for both good and evil: commerce.

THE BREAK WITH ANALOGICAL ECONOMICS

Barbon wrote political theory for the emerging age of commerce. He sought to understand the moral, political, and economic changes that occur as a result of commerce's ever increasing sweep. At the outset of *A discourse of trade*, Barbon suggests that the central significance of his discussion of trade relates to national power. As a result, and in a manner reminiscent of Machiavelli's method in *The prince,* Barbon begins by freeing political economy from the constraints of an intellectually and politically confining morality. In the first section of *A discourse of trade*, Barbon asserts that the principles which inform the economic conduct of individuals are different from, even at odds with, the principles that inform the economic conduct of nations. He illustrates this claim by contrasting the annual expenditures of an individual and a state. In considering these two sets of expenditures, Barbon observes an almost perfect contrast in economic qualities. To Barbon, an individual's resources appear finite, demanding frugality. In contrast, a nation's resources seem infinite. When appraising the consequences for an individual and a state spending near to their total income in a single year, Barbon contends that only the individual courts ruin. That ruin, or its avoidance, rests on a fundamental difference in the character of their respective resources.

> This sheweth a Mistake of Mr. *Munn,* in his Discourse of *Trade,* who commends Parsimony, Frugality, and Sumptuary Laws, as the means to make an Nation Rich; and uses and an Argument, from a *Simile*, supposing a Man to have 1000 *l.* per *Annum,* and 2000 *l.* in a Chest, and spends Yearly 1500 *l. per* Annum, he will in four Years time Waste his 2000 *l.* This is true, of a Person, but not of a Nation; because his Estate is Finite, but the Stock of a Nation Infinite, and never can be consumed. For What is Infinite, can neither receive addition by Parsimony, nor suffer Diminution, by Prodigality (Barbon 1905 [1690], 11).

There is scarcely a more essential distinction in kind than that between finite and infinite. Individual economy and national economy stand not only differently appraised, but seem to be defined by fundamentally different principles. More radically still, the change of scale and arena generates fundamentally different outcomes for the *same behavior*. In *The prince,* Machiavelli suggests only that social and political location colors how we evaluate the actions of an individual, especially a prince.

> And I know that everyone will confess that it would be a very praiseworthy thing to find in a prince all of the above mentioned qualities that are held good. But because he cannot have them, nor wholly observe them, since human conditions do not permit it, it is necessary for him to be so prudent as to know how to avoid the infamy of those vices that would take his state from him and to be on guard against those that do not, if that is possible; but if one cannot, one can let them go on with less hesitation (1998 [1532], 62).

Barbon significantly radicalizes Machiavelli's position. For Machiavelli, the context of an action's occurrence and its ultimate consequence determine the range of appropriate action and its evaluation. Barbon expands Machiavelli's position, arguing that profligacy on the part of a prince is not only *judged* differently, it entails a different outcome by virtue of its performance by a prince. Appearance, appraisal *and* outcome are all changed by the relocation of economic activity from the domestic to the political arena.

Nowhere is the radical nature of Barbon's economic insights, and their implied political consequences, more clearly demonstrated than in his discussion of the aspect of infinity in national economies. In rejecting "parsimony, frugality and sumptuary laws" (Barbon 1905 [1690], 11), Barbon aims at opening up not only new opportunities, but new ways of thinking about economic phenomena. For Barbon, breaking with past errors opens up the political possibilities of commerce in a fashion that echoes Machiavelli's account of his relationship with Livy and the role he envisions for history in politics. In *Discourses on Livy*, Machiavelli contends:

> Nonetheless, in ordering republics, maintaining states, governing kingdoms, ordering the military and administering war, judging subjects, and increasing empire, neither prince nor republic may be found that has recourse to the example of the ancients […]. Wishing therefore to turn men from this error, I have judged it necessary to write on all those books of Titus Livy that have not been intercepted by the malignity of the times whatever I shall judge necessary for their greater understanding, according to knowledge of ancient and

> modern things, so that those who read these statements of mine can more easily draw from them that utility for which one should seek knowledge of histories (Machiavelli 1996 [1517], Preface).

A discourse of trade echoes Machiavelli's expansion of political theory in *Discourses on Livy*. Like Machiavelli's political recovery of history, Barbon's economics provides new insights into the fuller nature of the political. Barbon values this new field of inquiry for quintessentially Machiavellian reasons. He pursues economics because of "that utility for which one should seek knowledge", the service of the state (Barbon 1905 [1690], 11). Barbon's insights about the curious character of national accounts matter as much for politics and the avenues of inquiry they open, as for the material realities they reveal. *A discourse of trade* possesses a larger purpose than disproving the confining economic moralism of Thomas Mun; it entails adding economics to the Machiavellian account of politics.[2]

THE CONTOURS OF THE POLITICAL AND THE CHARACTER OF INFINITY

Barbon's argument for economic infinity seeks to demonstrate two things. First, it aims to demonstrate the unique qualities of national economics freed from the assumptions of the individual/state analogy. More subtly, it aims to show the relevance of economics per se to what Adam Smith will eventually describe as the science of the legislator. Turning to the first of these tasks, Barbon sets aside the occluding lens of Mun's economic moralism to consider the economy as it actually is. Considering the argument for infinity, Barbon claims that common experience validates what appears to be an extraordinary claim. *A discourse of trade* reveals the economy's infinite aspect in agriculture and the productive cycle of the seasons. According to Barbon, the infinite emerges in agriculture's apparently endless capacity for

[2] Barbon treated the limits of this analogy in a more explicitly political fashion in his earlier *Apology for the builder* (1859 [1685]). Once again relying on Machiavelli to illustrate the connection between his economic and political ideas, Barbon wrote: "And if those gentlemen that fancy the city to be the head of the nation, would but fancy it like the heart, they would never be afraid of its growing too big; for I never read of such a disease, that the heart was too big for the body. And if we are of Machiavel's [sic] opinion, this *simile* is the best, for he saith, that citizens make no good counselors, for having raised their fortunes by parsimony and industry, they are usually too severe in punishing of vice and too niggardly in rewarding virtue" (Barbon 1859 [1685], 22).

regeneration. Every year the economy harvests and sells the vast majority of its product, retaining almost nothing for the next year save seed. In the following year all is completely replaced and often expanded. As Barbon observes:

> The Native Staple of each Country is the Riches of the Country, and is perpetual, and never to be consumed; Beasts of the Earth, Fowls of the Air, and Fishes of the Sea, Naturally Increase: There is Every Year a New Spring and Autumn, which produceth a New Stock of Plants and Fruits. And the Minerals of the Earth are Unexhaustable (Barbon 1905 [1690], 10).

While the notion that the earth's mineral resources are infinite may jar the modern ear,[3] the principle of infinity as endless replenishment, for agricultural production at least, appeared obvious to Barbon.[4] Tellingly, in the next paragraph Barbon expands his original account of England's economy to consider the particular products of neighboring nations and the relative advantages and disadvantages provided by these products and their trade. In this comparison, Barbon subtly begins to re-conceptualize previous views concerning the national economy. He sees the possibilities that these insights create in the explicit light of national disputes and contests. For Barbon, the state that recognizes the peculiar qualities of a nation's "native staple", especially its infinite aspect, stands positioned to benefit in ways far beyond the simply economic.

Barbon continued his study of the infinite nature of national economy and the advantage realized in its recognition, by discussing commerce's capacity to accommodate, with minimal dislocation, a steady increase in demand. To make his case Barbon cited the Doomsday book's census numbers. To Barbon, the Doomsday data suggested that the population of England had doubled since the time of William the Conqueror (Barbon 1905 [1690], 25).[5] He observed that,

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[3] For a fascinating treatment of Barbon's claim about minerals and seventeenth century ideas regarding their replenishment, see Finklestein 2000, 94.

[4] Barbon's description, in its sense of wonder at the emerging economy's potential, echoes in tone and broad substance, if not specifics, John Locke's (1988 [1689]) account of the productivity of agriculture in his *Second treatise of government*, II. 37.

[5] In his first treatment of the question of population growth, in *An apology for the builder*, Barbon drew on the analysis of the Doomsday book as presented by Matthew Hale in his *Origination of mankind* (1677) to argue for population growth but equally

unlike a household whose denizens double in number, England seems neither strained nor strapped by its enlargement. Drawing on earlier work by John Graunt and William Petty, work assessed explicitly in Barbon's earlier essay *An apology for the builder*, he noted that far from ruin the doubling of the national number appeared to have made England wealthier (Appleby 1978, 165). Indeed, in an opposition that possesses further Machiavellian intimations, Barbon noted that what constitutes a burden for a single home represents a boon for an entire economy. In both questions of account, Barbon demonstrated that the analogy of personal and national wealth, with all its ethical and political baggage, is false. Freed of the confines of analogical reasoning, the state can consider the almost inestimable potential of the economy's expansive capacities.

NEW SOURCES OF NATIONAL POWER

Barbon immediately linked the new thinking about national revenue, its nature and the sources of its increase, to the circumstances of England and its neighbors. In this application and despite its enormous economic impact, Barbon indicated that the primary reason for rejecting the individual/state analogy was political not economic. However, to accomplish that political end Barbon needed to set aside at least two more errors concerning the nature of national revenue, one political and one economic. In economics Barbon redefined the sources of a nation's wealth almost eighty years before David Hume made this position famous in his essay "Of commerce" (Hume 1985 [1752]). Rather than bullion or land, Barbon argued that the true source of a nation's wealth was its citizens. In *A discourse of trade*, Barbon stated unequivocally "people are the riches and the strength of the country" (Barbon 1905 [1690], 29). Barbon's assertion, as with so much in the rest of the text, contains not merely an economic assertion but an equally important, albeit implied, political argument. This apparently simple statement, in its pairing of riches and strength suggests that the rise of the modern

for population migrations within England (1859 [1685], 9). Unlike *A discourse of trade*, in *An apology*, Barbon also explicitly acknowledges a debt to William Petty (Barbon 1859 [1685], 20) who had argued in his *A treatise of taxes and contributions* that "Fewness of People" was a burden not a benefit to government in the performance of its duties (Petty 1899, 21).

market transformed the sources of national strength and the purpose of conquest. This claim—contained within a discussion of imperial projects—provided Barbon with grounds for the revolutionary claims to follow, most especially a fundamental reassessment of the character of trade. Barbon acknowledged as much when he declared:

> […] for until TRADE became necessary to provide Weapons of War, it was always thought prejudicial to the Growth of Empire, as too much softening the People by Ease and Luxury, which made their Bodies unfit to Endure the Labour and Hardships of War (Barbon 1905 [1690], 6).

In a subtle working out of the claim concerning the commercial character of the people in the discussion of empire that follows, Barbon presented a near complete rejection of the claim that trade is prejudicial to empire. Indeed, the account that follows asserts that trade comprises the new means to empire. In characterizing the people as the "riches and the strength" of a country, Barbon presented commerce as the uniquely modern source of national power.

Barbon acknowledged the breadth of consequence attached to his reassessment of the relationship between economics and politics. He recognized that the new commercial conception of the people's strength entailed the reordering of the relationship between martial and merchant virtues. Indeed, it entailed a reconsideration of the essential character of civic virtue. Barbon's account, in its most basic assumptions about virtue, shares the same ethical ground as Machiavelli's *The prince* and *Discourses on Livy*. The definitions of the good and the interests of the state overlap. However, here the sympathy ends. Barbon's essential innovation consists in a complete inversion of the early modern account of the relationship between self-interested citizens and professional armies (Smith 1979 [1776], V. 1. 39). Sidestepping entirely the problematic relationship between wealth and virtue, Barbon argued that it is not self-interested citizens who need modern armies and state *apparati*, but rather it is the modern state and its army that need self-interested citizens. Trade rapidly revolutionizes the political and social elements, population, prosperity, and urbanization, which initially prompted its ascension. In making this argument, Barbon began to reveal the ways in which his new political theory for a commercial age involved not merely completing Machiavelli and Livy, but, in important respects, displacing elements of their argument, especially their arguments concerning empire. In the *Discourses on Livy* Machiavelli writes:

> I say therefore that not gold, as the common opinion cries out, but good soldiers are the sinew of war; for gold is not sufficient to find good soldiers but good soldiers are quite sufficient to find gold (Machiavelli 1996 [1517], II. x. 2).

Machiavelli understood that money was necessary for war; Barbon countered that ultimately money, or more precisely commerce, could achieve the same political ends as war. The preface to *A discourse of trade* declares: "Trade is now as necessary to Preserve Governments, as it is useful to make them Rich" (Barbon 1905 [1690], 5).

Barbon renders his case in at least two rhetorical registers. First he demonstrates in a resigned tone the truth of his central insight, that a citizen's riches and not their virtue are the new sources of national power. An apparently unwilling revolutionary, Barbon calls for a transformation in the citizens and state of England necessitated by realities on the continent. Turning to France, Barbon looks beyond traditional condemnations of *ancient regime* extravagance to the economic and ultimately political consequences of that literally sumptuous social order. He argues unequivocally that the Bourbon rejection of sumptuary laws and the embrace of luxury explain the rise of French power.

> It is from Fashion in Cloaths, and Living in cities, That the King of France's Revenues is so great, by which he is become troublesome to his Neighbours, and will always be so, while he can preserve Peace within his own Country; by which, those Fountains of Riches, may run Interrupted into his *Exchequer* (Barbon 1905 [1690], 34).

Barbon does not mince words. The urban vices, in particular fashion in its essential inconstancy, sustain the fountain of riches that promises the Bourbon monarchs a global empire. Indeed, Barbon suggests, a conspiracy of frugality on the part of the rich would so impoverish the public coffers as to be "as dangerous to a Trading State as a Forreign War" (Barbon 1905 [1690], 32).

Barbon then investigates the charges that the political makes against the economic: that it softens, it feminizes, and it enervates. Considering these factors, Barbon restates and expands his critique of the individual/state analogy. He notes that as with profligacy, the consequences of luxury differ for states as opposed to citizens. In the Bourbon example the citizenry is indeed weakened by the ephemera it pursues but, in a moment of economic and political alchemy, that pursuit translates upward into an all-too-real political strength. Barbon completed the political account of this new reality by identifying the Sun King's exchequer, and not his generals, as the new source of strength for France and concern for its neighbors. For England, the rise of commercial power in France imposes a new reality. The new politics of commerce does not merely bankroll political power, in its transactions it *is* political power. England must escape the old thinking, about frugality, sumptuary laws and the ambiguous relationship between wealth and virtue or perish.

Barbon's *A discourse of trade* examines the series of revolutionary breaks that the market makes with existing assumptions concerning morality and politics. In breaking with the laws of nature and finitude and transforming the relationship between luxury and power, Barbon charted the growing disconnect between morality and economy. By discrediting the analogical relationship between the citizen spendthrift and the profligate state, Barbon deepened the moral break between macro and micro in politics begun by Machiavelli. Finally, in replacing the soldier with the merchant and the general with the exchequer, Barbon pointed to the new centers and sources of political power. Barbon's political examination of the market completed the inquiry begun by Machiavelli. Barbon's application of those insights to politics sought to complete Machiavelli's reordering of the political and moral horizons of the emerging modern world.

THE NEW POLITICS OF COMMERCE

In writing *A discourse of trade*, Barbon set out to transform the most basic assumptions about the nexus of politics, economics and morality. In the opening sections, Barbon subtly moved economics out from under domestic ethical and moral assumptions and analogies and into the realm of the political. More explicitly, as his introduction indicates, he moved economics into the realm of the political as understood by Machiavelli. The second half of *A discourse of trade,* in treating trade between nations and trade within empires, aimed to transform the Machiavellian politics it so recently extended.

Matching the rhetorical structure of so much that follows, Barbon began *A discourse of trade* by marveling at the capacity of trade to overcome traditional expectations and limitations. In particular, he wondered at the capacity of trade to escape the assumptions concerning scale and strength that informed European politics. As the introduction indicates, the unexpected escape from the politics of scale appeared most dramatically in the rise of the United Provinces and Venice, small states made great by commerce.

> The Greatness and Riches of the United Provinces, and states of Venice, Consider'd with the little Tract of Ground that belongs to either of their Territory, sufficiently Demonstrate the great Advantage and Profit that Trade brings to a Nation (Barbon 1905 [1690], 5).

Reiterating the earlier equation of "strength and riches", Barbon connected two terms, one political and one economic, to suggest a new relationship between the two. Advantage, as applied to Venice and the United Provinces, appears to be defined by benefits generated by, but hardly restricted to, trade. In their rise, these two states express the missing element of Machiavelli's political theory, namely the overlooked political potential of trade. Venice and the United Provinces, which both demonstrated an inverse relationship between size and power, suggested that the development of international commerce, as opposed to simple conquest, represents the modern route to power. In considering this emerging modern mode, Barbon continued the rhetorical strategy begun with his account of the rise of commerce more generally. His argument for commercial empire combines both responsive and innovative elements. First, Barbon argued that commerce has so transformed the world that the traditional routes to empire have closed. Second, he explored the possibility that these transformations facilitate a new and novel form of empire, one uniquely suited to English circumstances.

Making the case from necessity, Barbon returned to his economic analysis concerning the growth of the population of England and extended his examination to the continent. Considering the new political and economic realities of Europe, Barbon suggested that population growth is both a response to and eventually an engine for the broad productive potential of commerce (Barbon 1905 [1690], 31). He also revealed the new restrictions population growth presents to traditional modes of extending political power. He declared "There is now no room, the world is so full of people" (Barbon 1905 [1690], 29). Barbon's Europe had outgrown the old modes of military conquest. Armies, penned in and limited by ever-larger cities, could no longer hope to establish anything like the ancient empires of Alexander and Caesar. On the cramped continent, the military was unable to displace peoples or to so capture and subdue populations as to preclude assistance from nearby neighbors (Barbon 1905 [1690], 29). Moreover, on an ethical note, Barbon admitted that the density and sophistication of populations meant that their dominion, in the fashion of the ancient empires at least, would require barbarism too terrible to contemplate. Finally, as Barbon had already argued, if it is the people that constitute the wealth of a country, then the ancient modes of conquest invariably entail squandering the object of empire.

> For the same Reasons, That the World is grown more Populous, That the Arts of War are more known. That the People of *Europe* live under a Free Government. It is as difficult to keep a Country in Subjection, as to Conquer it. The People are too Numerous to be kept in Obedience: To destroy the greatest Part, were too Bloody, and Inhuman; To Burn the Towns, and Villages, and so force the People to remove, Is to lose the greatest share in Conquest (Barbon 1905 [1690], 29).

This is a uniquely post-Machiavellian argument. The critique of empire-building, or rather traditional empire-building, is not moral but practical. The ambition may be sound, natural or inevitable, but the avenue is closed. More importantly, the same agent that closed the avenue will open a new route for the modern prince: commerce. Once again, Barbon found the example of the United Provinces instructive.

Confronted with the ambitions of Europe's traditional empire builders, Spain and France, commerce permits the much smaller state to resist.

> And Amsterdam, that was not long since, a poor Fisher-Town, is now one of the Chief cities in Europe; and within the same Compass of Time, that the *Spaniard & French* have been endeavouring to Raise an Universal Empire upon the Land; they risen to that Height, as to be an equal Match for either of them at Sea (Barbon 1905 [1690], 30).

According to Barbon, only the preoccupying power of France and Spain prevent the United Provinces from becoming a new, distinctly commercial empire. England, spared from the continent by the Channel and already a naval power, stands positioned to become the next great empire, a commercial empire, necessarily borne upon the seas, the last open spaces in the age of commerce.

The end of old modes of extending power and influence, especially on the European continent, forced open new avenues of prosperity and power for Barbon's England. Barbon focused on these new avenues, but also the new relationship between prosperity and power. In delineating this new relationship, Barbon marked out the signal qualities of the nascent British Empire. His account even seems to hint at the not yet seen forms of commercial empire of the more distant future, the "voluntary empires" (Kagan 2009, 21) of NAFTA and the EU that inform much of modern international relations. Barbon recognized that such an empire, in accessing new means to imperial expansion, demanded new modes of imperial occupation. In order to succeed, imperial occupation could no longer rely on the practices of imperial tribute, tariff and appropriation that constituted the economic practices of previous empires. To succeed, the emerging commercial empires must be founded on the key insights about politics and economics that Barbon identified.

Barbon's international project begins with an application of the basic economic principles he first located within the domestic economy. Examining the sources of prosperity, Barbon identified the curious and perhaps counter-intuitive relationship between the rich and poor. In market societies, Barbon contended that: "The Chief Causes that Promote *Trade,* (Not to mention Good Government, Peace, and Scituation, and with other Advantages) are Industry in the Poor, and Liberality in the Rich" (Barbon 1905 [1690], 31).[6] Note that once again, the causes that promote trade bring with them other, explicitly political benefits. In his discussion of empire, Barbon recommended this morally suspect but mutually beneficial relationship as the framework for the new commercial core and colonies.

In instituting this arrangement a commercial empire radically inverts the assumptions about national advantage and the flow of people and products that undergirded ancient empires. Instead of conquest and seizure, Barbon maintained that the successful modern empire should seek to improve the material conditions of its colonies, founding and drawing people to cities as market centers. The empire's new subjects, like England's industrious poor, should benefit from the liberality of the imperial center. Commerce (and not justice) requires this new mutuality.

The new empires, forced to look overseas by the crowding of Europe, must adopt novel modes of influence over newly acquired populations. Commercial realities, if not necessarily requiring an empire by invitation, nonetheless permit only an imperial rationale that differs dramatically from conquest-based incarnations. Indeed, it quickly becomes clear that while there may be an initial moment of conquest, the exigencies of the new imperial form preclude traditional forms of subjection.

> The ways of preserving conquests gain'd by Sea, are different from those at Land. By the one, the Cities, Towns and Villages are burnt, to thin the People, that they may be the easier Governed, and kept into Subjection; by the other, the cities must be inlarged, and New ones built; Instead of Banishing the People, they must be continued in their Possession, or invited to the Seat of Empire; by the one, the Inhabitants are inslaved, by the other they are made Free (Barbon 1905 [1690], 30).

Barbon concluded by revealing the ultimate consequence of his declaration that people are the "strength and riches" of the nation. The nature of power in a commercial age leads inevitably to a radical

[6] Compare with Machiavelli: "For in every city these two diverse humors are found, which arises from this; that the people desire neither to be commanded or oppressed by the great, and the great desire to command and oppress the people. From these two diverse appetites one of three effects occurs in cities: principality or liberty or license" (Machiavelli, 1985 [1532], 39).

and prescient reappraisal of the relationship between the center and colonies. The strength that emerges from the profit of trade and that extends the scope of the nation's economic power and influence can do so only by easing the scope of its political interventions. As Barbon's argument develops, it becomes increasingly clear that the non-economic benefits that commerce brings with it are not collateral benefits, but necessary conditions for trade to thrive. The commercial empire must be an empire of liberty.

More radically still, the freedom founded upon economic necessity entails foregoing traditional assumptions about inequality and primacy between the center and colonies. Barbon's argument highlights a commitment to rough equality between center and colony in moving from a discussion of that equality to a recommendation that the center, England, open its frontiers to the most successful citizens of its new commercial colonies. This opening of frontiers, justified by economic considerations, nonetheless promotes an expansive understanding of equality. Aware of the political and cultural ramifications of his argument, Barbon writes with great rhetorical subtlety, further blending the economic with the political while drawing out the final consequences of his equation of riches with strength. He begins his argument with a heavy-handed gesture to prevailing prejudice when he declares that the English are suited for commercial empire by a climate that renders citizens, game-cocks, and mastiffs "nowhere else so stout" (Barbon 1905 [1690], 31). However, in his actual recommendations, the language of stock and soil disappears entirely. Passing his arguments for equality and liberty through the reassuring framework of a treatment of the promotion of trade, Barbon suggests:

> And were there an Act for a General Naturalization, that all Forreigners, purchasing Land in *England*, might Enjoy the Freedom of *Englishmen*, It might within much less Compass of Time, than any *G*overnment by Arms at Land, arrive to such a Dominion: For since, in some Parts of *Europe*, Mankind is harrassed and disturbed with Wars; Since, some Governours have incroached upon the Rights of their Subjects, and inslaved them; Since the People of *England* enjoy the Largest Freedoms, and Best Government in the World; and since by Navigation and Letters, there is a great Commerce, and a General Acquaintance among Mankind, by which the Laws and the Liberties of all Nations, are known; those that are oppressed and inslaved,

> may probably Remove, and become the Subjects of *England* (Barbon 1905 [1690], 31).

Barbon's endorsement of centripetal migration represents the final and fullest elucidation of the political and imperial consequences of the commercial transformation of politics. If the people constitute the economic and political power of an empire then those people, and not their produce, constitute the resource most in need of extraction from the colonies.

Commerce provides the means and rationale for a novel form of empire, but also implicitly suggests a new remedy for one of the oldest dilemmas of empire. Every empire eventually encounters the problematic relationship between claims of autochthonic privilege and the demands of long subjected populations. Each empire aims to solve this problem in its own way.[7] Barbon's commercial account points to a uniquely modern solution. The modern commercial empire, associating its citizens' strength with their riches, stands upon a perfectly transferable vision of civic value if not virtue. The commercial conception of the citizen as wealth creator detaches the individual's status from claims to shared history, common culture or original possession. Citizenship emerges out of economic transactions, the immediacy of commercial self-interest that entirely avoids the fraught territory of blood and soil. Commerce provides for a new imperial expansion that empowers the state at its frontiers and eases the absorption of the most vital colonial resource at its center.

Yet, Barbon's argument goes farther still than the commercial reframing of citizens and civic virtue. Barbon's new view of naturalization amounts to a restatement of his earlier account of commerce's effect on Europe. Facilitated by commerce, the trade routes, the intensification of population density and the increased ease of communication, all mean that English liberty is better known abroad. Commerce provides the pathways for a political appeal. Those seeking freedom and prosperity head for England, either from the colonies commerce built or from the countries that commerce has made aware of English liberty. As a result, England fills with the strength and riches

[7] For the Ciceronian treatment of this dilemma see my "The rise of global power and the music of the spheres" (Kellow 2009).

precipitated by the inflow of people. Here again, the political ends up being both complemented and transformed by the economic. The inflow of people to England empowers but, at the same time, transforms the substance of the nation. The commercial empire's external modes redefine its internal essence.[8] A commercial empire, built by mutual benefit and sure in its liberty empowers the economy to access people, the primary sources of riches and ultimately, strength.

CONCLUSION

Barbon's rhetoric of the new politics of commerce moves ineluctably from revelation to resignation and ultimately to revolution. The essay begins with Machiavelli and Livy, whose vision, lacking an account of trade, remains incomplete. *A discourse of trade* emancipates economics from the confines of morality, most captivatingly incarnated in the individual/state analogy.

Barbon then illuminates the new politics in tones of resignation, inevitability, and necessity. In the discussion of trade, especially international trade, Barbon's tone changes; the emerging world of markets may not be of our choosing, but the political advantage to be taken from it can be. Finally, in discussing commercial empire Barbon returns to political theory and conspicuously to the language of economic and political liberty. Just as his essay begins with Renaissance meditations on power, so too does it end. *A discourse of trade* concludes with Campanella, writing "an hundred years since" (Barbon 1905 [1690], 42). Barbon turns back to political theory and sees Campanella warning of the same Gallic threat he identifies.

> Campinella [sic], who Wrote an 100 years since, upon considering of the Great Tract of the Land of *France*; says, That if ever it were United under one Prince, it would produce so great a revenue; It might give Law to all Europe (Barbon 1905 [1690], 42).

In its specifics, Campanella's claim provides a curious coda to *A discourse of trade.* Throughout the essay, Barbon claims as his central insight a new account of the relationship of prosperity to power. Now, in the concluding passages, Barbon presents Campanella offering a version

[8] Consider Aristotle's *Politics* (1996, 30-40).

of the argument that he has been making all along. In fact the passage from Campanella's *Hispania Monarchia* represents a dual reprise. First, it relocates Barbon within the tradition of political theory, second Barbon places his enterprise and its concerns alongside Campanella's. In doing so, Barbon announces his return to the political theory fold, having completed the work begun by Machiavelli and Livy.

Campanella also serves, by way of Barbon's return to political theory, to restate the risks posed by France. Campanella legitimates not just Barbon's enterprise but his threat analysis. Barbon and Campanella agree that commercial revenues represent the key to French power. Barbon offers the English alternative, a new politics of commerce that is fundamentally free. Barbon ends *A discourse of trade* with Campanella, with political theory, but with a political theory that faces not merely a new reality, the politics of commerce, but a new question, what sort of politics of commerce? What sort of commercial state should provide the emerging market world with its new universal law? In ending with the conflict and contrast between the French and English states, Barbon restates the rhetoric of resignation and revolution that informs *A discourse of trade* as a whole. In placing the perennial Anglo-Gallic contest at the conclusion of his analysis of trade, restating it through Campanella's assessment of Bourbon power, Barbon forcefully locates his economic ideas within the context of a newly expanded political theory.

Ultimately, Nicholas Barbon's *A discourse of trade* presents, in its construction, substance and rhetoric, an early outline of a new science of the legislator for the new politics of commerce. Nearly a century later Adam Smith famously argued that political economy was "a branch of the science of a statesman or legislator" (Smith 1979 [1776], IV. i. 1). By then, the urgency of placing economics within the context of political theory was slowly, ineluctably, giving way to a new urgency, to retain for political theory a place within economics. The sense of rank and priority between disciplines had changed. Nonetheless, Barbon's central insight endures, Barbon's account of trade is an account of politics, his account of politics is an account of trade. Barbon's economics and politics are inextricably linked and mutually informing, the comprehension of one requires the comprehension of the other.

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Erasmus Journal for Philosophy and Economics,
Volume 4, Issue 1,
Spring 2011, pp. 23-41.
http://ejpe.org/pdf/4-1-art-2.pdf

Against the pragmatic justification for realism in economic methodology

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Abstract: In recent times, realism in economic methodology has increasingly gained importance. Uskali Mäki and Tony Lawson are the best-known realists within the discipline and even though their approaches are fundamentally different, both provide (among others) pragmatic defences of realism by claiming anti-realism to be the reason for the low quality of (some) economic models. My paper will show that a pragmatic defence of realism is untenable and furthermore, I will show that for both Mäki's and Lawson's *normative* ideas there is no need for realism.

Keywords: realism, anti-realism, Uskali Mäki, Tony Lawson, pragmatism

JEL Classification: B40, B41, B49

Every discussion of realist philosophy of science must begin by distinguishing the different forms of realism and by declaring what is exactly at issue. The following list provides an overview of different realist positions in philosophy of science, in ascending order, by the strength of claims being made:

> **1. Ontological realism**: This is the most modest realist claim and merely entails the belief in the theory-independent *existence* of an external reality.
>
> **2. Weak epistemic realism**: Scientific theories refer to an external reality and *may be* right in their claims about it, i.e. they are capable of being true or false. This includes the *semantic* thesis that theories are true if and only if they correctly refer to an external reality.

AUTHOR'S NOTE: I wish to thank the editors of EJPE, Manfred Stöckler, Dagmar Borchers, Andreas Pyka, the members of the PhD-seminar of the Institute of Philosophy in Bremen, the audience of my talk at GAP.7, and two anonymous referees for their very helpful comments.

3. Scientific realism/strong epistemic realism: Well-confirmed scientific theories refer to an external reality and *are* basically right in their claims about it.[1]

Both weak and strong epistemic realism are deeply connected with a correspondence theory of truth, because their central point is to make claims about the properties of an external reality. If those realists would rely on a coherence- or consensus-theory of truth, this would directly beg the question. In this paper, I take anti-realism as the thesis that we should suspend judgement on the truth and truth-worthiness of our theories or avoid talking about the truth of theories altogether in order to minimize the confusions that surround this concept.[2] I analyse the pragmatic aspects of the justifications for realism that one might interpret from the distinctive projects of two prominent realists in economic methodology: Uskali Mäki and Tony Lawson. I argue against these pragmatic aspects and try to show why an anti-realist perspective is preferable.

USKALI MÄKI'S REALISM

Uskali Mäki's overall strategy consists in developing a discipline-sensitive brand of realism that is tailored to analysing many of the traditional problems in economic methodology. His approach can be described as "bottom-up", which means that he tries not to invoke external philosophical concepts for criticising economics, but attempts to first understand what economists are doing before seeking a realist interpretation for it. Mäki's justification for taking a realist position is pragmatic insofar as he fears that giving up realism "would result in the worst kind of complacency" (Mäki 2002, 102). I call this a pragmatic justification, because it focuses on the good consequences that an adoption of realism would have.

Mäki believes realism can offer arguments against the well-known defence of abstract economic reasoning that jumps from the premise that all models are false anyway to the claim that all criticism against the falsehood of economic models is to be rejected (Mäki 2009a). In a definition of realism that Mäki gives, it becomes clear that his

[1] This list is not meant to be exhaustive. The qualification that theories are only "basically" right allows for structural realism as well. See Worrall 1989, for the *locus classicus*.

[2] Note that I do not claim that no theory can be possibly true—there may well be theories that are true (even if just by chance) but we should avoid talking about the truth of theories.

realism is based on a correspondence theory of truth, as he argues that good science pursues theories that are true by corresponding to reality ("the objective structure"):

> […] theories and models are true or false by virtue of the ways of that objective structure—not by virtue of whether evidence supports them or whether we are otherwise persuaded to believe in them, for example. Finally, good science pursues theories that are true, while being prepared for the possibility of error (Mäki 2009a, 74).

Mäki's realism allows him to discuss whether economic models resemble the real world. He distinguishes between models, whose internal analysis is for economists a complete substitute for analysing the real world to other models that are a useful surrogate for doing this (Mäki 2009a). While the terminology of substitutes and surrogates may be confusing, the claim that some economists are getting lost in abstract formal analysis is quite plausible.

A main point of Mäki's work consists in demonstrating that highly idealised economic models *can* relate to reality so that their analysis can be a useful surrogate for conducting direct empirical research. Mäki states that economists "can be philosophical realists about their models even though these describe imaginary situations" (2009a, 79). Indeed, he turns the above argument against the relevance of falsehood upside down: even if all models are necessarily false in their details, we can believe them to be *essentially* true because the idealisations are strategic and necessary falsehoods, which aim at isolating the true core of a model. Referring to Hausman (1992), Mäki takes the high degree of theoretical isolation in economics to be the reason why it is an "inexact and separate" science. Mäki compares his approach to Nancy Cartwright's (1983) point that economics lies because the world is messy and the models are cleaned of disturbing factors, but in contrast to Cartwright, he sees the chance for models to be true of basic causal mechanisms, even if the messy world seems to contradict them (Mäki 2009a, 81). Yet it is undeniable that some assumptions in economics are merely introduced for tractability reasons and not because they isolate central factors (take, e.g., the assumption of perfect knowledge, the ignorance of transaction costs, or constant returns to scale). Mäki acknowledges this detail and asserts that relaxing such assumptions (and not the ones required for theoretical isolation) as a major driving force of economics becoming more realistic, in the right sense (Mäki 2009a, 83-85).

It should be recognized that Mäki borrows an important argument in favour of realism from Lionel Robbins (1945): the view that economics does not create new "unobservables" but deals with entities that are close to commonsense (which he calls "commonsensibles"), such as firms, households, and prices. These entities have a certain amount of "reality" because we deal with them in our daily life (in contrast to physical entities like electrons or quarks). Even if the commonsensibles economic theory deals with are highly idealised, the idealisation is "strongly constrained by economists' commonsense intuitions" (Mäki 2009a, 88). This leads to the rejection of models that contradict commonsense, making the differing commonsense convictions of economists a highly crucial point in theory choice. But if we accept that the basic entities of a certain economic model are based on commonsense notions, it becomes clear why the existence of the basic entities is not the main point of a realistic position in economic methodology. Instead, the main point is about the reality of the causal mechanisms postulated by economic models.

In the end, Mäki admits that it is quite impossible to know whether his philosophical meta-theory of realism is true, and even worse, when we agree that economic models may be false (due to epistemic and institutional factors), we are forced to admit that the meta-theory may be false for the very same reasons. This leads Mäki to adopt fallibilism as the super rule (Mäki 2009a).

In a recent text called "Some non-reasons for non-realism about economics", Mäki rejects several premises that seem to support an anti-realistic interpretation of economics. Here is a short summary of his counter-arguments (Mäki 2002, 92, *et seqq.*):[3]

> **Thesis 1**: "Economics postulates unobservables, therefore it is better interpreted by non-realism". Mäki responds that this happens in every science and is no reason for non-realism, especially because many of the unobservables in economics are "commonsensibles" as explained above.
>
> **Thesis 2**: "Economics is based on false assumptions; this is an argument for interpreting it by non-realism". Mäki responds again that this is true for all sciences in a strict sense, so it is no reason for

[3] See Hodge 2008, for a discussion.

non-realism. The relevant question is, whether the false assumptions help to isolate parts of reality or not.

Thesis 3: "Economics is not predictively successful, so the basic premise for the no-miracle argument is missing, which is an argument for non-realism". Mäki responds, as explained above, that we have more direct access to economic phenomena by our commonsense, so believing in the reality of basic economic premises does not need to be justified by the no-miracles argument. Besides that, he claims, taking into account the complex nature of economic systems, it would be rather a miracle if economics was indeed predictively successful.

Thesis 4: "When accepting a theory, economists are persuaded (and not rationally convinced!) by many social factors, which is an argument for non-realism". Heavily abbreviated, Mäki responds by arguing that persuasion is completely orthogonal but not antagonist to truth, and therefore the argument is not directed against realism. Even if the influence of "irrational" factors is strong, the resulting theories can still be true.

These arguments show how anti-realism should not be justified, according to Mäki. They also show that his justification of realism often consists in attacks against anti-realism combined with an appeal to realist intuitions. However, as mentioned above, it should be noted that Mäki also provides a pragmatic justification for his realism when he expresses the fear that giving up realism could lead to justify *anything* in economics, even if it was only "a game of just playing with fictions" (Mäki 2002, 102). Obviously Mäki believes in the good methodological consequences of realism and, again, this is what I call a pragmatic justification. While Mäki is doubtful whether a strong epistemic realism can be achieved, he clearly sets this as an aim (Mäki 2002).

Below I will consider whether Mäki can live up to the task of improving economics by means of his realism. Before this, in the next section, I will present the other key realist position in contemporary economic methodology, Lawson's critical realism.

TONY LAWSON'S REALISM

Tony Lawson's critical realism differs fundamentally from Mäki's realism. Where Mäki is generally neutral or even affirmative concerning mainstream economic theory, Lawson decidedly wants to use realism as a tool for criticising current mainstream economics. Lawson starts with the premise that mainstream economics is in a state of disarray, because it focuses too much on formalised deductive modelling and does not deal with real world issues (Lawson 2001). He locates the fundamental error of mainstream economics in its anti-realist methodology which sees truth as an irrelevant criterion for theory evaluation. His basic argument is that anti-realism leads economists to ignore the central problem of their field by rendering the lack of realisticness of theories unproblematic by definition. According to Lawson, the anti-realist is in a desperate situation, if theories are *not* successful at predicting empirical data. In this case, the anti-realist usually recommends trying harder, digging deeper, and searching for regularities at a more disaggregated level—realism is the recommended way out of this problem.

Lawson states that in some sense nearly everybody is a realist because even methodological anti-realists often accept ontological realism. For this reason he defines his blend of realism by its "sustained concern with ontology" (Lawson 2001, 168). By this focus on ontology, Lawson hopes to learn something about the nature of social phenomena, which he thinks will enable him to give better methodological advice to economists than anti-realists can. This is a pragmatic defence of realism as it concentrates on the positive consequences of adopting critical realism. Indeed, it is much more explicitly pragmatic than the defence Mäki gives, because Lawson's project is much more normative. In his most recent book *Reorienting economics*, Lawson even suggests that all heterodox traditions are best understood by looking at the "social ontology" they presuppose (Lawson 2004).

Lawson's most important critical point concerns deductivism. He states that the formalistic models of mainstream economics necessarily rest on a deductivist mode of explanation, even if that fact may be concealed by the usage of stochastic variables or non-linear equations. According to Lawson, the fundamental problem of deductive reasoning is its dependence on closed systems that are characterised by stable observable event regularities. However, Lawson suggests, "that the social realm is everywhere open, that scientifically interesting event regularities rarely, if ever, occur" (Lawson 2001, 170). This makes deduction of future events or using theories as tools for prediction not only difficult, but inherently wrong. Lawson argues that deductivism in economics needs an ontology

> of structures, powers, mechanisms and tendencies, etc., that are irreducible to, but which underpin the actual course of events and

> states of affairs. Once this ontology is established it supports a conception of science as moving from phenomena at one level to its conditions or causes at a different, deeper, one (Lawson 2001, 172).

Lawson states that deductivists (including predictive-anti-realists) cannot discuss these matters and are therefore unable to explain why science is in fact successfully applied to open systems where event regularities do not hold (Lawson 2001).[4]

In Lawson's view, economic laws should not be made to represent observable event regularities, but rather the *underlying* workings of mechanisms and tendencies. He argues that his realist perspective should be accepted due to its greater explanatory power concerning the question as to how it is possible that results which hold in closed systems can often be meaningfully transferred to open systems, even if the predicted event regularities do not hold there (Lawson 2001).

His studies in "social ontology" lead Lawson to claim "that economics ought really to move in a different direction entirely, to develop ways of uncovering causal mechanisms in a seemingly quintessentially open, as well as intrinsically dynamic, and highly internally-related, social reality" (Lawson 2001, 175). The described social reality has not the same ontological independence of human thought as natural reality, because it is a human construct and hence depends directly on human thinking. Lawson rejects the view that all causal forces in the social realm are reducible to individuals, because the socio-economic structures exist prior to individual action (Boylan and O'Gorman 1995).

Even if Lawson's social ontology is supposed to reveal "deeper structures" and "essential features" it does not include the claim of ultimate knowledge about these matters and Lawson admits its findings are fallible (Lawson 2001). If one accepts Lawson's ontological claims, a methodology that takes individual reactions to changes in relative prices as its basis is ill-conceived, because it neglects the freedom of human choice and the power of social structures systematically. Orthodox economic theorising therefore often employs convenient fictions that state very general and tractable connections between variables, instead of looking after the real and essential forces (Boylan and O'Gorman 1995).

[4] Note the similarities between Lawson's view and Hausman's notion of tendency-laws.

Adopting his methodological views will lead, according to Lawson, to an economics that is a much more complicated and messy affair than the current mainstream. In this context, the best one can hope for is a kind of interpretative explanation (i.e., not prediction) of so-called demi-regularities, a term that is essentially equivalent to Kaldor's "stylised facts" (Lawson 2003). In short, Lawson states that economics should be concerned with the *essential* features of economic systems and his critical realist methodology is made for knowing what they are.

CRITICAL DISCUSSION

We have now seen in some detail how the two main protagonists of realism defend their philosophical thesis. I will argue that "truth" is almost always replaceable by other terms that are ontologically more parsimonious (such as empirical adequacy[5] or fit with the totality of current knowledge[6]) and may nonetheless fulfil the intentions the respective author had. While I accept many of the conclusions that Mäki draws (and some of Lawson's), I cast doubt on whether realism is necessary for justifying these conclusions. The next sections will elaborate on these doubts.

Discussion of general justifications for realism

The philosophical dispute about realism is of course not easily settled. I will first sketch some general arguments against realism, before I deal specifically with Mäki's and Lawson's arguments.

Let's start with the famous "no-miracle" argument for scientific realism. In its most basic version it simply states that realism is the "only philosophy that doesn't make the success of science a miracle" (Putnam 1979, 73). It states that the success of scientific theories can be *explained* by claiming that these theories capture elements of an external reality. It is true that anti-realism cannot offer such an explanation, but the crucial question is, whether the realist move is an explanation at all. Often, it seems that the realist's arguments are begging the question of the anti-realists, and vice versa. I think this is the case for the "no-miracle" argument as well. The anti-realist would claim that we are *not* justified in explaining the success of science by its

[5] See van Fraassen 1980, for the *locus classicus* of a defence for this criterion.
[6] In the sense of Quine and Ullian 1970.

truth[7] because theories could possibly be successful without being true, due to empirical underdetermination.[8] In short, scientists often accept those theories that work well and that is all there is to say. Accepting truth (in the sense of correspondence) as the best explanation for their success means to go beyond the borders of what we can legitimately infer. From this view, the suggestion that truth explains the success of theories is no explanation at all—it is rather an illegitimate *ad-hoc* statement. We could equally argue that the existence of God is the best explanation why our theories work, but anti-realists are convinced we should not do that on the same grounds why we should not "explain" success by a correspondence to an independent reality. In both cases, the explanation is based on uncertain ontological claims. But we can know whether a theory is helpful for solving our problems because that is a completely subjective judgement which does not involve an ontological claim.[9]

A stronger argument in favour of anti-realism is the fact that even inconsistent theories can "work" which shows that taking truth as an explanation for success is problematic because the truth can hardly be inconsistent (da Costa and French 2002).

Once we talk about the acceptance of the "inference to the best explanation" the quarrel between realists and anti-realists gets more complicated. In her daily work, the anti-realist may accept and use some theories because she holds them to be the best explanation for a phenomenon under scrutiny. For example, the anti-realist may accept increased demand for oil as the best explanation for a rising oil-price. Now the realist can ask why the anti-realist stops short of accepting realism as the best explanation for the success of theories and hence does not give up his anti-realist position. At this point, it becomes clear why the "no-miracle" argument is question-begging and cannot settle the argument between realists and anti-realists: both may be willing to *accept* best explanations, but the anti-realist never asserts the *truth* of the explanations she accepts and so will not accept truth as the best explanation for success. Furthermore, the argument that scientific theories can fail does not refute anti-realism. It merely supports what

[7] Keep in mind that I assume that realism is committed to a correspondence theory of truth by definition.

[8] Underdetermination claims that two theories can both be empirically adequate while making different claims about reality.

[9] Larry Laudan (1996) provides details on the problem-solving approach to scientific progress.

I have dubbed "ontological realism", that is, the view that there is an external reality, which can be *in*compatible with our theories. However, it does not show that those theories which *are* compatible with the external reality are such because they are "true" or "realistic".

The arguments given in this section show at minimum, that the traditional justifications for realism cannot settle the dispute. Let us see if Mäki and Lawson have something to add.

Against Mäki's pragmatic justification for realism

Before criticising some of Mäki's arguments in support of realism, I should state that I accept many of his arguments and generally share his point of view—except for its realist branding. I welcome his bottom-up approach, I accept his distinction between "realism" and "realisticness", and I accept his point that many assumptions in economic models serve the tractability of models rather than their epistemic value. His arguments concerning these points are careful and convincing which is why they do not have to be repeated here.

The main point against Mäki's usage of the term "realism" is that it is merely a brand-name. Mäki explicitly admits that many other methodologists contribute to the realist project, even if they do not do it "under the banner of realism" (Mäki 2007, 438). This, of course, raises the question if the term "realism" as Mäki uses it is informative at all. The main problem when trying to refute Mäki's realism is the lack of a real defence that could be attacked. His lack of the defence is evident in his attempt to defend realism against McCloskey's postmodernist (McCloskey 1985) charges in opposition to the very notion of an external truth:

> In my alternative realist account of rhetoric, the world and truths about the world are not dependent on persuasion amongst economists and their audiences. I reject the presumption that the occurrence of rhetorical persuasion alone rules out the possibility of attaining and communicating persuasion-independent truths about economic reality (Mäki 2009a, 91).

Instead of defending realism with arguments Mäki admits beginning with the *intuitions* of a realist Mäki (2009a). He then proceeds by showing how much of the economics literature can be rendered intelligible by his realist interpretation. I am the last to doubt that Mäki is immensely successful in this, but I doubt whether this really is a justification for realism instead of preaching to the already converted (Schliesser 2010). Mäki's work does show that realism offers a good way of talking about problems of economic methodology. However this is not enough for refuting anti-realism. Now, if Mäki wants to defend his brand of realism pragmatically, he needs to show how his version of realism would lead to an improvement of economic research and which standards it would specifically employ apart from standards that are compatible with anti-realism such as problem-solving capability or empirical adequacy.

The lack of this discussion in Mäki's work and, as I would say, the impossibility to show specifically how realism would change economic research, makes a *pragmatic* justification for realism difficult to provide. Mäki, at best, gives reasons which show that it is sometimes just natural to assume an external world and economic models relating to it and the realist can talk about unrealistic models that do or do not capture features of the world. Here, however, the anti-realist would talk about making assumptions that diverge from our current believes about the world, but nonetheless make successful (structural) predictions and by this, offer plausible explanations.

Mäki uses his realist rhetoric to argue against mere derivational unification (deriving more outcomes from the same set of premises) and in favour of ontological unification—establishing more "ontic unities" between phenomena, i.e., showing that they are of the same kind (Mäki 2009a). This sounds convincing, but is it really a normative guideline that differs substantially from what an anti-realist would advocate? As long as realism does not provide a unique standard to distinguish the two modes of unification, we are left with commonsense arguments that are not opposed to anti-realist positions.

This, of course, undermines any normative thrust for realism as we are still left with anti-realism-compatible standards such as empirical adequacy plus some pragmatic values like simplicity, fertility, modesty, and conservatism. Mäki often speaks about the way the world works (www) constraint (Mäki 2009b), which refers to economists convictions about real causal connections in contrast to their model results. But this is hardly a constraint at all if we cannot know when it is met. In another recent paper Mäki seems to mean that the "www constraint" is nothing more than the idiosyncratic ontological intuitions of economists (Mäki 2009a). This is of course unproblematic for the anti-realist, because it is only a consistency criterion and as such is a far cry from making ontological claims in a stronger sense.

As I said, Mäki pragmatically justifies his realism as a powerful instrument of criticism for economic models (Mäki 2002). To me, the issue seems the other way round: realism is the less critical methodology when compared to anti-realism because it allows talking about truth where anti-realism suspends judgement on this matter. Mäki's recommendation for developing useful surrogate models instead of getting lost in internal formal analysis or his suggestion to check models against commonsense intuitions can be kept without subscribing to realism of any form.

Against Lawson's pragmatic justification for realism

Now let us see how Tony Lawson's critical realism scores against critical scrutiny. Where Mäki's work is rather neutral towards economic methodology, Lawson intends to overthrow economic orthodoxy. If one is inclined to accept the methodology of mainstream economics as it is and therefore does not share Lawson's view that the search for observable event regularities fundamentally contradicts the ontology that underlies social processes, there is little reason to follow his demand for more realism. And, even if one disagrees with much that is going on in mainstream economics there is no need to accept Lawson's realist critique. It is important here to keep in mind that Lawson proposes a normative *methodological* realism. In his view, economics should deal with the real forces that move societies and these cannot be modelled in the deductivist style. As Jack Vromen (2004) notes, where mainstream economists cherish elegance, simplicity, parsimony, tractability, unifying power, and the like, Lawson wants to assign greater weight to other epistemic virtues such as truth, or realisticness, credibility, and plausibility.

But is Lawson justified in demanding this? There are at least three reasons why I disagree with his position: first, we cannot know what the "real forces" are; second, his proposal can be turned against any form of idealisation; and third, it is doubtful whether mainstream economics is well characterised by Lawson's interpretation of the term "deductivism" at all.

I will not deal in much detail with the first point here, as I laid it out already in quite some detail in my discussion of Mäki's realism. It is important to note that this point is even more crucial for Lawson because of his strongly normative orientation. Lawson urges economists to deal with the true and essential powers, but fails to show how anyone can have such knowledge apart from criteria that are acceptable for anti-realists as well. Lawson argues in the typical question-begging way that characterises the debate between realists and anti-realists; he accuses anti-realism of ignoring the central problem of realisticness (Lawson 2001). But a main conviction of anti-realism is to reject the idea of talking about realisticness (in the realist's sense), and therefore this is not an argument against anti-realism at all.

Second, let me grant that Lawson's continuous demand to search for the real structures in inherently open social systems may lead to a more realistic and detailed description, but taken seriously it is headed against many forms of abstract theorising and idealisations. There are many theories that would have to be abolished right away if Lawson's normative realism was uniformly accepted. Just think of formal decision theory, game theory, any theory employing folk psychological reasoning, any form of hypothetical contractarianism and even political liberalism, because they are all admittedly based on unrealistic assumptions.

It is questionable, whether looking for the real essential powers that drive human behaviour will soon lead to theories of any use for economic problems. It seems more likely that such a procedure will set-off a quest into the mysteries of the human brain and the freedom of the will. Lawson does not promote this, but takes his favourite project called "social ontology" as a starting point. The sustained concern with social ontology is bound to realism by definition in Lawson's project (Lawson 2001). But is Lawson justified in his demand that economics should be reoriented to become a science based on social ontology? This can be denied at two different levels: first, it is not obvious his social ontology gives us a realistic representation of the social world. Surely, the attempt to incorporate our commonsense knowledge about social systems (e.g., the claim that social processes are dynamic and inherently open processes) into the fundament of economic theory will make this fundament more realistic by our commonsense standards. But again, there is no viable criterion to judge in which sense a "reoriented economics" that is based on social ontology approaches or mirrors an external reality except for the notoriously vague commonsense. Or, as Wade Hands puts it, "critical realists […] offer no unique method […] that gives us access to those enduring structures" (Hands 2001, 327). Besides that, it is even doubtful if Lawson's ontology that assigns social structures an individual-independent existence is indeed more realistic even by commonsense standards.

Second, for the sake of Lawson's argument let us accept that an economics that depicts the inherent dynamics and openness of social systems fits better into the totality of our current beliefs than the mainstream mechanistic picture. This fit is surely not an absurd standard for "realism" in economics. But is it a helpful normative guideline to improve this fit? I have my doubts. Also at the methodological level more realism may not be helpful, because the increased detail of research based on "social ontology" is not likely to be a useful basis for theorising, because the emerging picture is too "messy" for that. While a deterministic picture of humans as rational agents may be false, it can be fruitfully so. To be sure, Lawson would deny this because he thinks the whole project of mainstream economics is on the wrong track. Alas, this fundamental assumption of his work is not carefully argued for. Lawson merely provides a collection of critical voices and adds the claim that mainstream economics is not successful with accommodating the data (Lawson 2001). This is at best only a half-truth: surely economics is very unsuccessful at predicting the next financial crisis or even the growth of the GDP for more than one year. But on the other hand there is a plethora of well-confirmed conditioned predictions of tendencies and progress in their prediction (during non-crisis situations) without the need to refrain from the underlying "deductivist" structure. Therefore, Lawson is not justified in completely rejecting the mainstream research programme. Of course, he is free to start his own project of critical realist economics that is based on social ontology, but as long as there is no agreement about the mainstream being in disarray, so the only escape would seem to consist in changing the *goals* entirely, and not accepting (even conditioned) prediction as one of them, Lawson will hardly gain many adherents. So even if we accept that Lawson's ontological approach is more realistic, it does not follow that it is pragmatically convincing and should be adopted.[10]

[10] This is of course the typical situation with any paradigm shift. Lawson is aware of this and therefore he mainly addresses those who accept that the economic mainstream is in inescapable disarray. Note that Lawson does not intend to use his ontological research for building an alternative economics with it. Rather he wants to support existing heterodox schools by showing that their foundations are ontologically more realistic than those of mainstream economics. His project is essentially about improving heterodox economics by reinterpreting and refining their presupposed ontological commitments. See Lawson 2003, part III.

Now let us turn to the third point, namely the question whether mainstream economics is adequately characterised by Lawson's label "deductivism". As he describes it, deductivism is necessarily committed to a notion that characterises scientific laws as observable event regularities. This is a great misunderstanding. The mathematical-deductivist style which is admittedly often used in mainstream economics does not commit economists to a "flat" ontology that forbids any talking about underlying structures that causes event regularities to occur. As Vromen (2004) notes, economists try to look for more than just event regularities and are even encouraged to do so by Friedman's (1953) classic methodological manifesto. Despite the common usage of mathematical deductions, mainstream economics aims at uncovering underlying structures of the social world—they do this by devising an axiomatic theory that offers a possible explanation for the observable data.[11] Instead of calling this method deductivism one is equally justified in calling it abductivism, for abduction is precisely the development of a theory trying to explain the facts. Mainstream economists of course reject the interpretation that they have found and even that they should find true and realistic underlying structures, as Lawson demands.

There is another confusing point about Lawson's sharp distinction between underlying structures and event regularities. If one accepts (as I argued most economists do), that scientific laws are not about the event regularities but rather about the underlying structures that cause them, one can still continue to dig for event regularities by arguing that an underlying structure must somehow show up in the empirical data. This would seem to imply that Lawson is much closer to mainstream methodology that he is willing to admit. One of his main points is the denial of strict event regularities in open systems, which he takes as an argument against deductivist modelling. He prefers "demi-regularities". Now it is hard to believe that mainstream economists would really insist on the strictness of the regularities in question and would reject searching for demi-regularities. Surely Daniel Hausman's (1992) characterisation of economic laws as tendency laws may indicate that the mainstream view is not all that distinct from Lawson's view.

There are many points in mainstream methodology that resemble aspects of Lawson's critical realism, but the realist parts of his methodology do not demonstrate why realism is justified or preferable

[11] See Reiss 2004, for a similar view.

to a more modest (anti-realist) methodology. Lawson commits the realist fallacy that assumes higher realisticness (even if seen as descriptive accuracy of the assumptions) should be an end in itself and by this excludes many forms of theorising that are commonly accepted to be useful or successful. This is the main reason why his plea for more realism is pragmatically unconvincing.

CONCLUSIONS

This paper has covered a lot of ground. Now it is time to step back and draw several conclusions: is realism pragmatically helpful for theory appraisal in economics? It should not surprise the reader that I answer this question negatively. If scientists (in contrast to philosophers) want to assess theories, they almost always want to know how well they work, not why. The on-going battle between realism and anti-realism in traditional epistemology can be separated completely from issues pertaining to theory appraisal. Even if there was a conclusive proof in favour of scientific realism this would still allow for a purely instrumental way of assessing theories, i.e., deciding how well they are suited for solving given problems, since this question can be completely separated from their truth-status. Put slightly differently, if one wants to make normative statements, pragmatic reasons are needed, however, as I tried to show above, it is difficult to defend realism on pragmatic grounds as adopting realism does not lead to normative implications that are unavailable to the anti-realist.

Such weaknesses notwithstanding, it should be clear that this does not imply that there is nothing acceptable in the realist's prescriptions, even if they stem from the wrong reasons. For example, within the assumption debate, the realists carefully distinguish between assumptions that isolate real factors and others that merely serve the tractability of economic theory. A certain type of anti-realism may accept the message that it is important to filter out the crucial, the fundamental or the necessary assumptions of a theory even if it would hesitate to call them real. Such a procedure could be called "anti-realist ontology" as it is a venture into the status of the very fundamentals of economics and by this it would save the lessons from one of the realists' preferred projects, without committing to a version of ontological realism (as defined above).

Another possible form of anti-realism may even agree with Mäki's recommendation of developing useful surrogate models for analysing the real world instead of playing with substitutes, but in contrast to Mäki, the anti-realist would not ask whether a model is representing "the real world" but would focus on its ability to shed light on real problems. If the problem to be solved is one of policy-consulting, it should be clear even to the anti-realist that research on the formal aspects of some general equilibrium model can become a dangerous substitute for practically relevant economic research. However if some formal aspects are indeed the problem a scientist wants to deal with, the anti-realist must accept this and cannot urge her to concentrate on surrogate models. A type of anti-realism could indeed accept a kind of "as-if-realism", which accepts many arguments and terminological points, but rejects the interpretation that theories or parts of them are literally true.[12] With this in mind, the anti-realist could actually talk about more "realistic" assumptions when he uses a coherence theory of justification instead of a correspondence theory of truth.[13] The debate about realism against anti-realism would then be merely about semantics bearing no pragmatic implications whatsoever. Then, the more realistic assumptions would be the ones that fit better to the totality of our current beliefs.[14] It is however another main point (that I argued for above), that more realistic assumptions are not always the better ones, but that we should rather look for adequate idealisations for the problem at hand instead of mechanically heading towards more realisticness. If one accepts these arguments, it is difficult to defend realism pragmatically as a critical therapy for economics. There are forms of anti-realism that can do the same, but are far more epistemologically modest concerning the ontological status of theories.

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[12] A similar argument is made in Carnap 1950.

[13] The addition of a semantic correspondence theory of truth to a coherence theory of justification is in fact the only feature that clearly distinguishes Mäki's realism from the anti-realism presented here. See Peter 2001.

[14] This suggestion is inspired by Quine and Ullian 1970.

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Erasmus Journal for Philosophy and Economics,
Volume 4, Issue 1,
Spring 2011, pp. 42-52.
http://ejpe.org/pdf/4-1-art-3.pdf

Puzzled by realism: a response to Deichsel

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Keywords: realism, anti-realism, truth, models, economic methodology
JEL Classification: B40, B41, B49

No realist project in and about economics is close to completion. There are many open issues that remain to be addressed and resolved. Simon Deichsel (2011) has written a healthy challenge that should offer some useful inspiration to anyone interested in assessing and perhaps contributing to the realist projects. He argues against realism and in support of some sort of anti-realism. My response first deals with some conceptual issues regarding the very ideas of realism and anti-realism. I will then discuss the role of pragmatics in relation to truth. Finally, I will address the issue of justifying realism—Deichsel's title, after all, suggests his challenge is directed against what he calls the pragmatic justification of realism. My remarks are both brief and selective.

REALISM, ANTI-REALISM, AND SUSPENDING JUDGEMENT ABOUT TRUTH

Deichsel defends what he calls anti-realism against realism. It is important to see how he defines "anti-realism" and that he does it disjunctively (Deichsel 2011, 24). Accordingly, anti-realism is the thesis that we should:

> **[1]** "suspend judgement on the truth and truth-worthiness of our theories" or
>
> **[2]** "avoid talking about the truth of theories altogether" and we should do so
>
> **[3]** "in order to minimize the confusions that surround this concept" (that of truth).

Each of these elements requires attention. Element **[1]** is particularly interesting when presented as a defining feature of anti-realism. It so happens that it is precisely this idea that I have emphasized elsewhere as (a) compatible with realism, and (b) important for realist accounts of some disciplines at some stages of their development (e.g., Mäki 2005).

Let me explain. (a) First, **[1]** is compatible with what I have called minimal realism for which it is enough if a theory has a chance of being true, and that it is true or false in virtue of how the world works. I take anti-realism to deny this and to claim that theories have no chance of being true in this sense: either no talk about truth makes sense or truth should be conceived in terms other than how theories relate to the world (such as usefulness, coherence, or consensus). (b) Second, there are many situations (fields of inquiry, disciplines, stages of their development, and so forth) in which one should not rush to pass judgement about the truthfulness of a theory; one should rather suspend judgement, sometimes for long periods of time. One is not entitled to pass judgement because of the high degrees of epistemic uncertainty characteristic of these situations. The reasons for uncertainty can be many, such as the subject matter being very complex or otherwise hard to access; the discipline being at its formative or explorative stages of development; research being heavily shaped by commercial or ideological interests; and so on.

Whatever the reason for uncertainty, in order to be able to suspend judgement on the truth of a theory in the first place, one must presuppose a minimal realism about theories having a chance of being true or false. This is independent of whether we are in an actual position to pass judgement. In short, I do not consider **[1]** an anti-realist principle at all. It is rather a realist principle well suited for research fields in situations characterized by severe epistemic uncertainty. Even radical scepticism—suspending judgement indefinitely—would be compatible with realism.

There are of course many notions of realism available, and while Deichsel acknowledges this, it often seems he wants a realist to subscribe to something stronger than just minimal realism. He would like the realists to tell how epistemic access to the real world is ensured—what precise criteria, procedures, and standards to apply so as to be able to pass judgement. So it seems he would like me to subscribe to the possibility of what he calls "strong epistemic realism".

But this is exactly what I have explicitly refused to advocate as the only sensible and defensible version of realism.

When an anti-realist suspends judgement in the vein of Deichsel, **[1]** seems to suggest that the judgement is about truth rather than something else. And as I said, this presupposes minimal realism about truth. But then saying that realism "allows talking about truth where anti-realism suspends judgement on this matter" (Deichsel 2011, 34) is a little confusing given that one can obviously talk about truth without passing judgement. One does not need a lot of "talking about truth" in order for those suspended judgements to be about truth.

What about **[2]** and **[3]**? Deichsel suggests that anti-realists avoid talking about truth in order to avoid confusions around the concept of truth. I am not attracted by this disjunct either. There are many confusing concepts around. Think of value, utility, preference, rationality, wellbeing, coordination, equilibrium, market, institutions (and economics!); or think of causation, explanation, theory, model, justified belief (or empirical adequacy and problem-solving capacity, Deichsel's favourites). Should we (economists, philosophers of economics, or others) avoid talking about those things just because there is confusion around the concepts? Should we surrender rather than meet the challenge of bringing light to darkness? Should we take the easy way and avoid the hard task of trying to remove or minimize confusion? No, we should not—regardless of whether we are realists or anti-realists. Scientists talk about truth and falsehood and no doubt often do it in a confused and confusing manner. But I take it as the task of philosophy to remove or reduce conceptual confusion. With respect to truth talk, philosophy is nowadays in a much better position than some decades ago to do this thanks to the recent resurgence of philosophical interest in theories of truth (see, e.g., Alston 1996; Vision 2004).

TRUTH AND PRAGMATIC MATTERS

As I see it, truth is not pragmatic, while pragmatics plays very important roles in the search for truths. Whatever we take the relevant truth bearers to be—such as thoughts, beliefs, sentences, propositions—they are true or false not in virtue of whether they are useful, convenient, justified, rationally acceptable, warrantedly assertable, persuasive, credible, collectively agreeable, or generally in virtue of having any such pragmatic property. Very roughly, truth bearers are true or false in virtue of the way the world is. An assumption of increasing returns in an industry is true if the returns are increasing in that industry. A model of a real-world mechanism representing it as a positive feedback mechanism is true if the mechanism is a positive feedback mechanism. The assumption and the model are not true or false based on whether they are useful or convenient or persuasive, whether evidence is taken to support them or the research community generally accepts them as solving research problems.

Even though I do not take truth to be pragmatic, it makes no sense to talk about truth in scientific inquiry without simultaneously talking about pragmatic matters. The relevant notion of truth is that of relevant truth. And relevance is pragmatic: whatever is relevant is so relative to goals, purposes, practices, questions, problems. This means relevant truths are relative to, or constrained by, purposes and problems, questions and quandaries. No truth is a relevant truth if it fails to serve a set purpose or to answer a posed question.

The notion of relevant truth has two important consequences. One is that it helps see why and how all theories and models necessarily represent only very limited and selective aspects of some subject matter, and can do so truthfully. *The correct selection or isolation is a function of the questions and purposes served.* One isolation serves one purpose, while another serves another purpose. Some questions can be answered in terms of very simple models, while other questions may require very complex models.

It is a mistake to think that a richer model is always more truthful per se. A related mistake is the common belief that a model can be taken closer to the truth by de-isolation, by relaxing its unrealistic assumptions and replacing them with more realistic assumptions, and in this way incorporating previously missing details. As I have frequently argued elsewhere (e.g., Mäki 2011a), a realist should be fully comfortable with simple models and unrealistic assumptions provided they serve good purposes such as the acquisition of relevant truths about simple facts of the matter. Therefore, it is not at all an anti-realist privilege to maintain that "more realistic assumptions are not always better ones" (Deichsel 2011, 39).

I also do not see why Deichsel thinks it is a "realist fallacy" (that he attributes to Lawson) to assume that "higher realisticness […] should be an end in itself" (p. 38). Realists should not commit such a fallacy. Many relevant truths can be attained—and often can *only* be attained—with lower degrees of realisticness. Naturally, this must be understood with a qualification that acknowledges the ambiguity of '(un)realisticness': unrealistic (*sensu* A) models can be realistic (*sensu* B).

The second important consequence of focusing on relevant truth rather than on truth per se is based on the recognition that relevance is a function of purposes and questions and that there are *a variety of possible purposes and questions that themselves can and should be critically assessed*. In order to be relevantly true, a truth bearer must be true and must serve a given purpose; relevance provides a link between truth and purposes. This means that a claim to relevant truth can be critically examined by separately raising questions about truth, about purposes, and about relevance. So if you want to challenge an economic model, or rather a family of models, you can ask (i) whether the models are true of their target; (ii) whether the models serve given purposes; and (iii) whether important purposes are being served. This simple classification gives us three forms of failure and helps us to be more focused in criticizing exercises of modelling. For example, within this framework, one can proceed to diagnose the alleged failure of macroeconomic models with respect to the present economic crisis, tracing the failure to its sources.

Deichsel seems to think of science in terms of problem-solving and that this might somehow speak in favour of anti-realism. So let me put forth a few remarks on this. Problems are in the family of pragmatic matters that provide criteria of relevance. But just to talk about problems and problem-solving in general sounds too abstract. All inquiry is problem-solving of some sort, but this alone is not very informative simply because problems come in so many different varieties. At one end there are problems related to the existence of an entity or a numerical value of its property, while at the other end there are problems that, say, relate to the formal details of a mathematical technique.

Varieties of problem-solving are differently related, if at all, to the big ambition of resolving the riddles of the real world. A realist would ask questions about this relationship, granting that there are many legitimate problem-solving activities that are only very indirectly related to the big ambition and that it is often difficult to determine whether they are so related at all. It is not clear to me on what basis an anti-realist would ask such questions if science were conceived merely as generic problem-solving.

JUSTIFYING REALISM?

It is important to see that "justifying realism" remains ambiguous as long as nothing more is said about the roles and goals of realism. One does not attempt to justify realism per se, one rather justifies realism in relation to the roles it plays and the goals it might help to attain. It is one thing to consider whether realism provides a correct (descriptive) account of economics. It is quite another thing to ask whether realism can somehow be used for making economics better.

It seems that Deichsel officially focuses on the latter role of realism, which he then takes to require what he calls a "pragmatic justification" for realism. However, even though this is his official focus, he also extensively deals with the former role and the associated justifications, but fails to clearly connect the two roles with one another. This is important since this connection is the key to seeing my weak version of the "pragmatic justification" of realism.

One example of considering realism as a philosophical account of science is Deichsel's discussion of the no-miracle argument. This is part of the standard literature on scientific realism in the philosophy of science. In the standard accounts, scientific realism is presented as a strongly pro-science philosophy. It is presumed that science is a great success story, manifested in its predictive and technological achievements. Scientific realism is offered as a philosophy that explains this fact and thereby removes the apparent miracle of success. Scientific realism—defined as the claim that science has mostly gotten its theories true of the unobservable world that exists mind-independently—is presented as the best explanation for why science is successful. Because realism best explains a property of science, it is the correct description of science. This is abductive inference applied in philosophical inquiry.

I have argued elsewhere that this argument is of little relevance in the case of economics—simply because there is no obvious fact of success to be explained. Yet most of my attempts to defend realism also deal with the first (descriptive) role of realism in regard to economics, and do so without appealing to the no-miracle argument.

One of my goals—that Deichsel fails to acknowledge—has been to check whether a scientific realist account of economics is feasible. This part of my work largely relates to the debates over realism in the general philosophy of science. In these debates, some contributors have argued that scientific realism is an adequate philosophy of parts of science only. Indeed, it turns out that standard formulations of scientific realism are hospitable to successful physical sciences, while the social sciences threaten not to be accommodated: the latter are neither obviously successful nor do they deal with mind-independent unobservables, and the like. I have argued that scientific realism must be reformulated so as to make it more encompassing. This project not only has shown what modifications are needed in scientific realism to make it more broadly applicable, but it has also highlighted interesting and important differences between (families) of scientific disciplines. This has been part of my larger project on interdisciplinarity: scientific realism provides a philosophical framework within which disciplinary diversity can be examined (see Mäki 1996; 2005; 2011b). This is a descriptive project in regard to economics and other disciplines, but at the same time, it has consequences for how to improve our *philosophical* understanding of science by way of acknowledging disciplinary diversity (see Mäki 2011c). One might think that insofar as realism plays either or both of these roles with success (illuminating scientific diversity and improving the philosophical understanding of science), this will provide support to it.

This last observation relates to another ambiguity in "justifying realism": the very idea of justification can be taken to mean a number of different things. It is not fully clear to me what Deichsel takes it to mean. Given that his general suspicion seems to be that I have not given arguments for realism, one could infer that he has a very stringent view of what counts as justification. I have provided arguments that support scientific realism or at least show that scientific realism is compatible with certain important facts about economics, but this may not be strong enough for Deichsel, given his implicitly strong notion of justification. He may expect to see arguments that show why realism is *necessary* for accomplishing the tasks assigned to it. I am not sure my arguments have this much power, but I am convinced they do have some power—enough to justify calling them justifications.

Many of the arguments I have developed over the years have the structure of even-if arguments. I have sought to argue that even if this or that feature of economics (or its parts) is granted, there is no compelling reason to adopt a non-realist or anti-realist view of the discipline. This stands in contrast to what one might expect or what has been argued by some commentators. Even if economics uses models with false assumptions… Even if the predictions yielded by economic models often fail, even miserably… Even if economics deals with highly formalized mathematical structures… Even if the economy and the scientific study of it are socially constructed… Even if rhetorical persuasion plays an important role… And so on. These are arguments against the necessity of anti-realism once those features are granted; or in other words, the arguments show that those features alone are not sufficient for anti-realism. At the same time, they are arguments in support of the *possibility* of realism about economics. They rule out arguments against realism rather than provide direct supportive arguments for realism. Yet I find it natural to say that ruling out certain arguments against realism is a way of supporting realism. To provide support is to provide justification. But it is not to prove, or to justify beyond any further doubt or question.

These arguments do not constitute what Deichsel calls pragmatic justification. So it is somewhat incomplete to say that "Mäki's justification for taking a realist position is pragmatic insofar as he fears that giving up realism 'would result in the worst kind of complacency' […] I call this a pragmatic justification, because it focuses on the good consequences that an adoption of realism would have" (Deichsel 2011, 24). His question is "whether Mäki can live up to the task of improving economics by means of his realism" (p. 27).

In pursuing a descriptive philosophical account of economics in interdisciplinary comparison, and in contributing to the revision of scientific realism in the philosophy of science, it is not my direct intention to help improve economics—and my proposals should be judged independently of such intentions or expectations. Yet I admit that this work is partly (but not completely) motivated by ideas about how realism *might* help improve economics, but these consequences are indirect.

It is also somewhat questionable to talk about "improving economics" and "good consequences" in the abstract as if these were well understood and shared ideas among people holding different philosophical outlooks—as if, that is, realist and anti-realist views of scientific progress were indistinguishable. But something like this may indeed be what Deichsel is suggesting, at least insofar as my realism and his species of anti-realism are concerned.

He asks me to show how my version of realism "would lead to an improvement of economic research and which standards it would specifically employ apart from standards that are compatible with anti-realism such as problem-solving capacity and empirical adequacy" (Deichsel 2011, 33). He himself does not expect realism and anti-realism to stand apart since "adopting realism does not lead to normative implications that are unavailable to the anti-realist" (p. 38). Therefore he says "realism" as I use it is "merely a brand-name" (p. 32). I suppose this implies that he takes "anti-realism" to be a brand-name as well.

I remain unconvinced. Consider the two distinctions I have proposed for distinguishing different research strategies and that Deichsel also discusses. One is between surrogate modelling and substitute modelling; the other is between merely derivational unification and ontological unification. The first couple highlights the importance of modelling in economics, while the second focuses on the highly valued goal of unification in economics. I always thought these distinctions only make sense against the background of some sort of realism and that a realist would emphasize the importance of surrogate modelling and ontological unification, while an anti-realist could be content with substitute modelling (a sort of problem-solving activity if you wish) and derivational unification (for which saving the phenomena and empirical adequacy will suffice).

One might say that even though the above two distinctions perhaps make conceptual sense, they do not make operational sense. There are no well-defined criteria or standards in terms of which we can tell apart the two kinds of modelling and the two kinds of unification. This seems to be what Deichsel thinks. For example, he believes that my distinction between ontological and merely derivational unification is useless without "a unique standard to distinguish the two modes of unification" (p. 33). Likewise, the ontological www (the way the world works) constraint on theories and models (one that I have claimed to have found in economic research practice) "is hardly a constraint at all if we cannot know when it is met" (p. 33). In the same vein, one may argue that there is no sensible distinction between realist and anti-realist conceptions of progress given that similar standards are being used.

What to make of this? My immediate reaction would be to say that I do not think operationism is any better as a principle constraining philosophical theorizing than it is in constraining scientific theorizing. In both cases, a realist insists on keeping apart the thing and our ways of measuring and knowing it. On the other hand, it is naturally a major challenge to develop ways of measuring and knowing and understanding things—these are the methods, procedures, criteria, and standards used in science. And just as there may be progress in theories and models of the world, there may be progress in methods and standards, and these two kinds of progress depend on one another. Moreover, just as we need many (kinds of) mutually interacting and progressing theories and models to represent and explain the world, we need many (kinds of) interacting and progressing methods and standards for building and assessing those theories (including Deichsel's favourites, empirical adequacy, problem-solving capacity, and fit with the totality of current knowledge—themselves hard to apply unambiguously). Against this background, asking for a "unique" (perhaps final and fixed?) standard does not sound entirely appropriate. My view is strongly fallibilist regarding both theories about the world and the criteria for assessing those theories as to how well they provide us with epistemic access to the world.

I do think realism is important for avoiding "the worst kind of complacency" associated with mere rhetorical games, substitute modelling, derivational unification, intellectual autism. It is in terms of realism that these practices can be (descriptively) conceptualized in the first place, and can then be (normatively) identified as instances of misguided complacency that should be avoided. Preaching realism—also by showing that most economists already share realism regardless of what their self-understanding happens to suggest—is a way of trying to bring all parties at the same table. A genuine debate cannot even begin if some participants play a very different intellectual game (a game, the realist might add, that escapes issues of accountability in trying to solve the riddles of the universe and to help us manage our ways in it). If realism can contribute to the articulation of a shared framework within which progress-enhancing debate can take place, it comes to play a role in improving economics. If this is taken to justify realism, it is not compelling enough to preclude all further inquiry and debate. I doubt such a compelling justification will ever be forthcoming.

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Erasmus Journal for Philosophy and Economics,
Volume 4, Issue 1,
Spring 2011, pp. 53-66.
http://ejpe.org/pdf/4-1-art-4.pdf

Anti-realism or pro-something else? Response to Deichsel

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Keywords: realism, anti-realism, ontology, mainstream economics
JEL Classification: B40, B41, B49

In those parts of his paper that have the clearest bearing upon my contributions, Simon Deichsel 1) elaborates various conceptions of realism; 2) declares himself an anti-realist of a specific sort; 3) seeks to identify and criticise pragmatic aspects of my justification for adopting a realist orientation; and 4) argues that his anti-realist perspective is preferable to realism.

An immediate problem with Deichsel's project, if intended as a critique of my own realist orientation, is that the sort of realism against which his anti-realism is oppositionally defined is not the version of realism I maintain. In fact the only one of Deichsel's formulations that I unambiguously accept *as a version of realism* is (in his terms) ontological realism. Realism as I understand the term is about existence. It is ontological in nature. At its most basic, it posits the existence of an 'external' reality.[1]

So understood, realism is not a theory of knowledge, or of language, or even of truth. Indeed so formulated it says nothing about knowledge or truth.[2] In particular it does not commit anyone to the correspondence theory of truth, or indeed to any other theory or conception of truth. In fact it is not a semantic theory at all.

[1] Use of the term "external" here should not be taken to exclude the *possibility* of a social reality whose existence is at least in part dependent on us.
[2] Of course, one could formulate a (truth) realism positing the existence of the truth of propositions or some such. But I have never sought to elaborate such a conception. To posit the existence of any phenomenon X is an ontological theory that can with reason be termed realism about X.

Of course I do accept notions (and the possibility) of truth, though I find the idea of a correspondence theory to be potentially misleading (there could be no literal correspondence between theory and any non-linguistic aspects of reality—see, e.g., Lawson 1997, chapter 17, for a discussion). At the simplest level (for truth is, or can be, a multifaceted and complex notion[3]), I accept an objective 'expressive referential' conception of truth whereby theories are true or not just in virtue of the way the world is; that is, our *expressions* are true or not just in virtue of the nature of the *referents* of our expressions. Clearly this conception, if it is not to be empty, presupposes that theories can refer. Perhaps we can think of the correspondence theory metaphorically as an expressive-referential account of truth. But whilst such a metaphorical version of the correspondence theory of truth does (like any other version) presuppose (ontological) realism, realism can be accepted without any acceptance even of the metaphorical version. Realism of the sort I maintain, to repeat, is not a theory of truth and it carries no necessary implications regarding theories of truth.

I might emphasise that I do not think it possible to prove the existence of an external reality. However, I think it is possible to show that my opponents and everyone else are as committed to an external world as I am, and are even committed to many of the existents to which I commit. In particular Deichsel reveals himself to be as committed as I am to the existence of discussion and debate in modern economics. He even tells us that "it is not obvious that [my] social ontology gives us a realistic representation of the social world" (Deichsel 2011, 35); and describes my "claim that mainstream economics is not successful with accommodating the data" as "at best only a half-truth" (p. 36). It does not take too much to see that in so arguing he presupposes the existence of an external reality (whatever we think of his claims), and even of a social reality that does not reduce to our conceptions.

Of course, in so arguing I am presupposing that there is intelligibility in the world, including in our linguistic communications. I cannot imagine that, or how, anyone would reasonably deny this, but it is an

[3] The word "true" has the same etymological root as the words "trustworthy" and "trust" and can mean reliable, as in a true friend. We might also talk of true believers, or true north. The essence of something is sometimes expressed by the (ontological) alethic truth.

assumption or principle worth making explicit.[4] But clearly to suppose that reality is intelligible first necessitates a commitment to realism as I understand the thesis.

So if Deichsel does not seem opposed to realism of the sort I maintain, to what kind of anti-realism does he subscribe? Deichsel's conception of anti-realism runs as follows:

> I take anti-realism as the thesis that we should suspend judgement on the truth and truth-worthiness of our theories or avoid talking about the truth of theories altogether in order to minimize the confusions that surround this concept (Deichsel 2011, 24).

Clearly, then, this is explicitly not anti- the sort of realism I defend at all. If ontological realism as Deichsel conceives it, and which I maintain, were the focus of Deichsel's anti-realism, then the latter would be formulated as something like the rejection of any theory- or representation-independent reality.[5] However the latter, as we have seen, is seemingly not Deichsel's position. His anti-realism is not an ontological doctrine at all. In fact, in a footnote to his statement on anti-realism, Deichsel adds that he does "not claim that no theory can be possibly true" only that "we should avoid talking about the truth of theories" (p. 24, n. 2).

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[4] Indeed I have. Thus in Lawson 2003, for example, I write:

> The alternative point of departure adopted is to suppose of scientific practices not that they are inevitably rational, but that they (and indeed all human practices) are *intelligible*. That is, it is accepted that all actual practices, whether or not scientific, and whether or not successful on their own terms, have explanations. There are conditions which render practices actually carried out (and their results) possible. Let me refer to this supposition as the *intelligibility principle* (to heighten the contrast with Popper's *rationality principle*, that individuals always act appropriately to their situations, see Popper 1967: 359). Thus, accepting the intelligibility principle, one strand of my strategy has just been to seek to explain (aspects of) certain human actions, to identify their conditions of possibility. Or, more precisely, my strategy has been to explain various generalised features of experience, including human actions, and so to uncover generalised insights regarding the structure or nature of reality. This of course, is precisely an exercise in ontology (Lawson 2003, 33).

[5] To the extent that we are implicitly, as I am, taking the notion of the 'external' to include the *possibility* of a social reality that may be in part dependent on but (again in part at least) *irreducible* to human representations, or at least of the representations of anyone including scientists currently examining it, then a relevant form of an anti-realism might choose to *emphasise* that the rejection of any representation-independent reality *incorporates* the rejection of any representation-independent (aspects of) social reality.

I must admit that, given this qualification along with the disjunctive (either-or) nature of Deichsel's formulation of anti-realism, I myself would be reluctant to interpret this as much of an anti-realist position of any sort. Rather his position simply seems to be that talking of truth can confuse, so if we cannot suspend making truth judgements of our theories we should, for clarity, at least avoid referring to them as true or false.

Certainly that is feasible, up to a point. For sure, on my conception a theory is true, when it is, not because of its degree of empirical support, but because of the way the world is. So the fact that a theory may be well supported by the evidence does not necessitate that it is true, certainly not in all respects, and so any caution in referring to a theory as true or even approximately true is understandable, especially in the context of scientific-explanatory work. Indeed in my own ontological-explanatory and scientific-explanatory research I do mainly talk of theories that are the most explanatorily powerful or better grounded. I do not think I ever claim explicitly that a theory that I support is true. So long as we *include* within our list of criteria of theory evaluation some that are ontological/evidential—and Deichsel apparently supports the use of criteria like "empirical adequacy", viewed as "ontologically more parsimonious" than truth (p. 30)—there does not seem a difference here. This doesn't mean that I do not believe that many of the scientific and ontological claims I make, are true, or contain significant truth (truth comes in degrees); but I tend not to mention this, and rather indicate merely some of the grounding for the claims or hypotheses in question.

Of course, it is impossible to indicate the evidential and other support for claims that are continually born out in everyday practice. Indeed, many lay people (and I suspect most of us) regularly use the terms true and false to denote representations that are continually found successfully to express their intended referents.[6] Given the highly qualified nature of Deichsel's pragmatic orientation, I assume he is not really bothered by statements like 'it is true that snow is white', or 'it is true that the grass in front of me is green', or 'yes, it is true that my name is Simon'. Technically perhaps such statements should be read as 'I believe it to be true that snow is white', and so on. But I am not sure

[6] And to the extent that most if not all claims or statements are representations, then if they are to be understood as statements they also need to be understood as representations (with associated truth values).

that such everyday comments really are capable of causing the sort of confusion that appears to be Deichsel's primary concern, at least not *in everyday contexts*.

In science, though, especially where discovery and explanation are involved, I agree there is a clear need for caution. I have used terms like realistic (which incidentally I think only a few, and mostly economists, have ever conflated with the term realism) to stand in as expressing theories or claims we take to be pervasively and perhaps without apparent exception grounded in every day experience and so forth.

The noteworthy feature of the output of modern mainstream economic modelling, in this respect, is that many features of the (believed-to-be) truths of every day social life—representations that are continually born out in practice, and which I therefore feel can reasonably be described as realistic—are regularly implicitly denied or anyway replaced by contradictory representations. So I do use terms like unrealistic (accepted-as-false or superficial, and the like) when describing the theories and models of the mainstream. Thus I have employed the term unrealistic to describe claims or "assumptions" like perfect foresight, omniscience, rational expectations, infinitely and perfectly calculating human individuals, two-commodity worlds, isolated economies, along with most of the (other) driving assumptions of modern economic modellers. Not only are such unrealistic claims everywhere apparent, it is even the case that they are mostly advanced for no better reason than their ability to facilitate model tractability.

Notice parenthetically that the terminology of 'unrealistic', 'superficial' and even 'false' is often that adopted by modellers themselves.[7] Deichsel may prefer different terms to those I use—though as we have already noted, when discussing my social ontology he is content to question whether it gives a "*realistic* representation"—but differences here, if they exist, seemingly turn on semantics and strategy at most. And none of this pertains to the positive social ontological and social scientific results I actually maintain. So whatever else Deichsel might be opposing in my contributions his anti-realism is not only *not* opposed to my basic formulation of realism, it is not even

[7] Robert Lucas writes for example: "To observe that economics is based on a superficial view of individual and social behavior does not seem to me to be much of an insight. I think it is exactly this superficiality that gives economics much of the power that it has: its ability to predict human behavior without knowing very much about the make up and lives of the people whose behavior we are trying to understand" (Lucas 1986, 425). And David Hendry is one of numerous econometricians who continually assert that "all models are false" (see, e.g., Hendry 1997).

inconsistent with the manner of my rendering of the results of my investigations.

AN ORIENTATION TO ONTOLOGICAL ELABORATION

It will be apparent from the foregoing that (I believe) we are basically all (ontological) realists. If that is so, the question can fairly be put as to why I have bothered ever to *identify* or *distinguish* my position as realist at all. What has been my purpose in doing so,[8] especially since—as Dan Hausman (1998) notes—there are few if any economists that profess to be opposed to (ontological) realism?

The answer is that, for strategic reasons, I have used the adjective realist to signal a particular orientation, namely one involving explicit, systematic and sustained attention to ontological elaboration. That is, I have argued for an ontological turn in social theory and at certain points, with realism itself being an ontological position, I identified such a stance as realist. *But in so doing I was careful to stress that I was seeking to distinguish my position not from anti-realism but from approaches in which the ontological commitments were left implicit and unexamined. That is, I was seeking to distinguish my position from others that, although realist in the noted sense, were, I believed, not being realist enough.*[9]

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[8] Notice that this is different to merely *acknowledging* a commitment to ontological realism. The latter too is essential not least because there are those who, however incoherently, seek to reject ontological realism. And the contributions of these anti-realists matter. It is this rejection of ontological realism that has been necessary to, and often even drives, the rejection of the possibility of objectivity or progress, including rational comparative assessment in knowledge, truth, human emancipation, and so forth. However these anti-realisms are positions rarely if at all to be found in economics. If they were, and were a commonplace, it would already be clear why I have chosen to *identify* or *distinguish* my project as realist.

[9] Thus in response to Dan Hausman's question as to why my project might be distinguished as realist I wrote:

> And it brings me to Hausman's second worry about explicitly labelling a project realist: that so doing 'inevitably suggests that the competing programs […] fail to be realist enough'. There is a sense in which this is exactly what I *am* suggesting.
>
> In identifying my project as realist I am first and foremost wanting to indicate a *conscious* and *sustained* orientation towards examining, and formulating *explicit* positions concerning, the nature and structure of social reality, as well as investigating the nature and grounds of ontological (and other) presuppositions of prominent or otherwise significant or interesting contributions. And I am wanting to suggest that it is precisely this sort of *explicit concern* with questions of ontology that is (or has been) lacking in modern economics. This is an absence, indeed, that I believe contributes significantly to the discipline's current malaise. In this sense of the term, in my view, most of the projects contributing to the development of modern economics are not nearly realist enough (Lawson 2003, 72).

Just as identifying some of us as cooks, or singers, or students indicates a sustained commitment to various practices associated with these positions without implying that the rest of us do not cook or sing or study (or even less that we are somehow anti-cooking, anti-singing, or anti-studying), so in identifying a sustained concern with ontology as realist I meant to signal a contrast only with those whose ontological commitments were relatively poorly elaborated; I intended no suggestion that ontological presuppositions were denied, or that some similar kind of anti-realist position was implied.

I might add that since the term ontology has, in recent years, become more commonplace in modern (heterodox) economics and indeed in social theory quite widely, I have been content to describe my basic project simply as one in social ontology.

The practice of using the adjective 'realist' to indicate an ontological emphasis is important here because it is primarily this emphasis that Deichsel seeks to show lacks pragmatic justification. I will now consider his arguments. But first let me note that even this use of the term realist clearly designates a position to which Deichsel's specific formulation of anti-realism does not of necessity stand in opposition. Even so, Deichsel's criticisms of what he identifies as my pragmatic reasons for accepting the stance that I do may be sound nevertheless. So let me now examine how they proceed. On this Deichsel writes:

> Lawson states that in some sense nearly everybody is a realist because even methodological anti-realists often accept ontological realism. For this reason he defines his blend of realism by its "sustained concern with ontology" (Lawson 2001, 168). By this focus on ontology, Lawson hopes to learn something about the nature of social phenomena, which he thinks will enable him to give better methodological advice to economists than anti-realists can. This is a pragmatic defence of realism as it concentrates on the positive consequences of adopting critical realism (Deichsel 2011, 28).

Notice, to repeat, that in my own terms such a pragmatic justification would suggest only that sustained ontological analysis is capable of providing insight that may be unavailable to those *who neglect ontology*. If the above passage does interpret those who do so neglect ontology as anti-realist this is Deichsel's terminology not mine.

Reconstructing Deichsel's argumentation a little, his critique of my contribution seemingly involves an opposition to 1) the whole endeavour in principle; 2) the way the ontological results are achieved;

and 3) the sorts of results achieved (including methodological implications).

Let me consider these three aspects to his critique in reverse order, starting with results achieved. In order to assess the effectiveness of Deichsel's criticisms here it is necessary that I first briefly sketch the features of my ontological results that are relevant to this point. They are the following.

First, and putting things momentarily in my own terms, I indicate that the familiar mainstream insistence that deductivist methods of modelling be everywhere employed presupposes (if these methods are generally to provide insight) that social reality is everywhere closed (that is, it consists of systems in which event regularities occur). And the restriction to addressing closed systems tends understandably to result in substantive economic formulations couched in terms of isolated atoms, an ontology that guarantees that closure is achieved.

Second, I argue that social reality is however not everywhere closed. So the mainstream insistence upon deductivism is a problem. However, I do not conclude that this assessment undermines explanatory analysis. For I additionally argue that social reality is structured in complex ways. Specifically, there are identifiable causal structures that underpin social events and co-produce them, structures that, I argue, are (amongst other things) emergent, agent-dependent, dynamic, and highly internally related (constituted in relation to each other).

So amongst the methodological implications of my ontological analysis is that the explanatory endeavour will often require that we seek to indentify the underlying causal structures responsible for actual events, and that reliance on deductivism as a generalised approach to social analysis is a mistake.

A further implication is that, given the open, emergent and internally-related nature of social phenomena, methods of theoretical idealisation, which rest on the initial formulation of closures as heuristic devices (involving conceptions of isolated causes or some such that might hopefully eventually be mechanistically/additively combined with others or otherwise elaborated) are unlikely to facilitate much insight into social reality; they are inappropriate methods for the ontological context (see Lawson 1997; 2009c).

So how does Deichsel proceed in criticising my assessment? The nub of his argument at this point seems to take the form of three basic "reasons" for rejecting my position. Using the terminology of "real forces" to express the causal structures which (I argue) underpin many social phenomena, Deichsel writes:

> There are at least three reasons why I disagree with [Lawson's] position: first, we cannot know what the "real forces" are; second, [Lawson's] proposal can be turned against any form of idealisation; and third, it is doubtful whether mainstream economics is well characterised by Lawson's interpretation of the term "deductivism" at all (Deichsel 2011, 34).

I have written so much on all this that I hope it is sufficient here to make just a few summary comments on each "reason" briefly in turn.

If the worry about not being able to know the real forces means not being able in principle to say anything about them, and in particular that it is not possible to rationally evaluate competing hypotheses about underlying causal structures (just as we evaluate all other hypotheses whether in social or 'natural' science, and this surely is all that matters here), then the worry is unfounded. Specific substantive theories or hypotheses about causal structures (or 'real forces') and the like, are defended, as in any other science, according to their relative explanatory power in relation to relevant empirical and other phenomena upon which the theories in question bear implication (see, e.g., Lawson 2003, chapter 4; 2009a; 2009b; 2009e).

Second, it is not the case (and certainly not a claim I make) that my "proposal can be turned against any form of idealisation", and in particular not against those idealisations formulated within natural science that can produce (typically experimental) conditions that approximate (but only approximate) closures. But my proposal certainly can be turned against most of the idealisations of modern economics. However, I cannot see that this itself is a self-evident defect of my results or position; not unless avoidance of criticism of the status quo, the hugely dominant current mainstream project, is the overriding objective. Deichsel adds, as if developing an argument:

> There are many theories that would have to be abolished right away if Lawson's [position] was uniformly accepted. Just think of formal decision theory, game theory, any theory employing folk psychological reasoning, any form of hypothetical contractarianism and even political liberalism, because they are all admittedly based on unrealistic assumptions (Deichsel 2011, 35).

The rhetoric of "abolished" is not mine. But I agree that these topics and approaches would likely attract significantly less attention if my assessment were to be widely recognised and accepted. But, to repeat, it is not much of a case against my position merely to indicate the radical or unpalatable nature of its consequences for current mainstream practice.

Parenthetically, in various places (for example in Lawson 1997; and 2009c), I do address the issue of idealisations in economics at length. In particular I identify the sorts of conditions in which methods of theoretical idealisation can be legitimately applied. I show that even where (or if) such conditions do (or were to) hold the methods in question remain both unnecessary and ultimately inhibiting. But I also argue, as an empirical assessment, that in the social realm, such conditions rarely emerge at all.

Third, Deichsel's rejection of my interpretation of the nature of modern mainstream economics as deductivist seems to rest on an assessment that mainstream economics can allow, and indeed sometimes posits, the existence of underlying structures.

But I do not deny this (see, e.g., Lawson 2003; or 2009d). That is why I characterise the mainstream project as deductivist rather than, say, positivist or empiricist (for its empirical wing). My argument rather is the following. A characteristic feature of deductivist explanation is its essential reliance upon closures (supporting real or constructed event regularities) of the sort that are a necessary condition for the sort of mathematical modelling endeavour in which mainstream economists insist on engaging. Now the relevant problem here (one of many overall) is that for any mainstream modellers also interested in causal structures the closure condition (and so adherence to deductivism) in practice *constrains the sorts of structures that can reasonably be maintained*. In effect they turn out always to be atomistic[10] (whereas, I argue, as noted above, social structures are not typically atomistic at all but are mostly internally related and continuously in transformation, see Lawson 2003, chapter 2).

In total then, I fear I do not find Deichsel's criticisms of my ontological results and their methodological implications especially compelling. How about his critique of the way the results are achieved?

[10] Further where a power is explicitly acknowledged it must be treated (unrealistically) as always exercised. For example, if a form of rationality is imputed to agents, in order for an event regularity to be guaranteed, it is behaviour that must always be treated as rational.

Here Deichsel seems to suppose that the only defence of the ontology I advance rests on 'common sense', and he then proceeds to dismiss this (his category, not mine) as notoriously vague.

This account of my defence of the ontological conception I maintain is so far from my position, that I can only infer that Deichsel missed important components of what I actually write. In my contributions I actually give various reasoned and lengthy defences of the nature of my ontological argumentation, and I can only refer Deichsel to certain instances (e.g., Lawson 1997, chapter 3; 2003, chapter 2; 2009b). But briefly, my methods employed for generating or defending the ontological results achieved include transcendental reasoning and immanent critique of contending positions. And indeed my assessment that social reality is structured, highly internally related, and so on, is defended both via transcendental analysis in terms of its relative explanatory power with respect to a range of phenomena (again see, e.g., Lawson 2003, chapter 2; or Lawson 1997, part 1; or even footnote 2 in page 53 above) as well as through imminent critique of contending ontological conceptions (see Lawson 2009b, for an overview). Until Deichsel provides a critique of such methods, there is little here for me to defend.

If Deichsel's critique of neither my ontological results nor the manner in which they are achieved is compelling, how about his critique of the whole ontological project in economics as a matter of principle? Deichsel opens the section headed "Against Lawson's pragmatic justification for realism" as follows:

> Now let us see how Tony Lawson's critical realism scores against critical scrutiny. Where Mäki's work is rather neutral towards economic methodology, Lawson intends to overthrow economic orthodoxy. If one is inclined to accept the methodology of mainstream economics as it is and therefore does not share Lawson's view that the search for observable event regularities fundamentally contradicts the ontology that underlies social processes, there is little reason to follow his demand for more realism (Deichsel 2011, 34).

This assessment is surely (logically) correct, though language like "overthrow" is not mine. The point here though is that the nature of Deichsel's argumentation as a whole seems to suggest that Deichsel is doing more than stating the logic of the case; he himself is actually "inclined to accept the methodology of mainstream economics" with its emphasis on "event regularities".

In fact, looking at Deichsel's paper in broad perspective, an assessment that there is little wrong with modern mainstream practice seems to be what is driving Deichsel's opposition to my contributions. I have already noted that my arguments against the usefulness of methods of (theoretical) idealisation in economics are not so much contested as dismissed because of their (likely) destabilising implications for mainstream practice. But Deichsel seems similarly keen to be dismissive towards specific problematic or critical implications for the mainstream throughout his paper.

Thus in addition to the various assessments by Deichsel that are sketched or summarised above, we further find dotted evaluations and statements like the following: theories of rational agents are false but fruitfully so (p. 36); within modern mainstream economics there is "a plethora of well-confirmed conditioned predictions of tendencies and progress in their prediction […] without the need to refrain from the underlying 'deductivist' structure" (p. 36); the picture that emerges from social ontology is "too messy" to support (presumably mainstream notions of) theorising (p. 36); [Lawson's] emphasis on empirical adequacy "excludes many forms of theorising that are commonly accepted to be useful or successful" (p. 38); and so on. But none of these assertions are backed up in any way; they only serve to persuade that the state of modern mainstream economics is just fine.

Parenthetically, support for the mainstream status quo seems equally evident in Deichsel's assessment of Uskali Mäki's position. Observing that "[in contrast with Lawson] Mäki is generally neutral or even affirmative concerning mainstream economic theory" (p. 27), Deichsel opens the section entitled "Against Mäki's pragmatic justification for realism" with the qualification: "Before criticising some of Mäki's arguments in support of realism, I should state that I accept many of his arguments and generally share his point of view—except for its realist branding" (p. 32).

So where does all this take us? If we put aside any semantic differences between Deichsel and myself, it is not clear that there is much necessary disagreement at the level of philosophy at all. Rather Deichsel seems to take essential issue with my assessment first of the (poor) state of health of modern mainstream economics, and then (and thus unsurprisingly) with the critical results of my ontological investigations including the methodological implications I draw for reorienting the discipline of modern economics.

As such, and bearing in mind the less than compelling case advanced against my ontological argumentation, I am inclined to conclude that it is my assessment that modern economics is far from being in a healthy state that is Deichsel's primary bone of contention. The topic on which we fundamentally disagree is the state of modern economics. This, however, is not something that is easily taken forward here and ultimately pinpoints a difference the reconciling of which has little to do with ontological realism anyway (the latter being something we both presuppose). But if the issues on which we actually differ are indeed ultimately matters of empirical assessment, not philosophical orientation, this at least points to the terms on which any further discussion can most usefully be taken forward.

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Erasmus Journal for Philosophy and Economics,
Volume 4, Issue 1,
Spring 2011, pp. 67-82.
http://ejpe.org/pdf/4-1-int.pdf

The inexact and separate philosophy of economics: an interview with Daniel Hausman

DANIEL M. HAUSMAN (Chicago, 1947) is currently Herbert A. Simon professor in the Department of Philosophy at the University of Wisconsin-Madison. He attended Harvard College, where in 1969 he received a BA in English history and literature. After completing an MA in teaching at New York University while teaching intermediate school, he spent two years studying philosophy at Gonville and Caius College at Cambridge University (UK) before earning his PhD in philosophy in 1978 at Columbia University.

Professor Hausman has taught at the University of Maryland at College Park, Carnegie Mellon University, and since 1988 at the University of Wisconsin-Madison. Most of his research has focused on methodological, metaphysical, and ethical issues at the boundaries between economics and philosophy, and he has been prominent in the development of philosophy of economics as a separate discipline. In collaboration with Michael McPherson, he founded the journal *Economics and Philosophy* and edited it for its first ten years. He also edited *The philosophy of economics: an anthology* (3rd edition, 2007). His most important books are *Capital, profits, and prices: an essay in the philosophy of economics* (1981), *The inexact and separate science of economics* (1992), *Causal asymmetries* (1998), and *Economic analysis, moral philosophy, and public policy* (co-authored with Michael McPherson, 2006). His latest book, *Preference, value, choice, and welfare* will be published in 2011 by Cambridge University Press. He is currently working on a book on the measurement of health.

In this interview, Professor Hausman offers some reflections on his approach to the philosophy of economics, and on various topics central to recent methodological discussions, such as the role of abstraction, idealizations, scientific representation, and causality in economics.

EJPE'S NOTE: This interview was conducted by Luis Mireles-Flores, PhD candidate at the Erasmus Institute for Philosophy and Economics (EIPE), Erasmus University Rotterdam, and co-editor of the *Erasmus Journal for Philosophy and Economics*.

EJPE: ***Professor Hausman, you did a BA in English history and literature and an MA in teaching before moving to do philosophy. How did you come to write a PhD thesis in philosophy of economics?***

DANIEL HAUSMAN: Well, originally I was doing biochemistry. My own intellectual strengths in high school and early college were really much more in the sciences and mathematics. But I started university in 1965, when the United States was undergoing lots of student turmoil—that and a combination of rebellion against my parents and being part of a movement committed to the view that the United States needed transformation prevented me from seeing myself simply as a scientist. I wanted to be doing something that seemed more relevant to people and their experiences.

English history and literature actually wound up pushing me in the direction of political philosophy. As an undergraduate I did a thesis on Shakespeare's play *Troilus and Cressida*, which is very much about the breakdown of the political order of the traditional late-medieval-world picture that Shakespeare was working in. And by the time I graduated, it was clear to me that I did not want to be doing English literature. I had naïve views that a revolution was coming, and I did not think I should immediately go on to graduate school. I did not know quite what to do. Initially I thought about doing some teaching as a way of avoiding getting drafted and going into the army, but I actually got a medical deferment so I was free of the army.

It still seemed that by doing some teaching I would be learning more about other parts of society, and also making contacts in preparation for the revolution. So I joined an MA in teaching program mainly for the possibility of teaching without already being certified as a teacher. Half of the program at NYU was basically teaching. I taught in the South Bronx, which at the time—and I think it is still the case—was an extremely poor area of the city. I visited some of the students' homes and, though the apartments were decent, the buildings were just horrible. You would walk through garbage two or three feet thick in the lobbies before climbing out of it on the stairways. The buildings were really quite frightening places. There was a heroin drop right in front of the school where at eleven in the morning the dealers would gather and portion out the heroin. The policeman who was usually posted by the school would leave just before eleven and come back after the drug dealers had left.

This was only for a year. I was not a very successful teacher. I was teaching 5th, 6th, 7th, and 8th graders who had many, many learning difficulties and psychological problems. I was unwilling or unable to be very authoritarian, and with almost no exceptions the only teachers who succeeded in the school were quite brutal to the kids in order to keep order. I did not have enough confidence that being brutal would do the students any good, so often my classes were semi-disastrous. I ran back to graduate school after this unsuccessful but in certain ways rewarding interlude.

I applied to Cambridge to do an affiliated degree in what at that time was called "moral sciences" rather than philosophy—there is now an undergraduate philosophy degree at Cambridge. I did a second BA precisely because I had not had much philosophy as an undergraduate at Harvard. I had done a few courses, including one with John Rawls, which was quite a special experience, and several political theory courses with Michael Walzer, who was a fantastic teacher. I also went to Cambridge for purely personal reasons: my girlfriend at the time was planning on going to England to work on English history. However there was a postal strike and she could not get her applications in, so I ended up going to England without her. That is a good example of my abilities to plan for the future.

So, I got my undergraduate training in philosophy at Cambridge. I still had no inkling that I would end up doing philosophy of science or philosophy of economics. At that time I was writing papers in history of philosophy and moral philosophy, and when I was at Columbia as a graduate student I still envisioned myself as mainly doing moral philosophy. When I started working on a dissertation, it was on the moral consequences of role theory as it was conceived of in sociology. I did not make much progress. Then I happened to sit in on a series of lectures by John Eatwell—now Lord Eatwell in England—on the Cambridge controversy. He is a wonderful lecturer. Though I wound up disagreeing with quite a lot of what he had to say when I wrote my dissertation, his account of the Cambridge controversy was very exciting. I was particularly struck with the contrast between his account of the nature of argumentation in the Cambridge controversy and what I had learned in studying philosophy of science.

This was a period when many philosophers of science were doing empirical work. Not empirical work studying nature, but empirical work studying the way scientists were studying nature. At the time, there had been very little of this done with respect to economics. The first work of this kind with respect to economics that I had learnt of was Alexander Rosenberg's *Microeconomic laws: a philosophical analysis* (1976). But I only came across this book when I was already working on my dissertation project.

Listening to Eatwell's lectures, I thought, gee, this particular controversy is methodologically very peculiar, very interesting, and this would be a rewarding topic for applied philosophy of science. It was never my view that philosophy of science had all the answers, which could then be mechanically applied to economics. I believed philosophers of science understood some things well, but I thought philosophy of science was imperfect. By triangulating between what I would learn about the economics and what I knew from the philosophy of science, I thought I could contribute to some extent to both enterprises. And that is the twisting story of how I ended up doing philosophy of economics.

Did you also have some specific training in economics?

I knew a little bit of economics at that point. I had taken one semester as an undergraduate of a year long survey course which seemed to me so stupid that it really was not worth my time. I found it very easy and of little interest. The first semester was on microeconomics and it was really at a baby level. It did not even expect calculus, but if you knew some calculus basically all you had to do was differentiate a couple of functions and you got an 'A' on all the tests. So the little bit of economics I knew at the beginning did not get me very far. Because of my political interests, I also sat in on a course on Marxian economics taught by a wonderful elderly immigrant scholar, Alexander Ehrlich.

When I started doing the dissertation I had to learn capital theory, some serious microeconomics, general equilibrium theory, and of course I studied the capital controversy, and Piero Sraffa's work which lies behind the Cambridge-England side of the Cambridge controversy. At one time I think I knew capital theory quite well. I also had a pretty good grasp of microeconomics and general equilibrium theory, and I studied several past accounts of capital theory, such as the works of Knut Wicksell and Frank Knight.

With respect to macroeconomics, I read J. M. Keynes, but that was not really knowing macro the way a student who has worked through a modern textbook would know macro. I figured out I needed to know what IS-LM analysis was, so I looked at that, but basically I did not really know macroeconomics. And related to capital theory, I studied a bit of growth theory, but it was a spotty kind of knowledge of economics. I did virtually no econometrics, and I am still not a great statistician by any means. I sat in on some other economics courses, but I did not formally take any later courses. So I am not a particularly well trained economist. I am largely self-taught.

The work of John Stuart Mill is an obvious influence on your writings. Were there any other particular texts or authors in the history of economics that had an important influence on your approach to philosophy of economics?

Apart from Ehrlich's course on Marxian economics, I might have sat in on a course on the history of economic thought, I am not certain. But I did definitely read the classic texts. Although I turned the pages very quickly when I got to all the discussion on the prices of corn, I read the whole of Adam Smith's *Wealth of nations*, and I definitely read David Ricardo and Nassau Senior.

I did not read J. S. Mill's *Principles of political economy* until later, but I read his methodological texts, and in writing my dissertation I was pretty careful about reading things in the history of methodology itself, so I read Mill, John Cairnes, John Neville Keynes, Lionel Robbins, and some of the methodological work of Frank Knight. I got reasonably well versed in the history of economic methodology, including the voluminous literature on Milton Friedman's "The methodology of positive economics" (1953). I also read some of Stanley Jevons, and I definitely read John Bates Clark, Eugen Böhm-Bawerk, and people like that, because of the capital controversy. So I certainly had more than a smattering of the history of economics.

You mention that you felt you should study the history of economic methodology, but this was at a time when there was no such thing as a clearly differentiated discipline called 'methodology of economics'. So how did you know what to read?

I do not know exactly how I knew what to read on the methodology of economics. It probably was just a matter of following the references in one author to another author. It would have been obvious enough to read people like Smith, Ricardo, and Mill, or the really important figures in the history of economics, but I do not know how I knew to read John Neville Keynes, for example. Perhaps it was Milton Friedman's reference to Neville Keynes in his essay on the methodology of positive economics. Somehow other readings put me onto it.

Already as an undergraduate student I had read some John Stuart Mill in political philosophy courses. I definitely read his *On liberty*, and knew of his *Utilitarianism*. I am sure I read his *Autobiography*—I do not know exactly why—and I remember thinking: wow, here is this really substantial philosopher who is also a really substantial economist. This is somebody who I have to look at.

The inexact and separate science of economics* (1992) *is to a great extent a criticism and elaboration of J. S. Mill's methodology of economics. Has anything changed in your thinking since the publication of your book?

In a couple of articles (Hausman 1995; and 2001), I explored a problem with Mill's views that I did not clarify in the book. Mill is very emphatic on the difficulties of learning about economic relationships by means of what he calls the "method a posteriori" or the "method of direct experience". He gives the example of trying to investigate by simply looking at data whether a tariff increase would decrease national wealth. We cannot learn the effects of tariffs by aggregate comparisons, because there are so many other causal factors apart from tariff rates that differ among nations or across time in an individual country. He is quite emphatic on this point, but I think he exaggerates the problem. In any case he is also emphatic about the need for verification, and if we really cannot learn anything from looking at comparisons of countries, then we also cannot verify the deduction that comes out of the theory that tariffs would in fact diminish national wealth. Mill is to some extent aware of this. Thus he held that if we can get an agreement between these two kinds of "evidence"—he uses that term—namely the deductive derivation from fundamental theory and what we observe, then such agreement will suffice to justify claims to knowledge.

He seems at this point to have forgotten his view about inductive Proof, with a capital 'P', that shows up in book 3 of his *A system of logic*, and in any case his view is questionable. It seems that if someone is going to be serious about learning about the economy, either using direct or indirect inductive methods, it has got to be the case that they have empirical tools to gather useful aggregate economic data that provide serious direct evidence about regularities such as those concerning the effects of tariffs. I think in fact that this is not an impossible thing, but Mill comes close to saying that it is impossible. There are some real tensions here, and I think that a successful philosophy of economics has to spell out and make room for more substantial uses of what Mill called methods of direct experience. Indeed, as problematic and flawed as different econometric methods are, I think that we can learn some things from them. But I do not think Mill teaches us much about how to do that.

More generally, I do not believe Mill solved the problems of economic methodology, but understanding his views greatly helps us to understand the problems. His views compare pretty well with the views of lots of contemporary thinkers.

In a recent seminar at EIPE, you referred to Mill's method a priori and quoted his claim that no economist "was ever so absurd" as to really believe humans are exclusively motivated by the pursuit of wealth. Economists make "an entire abstraction of every other human passion or motive" but only as part of their method. Could you clarify your position on the role of abstraction in economics?

For Mill, as I read Mill, abstraction is not the view that economists should create fictional artificial unrealistic models where they simply consider how one causal factor would operate all by itself. It is rather that economists start out knowing—and I think it is problematic whether economists do know this—that there is one causal factor which is of predominant importance with respect to economic phenomena. But I do not think he has an answer to the question of how do you know such a thing, John Stuart? That the pursuit of wealth is the predominant cause is what he grew up knowing. His father probably drummed it into him by age six.

Mill denies that his method is just a hypothetical method, yet it is to some extent hypothetical. Although economists are abstracting and simplifying, they know that they are abstracting and simplifying from the minor causes, from the lesser causes, not from the greater causes. Knowing this does not imply that economists might not sometimes get things radically wrong, because those minor causes are not completely trivial, and they can add up and falsify predictions that focus on only the effects of the pursuit of wealth. Nevertheless, on average the other causal factors would be weaker, and they may cancel one another out. So although economics is a science of tendencies, these are tendencies that economists ought to be seeing in the data. If they are not seeing them in the data, then they have failed to verify their models, and they need to go back and perhaps question whether they have captured the major causes, whether some of the things left out really are relatively minor and could legitimately be left out, or whether they have botched their model and drawn false inferences. But ultimately Mill still is really an empiricist. He insisted that these are inductive methods, and he is serious about that, as I read him.

Is that what has led you more recently to explore the behavioral assumptions of economics, the fact that traditional approaches are obscure about where these behavioral assumptions come from in the first place?

I do not think that came so much from reading Mill. It is rather that, if one looks at fundamental mainstream economic theory itself, one should have less confidence than Mill had that introspection and everyday experience justify being confident that the theory has picked out not only significant generalizations but the most important causal factors governing economic behavior. I think one really needs to raise the question of to what extent the kinds of abstractions that economists are making are useful, especially given the experimental work which directly challenges many of the assumptions of mainstream economic theory and shows not just that people do not always live up to the axioms, but that there is systematic divergence from the axioms.

If one is serious about being an empiricist about science (there are all kinds of empiricism—I am not a behaviorist or something like that, but I am an empiricist about science), then the only excuses for using such an idealized theory would be either that there is no alternative or that it really is doing valuable work for us. But there are alternatives, at least in some domains, and in various applications it is not obvious that the theory is doing valuable work. Many economists may disagree with me here, but if one takes economic theory seriously and recognizes the complexity of many of the possible circumstances in which it will be applied, the standard implications economists would like to draw from theory cannot be drawn.

Consider for example the implication of the theory of the firm that an increase in the minimum wage will increase unemployment among unskilled workers. Well, if it is a big enough increase, then (other things being equal) I am convinced. If the U.S.A. Congress were to set the minimum wage at $30 an hour, there would be lots more unemployment among unskilled workers. I believe what the theory apparently implies thus far. But if we are thinking about the actual policy alternatives, which involve relatively small increases in the minimum wage, it is not easy to derive any implications about the effects on employment, and there is typically no way from the theory to get any sense of what the magnitudes are. And the magnitude really matters. If you wound up with 0.1 percent increase in unemployment, that is going to be a small social cost; while if you wound up with a 10 percent increase in unemployment, that is going to be serious—and the theory itself is not going to really help you answer those questions.

Idealized mainstream economic theory provides a powerful fundamental framework, and I do not claim that people should not learn it. But the notion that it should have a monopoly on the way economists model economic phenomena seems to me unjustifiable. This is stated a little bit differently, but it is very similar to the conclusions I draw in *The inexact and separate science of economics* (1992).

What is your current view on modeling and scientific representation in economics?

I am inclined to think that there is a very simple and useful characterization of modeling in economic theory, though not of modeling in econometrics, which is very different. Basically I think it is useful to regard modeling as a kind of conceptual exploration, a way of using mathematical tools to ask: what if?

In mainstream economics, models are narrowly constrained by the requirement that they be consistent with a set of basic axioms. Conformity with these axioms (rationality, self-interest, profit maximization, and so on) makes something part of mainstream economics. Although particular assumptions can be relaxed, most will hold. Mainstream models not only agree on their basic generalizations, they also share certain stylized descriptions of the agents and their environment.

So economic models resemble what Max Weber called “ideal types”. Models depict fictionalized simplified worlds which are governed by certain kinds of generalizations. Specific models will contain additional specific assumptions, but at their core are the basic principles of mainstream economics and standard stylized descriptions of what the circumstances are. With the elements in place, economists then reach into their mathematical toolkit and see what kinds of things result.

There are a variety of reasons to engage in constructing models such as these and investigating their mathematical implications. One reason is that economists might think that their stylized descriptions of the circumstances actually are in some sense a reasonable approximation to the actual circumstances, that the generalizations are actually true, and that economists can then use these models to predict and explain features of the actual circumstances. Economists can also use a model (as Max Weber often suggests with respect to ideal types) as a diagnostic tool to identify ways in which reality differs from the model. There would be no point in doing that, unless economists thought that there is something significant about the model. I can talk about a number of ways in which the moon differs from a great big piece of green cheese, but there would be no point in doing that. Nobody cares how it differs from a piece of green cheese, because nobody thought it was a piece of green cheese beforehand.

Since there is such general commitment to the basic structure of neoclassical modeling, conflicts between what a mainstream model predicts and what is observed are of interest. If economists find that a model is inapplicable to some actual circumstance, that finding could in principle challenge the scope or validity of the explanatory generalizations built into the model. It will likely lead mainstream economists to look harder at their stylized descriptions of the circumstances, and it may help them to realize that they are missing something significantly different or additional about the real world. In this way current theoretical commitments can provide stepping stones toward building a better theory. Having constructed a simple model and examined its flaws, economists can construct more complicated models and, using more complicated mathematics, derive implications that are of explanatory and predictive use.

What sort of entities can be used in these three ways, namely to apply mainstream economics, to study the ways in which reality differs from what theory implies, and to develop more complex and subtle applications of theory? In my book (1992), I suggest that one should regard models in economic theories as either predicates or as definitions of predicates. (These two views are obviously different, but they are easily intertranslatable.) Of course to apply models one needs more than definitions or predicates, but that is a different question from what constitutes a model. This account is extremely simple, but I am not taken with any of the more elaborate alternative accounts in the current literature on modeling and scientific representation, where one finds almost everything called “a model”.

But in your book* (Hausman 1992), *you also present a much more specific account of scientific representation in which theoretical hypotheses are what connect economic models to reality—very close to Ronald Giere’s view of scientific representation. Could you elaborate on this account?

Yes, but that account is extremely simple too. If the model is simply a definition of a predicate or a predicate or a depiction of a hypothetical world, then it does not make any claims that are true or false of the actual world. To make substantive claims, one has to make some claim about the relationship between reality and the model. Such claims are what I call theoretical hypotheses. I follow Ronald Giere in using this fancy terminology. Suppose, for example, that you propose a simple model of rationality: someone is *rational** if and only if his or her preferences are complete and transitive and determine what is chosen. This makes no claims about actual people. Given this definition, one can offer a variety of theoretical hypotheses. One could say that all people are *rational**. Or one could say that when the moon is full, Americans are not *rational**. The model provides a conceptual resource that the person concerned about reality can put to use by formulating theoretical hypotheses.

Economists have to be willing to assert some theoretical hypothesis: that the world is just like some model, that it is not like this model, or that it is like a model in this way and not like it in this other way. But until economists assert some theoretical hypothesis they have not said anything about any actual economic circumstances. The model itself says nothing about reality. One can treat models as trivially true (as a definition), or as a predicate rather than a proposition and thus not the kind of thing that could be true or false. The assessment of the model is a question of how useful the model is. It is not whether the model is true or false.

What about cases like Schelling’s segregation models or von Thünen’s isolated state where there seems to be no intended theoretical hypothesis about a resemblance relation between the model and

reality, but rather they seem to be intended to represent some existing mechanism so as to show its workings in a particular fictional setting?

I have read about von Thünen's model, but I have not actually read von Thünen, so I would rather not comment on him. With respect to Schelling's (1969) model, I see it as addressing a theoretical question: is it possible to get racial segregation without the population being overwhelmingly racist? Can you get racial segregation where in fact you have relatively few racists? And he then gives you a story showing how that is possible. This use of the model is quite different from employing it to describe what happens, because I do not think that Schelling is committed to the claim that this model resembles reality in any significant way. Nor is he committed to the claim that the mechanism whereby we get segregation in his model is in fact the mechanism that leads to segregation within the actual world.

He is not using the model as a contrast to the actual world either. He is not saying, if you look at the model and see the ways that it does not fit the actual world that is what will lead us to notice important other things about the world. The model is instead being used to answer a how-possible question; and it is very powerful in this sense—it is lovely! Although Schelling's use of the model is different from the way in which economists might use the model of *rationality** that I sketched above, his model matches my conception perfectly naturally. He defines an artificial system, an artificial world, and is not making any claims about this being in itself true or false. He investigates its mathematical properties, and shows how a little bit of "bias" leads to very strict segregation. That is really interesting, but what it shows us is that something that we might not have thought of as possible is in fact entirely possible.

This idea of showing "that something that we might not have thought of as possible is in fact entirely possible" brings to mind David Ricardo's model of comparative advantage. But that also seems a quite different type of modeling from that employed in Schelling's segregation model.

What Ricardo shows in his model is that if country A has comparative advantage over country B with respect to the production of a commodity then it can trade with B, even if B has an absolute advantage with respect to the production of all commodities, and that that trade will be mutually advantageous. Unlike Schelling he is not just demonstrating a possibility (though he is doing that too). He is prepared to assert the theoretical hypothesis that the mechanism he identifies operates in reality—that the predicate he defines is (to some degree of approximation) satisfied by actual countries. He is inclined to make the claim that in its significant details the actual world is like his model—despite the fact that with respect to many details it is utterly different, since there are many more than two commodities, and all sorts of other possibly relevant circumstances. If, like Ricardo, you are prepared to take the leap and say that those differences really do not matter (it is not that they do not matter at all, but that they really do not make that much difference), then the actual world is like the model's world, and then you can derive the conclusion that trade will be mutually advantageous in the real world.

The model itself does not show you that result. You have to have this additional risky hypothesis that the differences between the model and the actual world are relatively minor, and if you can take the model as defining a comparative advantage world, you are then prepared to say that the actual world is a comparative advantage world. That would be a way of fitting it into the language that I used in *The inexact and separate science of economics* (1992).

The comparison between the two models is very interesting—I never thought of it—because Ricardo's partakes to some extent of the same kind of thing that Schelling's does, but there is nothing in Schelling's model which purports to show that, regardless of the institutional arrangements, whenever there are certain preferences for proximity you always get segregation, and that this is the one and only mechanism that will get you there. So Schelling is much more just saying: look, here is something that you did not think was possible before I gave you the model. Ricardo is doing that too, but he is also saying: and this is going to be the inevitable result of comparative advantages regardless of absolute advantages in any real world circumstances which in the relevant respects resemble my model.

Recurrent notions in your work since the 1980s have been: causal factors, causal judgments, causal mechanisms, causal explanation, and causal priority, and you wrote a whole book devoted to causal

asymmetries* (Hausman 1998)*. How do you understand the relation between causation and economics?

In my very first book, on capital theory (Hausman 1981), I ran into a slew of difficult causal issues in trying to think through Piero Sraffa's work. So already at the very beginning, my work in philosophy of economics pushed me to think about causation. It did not push me hard enough, because I would have done better to incorporate much more causal thinking in *The inexact and separate science of economics* (1992) and in my other work before that. In particular, rather than attempting to construe generalizations such as "People prefer more commodities to fewer" as inexact laws that contain in their antecedents vague ceteris paribus conditions, I would now argue that these generalizations be understood as stating causal tendencies and that what I construed as ceteris paribus conditions be for the most part understood as specifying the domain in which the tendency operates.

The work I have done on causation—and I have done quite a lot of work on causation—really came out of reading in philosophy of economics. Herbert Simon's views have had a huge influence, for instance, on my interest in exploring the issue of the direction of causation (e.g., Hausman 1984; 1998). If you look at Simon's essay "Causal order and identifiability" (1953), or at Guy Orcutt's "Toward partial redirection of econometrics" (1952), it is causal order that is important. They do not use much philosophical jargon, but what is really crucial about causal relations as opposed to mere correlations is that you can control events by intervening on their causes. Mere temporal ordering is not enough, because one effect of a common cause may precede another. In both Orcutt's story and in Simon's more theoretically elaborated account, what is crucial to causation is not temporal order but the direction of influence and possible control. There are many similarities with more elaborate contemporary philosophical accounts such as those defended by James Woodward (2003).

The remarkable developments in the causal modeling literature to which computer scientists, statisticians, philosophers, and economists have all contributed have also helped to shape my views on causation. The issues are, however, too lengthy and technical for us to pursue them much further here.

Some of your most recent work is on the analysis of preferences. As a closing comment, could you give us an outline of this project?

I have just completed a book, *Preference, value, choice, and welfare* that should be out from Cambridge University Press near the end of 2011 or the beginning of 2012. The book is about preferences, mainly as they are and ought to be understood in economics, but I also have some things to say about preferences in everyday language and action, in psychology, and in philosophical reflection on action and morality. In this book I clarify the notion of preferences that economists rely on and to a considerable extent defend the way economists use the notion of preference. But I am also critical of misconceptions concerning preferences that many economists and other social scientists hold.

In the economist's picture of choice and welfare, agents rank alternatives in terms of everything that matters to them. Preferences are, in this sense, total comparative evaluations. Among the available alternatives, the agent then chooses as far up the preference ranking as the constraints—such as prices or availability—allow. How far up the agent is able to go determines how well off the agent is.

In positive economics, this preference ranking governs people's choices. In normative economics, the objective is to move people up their preference rankings. The principles of positive economics are mostly generalizations concerning preferences and what they imply for choice. Normative economics is concerned with how best to satisfy preferences. Preferences lie at the core of mainstream economic theory, and my book aims to clarify what preferences are and how they figure in economic theory and practice.

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Erasmus Journal for Philosophy and Economics,
Volume 4, Issue 1,
Spring 2011, pp. 83-87.
http://ejpe.org/pdf/4-1-br-1.pdf

Review of Roger E. Backhouse's *The puzzle of modern economics: science or ideology?* Cambridge: Cambridge University Press, 2010, 214 pp.

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Roger Backhouse is one of the most insightful observers of modern economics around. Thus, one can only welcome a new book by him on the economics profession. Despite my admiration of him, and perhaps because of my high expectations, I found *The puzzle of modern economics* a bit of a disappointment. Part of that disappointment could simply be that the book is written for multiple audiences, including both academic economists such as me who are inherently interested in the material he is discussing, and intelligent lay readers whose interests are more general and who need more background. Such multiple audiences create a representative reader who is between the two.

But my reaction can also in part be explained by a substantive difference I have with him on the story he is telling. This difference is not about what modern economics is—there I think we largely agree. The difference concerns how the profession got here. He, far more than I, sees pro-market ideological forces as the driving force in the evolution of economic thought. I see internal professional incentives, which led to a departure from the Classical methodology that required strict separation of science and policy, and the adoption of a methodology that blended science and policy. In my view, this combining of policy and science, initiated by pro-government activist economists, led to a reaction by pro-market economists and a hopeless intertwining of science and ideology by both groups.

The book consists of ten chapters divided into an introduction and three parts. The introduction begins with a discussion of some recent critiques and defenses of the economics profession, both by lay people and academic economists. While the nature of these differs, Backhouse blends them together into an overarching critique that economics is ideologically tainted. He suggests that the existence of these critiques suggest a puzzle about the profession. He asks how such different views of a field can exist. He writes: "Is economics the most rigorous of the social sciences, then, or little more than the expression of a free-market ideology?" (p. 14). For the intelligent lay person, his question probably has great interest, but for me it is a bit too general and diffuse. The economics profession is multidimensional and encompasses economists with many different views. It is rigorous in some ways, un-rigorous in others. It has inherent ideological aspects in some ways and is ideologically neutral in others. Given this multidimensionality, I am more surprised by the lack of effective critiques than I am by the existence of critiques.

Part I, "Economics in action", consists of four chapters that discuss examples of where economics has recently been applied to real-world problems. This includes the role of economics in creating new pollution markets, in creating market economies in formerly communist countries, in globalization, and in money and finance. These chapters seem directed at the interested lay person. They are overviews of general issues and do not break new ground. For example, in the "Money and finance" chapter, Backhouse first goes over the basics of derivatives and Black Scholes and then discusses the Long Term Capital Management incident and UK monetary policy history, rather than discussing new consensus in macroeconomics and the empirical use of the DSGE model by central banks, topics that would have been more relevant for his academic economic readership. Surprisingly, he spends very little time here discussing the most recent financial crisis, suggesting that since macroeconomic theory will be covered in a later chapter that discussion can be put off until then.

His conclusion from these chapters is that "economics is most successful where problems are narrowly defined and that its applications is most problematic when wider issues, involving politics or social phenomena, need to be considered" (p. 97). He further concludes from these case studies that "it would be wrong either to dismiss economics altogether […] or to sweep such concerns aside" (p. 97). These conclusions are quite reasonable, but for academic economists interested in these issues, four chapters were unnecessary to arrive at them.

Part II, "Historical perspectives", is designed to allow the reader to see the bigger picture behind the case studies of the earlier chapters. Chapter 6 considers the history of microeconomics. It starts with a discussion of Lionel Robbins's definition of economic science, which Backhouse sees as an important statement of economic methodology, even though he agrees that economists have seldom followed this definition. He sees Robbins as a central figure in the evolution of economics to its current ideological structure. The lesson he draws from the chapter is that "many of the developments in thinking about microeconomics during the period since the Second World War need to be understood as the result of economists trying to be scientific" (p. 115).

Chapter 7 considers macroeconomics. It starts with an overview of the history of macroeconomics from the time of Keynes, and then discusses more recent macroeconomics in the last couple of pages. While this discussion is reasonable, it is somewhat less than the in-depth consideration Backhouse seemed to promise when he dropped the issue of economists' role in the financial crisis in chapter 5.

The second two chapters of Part II return to the title theme: science and ideology. The first of these looks at how economics has evolved in its policy views from supporting a mixed economy from the 1940s to the 1960s, to supporting a free-market economy more recently. Backhouse suggests that economists who supported government intervention were looser in connecting ideology and their analysis than were those who supported free markets. It is here where my interpretation of the history differs from his. Backhouse sees the evolution as driven by pro-market economists leading the economics profession away from a non-ideological scientific approach. My interpretation of the history is different. In my view there can be no non-ideological scientific theory which leads to policy conclusions, and it was the largely pro-government activist economists' combining of economics and science, starting in the 1930s in both micro and macro, that first departed from the Classical methodology of strict separation of policy and science. It was this separation that Robbins was attempting to defend in his well known essay; I see Robbins's definition of economics as descriptive, not prescriptive, and Robbins's prescriptive path as being the path not followed by the profession.

Classical economists, and neoclassical economists who followed a Classical methodology, handled the ideological problem by accepting that their policy views were based on values, their reading of history, their intuition, and economic science. To keep the science of economics as value free as possible, they separated the policy branch of economics from the pure science of economics. Doing so admitted that any policy view necessarily had non-scientifically derivable value judgments underlying it. Policy discussions were placed in the art of economics:

it was agreed that reasonable economists could agree about the science of economics even while disagreeing fundamentally about policy.

In the 1930s and 1940s this classical separation of art and science was abandoned, as economists started drawing policy conclusions directly from their models. Students began to be taught that economic science called for government intervention to correct externalities or to smooth out macro-fluctuations. Textbooks changed from teaching the wisdom of economists—broad arguments that integrated moral judgments, psychological insights, history, economic science, and common sense—to teaching the science of economics. The careful discussion of the limitations of models was lost, and students began to believe that policy followed directly from scientific models. This change in pedagogy is, in my view, central to the blending of ideology and economic science that currently exists.

The best of the scientific economists, such as Paul Samuelson, avoided the most flagrant connecting of scientific models and policy, but even they moved away from the careful qualifications of the formal models found in Mill, Marshall, and even Pigou. In the textbooks, the policy art and science of economics became blended into one. It occurred in both micro and macro, where Keynesian economic policy was presented as scientific, and Classical policy portrayed as ideologically motivated, or scientifically wrong. That provoked a reaction among pro-market economists and the ensuing debates hopelessly intertwined policy, models, and ideology. One could be sure that a model coming out of Yale would come to pro-government activist conclusions and a model coming out of Chicago would come to pro-market conclusions. It is highly unlikely that such models were "scientific" in any meaningful sense.

Our different interpretations of how ideology became embedded in science also affect our views of the role of rational choice theory in the process. Backhouse seems to accept Sonja Amadae's argument that rational choice theory provided an intellectual framework opposing communism. He argues that Kenneth Arrow's work on social choice showed that collectivism conflicted with liberal values, and that rational choice theory had ideological implications. I do not see it that way: those opposing communism or statism had no need for a formal scientific framework opposing communism; they could have relied on a variety of historical and philosophical arguments that had nothing to do with formal rational choice models.

The final chapter in Part II deals with heterodoxy and dissent. It argues that organized dissent in economics has been largely ineffective, an assessment with which I agree. But the reason I see it as ineffective is that it too has blended policy and science, rather than separating the two.

The concluding chapter, “Economic science and economic myth”, summarizes the argument of the book, and reiterates Backhouse’s view that pro-market policy is based on a free-market myth. I agree that it is (although I prefer the term story to myth). But what Backhouse does not emphasize enough is that pro-government activist policy is also based on a pro-government myth. Unfortunately, the complexity of the economy means that reasonable stories are the best that we can do. My methodological prescription for the profession has been and continues to be that we should accept our limitations and base policy on the best stories we can develop. But to arrive at the best story, rather than to tell two separate stories, both sides have to admit that they are debating stories not science. Currently neither is willing to do so.

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Erasmus Journal for Philosophy and Economics,
Volume 4, Issue 1,
Spring 2011, pp. 88-94.
http://ejpe.org/pdf/4-1-br-2.pdf

Neuroeconomic reductionism at work? A review of Paul W. Glimcher's *Foundations of neuroeconomic analysis*. New York: Oxford University Press, 2011, 488 pp.

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Recent years have seen a wave of interest in connections between neuroscience and the models and generalizations of neoclassical and behavioral economics. Interdisciplinary investigations of decision-making in humans and non-human animals have yielded a flagship neuroeconomics textbook (Glimcher, et al. 2009), hundreds of journal articles, and high-profile academic conferences. They have also attracted constructive and destructive criticism—not to mention charges of hype and irrelevance—from cognitive neuroscientists (Gallistel 2009), economists (Gul and Pessendorfer 2008), and philosophers of science (Ross 2008).

Paul Glimcher is one of the most creative, interdisciplinary, and philosophically inclined neuroscientists currently working on decision making. His book *Decisions, uncertainty, and the brain* (2003) put his research on primate visual decision-making in the context of a brief history of neuroscience since Descartes's work on the reflex and even included a short discussion of consciousness and philosophical zombies. Glimcher's latest, cheekily titled *Foundations of neuroeconomic analysis* (hereafter *FNA*) presents the case for his laboratory's research program seeking no less than a "partial reduction […] of economics to psychology and thence to neuroscience" (Glimcher 2011, xv).

FNA is organized into four main sections, the first of which tackles the difficult issues of inter-theoretic relations and reductionism. Here, Glimcher partially traces the history of the idea that all scientific theories may be reducible to fundamental physical theory from the logical positivists through Ernest Nagel, briefly discussing critiques from C. D. Broad, Jerry Fodor, and others along the way. Nothing he says here will be new to philosophers of science, and unfortunately there is no discussion of more recent work on reductionism from philosophers of biology or the social sciences (for a review of these issues, see Sarkar and Wimsatt 2006). To cite one example, William Wimsatt's (2007) discussion of the "functional localization fallacy"—mistakenly attributing a property of a whole system to a functionally important part of that system—may turn out to be relevant to attempts at neuroeconomic reduction. In general, Glimcher seems unaware of the bewildering variety of ways in which reduction is construed, in terms of theories, models, entities, explanations, methodologies, and so forth. Indeed, some philosophers have gone so far as to suggest that talk of reduction should simply be eliminated in favor of more precise terminology (Maclaurin 2011).

Here the crucial question for Glimcher turns out to be whether mathematically explicit theories from economics may be mapped onto those from neurobiology homomorphically, that is, preserving their mathematical or logical structure. It is well known that this cannot be taken as a plausible account of reduction in general, as a homomorphic mapping between two mathematical models need not imply inter-theoretic or intra-theoretic reduction (Schaffner 1967). For example, a single well-known set of differential equations models phenomena in epidemiology and certain predator-prey systems. While this may be biologically suggestive, it need not imply a reduction. Homomorphism between models could be taken as merely a necessary condition on successful reduction, or one might opt for a stronger condition like isomorphism, an idea originally suggested by Suppes (1957) that has long since been challenged (Sarkar 1992).

Philosophical problems aside, on this issue Glimcher is careful to hedge his bets, claiming that while "there almost certainly will be regularities that homomorphically map some economic kinds to neurobiological kinds" (p. 31), we should not expect such attempts to proceed without exceptions (genuinely emergent properties) or without *modifying* existing higher and lower-level theories as we go. What really matters is producing more predictive, more explanatory theories, and the history of inter-theoretic reductionism, in biochemistry for example (pp. 26-28), gives us empirical grounds to conclude that his explicitly reductionist research program will bear fruit whether or not strict reductions of the logical objects of *current* economic theory to *current* neurobiology are in the offing. Thus in practice, 'partial reduction' just means 'interdisciplinary synthesis'. He has already argued persuasively in his earlier work that ideas from economics can help structure our theories about what the brain does—here a higher-level theory swoops in to save a lower-level theory from absurdity (Glimcher 2003).

The remainder of the first section of *FNA* is devoted to laying out for non-specialists the theories to be connected to neurobiology: neoclassical economic theory, the psychophysics of perception (particularly signal detection theory), and the famous “anomalies” of expected utility theory (Allais’s and Ellsberg’s results, the endowment effect, and risk-seeking over losses). Glimcher’s interdisciplinary sweep serves three purposes. The first, just mentioned, is to provide compact summaries of disparate fields for non-specialists. Whether he succeeds here is not for me to say, but I suspect readers will appreciate his clear explanations and examples. It is also worth noting in this context that each chapter of *FNA* contains a helpful précis that often advises practitioners of a particular discipline to skip ahead (most readers of this journal will probably not need to be reminded of the von Neumann-Morgenstern axioms of expected utility theory).

The second goal is to provide a kind of preview of his strategy of reductionistic linkage. He does this by suggesting how the psychophysics of perception could be connected to random utility models of economic theory. The idea is that a noisy perceptual intensity curve mapping, say, concentration of sugar in solution to perceived sweetness, could be connected to a noisy or random utility curve (McFadden 1974) describing choices of hungry subjects between solutions with these sugar concentrations (Glimcher 2011, 93-98) —random utilities turn out to be quite important for Glimcher: he stresses the fact that the brain is a stochastic organ, so some of its processes cannot be accurately modeled by deterministic algorithms (chapters 9 and 10).

The third goal of section one is to motivate a rejection of the instrumentalist-behaviorist tradition in economics, the insistence that economic theories are only as good as their predictions about choice behavior (Friedman 1953; Gul and Pessendorfer 2008). On this view, whether individuals or firms actually compute expected utilities, consciously or unconsciously, is irrelevant to testing expected utility theory. What matters is that they choose *as if* they performed such computations. Glimcher calls such *as-if* theories “Soft theories” and proposes instead that we consider *because* theories or “Hard” economic theories that predict that the relevant computations *are* being performed somewhere in the brain. There is no knockdown argument against the diehard instrumentalist, however: "We can make mechanistic test irrelevant by assertion […] but that is a political rather than a scientific operation" (Glimcher 2011, 132). Rather, the only way to convince the *as if* theorist is to produce successful *because* theories.

The rest of *FNA* is an extended argument that successful *because* theories are possible, so neuroeconomic reductionism is a viable research strategy. The second and third sections concern the neural mechanisms of choice and valuation, respectively. Those interested in Glimcher's neuroscientific work would do well to skip immediately to these sections and read them carefully. In summary, Glimcher argues that our brain contains networks for valuation, mediated by midbrain dopaminergic neurons that allow us to learn the subjective value of behaviors, which feed to choice networks in the prefrontal and parietal regions, which in turn feed to motor output.

In the fronto-parietal choice network, topographically organized neurons encode, by their mean firing rates, the "relative expected subjective value" of particular motor actions (p. 242), for example moving the eyes towards a particular target. The valuation circuit feeds the cortical choice network these value-signals over actions, which are (somehow) normalized over choice sets (pp. 236-250). The valuation signal and the choice network itself have some degree of stochasticity, which can apparently be modulated by adjacent cortical neurons. Thus choice may appear more or less random, depending on contextual factors, for example the size of the choice set (pp. 246-247).

Choice occurs when firing rates exceed a certain threshold, which apparently may happen in one of two ways: either a "winner-take-all" computation is performed and the action with the highest associated firing rate is performed, or else a "reservation price" is (somehow) set by the network, and the first action whose firing rate exceeds the threshold is performed. Glimcher argues that these correspond to the "arg-max" operation of expected utility theory and the satisficing, reservation-price-based algorithms due to Simon (1955), respectively. Lingering empirical difficulties include whether the model, based mostly on studies of visual decision-making in monkeys, can be generalized to more complicated behaviors and actions, how cortical normalization occurs, and why and under what conditions the two different kinds of computations leading to choice behavior are performed.

The third section of *FNA* deals with the valuation network, where again Glimcher seeks an interdisciplinary synthesis of contemporary neuroscience with models from psychology, computational learning theory, and economics. Here Glimcher introduces temporal difference models of reinforcement learning, and he reviews evidence that these are implemented by midbrain dopaminergic neurons, particularly in the ventral striatum (chapter 13). The basic idea behind these models is that an organism learns the value of an action by predicting its expected value and then using the difference between experienced reward and the prediction (the reward prediction error) to update their expected value prediction. Recently, Caplin and Dean (2007) axiomatized reward prediction error systems and Glimcher and his colleagues found that activation patterns in the striatum follow these axioms—a major success for the neuroeconomic research program.

Activation patterns in the medial prefrontal cortex have also been correlated with subjective valuation and preference, relative to a baseline or reference-point (Glimcher 2011, 349). Upward shifts relative to the baseline firing rate (representing *gains*) have been shown to be less than downward shifts (representing *losses*), the degree of asymmetry predicted by standard behavioral measures of loss aversion. Glimcher argues that the data suggest a neural implementation of Kozegi and Rabin's (2006) models of reference-dependent preferences. The remainder of the section on valuation reviews what little else we know about how subjective values are constructed and stored, including uncertain roles for the amygdala, insula, dorsolateral prefrontal cortex, and orbitofrontal cortex. This is the most speculative section of the book: we know very little about how all of these parts of the brain work together to construct and store a subjective value signal.

There is no doubt that Glimcher has succeeded in providing, at the very least, an outline of a causal-mechanistic microfoundation for microeconomics. While it may be a difficult read at times, fans and skeptics alike will profit from carefully absorbing *FNA*. Glimcher has revived Bentham's view that "utiles" may someday be identified in the brain. However, difficult questions remain. How is the subjective value signal generated and stored? Can Glimcher's simple model of choice be extended to complex behaviors and tasks? How much of our economic agency is located *outside* of the head in the environment and our technologies? What about the role of language and symbolic thinking? Which individual and social properties will be resistant to relentless neuroeconomic reductionism? If neuroeconomic research outlives the hype and overblown criticism, hopefully we will get some answers.

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Erasmus Journal for Philosophy and Economics,
Volume 4, Issue 1,
Spring 2011, pp. 95-98.
http://ejpe.org/pdf/4-1-br-3.pdf

Review of *Economic pluralism*, edited by Robert Garnett, Erik Olsen, and Martha Starr. London: Routledge, 2009, 336 pp.

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This volume brings together in twenty accessible chapters a large number of authors contributing previously unpublished work on economic pluralism. The editors present it as *the* collection of economic pluralism for the twenty first century. This sounds very ambitious but the table of contents is promising, because it reflects pluralism in themes and approaches as well as diversity in authors' geographical origin and gender. In this review, I will try to assess to what extent the contents of the book also reflect this wide diversity.

The introduction to the volume by the three editors distinguishes first-wave and second-wave pluralists. Garnett, Olsen, and Starr characterize first-wave pluralists in terms of paradigmatic self-sufficiency, striving for an "analytically unified and self-contained school of thought whose practitioners need not engage in scholarly dialogue beyond the boundaries of their own tradition" (p. 4). Second-wave pluralists, in contrast, aspire to a pluralism derived from John Stuart Mill's arguments against the tyranny of the majority in *On liberty*; as the editors put it, "a positive valuing of a diversity of views in the minimal sense that one who is so committed would not want to reduce the number of available narratives or views" (p. 4.). This is formulated more simply by Tony Lawson in chapter seven, as "the affirmation, acceptance, and encouragement of diversity" (p. 99). *Economic pluralism* presents an overview of second-wave pluralism in three parts. The first set of chapters discusses the philosophical realms of epistemology, ontology, and methodology, whereas the second set of chapters goes on to real-world economies, and the third part discusses economics education.

The first chapter, written by Fred Lee, makes the case for second-wave pluralism by showing how different schools of thought have engaged with each other recently. He provides an insightful table with examples of publications at the cross roads of different heterodox traditions, such as post Keynesianism and feminist economics, or institutionalism and social economics. William Waller, in the third chapter, makes the same argument, but, like Lee, focuses on heterodox schools of thought rather than neoclassical or mainstream economics. The second chapter, by David Colander, makes a very different point. It proposes an 'inside the mainstream' strategy for heterodox economists in order to further the cause of economic pluralism beyond the relatively small group of heterodox economists. His argument is strategic. Colander pleas for training heterodox students to a high level in the quantitative skills of mainstream economics because "the only ones who are allowed to break the rules are those who have demonstrated a full command of them" (p. 41). I find both viewpoints appealing: Lee's and Waller's appeal for continued, and increased, mutual engagement between various heterodox traditions, and Colander's appeal for engagement between heterodox schools and the mainstream. Of course, the one position does not exclude the other, but second-wave pluralism would benefit, in my view, from a combination of the two, so that mutual engagement between a particular heterodox school of thought and a mainstream school would also be encouraged. And here we can also find successful examples in the literature, for instance between feminist and experimental economics, or between institutional and behavioural economics.

The fourth chapter, by Strassmann, Starr, and Grown brings a very different issue to the pluralist table. The authors argue convincingly that heterodox pluralists focus too much on diversity in theoretical and methodological approaches and too little on economic problems concerning the improvement of human lives, which requires a focus on gender, class, and race. They point to Geoffrey Hodgson as an example of a heterodox author who tends to ignore gender diversity, quoting only 16 women in a book containing more than thousand citations, and contrast this with Amartya Sen as an example of a pluralist economist who has always taken a gender perspective on board.

Chapter five, by Marqués and Weisman, takes us into the philosophy of science and considers the relevance of Thomas Kuhn's work for pluralism in economics. They argue that Mill's fallibilism is a more suitable foundation for pluralism than Kuhn's incommensurability thesis. Furthermore, they find Kuhn's philosophy corrosive of pluralism because "those who believe in incommensurability (even in its weaker sense) will lack the incentives for engaging in a conversation with 'foreign' positions" (p. 78). The authors strengthen their point by noting that although truth matters, knowledge is key for pluralism, because knowledge requires a clear perception of the motives and reasons which give rise to the idea one takes as true. In other words, they make a clear case for second-wave pluralism as openness and engagement with diversity.

The second part of the book focuses on real-world economies, starting off with a chapter by Greenwood and Holt arguing that development economics should go beyond concern with GDP growth. Although development economics is among the fields that tend to be most open to pluralism, because of its real-world orientation, there are still pleas for monism around. For example, in an influential recent paper Angus Deaton (2010) defends, in a strikingly positivist manner, deductive hypothesis testing as *the* appropriate method for development economics. Other chapters in the section discuss themes like equity, capitalism, and local exchange networks.

The third and final part of the book deals with economics education. This part is kicked-off by McGoldrick who emphasizes non-lecture based pedagogical practices. I fully support this approach to pluralist economics teaching, as it is foundational for getting across second-wave pluralism as really engaging with pluralism in methodology, theory, themes, and policy recommendations—a good example of pluralist economics education is presented by Jack Reardon (2009) in his edited volume on how to make economics teaching pluralist. An important methodological point is made by Butler, who argues that economics teaching should go beyond the false dichotomy of positive/normative economics. This is precisely what only very few textbooks and handbooks do, even those coming from heterodox schools of thought.

The final chapter, twenty, by Varoufakis, provides an interesting overview and self-assessment of a pluralist doctoral programme at the University of Athens. But this chapter should not have been placed at the end of the volume because it undermines one of second-wave pluralism's objectives, namely an open, unprejudiced engagement with any school of thought, including the neoclassical. The chapter clearly distances itself from neoclassical economics with characterizations of it as "mystification" and "witchcraft". By ending with this chapter, the volume leaves behind the impression that it is a critique of neoclassical economics after all, rather than a plea for second-wave pluralism.

Moreover, this ending to the volume draws attention to the lack of a closing chapter by the editors. It would have been helpful to see this volume end with a final word on the pluralism of perspectives they have offered to their readers, and on how to really move away from first-wave pluralism—which chapter twenty still resembles—to second-wave pluralism. But apart from this unfortunate ending, I find *Economic pluralism* a highly recommendable book that points at directions for furthering pluralism in economic methodology, theory, applied economics, and economic education.

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Erasmus Journal for Philosophy and Economics,
Volume 4, Issue 1,
Spring 2011, pp. 99-107.
http://ejpe.org/pdf/4-1-br-4.pdf

Review of Debra Satz's *Why some things should not be for sale.* Oxford: Oxford University Press, 2010, 252 pp.

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One of the major points of resistance that proponents of unrestricted markets have always encountered has been the repugnance that many people experience at the thought of certain goods and services being subject to commercial exchange. Friends of the free market have found—much to their chagrin, and occasionally, surprise—that merely pointing to the marvelous efficiency gains that can be achieved through the introduction of markets for these goods does not instantly dissolve all resistance. It is thanks to this stubborn resistance that, to this day, you cannot (in most jurisdictions) pay someone to stand in line for you, bear you a child, provide you with replacement organs, or bring you to orgasm.

On its own, this phenomenon might be regarded as little more than a curiosity, perhaps an interesting example of how cultural mores can constrain markets at the periphery. (After all, there was a time when people expressed equal abhorrence at the ignoble thought that individuals should be able to acquire *land* merely because they had enough money to pay for it.) The stakes were raised quite considerably, however, by Michael Walzer, who in his *Spheres of justice* (1983) argued that this sort of repugnance provides, not just an account of why certain markets are prohibited, but an all-purpose normative rationale for the welfare state. Specifically, he tried to show that the reason certain goods and services are provided by the public sector is precisely that it would be unethical for them to be provided by the private sector.

The first thing to be noted about Debra Satz's recent book is that, despite her many disagreements with Walzer, her work remains squarely within this tradition. Unlike theorists like Deborah Spar or Kimberly Kraweick, who are interested in "forbidden markets" as primarily local phenomena, she agrees with Walzer (and Elizabeth Anderson) that the moral intuitions at play in the domain of prostitution, reproduction, and transplantation are the same intuitions that justify the role of the public sector in the provision of health care, education, and old-age security. At first glance this might seem like quite a leap, so it is worth reviewing briefly what sorts of arguments are thought to be capable of carrying us across.

Walzer argued, famously, that it was a substantive feature of the goods in question that made it unethical to exchange them. Different goods belong to different socially defined "spheres", each with its own distributive logic. Thus votes are to be distributed in accordance with a principle of equal citizenship, health care in accordance with need, love in accordance with free choice, and commodities in accordance with ability to pay. Thus trying to buy votes, health care, or love, constitutes an illegitimate boundary-crossing.

There are some obvious problems with this argument, which critics were not slow to point out. The most common sort of concern, echoed by Satz (p. 81), takes as its point of departure what John Rawls referred to as the "fact of pluralism", viz. that one can expect a free society to be marked by reasonable disagreement over the values at stake in each of these spheres, as well as the appropriate principles of distribution. If, however, people assign different value to goods such as health, then it seems obvious than any principle of distribution governing such a good should be sensitive to these differences in valuation. One obvious way of satisfying this constraint is to create a market for the good, so that people can buy the amount that they want, based on their own estimation of its importance in their overall plans.

As if this were not enough, serious doubts have also been raised about the extent to which the *exchange* of goods is really what triggers repugnance, or whether people are merely reacting to the background inequality that underlies certain exchanges. In this respect, the work done by Alvin Roth (2007) on paired kidney exchange is extremely significant. It turns out that most people, while being offended at the thought of transplant organs being sold for cash, are not actually offended by the prospect of such organs being traded. Many people in need of a kidney transplant have family members who are willing to donate, yet cannot because of incompatibility. Consider the case of two patients in such a situation, each of whom has an incompatible donor, but each of whom is also compatible with the *other*'s donor. Would there being anything wrong with bringing the four of them together, in effect, swapping kidneys between the two donors? There tends not to be a strong reaction against this arrangement.

But if two people can swap donors, it does not seem unreasonable that three people should be able to do so, or that four should be able to do so, or that arbitrarily long chains of paired donors should be arranged. The end result is the creation of a barter economy for transplant organs, something that, again, most people find unobjectionable.[1] After all, it produces significant efficiency gains (which, in this case, mean many lives saved).

What is the difference between an ordinary market and this barter system? The only morally salient difference seems to be that, in the kidney exchange system, endowments are necessarily equalized, since the only thing you can use to "buy" a kidney is another kidney. The problem with being able to use cash to pay for a transplant, rather than another donated kidney, is that it allows people to take potentially undeserved advantages they have acquired in other domains of social exchange (e.g., inherited wealth, citizenship in a first-world country, and so on), and transfer it over into the domain of kidney acquisition. Thus the prohibition on markets for kidneys starts to look like an egalitarian intuition, not one having to do with the sacredness of the human body or anything like that.

To admit this, however, is to risk undermining the idea that there should be any prohibited markets. This is because (as Satz rightly observes) there is a familiar line of reasoning in welfare economics which shows that, if inequality is the problem, then the best way to address it is by making adjustments on the income side, not by interfering with particular markets. Why? Because this both permits a more effective solution to the inequality problem and allows participants to realize the efficiency gains associated with market exchange. As Abba Lerner put it: "If a redistribution of income is desired it is best brought about by a direct transfer of money income. The sacrifice of the optimum allocation of goods is not economically necessary" (Lerner 1970, 48).

Because of this, there is a very slippery slope that leads from Walzer's position directly to a view that Satz, following James Tobin, refers to as "general egalitarianism", which justifies no restrictions in principle on the scope of market exchange. To the extent that a case can be made for restricting a particular market, it will be due to 1) efficiency

[1] Some may regard this as permissible because it is an extended system of gift exchange. But this is a reduction of the communitarian intuition. If it were true, then the market itself would be nothing but a gigantic system of gift exchange.

concerns arising from market imperfections (externalities, asymmetric information, market power, and so forth), or, 2) paternalistic concern that improving the distribution of income will not result in the right sort of improvements in final outcome. (The latter sort of rationale is, of course, dubious given the "fact of pluralism".) If a market raises neither of these two concerns, then the general egalitarian would regard any repugnance we may experience as nothing but a "yuck" response, which we must learn to overcome.

The best way of describing Satz's position would be to say that she wants to embrace a fully liberal perspective, while nevertheless stopping somewhere short of general egalitarianism. Thus she accepts that, to the extent that markets are prohibited, it will be on the basis of general principles, not on the basis of anything specific to the particular good being exchanged.[2] She also seems to want the principles that do the prohibiting to satisfy a neutrality constraint. By contrast to the general egalitarian, however, she wants to offer a broader interpretation of the considerations that could justify prohibition of a market. For starters, she provides what could best be described as a generous interpretation of the egalitarian and efficiency principles. Thus she identifies four characteristics that make a market "noxious": that it produces harmful outcomes for individuals, or for social relations, or that it involves highly asymmetric information or agency, or that one of the parties exhibits extreme vulnerability.

Going through the examples she provides, however, one gets the sense that all of them could be construed as problematic from the general egalitarian view as well: "markets whose products are based on deception, even when there is no serious harm" (p. 97), (asymmetric information); "markets in urgently needed goods where there is only a small set of suppliers" (p. 97), (market power). Furthermore, the example that she gives of a market that should be restricted for egalitarian reasons, viz. "a grain market whose operation leaves some people starving because they cannot afford the price" (p. 94), is one that seems more appropriately handled by the general egalitarian remedy of income redistribution.

Of course, while the general egalitarian *might* be able to accommodate these concerns, Satz is certainly correct in pointing out that the standard version of this position interprets both the efficiency

[2] Thus Satz grants that "perhaps many of our reactions are little more than an irrational repugnance at that which we dislike" (p. 112).

and the equality principle quite narrowly. For example, she observes (quite astutely) that an enormous amount of normative work gets done by what economists are willing to classify as an externality (p. 32). Typically the set of externalities is limited to what John Stuart Mill would classify as "harms", even though this is in no way entailed by a general welfarist framework. If one looks further, one can see all sorts of cultural and social consequences of market interactions that are simply ignored in standard economic analysis.[3] For example, Satz notes that in jurisdictions where kidney-selling is legal, kidneys are increasingly used (and demanded) as collateral for loans. This is obviously an untoward effect, but one that is difficult to classify using the traditional categories of external effect.

With respect to equality, Satz also wants to expand the traditional understanding to include more than just unequal endowments and asymmetric bargaining power. She argues that the operations of particular markets may "undermine the conditions that people need if they are to relate as equals" (p. 94), and undermine the ability of some to "participate competently and meaningfully in democratic self-governance" (p. 101).[4] This cannot be remedied through income redistribution, in her view, but requires that some exchanges be prohibited, and that other types of goods be provided by the welfare-state in-kind (p. 102).

Satz spends a fair bit of time defending her view on equality (essentially a type of non-responsibility sensitive egalitarianism with a "basic needs" flavor), something that strikes me as being a slight misdirection of effort, since there is very little in her view of equality *per se* that distinguishes her position from that of the general egalitarian. In particular, it is far too easy to assume that, because the state has an obligation to ensure that the basic needs of all citizens are met, that the state must do more than just redistribute income. Why should that be? If people have sufficient income, and if their basic needs are indeed basic, then why would they not go out and purchase everything that they require to satisfy these needs on the market? The idea that guaranteeing minimal income is somehow different from

[3] The exception to this is Fred Hirsch, who made a number of suggestive observations about the cultural consequences of commodification, particularly with respect to the way that charging for a good can change its social meaning (Hirsch 1978, 84-101). These observations, however, have not received much uptake.

[4] There are interesting parallels between this view and the one developed by Kevin Olson (2006, 15-18).

guaranteeing basic needs presupposes a seemingly paternalistic concern, i.e., that people will not actually spend their money satisfying their supposedly basic needs.

Thus the most important difference between Satz's view and the general egalitarian's stems from the way that she justifies these restrictions (or "blockages") on individual choice. "The basis of this blockage is not paternalistic", she argues, "it is focused on a view about the source of the donor's obligations, not on a view about what is in the recipient's best interest" (p. 79). In other words, she claims, the state must provide for certain needs in-kind, without any opt-out, because it is under an obligation to achieve a certain sort of outcome, regardless of whether the individuals in question happen to value that outcome.

This seems fine, as far is it goes. Unfortunately, she says little about where this obligation comes from, or more importantly, how one could justify an obligation on the part of the state to ensure that a particular person's basic needs were satisfied *without making any reference to what is good for that person*, and without presupposing some sort of perfectionism. One would like to have seen more development of this point, since it seems like the one issue on which there really is a significant disagreement between Satz and the general egalitarian.

After outlining her basic normative framework, Satz moves on in the second half of the book to present a series of applications of this framework to particular issues that have generated philosophical discussion. (It is noteworthy that these are all questions about "forbidden markets", such as prostitution, organ donation, child labor, and so on, not welfare-state staples like education and health care.) There is plenty of common sense on display throughout. Furthermore, because she does not think that any of these exchanges are intrinsically wrong, Satz exhibits admirable receptivity to the range of empirical evidence that is relevant to the assessment of these markets.

There is a fair amount of pointed criticism of opposing views in these sections. For example, Satz repeatedly makes the observation that in order to justify prohibition of a particular exchange, it is not adequate simply to come up with a reason why it should be banned. One must also show that this would *not* result in the prohibition of all sorts of other markets that no one has any particular problem with. (In other words, one must worry not just about the confirming inference, but also about the disconfirming contrapositive.) This may seem like a simple point of logic, but she uses it to cut an extraordinarily wide swath through the philosophical literature, often with a measure of subtle wit. For example, she dismisses the argument that prostitution is an exchange that women enter into only out of "desperation" on the grounds that "there is no strong evidence that prostitution is, at least in the United States and certainly among its higher echelons, a more desperate exchange than, say, working in Walmart" (p. 141).

However, having praised Satz's receptivity to empirical considerations, there is one small complaint that I would like to register. At two rather key points in the argument, Satz appeals to what she, following Jonathan Wolff, calls the "Titanic puzzle". This puzzle arises from a rather throw-away line in Thomas Schelling's *Choice and consequence,* in which he suggested that the Titanic had an inadequate number of lifeboats because passengers in 3rd class (or "steerage") were expected to "go down with the ship" (Schelling 1984, 115), and that this was somehow part of the conditions of carriage associated with the less expensive tickets. The puzzle is then as follows: assuming that we find it outrageous for passengers on the same ship to have differential access to lifeboats, on the grounds that some did and some did not pay for this safety feature, how then can we accept an arrangement under which passengers on *different* ships, having paid different prices for carriage, have access to different levels of safety?

The puzzle is fine so long as one is simply looking for an intuition-pump. It is important to realize, however, that this account of conditions on the Titanic is entirely fictitious (indeed, the suggestion that there was a policy of denying 3rd class passengers access to the lifeboats was vehemently denied by White Star Lines). Differential rates of survival among Titanic passengers were very much a product of early 20th-century social mores, not *ex ante* contracting. First priority was given to women and children, and after that, male passengers (on one side of the ship men were barred entirely from entering the lifeboats). This was reflected in the fact that survival rates among female 3rd class passengers was higher than among any group of male passengers, including those in 1st class. Indeed, much of the discrepancy in survival rates between 1st, 2nd, and 3rd class passengers was due to the lower proportion of women in steerage, along with the physical positioning of the lifeboats on the upper decks (Butler 1998, 105-106).

I am drawing attention to these facts not just in the hope of preventing an urban myth from taking hold in the philosophical literature, but also to make a point that is relevant to the normative assessment of the thought-experiment. Satz claims that in the Schelling scenario, the selling of tickets with differential access to lifeboats is impermissible because it undermines the conditions of equal status among passengers, by treating the lives of some as worth more than those of others. Yet the fact that we routinely pass over in silence arrangements in which men are exposed to much greater risk than women suggests that there is no general norm requiring equal safety in our society.

This has broad ramifications in many areas of economic life. In the typical wealthy country physically dangerous work is done almost entirely by men. In Canada, for instance, in 2005, over 97% of workplace fatalities were among men—in numbers, out of 1097 deaths, 1069 were of men, 28 of women (Sharpe and Hardt 2006, 25-26). Yet instead of being met with outrage, the standard response to this statistic is to say "well, they get paid more to do this sort of work". This is, of course, precisely the response that we find unacceptable in the fictitious Titanic scenario.

What this suggests, in my view, is that there is no general norm of equality underlying our response to the Titanic case, because we do not actually believe that equal safety is required for equality of status. One possibility is that the situation of a sinking ship evokes a particular set of social norms, similar to those governing what G. A. Cohen described as "the camping trip" (2009). A more likely explanation is simply that we find male victims of class discrimination more sympathetic than male victims of sex discrimination. If this is true—and if we are not committed to any general principle of equal safety—then by Satz's argument our reaction to the fictitious Titanic scenario may just be a type of repugnance that we need to get over.

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Erasmus Journal for Philosophy and Economics,
Volume 4, Issue 1,
Spring 2011, pp. 108-113.
http://ejpe.org/pdf/4-1-br-5.pdf

Review of Willie Henderson's *The origins of David Hume's economics*. London and New York: Routledge, 2010, 228 pp.

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The "origins" in the title refers narrowly to the link between Hume's *Political discourses* (1987 [1752]) and *A treatise of human nature* (2002 [1739-1740]). As this locution indicates Henderson wants to argue for a unity and continuity in Hume's thinking—hence a detailed discussion of the "Abstract" (of *A treatise of human nature*) and a chapter on the two *Enquiries* (Hume 1999 [1748]; and 1998 [1751]). However, he argues that notwithstanding this coherence Hume deliberately changed his "textual strategy" and embarked on what Henderson calls a "rhetorical turn" (Henderson 2010, 4, et passim). In the context of this book's own "strategy" the way this argument is executed produces an odd outcome. Henderson is careful to advertise that this is not a book of "advanced scholarship" but is a "general book" intended "to help those new to Hume" (pp. xvi, 20, xv), yet the first third of the book is devoted to a discussion of textual analysis in general and of some passages of Hume in particular. The more general analysis goes on at some length about the various meaning of "summarization" and "selection", citing in the process some standard histories of economics. It is questionable whether Henderson's intended audience is as concerned as he is with this issue.

Henderson's own commitment to textualism is relatively unreflective. There is not here any excursus into Derridean or Foucauldian concerns about "authorship" (merely some occasional second-hand references to post-structuralism) nor is there any acknowledgement of the Cambridge School's and their critics' deliberations on intentionality. This unreflectiveness does reveal itself in some naivety, as when he states "meaning is rarely given in a sentence, being rather constructed within a sweep of sentences in a surrounding discourse" (p. 36). He also does not subscribe to this dictum wholeheartedly since he is able to declare that Hume's admiration for commercial society is exhibited "at sentence level" (p. 89). At the heart of Henderson's textualism is what he calls "close reading" which he defines as an "exercise" (p. 61) that looks into a text, isolates it from other texts "in the first instance", in "the hope that we emerge with a clearer understanding of the examined text" (p. 69). What, in practice, does this amount to? He extracts some paragraphs, numbers the sentences and proceeds systematically to outline the language, the connections and the unfolding argument. One application of this is to the well-known passage in Book III of *A treatise of human nature* on the origin of justice, where three paragraphs (Hume 2002 [1739-1740], III.2.2.1-3) are supplied and followed by six pages of commentary. Some relaxations from "closeness" occur (a case perhaps of going beyond "the first instance") when references to other Humean texts are invoked and there are frequent asides to Adam Smith. In fact these are made not only here but throughout the book, including some pages on Smith's "four-stages" theory (in the context of enquiry as to whether Hume has a stadial theory; answer "not really" (Henderson 2010, 186ff.).

Albeit that it is undertaken intelligently and not without insight, there are a number of problems with this "method" both extrinsically in execution and intrinsically as a method. Confining these remarks to the "justice" passage mentioned above, in its execution the commentary inserts comparisons that either are unhelpful, as when it is simply stated that Hobbes is Hume's target (p. 83), since we are given no explanation of why this is (disputably) the case, or are simply ad hoc, as when Pufendorf is quoted—from John Stewart (p. 194n.). Another insertion oversimplifies when it is claimed that "selfishness" is "the" source of justice, property and government (p. 85), especially since a few pages later Hume is quoted identifying "scanty provision" as another source. Henderson does talk of "scarcity" but the issue is rather that, for a close reader, it is surprising that he does not comment on the meaning of Hume's actual wording here which refers to "selfishness and confin'd generosity", since the force of the latter phrase needs exploring, bearing as it does on Hume's view of familial relations (on which Henderson does comment).

Two other problems are possibly of more moment. Henderson moves very swiftly over one of the most contested aspects of Hume, namely, his "restriction" of justice to property relations (p. 86). What is at stake here is not so much the contestation as an implication of Henderson's own methodology. Arguably the meaning of Hume's reduction of justice to property relations lies principally in what he does *not* say in the text. Justice for Hume is properly expressed in inflexible rules and he does not discuss the commonly held (both historically and contemporaneously) wider, less restricted, notion that equates justice with a general code of conduct characteristic of Aristotelian/Natural Law ethics—witness Hutcheson's declaration that the highest branch of justice is piety to God (Hutcheson 2007 [1747], I, 8). For Hume this latter approach would introduce flexibility and "infinite confusion in human society" (Hume 2002 [1739-1740], III.2.6.9). This has direct bearing on Hume's "economics" since stability of possession is a prerequisite. In short, that Hume is deliberately distancing himself from key prevalent arguments is not derivable merely from close reading, notwithstanding the "relevance" of such distancing to an appreciation of the "foundation" (Henderson 2010, 68, 91) of Hume's economics in *A treatise of human nature*.

The remaining problem in this exemplifying passage bears on the book's aim. After the exercise in close reading Henderson proceeds to note aspects from elsewhere in that chapter of *A treatise of human nature* that he has not analysed, including questions of economic motivation, the development of "new wants", the free rider problem, the emergence of money, and so on. For a book designed "to help those coming new to the study of Hume" (p. 32), it might be reasonably thought that a fuller treatment of these issues was in order. Nor is this an isolated occurrence. In the chapter most explicitly devoted to the "economics" Henderson is explicit that his focus is on money—he considers closely, in addition to "Of money", "Of interest", "Of the balance of trade" and "Of public credit" (in an earlier chapter he had treated similarly "Of commerce" and in his final chapter on Hume on progress he deals with "Of the populousness of ancient nations")—and advises the reader to look elsewhere for "issues not here dealt with" and for "wider" discussion (p. 141). At the very least, this selectiveness sits awkwardly with the book's intent.

While still upholding paragraph by paragraph "internal" reading (p. 141) Henderson's later discussion is less abstemious with external sources. For example, he is not averse to throwing in passing references to Aristotle and Locke on moral limits to accumulation (p. 115) or, in the context of debates about interest, to referring to Locke, Petty, and Massie (p. 151) or to invoking the names of "late mercantilists" like Defoe, Davenant, and Postlethwayt (p. 139). Regarding this last example, if a reader was looking for a line on Hume and mercantilism they would be disappointed since we are given a mix of interpretative statements, such as that Hume is offering a "direct challenge" in his definition of money while "mercantilist ideas permeate Of commerce" (p. 137). It is not, of course, that Hume has to be consistent but that the intended reader would not be helped much by such a range of judgements.

These "external" references betray an uneasy attitude toward context. Henderson's commitment to close reading produces claims that "whatever the wider context" the analysis "has shown" how Hume's "economics thinking"—the structure of its writing and its development—is related to *A treatise of human nature* and the *Enquiries* (p. 150). Against this he states that Hume's "economic concerns" need "to be read in context" (p. 140) and, more substantively, he interpolates at one point that "Hume is writing in the context of the recoinage debate" (p. 148). However, the reader is given no more information and is left little wiser. There is, indeed a disarming footnote where he declares that though this is an "internal" study it is "appropriate to look at outside influences from time to time" (p. 199). The reference here is to Joshua Gee and, seemingly running counter to the statement on page 69 quoted above, it is justified by the claim that a comparison "will help secure an understanding of the advantages of Hume's approach" (p. 155). Gee is at least cited by Hume, but Henderson also includes some pages on Hume's relation to Cantillon (p. 163ff.). This context, however, is generated by inconsequential commentary and pace Henderson it would be very possible to omit this.

One point made in the assessment of the Hume-Cantillon issue is that they were working in different genres and this intimates a pervasive theme in the book. The strongest aspect of the book is the discussion of Hume's attentiveness to his audience following the perceived failure of *A treatise of human nature* to gain a readership, and his corresponding "communicative strategies" thereafter. Henderson makes a particularly enlightening point about Hume's use of the essay format to forestall the difficulties attendant upon a "long chain of reasoning" (see pp. 42, 94, 118, et passim). (Compare Hume's reference to "compleat chain" in the Advertisement to *A treatise of human nature* with the remark at the outset of "Of commerce", that Henderson quotes (p. 133), that arguments ought not to be drawn "too fine or connect too long a chain of consequences together".) This change is implicitly a case of the "rhetorical turn" but, despite being trailed in the opening pages, the meaning of this term is not explicitly discussed at any length. What seems to pass for that discussion are references to Cicero.

These themselves are not systematic. Not surprisingly, and reasonably enough given there is not "world enough and time", no effort is made to identify Cicero's particular influence and there is a tendency to fall back on locutions like "to some extent" (pp. 122, 130) or "in a sense" (p. 127) and to remark "it is interesting" (pp. 103, 106, 115). At times this does produce near meaningless comments like it is a "possible link" that both Hume and Cantillon had read Cicero (p. 199n.). Notwithstanding such platitudes Henderson's line is helpful, as, with specific reference to the "economics", it builds, with acknowledgment, on Box's work.

My final set of remarks pertain to why "economics" is in scare-quotes. Henderson takes a relaxed attitude to the definition, saying at one point that he is interpreting "economic ideas" "fairly widely" (p. 68, and see p. 99) and refers to "essays conventionally classified as economic" (p. 127). Within this width there is some narrowing. Hence despite being a fairly obvious subject, the essay "Of taxes" is not treated at all while "Of refinement in arts" is only dealt with in passing, nor, perhaps less obviously, is "Of national characters" considered despite Henderson's emphasis on causal analysis as a continuing thread in Hume. At times, too, he plays fast and loose with relation to the "political". While admitting that Hume's "economic concerns" are "politically located" (p. 140) he also judges that Rotwein "correctly" excluded "Of balance of power" from his list of Hume's economic essays (p. 154).

In sum, this book does not offer anything especially novel or controversial regarding the substance of Hume's arguments. In a book not designed for an advanced readership that is not an issue, but what is amiss is the means of delivery. It is for that reason that this review has focused on Henderson's methodology. The book is something of a missed opportunity. Its unevenness, and selectivity, of content means it does not a fill what is a real gap, namely, the provision of a non-sophisticated review of Hume's economics.

Alas, I cannot conclude without observing that the book is marred by sloppy editing. There are frequent mis-spellings (e.g., "Malebranch", "Berkley"), typos some of which make sentences gibberish (e.g., p. 38) or which confuse (e.g., "casual" for "causal", p. 151) as well as an egregious misquotation ("collabourate" instead of "corroborate" from *A treatise of human nature*, p. 43). Some redemption may be found in a good index.

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Erasmus Journal for Philosophy and Economics,
Volume 4, Issue 1,
Spring 2011, pp. 114-118.
http://ejpe.org/pdf/4-1-br-6.pdf

Review of Johan J. Graafland's *The market, happiness, and solidarity: a Christian perspective.* London/New York: Routledge, 2010, 186 pp.

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Christian economics has always had a love-hate relationship to the market. Some Christian economists consider the market mechanism as a manifestation of God's providence and defend it as a divine solution, for example, to poverty; whereas others portray it as a modern idol and point to its sinful and destructive side-effects. Outsiders may find it puzzling that Christian economists can be found supporting capitalism, socialism, and communism alike. However, the fact that Christian economics has many faces is less surprising when we recognize that the Bible, Christian tradition, and Christian theology all take an ambivalent attitude towards "the economic" in general. To give some examples, Job's loyalty to God was rewarded by making him rich; Christ preached that it is easier for a camel to pass through the eye of a needle than for a rich man to enter into the Kingdom of God; Augustine stated that there can be no sinfulness in trade but only in the trader; and Aquinas argued that to demand interest for lending money is unjust. From a Christian point of view, in short, economic success can be a curse as well as a blessing. "It all depends". And the same holds true for the market, as Johan Graafland shows in his new book.

The primary aim of *The market, happiness, and solidarity* (a translation and revision of Graafland 2007b) is to clarify the links between ethical values, Christian belief, and economics. More specifically Graafland, a professor of economics, business, and ethics at Tilburg University in the Netherlands, tries to contribute to the Christian debate about the market, market operations, and the market economy by formulating a Christian perspective on it. In this perspective three disciplines are combined, namely economics, ethics, and theology. Although the revision thus complements Graafland's book (2007a) with a theological dimension, it is still ethically rather than theologically orientated. The core chapters examine the effects of the market on three more or less ethical issues, to wit welfare or happiness, justice, and virtue. The point is that for the author "[t]he link between Christian faith and economics is ethics" (p. 9). Ethics is, in other words, where Christian faith and economics meet and a Christian perspective on the market is in the end an ethical perspective.

Before discussing the three ethical perspectives on the market in somewhat more detail, it is useful to hold the methodology of the book up to the light. Why, first of all, does it focus on welfare or happiness, justice, and virtue? According to the author, these are three typical "effects" of the market about which its defenders and critics disagree. Some economists and politicians claim that the market has positive effects on welfare or happiness, contributes to justice, and reinforces virtues, whereas others argue the opposite. The reason for this disagreement is not only that scientific knowledge of the effects of the market is scarce (the positive aspect), but also that economists and politicians have different value orientations with respect to the market, i.e., different views on which values it should serve and how it in practice contributes to the realization of these values (the normative aspect). A Christian view of the market, Graafland argues, should therefore "link biblical teaching on the economy with recent theoretical and empirical research" (p. 9) and this is indeed what the book does. However, such a view is not only based on theology and this is reflected in the way the chapters are organized. In each of them, firstly some relevant "secular" ethical theories are discussed; secondly these are confronted with a Christian ethical account of the topic; thirdly an overview of current theoretical and empirical economic knowledge is provided to prevent "economic illiteracy"; and finally a Christian view is formulated by evaluating and relating the foregoing findings. A Christian view of the market, in other words, combines secular ethics, Christian ethics, and economic research.

The word "biblical" instead of "Christian" in the above quotation betrays Graafland's consistently Protestant approach. In fact, the terms Bible, theology, and Christian faith are taken as more or less synonymous. However, Graafland's Christian view of the market is not based on the theological or church tradition, but merely derives principles on welfare, justice, and virtue from the Bible, whereas the book's subtitle "a Christian perspective" suggests something broader.[1] It is true, the author acknowledges, that the Bible is not a handbook of economics and ethics, and he sometimes relies on Calvin and

[1] But certainly not "a Christmas perspective", as an unfortunate typo puts it on p. xii.

contemporary Christian economists, but still this Protestant approach is a clear limitation of the book.

In the first chapter, after the introduction in which methodological questions are discussed, Graafland examines whether or not the market creates welfare and contributes to happiness. He argues that the market economy under certain conditions, namely the right mixture of neoclassical, neo-Austrian, and Keynesian elements, at least fosters economic growth, which does not eliminate scarcity but does generally make people happier. Also from a Christian perspective, economic growth can be defended as it creates employment and helps to fight poverty. Instead of a stationary "economy of sufficiency", as proposed by the influential Dutch Christian economist Bob Goudzwaard, a concept of selective growth is to be preferred. That is to say, economic growth should be directed at serving real human needs, for example the reduction of poverty. The biblical ideal in this respect, the author states in an interesting section, is moderate scarcity. Whereas the complete abolition of scarcity and restrictions on human needs could be "the gateway to hell", a situation of moderate scarcity still requires our use of talents, creativity, and power in order to meet our responsibilities towards our fellow humans and the environment. Graafland concludes that "the perfect free market is a beautiful and inspiring ideal" (p. 54) that requires good government intervention to function well in reality.

The second chapter considers how the market relates to principles of right and justice. Graafland introduces twelve principles of distributive justice in ethics, including the theories of Robert Nozick, John Rawls, and Amartya Sen, and shows that several of them are supported by the Bible too. This means that egalitarian as well as capitalist or libertarian views on justice are promoted in different biblical contexts. The fact that secular ethics and Christian ethics often resemble each other is not surprising, since the former has been influenced by the latter and vice versa, something the author recognizes. Economic research shows that well-functioning markets without severe market imperfections and too intensive competition can indeed respect both positive and negative types of justice. Typical of a Christian view on the market and justice is the priority of the poor. Governments of Western countries should not only give priority to meeting the basic needs of their own citizens and therefore to a certain degree redistribute income, but also play an active role in the development process of poor countries. At the same time, "the range of income inequality that is still legitimate from a Christian point of view is quite large" (p. 94).

In the penultimate chapter, Graafland examines whether the market strengthens or excludes virtues, and especially Christian virtues like faith, hope and love. Graafland here tries to follow up Albert Hirschman's discussion (1982) of the *doux commerce* versus the self-destruction thesis, i.e., the question of whether commerce and competition have a favourable impact on human manners and virtues overall or just undermine them. Graafland starts by discussing and relating the classical virtue ethics of Aristotle and the biblical virtues and vices, which again partly overlap. He then brings in empirical research to analyze the effects of market operations on both classical and Christian virtues. The problem with this virtue ethical perspective on the market, however, is that little empirical research has been done, possibly because this relationship is so un-amenable to measurement. As a result, the results of existing (theoretical) economic research are often vague, conflicting, or at the least uncertain. This renders the chapter somewhat ineffective. Nevertheless, Graafland here brings together an extensive literature on the relationship between the market and a variety of virtues, which is valuable in itself. His conclusion is that there may be a curvilinear relationship between competition and virtues, so that whether the market will erode virtues or reinforce them depends, among other things, on the degree of competition. Whereas a moderate form of competition has a healthy impact on virtues, a lack of competition as well as fierce competition will be destructive. The same, he argues, holds true for Christian virtues. Especially because fierce competition may erode its most central virtue, namely the virtue of love, a Christian view about the market and virtues has to be sceptical about unlimited competition.

All in all, *The market, happiness, and solidarity* is a valuable contribution to the Christian literature about the market, and also about economics in general. Whether or not the market can be seen as a blessing from a Christian perspective depends on a lot of factors and Graafland discusses them carefully. For him, the question is not so much *whether* Christians should accept the market system, but rather *how* its harmful consequences can be diminished. The distinctive strength of the book is its emphasis on a great variety of (recent) economic research into the market, both theoretical and empirical. As noted, it does not begin from a theological point of view as is often the case in Christian economic literature, but combines and does justice to economics, ethics, and theology. A disadvantage of this multidisciplinary approach is that the book often arrives at rather nuanced if not too nuanced conclusions. In the final “Integration and application” chapter, for example, in which Graafland applies the findings of the book to a case study of replacing a progressive tax system by a flat-rate income tax, it appears to be hard to draw clear conclusions. More generally, the book successfully shows that all three ethical perspectives—welfare or happiness, justice, and virtues—legitimate the market and its plea against too much government regulation, but also suggest a need for limitations and a strong state to correct the market. These and other nuanced views in the book are not really surprising, but nevertheless plausible. After all, the truth is often in the middle.

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