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Interdependent preferences and policy stances in mainstream economics

FRANÇOIS CLAVEAU
EIPE, Erasmus University Rotterdam

Abstract: An individual's preferences are interdependent when they can be influenced by the behaviour of other agents. This paper analyzes the internal dynamics of an approach in contemporary economics allowing for interdependent preferences, the extended utility approach (EUA), which presents itself as a mild reform of neoclassical economics. I contend that this approach succeeds in broadening the policy perspectives of mainstream economics by challenging neoclassical policy stances. However, this success comes with a limitation: the EUA is unable to supply new consensual policy stances as alternatives to the challenged ones. The reason for this limitation is that the EUA opens the possibility of a wide variety of specifications for the utility function, and policy conclusions are sensitive to the details of these specifications.

Keywords: interdependent preferences, extended utility function, neoclassical economics, Pareto efficiency, welfare analysis, policy recommendations

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The face of mainstream economics has changed dramatically in the last three decades (Colander, et al. 2004; Colander 2005; Davis 2006; Davis 2008). This development has come neither from a single breakthrough nor as a consequence of a revolutionary outcry. In fact, many of the changes have resulted from attempts by economists to meet the following reformist challenge: "what can you explain if you accept all of the standard assumptions except one?" (Ackerman 1997, 656) It is

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mainly by the cumulative impact of rounds of this game that mainstream economic theory has moved on.

This paper focuses on one axis of research stemming from this reformist challenge. This axis tries to address a common objection to neoclassical economics regarding its extremely narrow account of the social character of humans. Critics have relentlessly said that individuals are intertwined in a much more complicated manner than neoclassical economics is ready to accept. To remedy this defect, some mainstream scholars have accepted to expand the social character of economic agents in the theory by endowing them with interdependent preferences. In a standard choice model, social interactions could be allowed through three channels: external constraints, information (beliefs), and preferences. The research covered here refers only to the work employing the third channel.[1]

The concept of interdependent preferences can be given a simple, intuitive definition: preferences that are influenced by the behaviour of other agents. For instance, in the context of demand functions, Robert A. Pollak defines them as "preferences which depend on other people's consumption" (Pollak 1976, 309). More generally, preferences can be influenced by many types of behaviour by peers far beyond strict consumption. Staying in the realm of traditional subjects in economics, we may underline the role of interpersonal effects on labour supply decisions: if my fellow workers accept enthusiastically to work overtime, I may be more receptive to this idea as well. Being more eclectic, we could also reflect on the consequences of culture on nutritional habits and physical activity.[2]

From the vast panorama of approaches in economics on the concept of interdependent preferences, I will focus on the internal dynamics of one approach: the *extended utility approach* (EUA). Basically, this approach involves extending the number of variables in the utility function while keeping the entirety of the other 'standard assumptions'. Given that a major motivation behind the EUA is to offer new policy insights, I investigate how the approach fares on this dimension. I contend that it succeeds in challenging neoclassical policy stances but that it does not supply consensual alternatives. Looking forward into how the EUA could overcome this limitation, I find that it will hit on

[1] There is also research in mainstream economics on the two other channels, e.g., Bikhchandani, et al. 1998, on information; Postlewaite 1998, on additional constraints.
[2] Note that nothing forces one to see interdependent preferences in such examples. They could be due to the information or the constraint channels.

even greater problems if more empirical evidence is brought to bear on the specifications of the extended utility functions.

More precisely, the paper defends four claims. First, the EUA is a typical exercise in the reformist challenge; it keeps the whole of the neoclassical framework except one assumption, i.e., *in*dependent preferences. To substantiate this assertion, I present in the first section what I consider to be the four pillars of neoclassical economics. I also use this section to list some of the policy stances typical of neoclassical economics. In the second section, I describe the general framework of the EUA and explain its relation to the neoclassical framework thus bringing home my first claim; the proponents of the EUA are indeed playing the reformist game. The second claim is that theoretical results of EUA often conflict with the typical policy stances of neoclassical economics, and is defended in the third section of this article. In the fourth section, I turn to my third claim: the EUA is not equipped to supply new consensual policy stances, because it lacks the resources to discriminate between competing specifications of interdependent preferences. In fact, the EUA currently proposes a cacophony of policy recommendations. This result is in agreement with the conventional wisdom of economists. Scholars have warned for decades that allowing for more factors to be included in the utility function would make it possible to reach any desired conclusion, thus given the flavour of ad-hocness to the whole exercise. In the fifth section, I speculate on what the proponents of the EUA could do next. I explore the possibility that they widen the range of empirical evidence considered in order to choose the specification of their extended utility function. The fourth claim I defend, thus, is that moving in this direction will force the EUA to reconsider its adherence to (at least) one of the four pillars of the neoclassical framework, namely: Pareto efficiency. Empirical studies on interdependent preferences readily disconfirm the neoclassical assumption—necessary for conventional welfare analysis—that individual choice tracks individual welfare.

NEOCLASSICAL ECONOMICS

Since I contend that the EUA, while allowing for interdependent preferences, tries to remain as close as possible to the neoclassical framework, I start by defining what I mean by neoclassical economics. I take Gary Becker's characterization of the "economic approach" as stating the core elements of the neoclassical research program: "The combined assumptions of maximizing behavior, market equilibrium, and stable preferences, used relentlessly and unflinchingly, form the heart of the economic approach as I see it" (Becker 1976, 5). This quote delineates three pillars of the *positive* part of neoclassical economics: (a) given preferences, (b) maximization and (c) equilibrium as the outcome of the interaction of optimizing agents.

It is crucial to note that the core of neoclassical economics also has a *normative* component in the concept of Pareto efficiency. Due to "economics' self-conception as a positive science" (Davis 2005, 195), economists tend to forget this component when they attempt a characterization of their core theoretical elements. However, Pareto efficiency, with "its role as the only policy recommendation generally accepted in economics" (Davis 2005: 195), is central to theoretical and applied neoclassical economics.

The fundamental role of a shared normative criterion for neoclassical economics—Pareto efficiency being the current one—can be appreciated if we go back to what is sometimes called the ordinal revolution in utility theory. Before the 1930s, many economists were routinely assuming the possibility of interpersonal comparisons of utility, i.e., that one's change in utility can be compared to the change in utility of another agent. In his *Essay on the nature and significance of economic science*, Lionel Robbins forcefully attacked the postulate of interpersonal comparability of satisfaction: "It is a comparison which necessarily falls outside the scope of any positive science" (Robbins 1949 [1932], 139). Robbins was quite aware of the consequences of his argument for welfare economics. Referring to a vibrant reply by Roy Harrod,[3] Robbins asserted "that economics as a science [can] say nothing by way of prescription" (Robbins 1938, 637).

At the same time, Harold Hotelling (1938), Nicholas Kaldor (1939) and John Hicks (1939) entered the debate drawing heavily on the feeling of a methodological crisis among economists. In his *Foundations of welfare economics*, considering his task to be "mainly one of synthesis", Hicks proposed "to set out briefly and simply the main lines of the new welfare economics" (Hicks 1939, 698). This new foundation, he claimed,

[3] Harrod wrote: "If the incomparability of utility to different individuals is strictly pressed, not only are the prescriptions of the welfare school ruled out, but all prescriptions whatever. The economist as an adviser is completely stultified, and, unless his speculations be regarded as of paramount aesthetic value, he had better be suppressed completely" (Harrod 1938, 397). In line with Melville (1939), he then argued that the postulate should still be used even if it places us at the border of science.

was rendered necessary by Robbins's criticism since "economic positivism might easily become an excuse for the shirking of live issues, very conducive to the euthanasia of our science" (Hicks 1939, 697). He then proposed the well-known Pareto efficiency and the compensation criterion[4] as the central concepts of new welfare economics. Economics was saved from euthanasia.

One of the central achievements of neoclassical economics in the last century is that the Pareto criterion, in conjunction with the standard model of the three pillars, has led to the ossification of a set of standard policy recommendations. This set is an integral part of the contemporary culture of economics. Today, students of neoclassical economics, apart from struggling with the intricacies of model building and equilibrium definition, learn this set of governmental 'good practices'. Lecturers of microeconomic courses commonly engage in comparative statics while asking: 'in what state agents are better-off?' With this training, students soon master the basic policy stances which include, for instance: aim at higher output, respect consumer sovereignty, prefer cash transfers to in-kind ones, and increase the cost of undesirable behaviour.

I should be clear: I do not claim that economics is monolithic, or that every economist endorses these prescriptions. The different surveys of economists give the picture of a community with a varying degree of homogeneity in beliefs depending on the issues (e.g., Kearl, et al. 1979; Frey, et al. 1984; Alston, et al. 1992; Fuller and Geide-Stevenson 2003). Furthermore, I do not believe that such broad policy stances are necessary implications of working with the four core neoclassical pillars defined above. Indeed, as we will see, the EUA works with the four pillars as well, but reaches different normative implications. My point is rather that the training of neoclassical economists makes these policy prescriptions salient. The usual interpretations of their models are in line with these recommendations and make economists often fall back on such stances when they face a policy issue. Let me take in turn each policy stance of the short list given above so as to illustrate how these prescriptions are made salient by receiving training in neoclassical economics.

[4] The compensation criterion soon lost its popularity when its theoretical defects were revealed (Gorman 1955). Pareto efficiency was left as the only consensual normative criterion of neoclassical economics.

'Growth is good' is a basic normative conclusion of the neoclassical culture. In general, higher output translates into a weakening of the budget constraint of agents, hence in a higher potential utility. A famous variant of this argument is due to Robert Lucas (1987, chapter 3). For him, the promotion of economic growth is far more important than policies aimed at stabilizing the business cycle, since the "potential gains from improved stabilization policies are on the order of hundredths of a percent of consumption" (Lucas 2003, 11). Lucas maintains that this sharp normative conclusion is the result of the progress of the neoclassical research program: "we are able to form a much sharper quantitative view of the potential of changes in policy to improve peoples' lives than was possible a generation ago" (Lucas 2003, 12).

The typical fondness of economists for the idea that 'the agent is the best judge of her interest' comes from the fact that textbook models are premised on this idea. The function maximized under constraints by the agent is at the same time the measure of her welfare. If she was not able to reach a higher utility given the constraints, no one else could do it for her. Hence follows the normative notion of consumer sovereignty: it is good that the agent be left to choose by herself.[5] Since consumer sovereignty is embedded in the assumptions of the neoclassical models, it is no wonder that a high proportion of economists endorse this view.

The idea that 'transfers should be in cash rather than in kind' is related to consumer sovereignty. Money can be allocated by the individual according to her preferences while goods are far less

[5] Jack Vromen pointed out to me that 'consumer sovereignty' is also (and probably more often) used to refer to the thesis that consumers, through their purchasing decisions, are the ones selecting what gets to be produced. In this case, consumer sovereignty is a descriptive concept, i.e., the thesis may be false. If one accepts the descriptive concept, the normative notion can be rephrased as 'it is good that consumers be the ones orienting production'. But one can also think that consumer sovereignty is false as a description but keep it as an ideal to strive for. In the case of neoclassical economics, the normative notion of consumer sovereignty is supported by the belief that 'the individual knows best' without any need to endorse the descriptive statement.

The reader should also note that 'consumer sovereignty' is also used by some authors to refer directly to the idea that 'the individual knows best', e.g., "the standard principle of consumer sovereignty according to which every individual is the best judge of his own interests" (Fleurbaey 2008). I will not conflate the two meanings because, if economists seem to endorse the normative notion of consumer sovereignty because they believe that 'the individual knows best', it is also possible to reject the latter claim and still endorse consumer sovereignty. For instance, Robert Sugden (2004) recognizes that individual choices are sometimes at odds with their interest, but maintains that the individual should be free to choose anyway.

fungible. If the government gives food stamps for instance, it presumes that individuals are in need of edible goods. By transferring money instead, the government leaves it to the individual to allocate her resources to what is more conducive to her welfare. Some opinion surveys asked economists if they agree with the following statement: "Cash payments increase the welfare of recipients to a greater degree than do transfers-in-kind of equal cash value".[6] In 1976, only 8% of respondents disagreed with the statement—68% agreed and 24% agreed with provisos (Kearl, et al. 1979). The same pattern showed up again in 1990, when disagreement with the statement rose to 14% of respondents—62% agreed and 24% agreed with provisos (Alston, et al. 1992, 206).

Neoclassical economics is also a discourse about incentives, about decision makers weighing the costs and benefits of the alternatives they face. If asked what strategy could help curb some undesirable behaviour, the typical response of an economist is 'to increase the expected cost of the behaviour'. For instance, the criminality rate should respond to the probability of being caught and to the length of the jail sentences. It is by playing on the expected returns of unwelcome behaviour that the State can control to some extent these 'deviations'.

EUA scholars challenge these four neoclassical policy stances. In fact, one of the main motivations behind the work in the EUA seems to be the broadening of the policy views in economics. According to most proponents of this approach, taking into account social interactions has an important impact on the normative conclusions of the analysis. Before showing how the EUA weakens the standard policy views, let me elaborate more on what the EUA actually is.

EXTENDED UTILITY APPROACH AND INTERDEPENDENT PREFERENCES

The general framework of the EUA is well illustrated by the work of Gary Becker (1996) and of Becker and Kevin Murphy (2000). In this section, I will use their model and their peculiar vocabulary to characterize the EUA as applied to interdependent preferences.

The focus on Becker's formulation does not mean that he is a sort of "leader" of the extended utility school. There is no such school: economists working with the EUA in the study of social interactions share a theoretical approach but they do not belong to any

[6] Unfortunately, I was not able to find survey questions that test the popularity of the other policy stances.

institutionalized school. Some of them would not even recognize themselves as members of a common approach. I use the Beckerian formulation as a depiction of the EUA, simply because it summarizes well the relationship between this approach and textbook economics.

In *Social economics*, Becker seems to have changed his mind on what are "the traditional foundations of [...] the economic approach to behavior" (Becker and Murphy 2000, 5). He cites only "utility maximization and equilibrium in the behavior of groups" while omitting the "stable preferences" component of his 1976 definition. One should not be misguided by this omission. Becker and Murphy still accept the neoclassical dictum of fixed preferences, but they give to it a somewhat odd twist. The evolution of Becker's definition of the economic approach is probably an indication that the authors are conscious that what they call 'stable preferences' would be viewed as highly unstable by most economists.

The key distinction between the usual neoclassical approach and that of Becker is his use of the concept of 'extended utility functions'. These are "utility functions that remained the "same" over time and are the "same" for different individuals" (Becker 1996, 6) even though the social context changes. In short, whatever happens in the social environment, Becker wants his objective function to stay intact, only the values of the variables will change. The innovation is thus to extend "the definition of individual preferences to include personal habits and addictions, peer pressure, parental influences on the tastes of children, advertising, love and sympathy, and other neglected behavior" (Becker 1996, 4).

From the standpoint of the extended utility function, the standard functions postulated by neoclassical economists are "subutility functions of goods [that] 'shift' over time in response to advertising, addictions, and other behavior" (Becker 1996, 6). The preference relation of an agent between, say, rock and jazz may appear to change if we focus on the subutility function, but it is simply because of an omitted-variables bias. To remove the anomaly, we need to include in the utility function, for instance, the musical habits of friends. The extended utility function can then be written as:

$$U = U(x; P, S)$$

where x is a vector of typical variables generically labelled as 'goods', P is "personal capital"—potentially including past consumption and other personal characteristics—and *S* "represents social influences on utility through stocks of 'social capital'" (Becker and Murphy 2000, 9).[7]

Since we are interested in interdependent preferences, let me keep the *P* in the background and focus on *S*. In a pure Beckerian style, the analogy between this 'social capital' and the usual physical capital is further explored by claiming, for instance, that the stock *S* is subject to some depreciation rate (which could be 100%). The stock inherited from the past may strongly affect present choices of *x* if it depreciates relatively slowly. For example, if one element of *x*, say x_i, and *S* happen to be strong complements, the individual's choice of value for x_i will increase with inherited *S*.[8] The concept of social capital in the extended utility function is broadly defined as "the effect of others' choice on own utility" (Becker 1996, 12). This definition places little constraints on what the modeller actually puts for *S*.

Students of neoclassical economics will feel at home with the EUA: the maximization-equilibrium framework remains, and even the stability of preferences—if appropriately understood—is left untouched. Moreover, by its loyalty to fixed preferences (stability of the extended utility function), the EUA makes it possible to keep the normative component of the neoclassical core (this condition was stated as early as: Kemp 1955, 218). If the preference ordering changed with the context, different contexts would be incommensurable. But the extended utility function gives the stable metric necessary for the use of Pareto optimality; in models of the EUA, we can tell when the agent is 'better-off' in the exact same way as in conventional models.[9] For Becker, it is

[7] The extended utility function has to be distinguished from meta-rankings defined as "rankings of preference rankings" (Sen 1977, 337; in fact, Becker explicitly rejects Sen's view; see Becker 1996, 17). Most importantly in the present context, meta-rankings do not generally allow a neat use of Pareto optimality as is the case for the extended utility function (Voorhoeve 2006).

[8] To make this relation more concrete, suppose that *x* is hours spent watching a given television series during summer vacation and *S* is the hours spent by colleagues watching the same series last month. If one wants to participate in lunchtime discussions, one may choose to sit more hours in front of a screen given that co-workers are known to be fans of such series. In Becker's jargon, hours spent by co-workers watching television is a complement to one's time 'spent' in front of the box.

[9] It may not be altogether clear to the reader that, since Pareto efficiency uses the individuals' own rankings of outcomes to determine which situation is socially better, these subjective rankings need to be stable across states of the world that we wish to rank. To make this requirement clearer, let me build a toy example where agent's *i* preference map depends on the context. Let $\boldsymbol{P}_{\mathrm{J}}$ denote the strict preference relation in context J. If *i* happens to be in context X where meals are usually served with potatoes, the conformist *i* will prefer potatoes to rice ($p\ \boldsymbol{P}_{\mathrm{X}}\ r$). If the agent is thrown in context Y where rice is the standard side dish, *i* will prefer rice to potatoes ($r\ \boldsymbol{P}_{\mathrm{Y}}\ p$). Can we say

'business as usual' in normative economics: "If the relevant utility function for welfare analysis includes personal and social capital, the effect on utility of advertising and public policy can be evaluated without any ambiguity" (Becker 1996, 20).

Beyond the retention of the Pareto criterion, the EUA is also following the neoclassical customs in using the vocabulary of externalities to characterize interpersonal effects. In the EUA, when an agent acts, she does not take into account the welfare effect of her action on the preferences of others. There is thus a potential wedge between the private valuation of an action and its social value. Akerlof's language is representative of this application of the notion of 'externality' to discussions of interdependent preferences:

> Except under rare circumstances, such interactions produce externalities. These externalities typically slow down movements toward socially beneficial equilibria but in the most extreme cases they will create long-run low-level equilibrium traps that are far from socially optimal (Akerlof 1997, 1005).[10]

Scholars associated to the EUA go on to say that the presence of these externalities opens the door to beneficial interventions by governments. To be sure, there is sharp disagreement among economists about the extent of the inefficiencies created by interpersonal effects and the promise of governmental interventions—I will return to this debate later. Beyond this controversy, the proponents of the EUA agree on what counts as an appropriate justification of

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something about the Pareto ranking of the bi-dimensional outcome X-potatoes relative to Y-rice? In other words, can we say something like '*i* prefers eating potatoes in context X than having rice in context Y'? If we take *i*'s 'partial' preferences—where partial means only defined over the meal consumed—as the standard, the two outcomes are incommensurable since the choice of metric ($\boldsymbol{P}_{\mathrm{X}}$ or $\boldsymbol{P}_{\mathrm{Y}}$) is arbitrary. If we take *i*'s preferences to mean '*i*'s preferences in context X' ($\boldsymbol{P}_{\mathrm{X}}$) outcome X-potatoes is Pareto superior. However, the inverse is true if we take '*i*'s preferences in context Y' ($\boldsymbol{P}_{\mathrm{Y}}$) as the relevant ordering.

For the advocates of the EUA, the way out of this dead-end is to consider the "total preference map" (Kemp 1955). Instead of defining the preferences only over goods consumed (e.g., *r* and *p*), the agent is now assumed to rank states of the world defined as the conjunction of goods consumed and context. In the example above, there are four states in the stable preference ordering. From the assumed choices of *i*, we already know the preferred meal given the context [(p, X) $\boldsymbol{P}$ (r, X) and (r, Y) $\boldsymbol{P}$ (p, Y)]. Now, we can also rank states of the world stemming from different contexts (the most interesting binary relation should be the one between (p, X) and (r, Y), that is: Does the agent prefer to eat potatoes among potato-eaters over the alternative of eating rice among rice-consumers?

[10] On "positional externalities" see also Frank 2005; 2008.

public intervention. The model discriminates between optimal and suboptimal outcomes. If we believe that a low-level equilibrium might prevail, intervention aiming at diverting the system to a better outcome should be considered.

Scholars of the EUA accept integrally the methodological principles of normative neoclassical economics. The shared understanding of the appropriate justification of public policies—correcting suboptimal outcome due to externalities—structures the debate between neoclassical economists. From the point of view of public policy discussions, the modelling ritual of economists appears as a speculative game about why some stylized social outcome may or may not be optimal.

We can distinguish two waves in this game. In a first wave, users of the extended utility function argued that accounting for interdependent preferences may change a lot, if not most, of the standard policy prescriptions stemming from neoclassical economics. In a second wave, the increase in the number of extended utility models resulted in a competition among models of the EUA, each leading to different, often contradictory, policy conclusions.

THE FIRST WAVE, CHALLENGING HARD-WIRED POLICY STANCES

Since policy guidance is such an important goal in economics, a fresh look at normative implications is probably the most important result of the EUA. In this section, I will illustrate some of the normative claims that the EUA raises against the standard neoclassical models. It has been argued that hard-wired policy prescriptions in neoclassical economics hinge on the assumption of *in*dependent preferences.

The most cited result of the EUA questions the idea that economic growth necessarily leads to improved welfare. The germs of this scepticism can be traced back to James Duesenberry's relative-income hypothesis (1949). Duesenberry rejected the usual specification of the utility function where only an agent's own consumption and own leisure appear. He argued that a measure of the consumption norm (the S in the case above) had to figure in this function. According to him, agents do not derive satisfaction from their absolute level of consumption but from their consumption *relative* to the consumption of their fellows. Duesenberry proposed to define the income term in the indirect utility function as the ratio of own income to average group income. The new stance toward growth follows from this specification of the utility function since economic growth with a stable social distribution of income leaves relative income unchanged. If one's income grows at the same rate than GDP per capita, the proportional increase of the two terms in the relative income variable will leave satisfaction unchanged.

The literature drawing on the relative-income hypothesis is now extremely voluminous (see Clark, et al. 2008). Starting with Richard Easterlin's seminal paper (1974), this hypothesis is widely cited to explain why the strong economic growth of Western countries in the second half of the twentieth century has not generally been accompanied by increased happiness. To be sure, even if we accept the strong claim that higher average real income does not imply more consumption satisfaction, "that does not mean that economic growth becomes a matter of social indifference" (Frank 1985, 36). It could well be that economic growth brings other benefits such as:

> [...] the link between the length of life and (aggregate) income; the link between the ability to withstand foreign aggression and economic activity; the ability to attract migrants when income levels are relatively high; and some status utility benefit to a country as a whole from having high income compared to other countries (Clark, et al. 2008, 124).

Nevertheless, the relative-income hypothesis implies that the neoclassical argument for growth is misguided. In the logic of the relative-income hypothesis, it is not because people can consume more that growth should be welcomed.

Despite its popularity, the relative-income hypothesis is arguably covering only a small part of the social interactions relevant to economists. Since it was first intended to explain the relationship between income and the savings rate, it only posits a connection between one's consumption level and the consumption levels of agents in one's reference group. Therefore it does not address the interpersonal impact of different types of consumption goods, since all goods are lumped into a unique value. To improve on this crude picture of the dynamics of comparison in which consumers are engaged, some authors argue that it is necessary to distinguish between conspicuous and non-conspicuous goods.[11] Some goods—such as houses, cars, and clothes—generate satisfaction partly by comparing one's bundle to the

[11] See Frank 1985; 1997; 2007; and see Hirsch 1976, for the related notion of 'positional goods'; also see McAdams 1992, for a discussion of the two notions.

others. They are said to enter the utility function in a relative form. Other goods—like insurance, time with friends, and rest—are almost only desired for their absolute (versus relative) attributes. A vast amount of resources is spent on conspicuous goods simply to 'keep up with the Joneses'. However, agents will be better-off if the conspicuous expenditures of everybody were kept low and the resources were reallocated to non-conspicuous goods. Hence, according to these scholars, the problem is not with economic growth per se, but with growth primarily channelled to conspicuous consumption.

The conspicuous good theory forces a reconsideration of the normative concept of consumer sovereignty which is at the heart of the neoclassical culture—as described in the first section of this article. Typically, economists believe that "each person [should be] free to get what she wants [… since she] is the best or proper judge of her own well-being" (Sugden 2004, 1016). Here, the externalities created by conspicuous consumption make the systemic effect of individual decisions unappealing: "Roughly speaking, the problem is that we work too many hours, save too little, and spend too much of our incomes on goods that confer little additional satisfaction when all have more of them" (Frank 2007, 103). Consequently, welfare economics prescribes that individuals should *not* get what they want. There is room for the government to limit the wastes due to competitive consumption. Since we are in presence of externalities, taxes are welcomed to realign the price mechanism. The literature thus offers various proposals to implement taxes on conspicuous goods or on consumption in general, e.g., Robert Frank's progressive consumption tax (2007, chapter 11).[12]

[12] It might sound surprising to many that I have put Robert H. Frank as a user (or a supporter) of the EUA. But let me give the microphone to Frank so that he can explain how he sees his approach:

> **Frank**: [...] I think that I am much closer to the neoclassical approach than most people in the new economics and psychology movement.
> [...]
> **Interviewer**: Do you see your work as trying to incorporate as much as you possibly can within the rational choice model?
> **Frank**: Yes, that's the way I see it.
> [...]
> Once you put in a taste for these things, then it's just like a taste for pushpins, it's the same model as before; it's constrained maximization [...] If you want to say that someone has a taste for doing the right thing, alright, you put a taste in for that, and there's a taste for own income and consumption.
> [...]
> I still think my intellectual capital is more with the old static maximization model (Interview with Robert H. Frank; in Colander, et al. 2004, 116, 117-118, 125).

In addition, the recommendation that governmental transfers should be in money instead of in kind rested on the acceptance of consumer sovereignty. Now that agents may not allocate their resources in the best way, in-kind transfers targeted to non-conspicuous goods may be preferable. In short, this part of the literature on interdependent preferences, which is limited to interpersonal effects through status concern, does away with some of the central normative propositions that a student of neoclassical economics would be encouraged to accept.

When I write that the welfare conclusion of the research on conspicuous goods is that individuals should not get what they want, it is important to see how this conclusion is in agreement with a long tradition in economics related to collective action problems. The conclusion does not come from the rejection of the belief that the 'individual knows best' because the agents are still making the optimal choice *given the decision context.* What happens is that one's choice affects the decision context of the other agents, i.e., we have externalities.

The argument for conspicuous goods is thus strictly analogous to the one for an arms race between two nations: each nation allocates an important part of its resources to military armament because the other nation does the same; they will both be better-off if they could make a binding agreement to limit military expenditures (Frank 2005, 138). What is peculiar to the literature on conspicuous goods is that, suddenly, a great proportion of individual actions typically considered private are now said to follow the logic of collective action problems. These private actions have a collective dimension because they are factored in the extended utility function of each agent.

Another branch of the literature on interdependent preferences, the 'identity' models (e.g., Akerlof 1997; Akerlof and Kranton 2000; 2002; 2005) challenges yet a different dimension of standard policy prescriptions in economics. In these models, the *S* in the extended utility function contains a vector representing the actions of other agents and a sub-function defining one's "identity or self-image" (Akerlof and Kranton 2000, 719). Identity depends on multiple factors including one's assigned social category (e.g., male or female). Associated to the social categories, there are prescriptions indicating "the behavior appropriate for people in different social categories in different situations" (Akerlof and Kranton 2000, 718). To illustrate their notion of social prescription, George Akerlof and Rachel Kranton give the following cliché: "the ideal man is male, muscular, and should never wear a dress, except perhaps on Halloween" (Akerlof and Kranton 2000, 718). Then, they use this payoff function in simple static games to offer explanations for phenomena like "gender discrimination in the labor market, the economics of poverty and social exclusion, and the household division of labor" (Akerlof and Kranton 2000, 718).

Equipped with this peculiar specification of the extended utility function, Akerlof and Kranton are up to challenge the view that the fundamental strategy behind public interventions is to change the 'price ratios' of different actions. If you want less crime, an economist would typically tell you to increase the cost of being caught. Similarly, if you want people to get more education, increase the benefits or lower the costs of education. However, if an agent's action is highly affected by the way she defines her identity, then changes in 'prices' may not have a great impact on her choice once her identity is fixed. One conclusion is that identity should be changed. Changing identities is not an easy task for public officials, but it points to quite different means than the standard strategy focusing on incentives.

Even free higher education will not bring a large proportion of children from low-income families into universities if their social background makes them think that college studies are not for them. Stricter criminal penalties will not have the promised impact if criminal behaviour is driven by neighbours' behaviours (Glaeser, et al. 1996). Neighbourhoods with a good mix of social categories may be more effective. In sum, "in important special cases the incorporation of [...] social factors into rational choice analysis results in behavior that more closely corresponds to the intuition of sociologists than of economists" (Akerlof 1997, 1006). Hence, the prescriptions derived from the models will also be closer to the ones of non-economists.

THE SECOND WAVE, DISCORDING VOICES INSIDE THE EUA

My goal in the previous section was to show how some results of the EUA challenge basic neoclassical policy recommendations, and to make clear how the usual results in welfare economics rested on a problematic premise, namely *in*dependent preferences. While the EUA has shown that standard prescriptions should be reassessed, it does not follow that the normative conclusions of any particular model accounting for interpersonal effects are better grounded. In fact, it comes with no surprise that, depending on the chosen structure of the interdependence, a model could produce radically divergent policy prescriptions.

Larry Samuelson (2006) illustrates this ‘specification sensitivity’ by considering different assumptions on the form of the relative consumption effects. In fact, his models pertain only to the class where the interdependence is strictly instrumental, i.e., the interdependence is not written in the utility function as is the case of the EUA but it comes from additional constraints to the optimization problem of agents (such as presented by Postlewaite 1998). Samuelson thus recognizes that “the set of possible sources of relative consumption effects is much richer” (Samuelson 2006, 264). Nevertheless, he concludes that policy prescriptions are highly contingent on the chosen assumptions:

> These examples indicate that once behavioral interdependencies are allowed into our economic model, even such straightforward questions as whether distortionary taxes improve or dissipate welfare are open to question. The answer depends upon the nature of the interdependencies and the market in which these effects find expression. Without further study, none of our conventional welfare conclusions can be taken for granted (Samuelson 2006, 263).

The success of the reformists’ promise to offer a better guide for governmental interventions is thus far from guaranteed. It hinges on a justificatory procedure for the choice of assumptions. The modeller will need to present good reasons for why she selects these “fine details of utility functions and market interactions” (Samuelson 2006, 261), instead of the countless other potential combinations, if she wants her policy prescriptions to be credible. Unfortunately, no common justificatory procedure exists among members of the EUA.

When the interdependence effect takes the form of an additional constraint imposed on agents (as in the cases considered by Samuelson), we may have the hope that studying the institutional structure of the relevant ‘market’ will tell us how to specify our model. This strategy will not do for the EUA. When the interdependence effect is located in the utility function (i.e., in the internal valuation mechanism of agents) looking at the working of the market will not be sufficient. There is a fundamental problem of underdetermination of the model by data here. One can think of different specifications of the extended utility function that give the *same predictions* for the market behaviour of agents (i.e., how individual demand will react to a change in prices or income) but, and this is the important point, have *different normative implications*, e.g., how the well-being of agents will be modified by a change in prices or income.

A range of specifications of Duesenberry's utility theory (see the previous section) entertains this relation with standard utility theory (with *in*dependent preferences). To make this point clear with the simplest example possible, let me define Y_i as the income of agent *i* and $\hat{Y}$ as the average income of agents in *i*'s reference group (national GDP per capita for instance). In standard utility theory, the simplest, one-period specification of the indirect utility function would be $U(Y_i)$, where U is strictly increasing. Given such a function, a manna increasing the income of all agents will make everybody better-off; the primitive intuition for 'growth is good'. With Duesenberry's utility theory, the simplest specification would be the strictly increasing function $U(Y_i/\hat{Y})$. In this case, the manna leaves at best all agents indifferent. It may even make some agents worse-off.[13] This example illustrates why Heinz Holländer maintains that "[i]n the realm of welfare theory [...] the standard approach and Duesenberry's approach often lead to diametrically opposed results so that it is of great importance which one is used in evaluating policies" (Holländer 2001, 230).

In the same paper, Holländer specifies the range of conditions under which the behavioural implications of both theories are identical and concludes that "[i]t is not to be expected [...] that observed behavior will enable us to discriminate empirically between the two approaches" (Holländer 2001, 232-233).[14] If the required conditions apply, we face the underdetermination problem: we have two theories indistinguishable from the perspective of standard empirical tests (i.e., how well a model predicts behavioural responses to changes in prices and income), but

[13] It all depends on the form taken by the manna increase. It is easy to verify that if each agent receives the same amount of manna independently of the agent's initial income (Y_i + m, where m is the same for all *i*), the ones with an income above the mean will be worse-off, while the ones below the mean will benefit from the change. Alternatively, if the income of all increases proportionally (m $\cdot$ Y_i), the manna changes nothing to welfare. To see how sensitive the welfare conclusions are to the specification of $U(\cdot)$, the reader can verify that the results are almost the opposite if the utility function is $U(Y_i - \hat{Y})$. Now, an additive manna has no effect on welfare, while the ones below the mean suffer from a manna increase proportional to income.

[14] It will lead me too far from my main argument if I was to explain the technical conditions needed for Duesenberry's utility theory to empirically mimic the standard theory, but I can still list them. First, the extended utility function—including as variables leisure (l), a consumption vector (x), and a vector of reference consumption (a)—needs to be weakly separable in (l, x). Second, "commodity preferences must be homothetic, and [third] the marginal value of leisure must be directly proportional to commodity consumption" (Holländer 2001, 230). For the explanation of the conditions, I refer the interested reader to Holländer's 2001 paper.

leading to divergent policy stances. To be sure, it is also possible that the conditions are not met in the case of the contest between the relative-income hypothesis and the standard utility theory, and that one of the two theories is actually better at predicting behavioural responses. But the point should be clear by now. By allowing for more terms in the utility function, the EUA has created a far more flexible tool which means that it is easier to reach contradictory policy conclusions based on different particular models, but harder to tell which model should be believed. The hard task for proponents of the EUA is thus to legitimize the choice of a given specification.

The lack of shared criteria among proponents of the EUA to justify the form taken by the interpersonal effects makes the policy debate obviously value-driven. At the present state of the research efforts, it seems hard to reject that the competing models using an extended utility are simply reflecting the modellers' divergent prejudices. The charge of Becker and Murphy against Frank's normative conclusions is a perfect illustration of this 'dialogue of the deaf'. Frank is drawing far-reaching implications from the presence of interdependent preferences. For instance, in his recent book, he argues that interpersonal effects, combined with the rise of inequality in the United States, profoundly harm the middle class (Frank 2007). In response to this kind of assertion, Becker and Murphy emit doubts that indeed interdependent preferences produce inefficiencies: "Strong, and often unreasonable, assumptions about the role of marital and other pricing lie behind criticisms by Frank and others" (Becker and Murphy 2000, 124). Accordingly, they tend to focus on models where "status can be purchased in a competitive marketplace" or on particular institutional setups where "women [are] fully [compensated] for the utility gain to their husbands or other companions from their wearing high heels" (Becker and Murphy 2000, 123). As one would expect, these models lead to efficient outcomes.

When they accept 'distorted' pricing due to interdependent preferences, Becker and Murphy are tempted to emphasize that it could well come to correct some otherwise inefficient outcome (e.g., underinvestment in risky activities such as entrepreneurial or scientific careers): "Competition for status might even raise efficiency compared with the situation when utility does not depend on status" (Becker and Murphy 2000, 124). Since no criteria is presented to select among competing models, these comments are pure speculations. In fact, Becker and Murphy recognize candidly that their position stems from their prejudice: "To put this differently, critics stress the 'rat race' aspects of the competition for status, whereas we believe in the American dream that competition to 'get ahead' makes a society function better, not worse" (Becker and Murphy 2000, 125). Who will they convince which is not already on their side?

Let me sum up. We have seen that the EUA has considerably reduced the support for the neoclassical set of standard policy recommendations. However, the EUA, after this phase, has failed to construct a new set of typical recommendations as a replacement. This failure is due to a structural problem of the approach. The EUA is an extremely flexible apparatus and there is no shared criteria among modellers on the actual way to use it, that is, on the way the interdependence is to be modelled. We thus see a proliferation of models, each presenting a particular recipe, and leading to divergent normative implications. One cannot help but concluding, with Daniel Zizzo, that, "in practice, the endogeneity is simply modelled by introducing fuzzy variables in the utility (or meta-utility) function, that are then allowed to change in ad hoc ways" (Zizzo 2003, 874).

For many decades, the conventional wisdom of economists was able to police the practice of model building: the variables allowed in the utility function were not subject to negotiation. The fear was that opening the utility function to other factors meant, in fact, opening Pandora's box. The above discussion supports the common apprehension of economists. The neoclassical framework could deliver a standard set of policy recommendations as long as the rule of using only *in*dependent preferences was generally followed. Once *inter*dependen*t* preferences are allowed on stage, the neoclassical framework cannot generate a harmonious prescriptive stance anymore. At least, it will not be able to do so until it finds a procedure to discriminate between all the competing models.

Given the diversity of extended utility models in the literature today, it becomes blatant to the observer how much the normative conclusions one reaches depend on 'arbitrary' assumptions about the form of the interdependence. This disillusion also affects the status of the standard neoclassical model since it is now only one among many models in the literature. The postulate of the standard model on the form of the social interactions, namely that there is no interdependence, is as weakly grounded as the numerous other possibilities. The EUA has not only dethroned the set of standard policy recommendations, it has made it far more difficult to build new consensual normative stances.

CAREFUL EMPIRICAL GROUNDING AS A WAY OUT?

To dispel the feeling of arbitrariness surrounding their models, EUA scholars will have to argue more persuasively for the specific utility function that they choose. Once we "appreciate that behavioral assumptions [...] tend to rule out some policy ideas and favor others" (Berg 2003, 424), we need to be far more careful in specifying the interdependence structure than modellers in the EUA typically are. Since no specification can be ruled out a priori, empirical studies investigating the structure of the interpersonal effects would have to be at the forefront of the conversation. We have seen in the last section that using behavioural responses to changes in prices and income—the official empirical procedure in neoclassical economics to infer the utility function—would most likely not be enough to discriminate between specifications implying contradictory policy conclusions. The additional desirable evidence that I have in mind would have to go beyond such a procedure. I am thinking of jointly using a set of carefully-designed experiments, subjective surveys, neurological data, evolutionary hypotheses,[15] and the like.

I am far from suggesting that it is something novel to claim that the specification of the extended utility function should be more empirically informed. Scholars participating in the now highly-influential economics and psychology movement have been already making the same point. The idea is clearly stated by Samuel Bowles:

> The need for empirical grounding of assumptions is nowhere clearer than in the analysis of individual behavior, where the process of enriching the conventional assumptions about cognition and preferences can easily descend into ad hoc explanation unless disciplined by reference to facts about what real people do (Bowles 2004, 16).

[15] The qualifier 'empirical' is probably less fitting for evolutionary hypotheses, but one has to keep in mind that, due to theory-ladenness, all the other alleged empirical types of evidence are also conditioned by background conceptions. The theoretical understanding is only more conspicuous for evolutionary hypotheses. When I argue that EUA scholars will need to be more empirical, I do not mean the naïve thesis that they should face the barren facts. I intend to say that the attitude should be one of active gathering of data from a diversity of sources.

The idea is also expressed by some members of the EUA that I have mentioned above. For instance, Robert Frank talks in an interview of the same problem of ad-hocness (Colander, et al. 2004, 117), and includes other types of evidence in his work, like evolutionary hypotheses (e.g., Frank 2005). Likewise, scholars drawing on the relative-income hypothesis have been supporting their ideas with happiness surveys already for some decades.

Is it just a matter of bringing all the evidence together to select the 'right' extended utility function and then derive the correct policy conclusions from this empirically-based specification? If this possibility comes true, it would be a terrific achievement for the EUA. Unfortunately, it appears that, if EUA scholars were attentive to all evidence instead of picking and choosing the evidence supporting their preferred specifications, the EUA would hit a wall. More specifically, I maintain that a basic presumption of the EUA will not survive the move toward a deep acquaintance with all available evidence. This approach—one variation of the reformist challenge discussed in the introduction—was erected on the belief that interdependent preferences could be allowed in the analysis while keeping intact the neoclassical framework (of fixed preferences, maximization, equilibrium, and Pareto efficiency). In fact, available empirical evidence on interdependent preferences readily disconfirms a core requirement of the neoclassical framework permitting the link between an agent's choice and its welfare. With this link broken, the Pareto criterion is adrift. Hence, if my analysis is correct, the EUA, in its attempt to justify its assumptions, has to face strong evidence against its conventional use of Pareto efficiency.

The utility function behind neoclassical welfare analysis is twofold. First, it is the objective function determining the economic choices of an agent. The story supporting the model is that the agent chooses, in the feasible set, the bundle giving her the highest utility level. In positive analysis, the utility function is thus at the centre of the choice mechanism. Second, the utility function also tells us the welfare of the agent. A feasible bundle associated to a higher utility will not only be chosen over a competing bundle, it will also make the agent better-off. Thus, the utility function enables the ranking of alternatives in view of the Pareto criterion. The combination of the *choice* and the *welfare* dimensions of the utility function sustains the neoclassical economists' idea that, given the constraints, the agent will make the welfare-maximizing choice for himself.

Many behavioural economists argue against this amalgamation of the choice and welfare dimensions. It seems that, in many contexts, "people do not appear to do what is best for themselves" (Loewenstein and Ubel 2008, 1795). If we wish to keep the concept of the utility function, growing evidence points to the possibility that choices are guided by one function, "decision utility", while satisfaction arises from a different function, "experience utility" (see Kahneman, et al. 1997; Kahneman and Sugden 2005; Kahneman and Krueger 2006). Put differently, it is as if agents, aiming at making the most satisfying choices, have a systematic tendency to mispredict the welfare effects of their choices. Why would they have this tendency?

Many scholars claim that one central source of this choice bias is that some dimensions of alternatives, the extrinsic attributes, are overweighted in the decision process (e.g., Bowles 1998, 90-91; Frey and Stutzer 2004; and 2006; Hargreaves Heap 2005, 201-202). In general, the intrinsic attributes are rewards stemming directly from an action, for instance, satisfaction derived from meeting friends. The action is pursued because it delivers this satisfaction. Conversely, extrinsic attributes "serve people's goals for material possessions, fame, status or prestige" (Frey and Stutzer 2004, 3). Actions chosen because of their extrinsic attributes are not valued for themselves but only as means to another end where the satisfaction lies. It is not necessary here to develop the psychological theory supporting this distinction (e.g., Deci and Ryan 2000). However, it is important to note that extrinsic attributes are the ones driving the race for social status, a phenomenon taking a central place in the EUA. Saying that extrinsic attributes are overweighted in the decision process is akin to assert that the benefits of higher status are overweighted.

Hence, behavioural studies on the effects of social interactions tell us that a dimension of these interactions—the one associated to status-seeking—is one source of the disjunction between decision utility and experience utility. If we take this disjunction seriously, the neoclassical welfare analysis is undermined, since this analysis assumes that the model of individual choices can also be used to evaluate individual welfare. With the insight of behavioural economics, it appears that explaining choices with an extended utility model should be sharply distinguished from evaluating the efficiency of the outcome.

I have three comments before closing. First, the divergence between behavioural economics and the EUA shows how much the notion of interdependent preferences is treated differently depending on one's starting point. If the researcher begins the enquiry by wondering how to add some variables in the utility function to account for our folk understanding of social interdependence, she is led in the direction of the EUA. Alternatively, if one starts by studying how an agent's ex ante and ex post valuations relate to each other for different types of choice, she comes to emphasize that our concern for relative standing makes us ultimately dissatisfied with our chosen option. The understanding that EUA scholars have of interdependent preferences thus hinges on the fact that they are responding to the reformist challenge.

Second, and more importantly for the present paper, the divergence between the results emphasized in this section and the presumption of the EUA demonstrates that the EUA has to be transformed by its confrontation with empirical evidence on the form of the interdependence. It is a chimera to think that, provided we have postulated an extended utility function, "public policy can be evaluated without any ambiguity" (Becker 1996, 20).

Third, to situate this section in the rest of my argument, I need to emphasize that the claim that decision utility cannot be used to evaluate welfare is logically independent from the developments in the EUA; the rift between decision and experience utility, provided it is serious, is devastating for standard welfare analysis regardless of the existence of the EUA. This result is however highly relevant when EUA scholars survey their options to pursue future research. Confronted with a wealth of incompatible policy conclusions, proponents of the EUA have to look for ways to circumvent this undesirable outcome. It is in this search that the relationship between status concerns and dissatisfaction is more likely to come to saliency. In other words, if no feeling of crisis was present, EUA scholars could go 'business as usual' and totally ignore other lines of research. In the present case, however, something must be done to redirect the EUA for the sake of policy relevance.

CONCLUSION

For more than a century, critics have complained about the "atomized, *under*socialized conception of human action" (Granovetter 1985, 483) in neoclassical economics. According to them, the specification of homo economicus has led neoclassical economics to sketch a systematically biased image of social issues. Consequently, welfare economics has often been stigmatized as being nothing more than a jargon used by some scholars to advocate their particular prejudice. In the public arena, argue the critics, 'scientific welfare' is one political strategy among many: "Different social groups struggle for their alternative social programme utilizing an arsenal of weapons that includes, for many, their respective efficiency calculi" (Wolff 2006, 188).

At first, economists paid little attention to the criticism regarding their undersocialized agents. It is only recently that a wealth of scholars started to allow complex social interactions in their neoclassical models. The extended utility approach (EUA) is a highly popular way to extend economic models in the direction of interdependent preferences. I have first argued that the EUA, by focusing on the relaxation of only one assumption (*in*dependent preferences) in the standard modelling procedure, is a typical variation on the reformist challenge.

Even with this somewhat mild change, the EUA has comforted the opinion of the critics of standard welfare analysis; this analysis seems indeed to be systematically biased. Indeed, the second part of my argument was exactly that, once we allow for *inter*dependent preferences, economists tend to conclude that "none of our conventional welfare conclusions can be taken for granted" (Samuelson 2006, 263). Moving on in my argument, I have maintained that an important shortcoming of the EUA is that, after blowing up the conventional set of governmental 'good practices', it is incapable of supplying new consensual policy stances.

To remedy this defect, it seems that a wider array of evidence should be used to discriminate between competing specifications of the extended utility function. The problem that I envisage for the EUA is that, if they attempt to account for all available evidence, they will meet other strands of research, strands that they could have completely disregarded otherwise. My last claim has been that this research dooms standard welfare analysis and, with it, the original project of the EUA. Welfare cannot be evaluated with the same function that is used to characterize choice.

This finding, presented in the last section, on the disjunction between choice and satisfaction is interesting regardless of the state of standard welfare analysis. But, in the past, other studies on the peculiarities of human satisfaction—e.g., Tibor Scitovsky's fascinating *Joyless economy: an inquiry into human satisfaction and consumer dissatisfaction* (1976)—have been totally ignored by the bulk of economists. The current episode is different because the results of the

EUA have debunked conventional policy stances, and there is a clear feeling of crisis. I maintain that it is because EUA scholars are forcing economists to look at a wider array of evidence to discriminate between alternative specifications that the standard welfare analysis is more exposed today.

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François Claveau is a PhD candidate at the Erasmus Institute for Philosophy and Economics, Erasmus University Rotterdam. His research focuses on the contributions of the new approaches in economics to the policy conversation and on the debate in economics over micro-foundations in the context of causal reasoning for policy use.
Contact e-mail: <francois.claveau@mail.mcgill.ca>

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The history of transaction cost economics and its recent developments

ŁUKASZ HARDT
University of Warsaw
Polish Academy of Sciences

Abstract: The emergence of transaction cost economics (TCE) in the early 1970s with Oliver Williamson's successful reconciliation of the so-called neoclassical approach with Herbert Simon's organizational theory can be considered an important part of the first cognitive turn in economics. The development of TCE until the late 1980s was particularly marked by treating the firm as an avoider of negative frictions, i.e., of transaction costs. However, since the 1990s TCE has been enriched by various approaches stressing the role of the firm in creating positive value, e.g., the literature on modularity. Hence, a second cognitive turn has taken place: the firm is no longer only seen as an avoider of negative costs but also as a creator of positive knowledge.

Keywords: transaction cost economics, Oliver Williamson, theory of the firm, modularity literature, cognitive turn

JEL Classification: B21, B31, D21, D23, D83

Transaction cost economics (TCE) has a long past since what we generally speak of as 'transaction costs' have been present in economic discourse for centuries. The past of TCE is rich in metaphors describing the idea of transaction costs, but the one with the most profound impact on the later development of TCE was the notion of frictions. That metaphor is strongly connected to the further metaphor of the market as a machine whose deviations from ideal functioning is characterized by frictions (e.g., Walras 1893). Therefore, the study of the past of TCE is guided by the study of its metaphors and particularly that of mechanical

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friction. The past of TCE was not encapsulated in a particular research program, but rather in uncoordinated attempts to give the elementary idea of "costly exchange" an operational counterpart.[1]

For centuries that elementary idea had been conceptualized as just costs of transportation (e.g., Aristotle's *Politics* and Smith's *Wealth of nations*).[2] Then, in the nineteenth century, Menger introduced the concept of friction into his *Grundsätze der Volkswirtschaftslehre* where it stands for various difficulties in the process of price formation. The growing popularity of the friction metaphor made it a useful concept for explaining given theoretical model's failures—economists simply introduced frictions (Klaes 2000a). That was the case for example in monetary economics at the beginning of the twentieth century, when economists considered why people hold onto cash rather than profitable assets.

It was John Hicks who first disagreed with general friction-based explanations: "The most obvious sort of friction, and undoubtedly one of the most important, is the cost of transferring assets from one form to another" (Hicks 1935, 6). Subsequently, in 1940, Tibor Scitovsky introduced the label of 'transaction costs' into the economic vocabulary (Hardt 2006). In the meantime Ronald Coase published his 1937 paper in which he attributed the existence of the firm to the cost of using the price mechanism (Coase 1937, 390).

It should be clear therefore, that TCE, understood as the study of the economic consequences of "costly exchange", existed a long time before becoming a research program within the framework of economics. It has a long past but as a science it has a short history.[3] That history began in the 1970s with the work of Oliver Williamson. The first appearance of the term 'transaction cost economics' was in the title of Williamson's article in 1979, "Transaction cost economics: the governance of contractual relations" in the *Journal of Law and Economics* and, as far as the study of transaction costs usually leads to the study of institutions,

[1] I use the term 'research program' in the entire article not in a strict Lakatosian sense, but merely as a theory or a set of theories developed in order to solve particular problems (for a further discussion, see the final paragraph of the forth section).

[2] The term 'elementary idea' is used here in the sense of Lovejoy (1982), namely as an idea present in various historical époques and in different cultures. Treating transaction costs as an elementary idea leads us to the conclusion that it is of crucial importance for economics as a whole, since "the number of essentially distinct philosophical [here: economic] ideas is decidedly limited" (Lovejoy 1982, 4).

[3] A reconstruction of the past of TCE can be found in Klaes 2000a; 2000b; 2001a.

he is also the father of the term 'new institutional economics', in *Markets and hierarchies*, 1975.

As it is indicated in the title of this article, my goal here is to reconstruct the history of TCE: the approach within economic theory emerging from Williamson's contributions in the 1970s. Since "[...] in order to evaluate the past [history] properly the historian of science must know the present" (Bachelard 1951, 9), I will try to find a theoretical bridge between the history of TCE—particularly of its emergence (the Williamsonian TCE of the 1970s)—and its recent developments. Consequently, I reconstruct the rise of Williamsonian TCE and claim that his approach, lying at the intersection of economics and organization, is to a great extent responsible for the first cognitive turn in economics: namely the limited transformation of the so-called mainstream economics (henceforth, ME) due to the study of economic activity as undertaken by agents characterized by limited cognitive capacity.[4]

First the character of Williamsonian TCE is analyzed and it is argued that what distinguishes his theory is his treating the firm *as an avoider of negative* (transaction costs). The underlying logic of the development of TCE can be described as a move from treating the firm as *an avoider of negative* (costs) towards conceptualizing the firm as *a creator of positive* (knowledge). I show that this was due to the (re)introduction of knowledge related problems into the realm of TCE following the incorporation of those elements of Simon's legacy which did not enter TCE in the 1970s.[5]

I describe this late incorporation of some elements of the Carnegie legacy as a second cognitive turn in TCE. Interestingly, as the first cognitive turn allowed for the limited incorporation of TCE into economic orthodoxy, the second one moved TCE back towards economic

[4] I define here mainstream economics simply as orthodox economic thought. For the purposes of this paper heterodoxy is understood as non-orthodoxy, where orthodoxy denotes the research perspective based on the framework of maximizing behavior. The further a given theoretical approach is from a maximizing (or cost-minimizing) framework, the more heterodox it is.

[5] The inveteracy of knowledge issues is an important distinguishing feature of modern TCE as opposed to the Williamsonian approach of the 1970s. In that sense TCE is not just one of many approaches dealing with the issue of incomplete information. If we treat information just as "data organized into a meaningful pattern", then even in the situation of possessing perfect information we may still have imperfect knowledge (treating knowledge as "information with a layer of intellectual analysis", e.g., beliefs about causality, see Hislop 2005, 16). That is why limited cognitive capacity leads to imperfect knowledge even in the presence of perfect information.

heterodoxy (Groenewegen and Vromen 1996). In other words, recent TCE literature can be seen as a more "new institutional" approach than was the case for Williamson's early writings, described by Dugger (1983, 96) as just "a more realistic and sophisticated neoclassicism". The closeness of recent TCE to "new institutionalism" is mainly due to the fact that nowadays its research apparatus is only partially built on the "economizing on transaction costs" principle. In that sense TCE, I claim, is more heterodox than in the early 1970s.[6]

THE FIRST COGNITIVE TURN: THE EMERGENCE OF TCE

While the term 'transaction costs' appeared in the economic literature relatively late, the notion of 'transaction cost economics' entered into economics even later, that is, in the work of Oliver Williamson from the late 1970s. Before that, the approach emerging from Coase's (1937) "The nature of the firm" was described as *transaction cost reasoning*, *transactional paradigm* or *transaction cost approach*. Surprisingly, even in his now classic papers from the early 1970s Williamson did not use the term 'transaction cost economics'. For Williamson the *transaction cost approach* was at that time outside the domain of mainstream economics, namely the orthodox economics based on the work of Arrow and Debreu.

In *Markets and hierarchies*, Williamson expresses his doubts about the place of *transaction cost reasoning* within economic theory as follows: "Whether such an approach qualifies as economics is problematic" (1975, 248). A few years later he adds: "[...] the origins of transaction cost theory must be sought in influences and motives that lie outside the normal domain of economics" (Williamson 1981b, 1538). In other words, in the economics built on the general equilibrium framework any attempt to incorporate transaction costs into the realm of ME would be treated as a heresy, and the term 'transaction cost economics' would seem an oxymoron.

In the 1970s, however, something had changed in ME: economic theory started to become more pluralistic again (as it had been in the 1920s and the 1930s).[7] On the one hand, many economists failed in their attempts to build a "whole" economic theory on the general equilibrium

[6] See Hodgson 1993, 12.

[7] I use the word 'pluralistic' here in a broad sense, namely that economics started to be rich in various theories (plurality of theories) and that economists gradually started to treat the growing plurality of theories as a positive phenomenon. See also Mäki 1997.

framework, e.g., because of the impossibility of formulating the so-called microfoundations of macroeconomics—the implication of the Sonnenschein-Mantel-Debreu theorem (Sent 2006). On the other hand, the introduction of transaction costs into the world of Arrow-Debreu resulted in claims such as that "[...] different social arrangements result in different transaction technologies purely as a result of legal ways of protecting property rights" (Kurz 1974, 4), i.e., that the set of possible transaction opportunities depends on the institutional framework of the economy. Consequently, ME has been transformed into various complementary approaches based on game theory, bounded rationality, experimental methods, and last but not least transaction cost reasoning.[8] In the late 1970s putting the term 'transaction cost' together with the word 'economics' became not only possible, but also desirable. The long past of TCE was over, and the history of TCE had begun.

This section is organized as follows. First, the emergence of Williamsonian TCE, described as the first cognitive turn in economics, is reconstructed. Then, in a second subsection, his theory is presented, focusing on his conceptualization of a firm as *an avoider of negatives*.

The rise of Williamsonian TCE

In the 1950s and 1960s the neoclassical theory of the firm started to be widely criticized for its unrealistic assumptions. The assumption of profit maximization was questioned as well as that of a firm's perfect information about market conditions. Katona (1951) claimed, for instance, that firms do not maximize profits, but act in order to satisfy managers' various ambitions. In the same way, argued Papandreou (1952), firms just maximize a so-called "general preference function", which aggregates the individual aspirations of members of an organization. Rothschild (1947) went even further and claimed that a firm's *raison d'être* is just to survive. Others did not reject the importance of making profits, but instead of the pure profit maximization assumption they preferred to talk about achieving satisfactory profits (e.g., Gordon 1948; Margolis 1958).

Such critique of the neoclassical theory of the firm opened up the black box of the Marshallian representative firm and shifted economists' focus of attention towards the study of the internal structure of the

[8] I do not claim here that the so-called degeneration or in other words great transformation of economics was only due to the two above mentioned facts. For an in-depth study of the reasons for the growing pluralism within ME, see Sent 2006; Colander, et al. 2004.

firm. Consequently, two kinds of theories emerged: those dealing with the issue of designing an incentive structure within the firm that would maximize the firm's chances of surviving in the market (e.g., Bernard 1938; Simon, et al. 1950); and an approach focusing on the issue of decision making within organizations, which took its origin from cognitive psychology (e.g., Simon 1957a). What links these two kinds of theories is a departure from the perfect rationality assumption and its replacement by the claim that individuals are characterized by bounded rationality: "[they] are intentionally rational, but only to a limited extent" (Simon 1957b, xxiv). *Economic man* is substituted by *organizational man* with limited computational and cognitive capacity (Simon 1978). For Herbert Simon the key to understanding the functioning of the economy is an analysis of the decision making process:

> The most important data that could lead us to an understanding of economic processes and to empirically sound theories of them reside inside human minds. Accordingly, we must seek to discover what went on in the heads of those who made the relevant decision (Simon 1997, 70-71).

Simon's research, particularly his concept of bounded rationality, had a profound impact on other economists working at Carnegie, and made the rapid development of organizational theory and behavioral economics possible. Two important features of research in the area of organizational theory undertaken at Carnegie were its interdisciplinary character and concern with empirical problems. Richard Cyert, one of the main proponents of behavioral economics at Carnegie, describes the character of the economic theory developed at Carnegie in the 1950s and the 1960s as follows:

> If you are doing behavioral economics you have to think about actual behavior and you also have to have the ability to move to the field of organization theory, and to borrow ideas from other fields too, such as psychology [...]. On the theoretical level it is important to learn to deal with bounded rationality and uncertainty. You have to deal with the real world (Interview with Cyert, in: Augier and March, 2002, 6).

The above statement by Cyert may suggest that economics at Carnegie was quite heterodox and was in opposition to ME, but that was not the case. One should note that apart from the behaviorist group there was also a strong ME group dealing with issues such as rational expectations and the theory of effective markets (for example, Franco Modigliani, John Muth, Merton Miller, Allan Meltzer, and later also Robert Lucas, Thomas Sargent, and Edward Prescott). Due to the relatively small number of economists at Carnegie, people from the two groups exchanged ideas and they quite often had very heated debates (Klaes 2001b). That intellectual atmosphere made Carnegie "an incredible place at which to be a student" says Oliver Williamson, a graduate student at the Graduate School of Administration at Carnegie in the late 1960s. In an interview from 1988 he adds:

> The Carnegie experience was extraordinary. I really enjoyed it [...], it was just such an interesting place to be. Interdisciplinary work was going on [which] included a good deal of work in organization theory [...]. I especially found the intersection of economics and organization fascinating, and I felt that there would be a lot of research opportunities here (Williamson 1990, 117).

Williamson's PhD dissertation entitled *The economics of discretionary behavior: managerial objectives in a theory of the firm* is situated just at the *intersection of economics and organization*:

> [...] although the objective function of the firm was reformulated in favor of realism in motivation, I worked out of a maximization rather than a satisficing setup. The dissertation therefore reflected some of the tensions between behavioral economics and orthodoxy (Williamson 1996, 150).

The research strategy of Oliver Williamson was to use the behavioral assumptions of organizational theory combined with the quantitative and marginal analytical framework of neoclassical economics (Allen 1999). The following statement by Williamson from "Hierarchical control and optimum firm size" clearly summarizes his research strategy:

> The strategy of borrowing behavioral assumptions from the organization theory literature and developing the implications of the behavior observed within the framework of economic analysis would seem to be one which might find application quite generally. Combining these two research areas so as to secure access to the strengths of each would thus appear to be quite promising (Williamson 1967, 135).

For Williamson, the theories and concepts of organization theory literature including those of Simon's behavioral economics were related to the analysis of individual decision making and hence had a very microeconomic character. However, the majority of organization theory's concepts were defined so broadly that it was nearly impossible to use them in empirical research. It became evident for Williamson that there was a need to translate the behavioral concepts of Carnegie into the language of economics (Simon 1997, 38).

In the late 1960s Williamson tried to explicate the rationale for vertical integration, but he could not find the answer within the framework of ME. That question is similar to the one posed by Coase in "The nature of the firm", but the answer given by Williamson is slightly different from that of Coase. Although Williamson was deeply convinced that the existence of market exchange costs was important for explaining the emergence of firms, "[he] was not persuaded of the possibilities inherent in the transaction cost approach" (Williamson 1990, 117). Then, while preparing a series of seminars on the theory of vertical integration requested by Julius Margolis, he discovered that the reasons for integration lie in the behavioral characteristic of contracting actors and first of all in bounded rationality:

> Bounded rationality is one of them. I don't know if I defined opportunism at the time, but we focused on two critical issues which are close to opportunism, namely limitations associated with promises and the fact that some promises need institutional support (Williamson 1990, 118).

Consequently, the problem of opportunistic behavior combined with that of bounded rationality arising in the situation of bilateral monopoly (small-numbers exchange) and uncertainty emerged as the defining features of his analytical framework. Subsequently, Williamson translated ideas from organization literature into concepts observable in the functioning of firms and markets: Simonian bounded rationality gave a theoretical foundation for formulating the idea of incomplete contracts and opportunism, and the search theories of Cyert and March (1964)—e.g., myopic search, trial-and-error learning, and local search—enabled Williamson to develop the concept of "feasible foresight". Next, he combined that conceptual framework with the "classical" assumption of neoclassical economics, namely that of cost minimization. The emerging transaction cost economics, here described also as Williamsonian TCE, followed. The first paper in which he used that framework was "The vertical integration of production: market failure considerations" (1971). Twenty years after its publication he says: "I really feel, at the time when I wrote the paper, that I cracked the problem. This was obviously a certain exaggeration. But I did have a sense that this reformulation [of concepts] really got to some of the basic issues" (Williamson 1990, 119).

The organizational theory of Carnegie was the first attempt within (broadly defined) economics of building a connection between (cognitive) psychology and (old behavioral) economics (Sent 2004, 739-740). That was possible mainly due to Simon's contribution to the so-called cognitive revolution: the successful attempt to bring psychological insights into the realm of economic theory and simultaneously to limit the role of behaviorism.[9] But still, organizational researchers at Carnegie remained quite dissatisfied with mainstream economics. Simon, for instance, left the Carnegie Graduate School of Industrial Administration in the 1970s for the psychology department of the same university, noting: "My economist friends have long since given up on me, consigning me to psychology or some other distant wasteland" (Simon 1991, 385).

Sent (2004) even claims that due to its distance from ME the organizational theory of Carnegie had a very limited impact on economic theory of the 1960s and 1970s; however, the emergence of Williamsonian TCE proves the contrary. There is no doubt that TCE had a profound impact on the state of economic theory in the 1970s, and that it is partly responsible for its current plurality. Moreover, there is no doubt that the rise of TCE in the 1970s was only possible due to the Carnegie revolution of the incorporation of psychological concepts into economics. In that sense, Carnegie, by making the rise of TCE possible, played an important role in transforming ME, and hence the rise of TCE can be treated as the first cognitive turn in economics.

The firm as an avoider of negatives in Williamsonian TCE

Every theory of the firm must answer the following question: why do firms exist? (Holmstrom and Tirole 1989, 165). According to Williamson, firms emerge when making transactions internally (within the firm) is

[9] One should note that the cognitive turn described here can be treated as an important step in the process of enriching economics with various ideas from the cognitive sciences. That turn is more advanced than "[...] the cognitive (half-) turn made at Cowles" (Mirowski 2001, 451), because the "(half-) turn" was mainly due to the incorporation of informational issues into the realm of economics (e.g., Marschak's work), and not the knowledge ones, as in the case of Simon's contributions.

cheaper than externally (on the market). That is similar to Coase's now famous statement that "the main reason why it is profitable to establish a firm would seem to be that there is a cost of using the price mechanism" (1937, 390); however, in "The nature of the firm" we do not find an in-depth analysis of the reasons for the positive costs of market exchange. Coase (1937) writes about "the cost of discovering what the relevant prices are", and "the cost of negotiating and concluding a separate contract", but does not elaborate extensively on these concepts, nor give any operational measures of transaction costs. Moreover, in his 1937 paper he does not study the interplay between institutions and transaction costs:

> […] as I came to realize when I wrote "The problem of social cost", all these interrelationships [between institutions and transaction costs] are affected by the state of law, which also needs to be taken into account in the analysis. But it is a theoretical scheme that incorporates these interrelationships that I believe will make my approach in "The nature of the firm" operational (Coase 1993a, 73).

Williamson goes a step further and offers a complex and rather complete analysis of the determinants of the mode of making transactions. First, he claims that:

> [...] the advantages of integration thus are not that technological (flow process) economies are unavailable to nonintegrated firms, but that integration harmonizes interests (or reconciles differences, often by fiat) and permits an efficient (adaptive, sequential) decision making process (Williamson 1971, 117).

Thus, economizing on transaction costs matters for selecting a given way of contracting. Although his work from the early 1970s was stimulated by empirical research, his papers from that period were of purely theoretical character. His aim was to build a conceptual framework which only later could be used as a tool in empirical research. The purpose of his subsequent articles (1971; 1973; 1975; 1979) was to conceptualize the interplay between various factors responsible for vertical integration.

> This was done in my article "Transaction cost economics: the governance of contractual arrangements" [...]. I think this is a key article [...]. This effort to so to speak "dimensionalize transactions" seemed to me at that time and since as an important step on the

> road of operationalizing this whole line of study (Williamson 1990, 120).

Although Williamson's 1979 paper shows the impact of asset specificity on the choice of organizational form, it does not elaborate on the interplay between the imperative of transaction costs minimization and the neoclassical rule of optimizing the size of production activity (economizing on production costs). In other words, he does not conceptualize the relation between asset specificity and the total production costs—neoclassical production costs plus transaction costs—(Menard 2007). That was due to his negligence of technological issues: "concentrating on the study of transaction technology resulted in disregarding the role of production technology" (Williamson 1988, 361).

In the late 1970s and the early 1980s he re-discovered the role of production technology in defining the mode of making transactions and hence the concept of asset specificity: naturally related to production technology, started to play a dominant role in the *explanans* of his theory. In his 1981 paper he writes:

> If assets are nonspecific, markets enjoy advantages in both production cost and governance cost respects: static scale economies can be more fully exhausted by buying instead of making; markets can also aggregate uncorrelated demands, thereby realizing risk-pooling benefits; and external procurement avoids many of the hazards to which internal procurement is subject. As assets become more specific, however, the aggregation benefits of markets in the first two respects are reduced and exchange takes on a progressively stronger bilateral character (Williamson 1981a, 558).

Simply speaking, Williamsonian TCE treats the firm as *an avoider of negatives* (Conner 1991). First, as *an avoider* of high exchange costs on the market. Second, as *an avoider* of the risks resulting from the hold-up problem. Third, as *an avoider* of opportunistic market relations. Since at the heart of Williamsonian TCE there is an assumption that "the same production activities can be carried on either within the firm or by a collection of autonomous contractors—that is except for problems of opportunism, the same inputs can be used equally productively in a firm or a market context" (Conner 1991, 142).

It is really hard to see here the firm as a creator of any positive value. That is contrary to earlier views of the firm such as that of the resource based literature that claimed that firm specific assets are more productive inside than outside the firm (e.g., Wernerfelt 1984). In the Williamsonian framework, if we do not have any opportunism, then any resource can be used with the same productivity within or outside the firm. Going even further, one could say that in the situation of the non-existence of opportunism, there would not be any reason for the emergence of the firm, but the actual nature of economic systems proves the contrary: firms emerge even when one cannot identify any opportunistic behavior.

The reason why Williamson treats the firm as *an avoider of negatives* lies in the fact that economists of organization focus their attention only on the role of the firm in constraining rent-seeking behavior resulting from imperfect knowledge (Langlois and Foss 1997, 6). They do not elaborate on the role of the firm in *productive* rent-seeking, namely the more efficient use of knowledge. "Economists have neglected the benefit side of alternative organizational structures; for reasons of history and technique, they have allocated most of their resources to the cost side" (Langlois and Foss 1997, 6).

In his early literature on TCE, Williamson concentrated on the issue of coordination: firms emerge in order to facilitate cooperation between various production inputs. In other words, firms materialize in order to avoid the market costs of coordination which are quite high in the case of boundedly rational agents confronting uncertainty.

> If, in consideration of these [cognitive] limits [resulting from bounded rationality], it is very costly or impossible to identify future contingencies and specify, ex ante, appropriate adaptations thereto, long-term contracts may be supplanted by internal organization [...]. Internal organization in this way economizes on the bounded rationality attributes of decision makers in circumstances in which prices are not "sufficient statistics" and uncertainty is substantial (Williamson 1975, 9).

Williamson summarizes his research strategy as follows: "A useful strategy for explicating the decision to integrate is to hold technology constant across alternative modes of organization and to neutralize obvious sources of differential economic benefit" (Williamson 1985, 88). So, in his work from the 1970s and the 1980s he neglected the role of firm-specific knowledge, i.e., the positive capabilities of a firm. He assumed that knowledge could be equally well transmitted between parties transacting on the market and those transacting internally. That claim is related to the ME assumption that firm behaviour—e.g., profit maximization—is invariant to its institutional form—e.g., ownership structure—(Foss, et al. 1999, 632). In the next section, I show that a departure from that very assumption of Williamsonian TCE opens the door for a new kind of theory of the firm in which an enterprise is treated as *a creator of positive* value.

THE SECOND COGNITIVE TURN

I have shown already that the first cognitive turn in economics was associated with the work of Simon and others from Carnegie. The role of TCE in this turn was to offer a link between neoclassical economics and cognitive psychology combined with Carnegie's organizational theory, e.g., TCE popularized the concept of bounded rationality in ME (Foss 2003).[10] In that sense, TCE played a significant role in making the economics of the 1970s more diversified. However, it did not incorporate into its *explanans* all the concepts and theories of the economics of information and cognitive psychology: "Williamson has taken only part of Simon's argument on board" (Hodgson 1993, 11).

TCE, for instance, neglects the role of knowledge formation and sharing in defining the way transactions are organized. Williamson's contributions from the 1970s implicitly assumed that knowledge can be equally well shared on the market and within the firm. It should be noted that even in the pre-Williamsonian theory many claimed that knowledge can be more easily transmitted within the firm (Malmgren 1961). That is due to the fact that knowledge often has a tacit nature and needs a stable environment to be efficiently shared: "the more often a particular transaction is made the more information the firm may have about that transaction" (Malmgren 1961, 414). That is not the case in anonymous market transactions. Such ways of understanding the economic role of the firm (half-) opened the door for theories conceptualizing the firm as *a creator of positive* value.

The introduction of knowledge issues into various theories of the firm, including TCE, has transformed the treatment of firms in economics (Grant 1996). For Williamson the way transactions are organized depends on asset specificity, uncertainty, and frequency of

[10] An interesting explanation of why Williamson built a "link" between ME and Simon's approach is offered by Pessali (2006). He uses a rhetorical analysis to show that the goal of Williamson was to persuade an ME audience to take his theory seriously and thus he had to relate TCE to their beliefs (Pessali 2006, 48). That is why, in his opinion, Williamsonian TCE shares some fundamental assumptions with ME (e.g., cost minimization).

contracting. When the focus is on knowledge, the three pillars of Williamsonian TCE do not offer a sufficient basis for predicting the emerging organizational form. It is quite intuitive that in the case of interactions rich in knowledge special governance structures should emerge. In the beginning of the 1990s it became evident that in a modern economy what really matters are the knowledge transactions (e.g., Starbuck 1992; for the study of knowledge intensive firms). But the way transactions are conceptualized in TCE is "at best incomplete for the purpose of treating knowledge transactions" (Foss 2006, 18).

According to Winter (1987), knowledge transactions can be conceptualized in terms of the characteristics of the underlying knowledge. Following from this, he offers four dimensions of knowledge transactions: tacitness versus explicitness, system quality versus stand-alone, teachability versus non-teachability, and complexity versus non-complexity. What follows is the so-called knowledge governance approach (KGA) which focuses on the problem of how to organize transactions to efficiently generate knowledge and capabilities (Nickerson and Zenger 2004, 617).

In the Coasian terminology a firm is an "island of conscious power in the ocean of market transactions" (Coase 1937, 5), and the reason for the existence of such islands is to economize on transaction costs. If we are to use the terminology of KGA, and particularly of the modularity literature, one can describe the process of firm formation as putting the interactions (transactions) within a single module: more precisely, a firm is a set of interactions (processes) that cannot be decomposed. A standard example, noted by Simon (1962, 470), is of the Tempus, a traditional Swiss watchmaker, who manufactured all the parts of a watch single-handedly without using any subassemblies supplied by external firms. The main reason for that specific way of organizing the production process is in the character of the underlying knowledge which is mainly of a tacit (subjective) nature. According to Langlois, "the firm exists because it offers a special kind of information exchange that somehow generates more knowledge than the 'sum' of the knowledge of participating individuals" (Langlois 2002, 34), and consequently, contrary to Williamsonian and Coasian tradition:

> Firms arise as islands of nonmodularity in a sea of modularity. They may do so in response to externalities arising from the likes of team production or asset specificity. More interestingly, firms may also arise in order to *generate* externalities, that is, to facilitate the

> communication of rich information for purposes of qualitative coordination, innovation, and remodularization (Langlois 2002, 34).

Consequently, "firms exist because they provide a social communication of voluntaristic action structured by organizing principles that are not reducible to individuals" (Kogut and Zander 1992, 384). In the presence of knowledge rich environments what is needed for effective knowledge sharing is a common language and powerful incentives. The KGA perspective still falls within the framework of TCE because its unit of analysis is still the transaction, but now this is a knowledge transaction, and due to the very nature of knowledge (e.g., its cumulative character, tacit nature, and public good characteristics) the *explanans* of TCE has to be enriched.[11] Since, according to the KGA approach, managers first choose valuable problems, and then the organizational mechanism that efficiently governs search (i.e., the search for knowledge), the character of a given problem is an important factor in determining the organizational choice. It should be noted here that the *explanandum* of TCE is still the same, i.e., at the heart of TCE is the question of how particular transactions should be organized.

If we have a decomposable problem (e.g, building a high-performance PC can be decomposed into manufacturing a high-speed processor, disk, and so on), the quality of the solution depends very little on interactions between knowledge sets (e.g., between the knowledge of the processor manufacturer and the knowledge of the hard disk manufacturer), and hence 'directional search' is best. Directional search is a search through trial and error, e.g., we put a given processor and a hard disk together and check whether we get a more efficient computer or not.

The contrary holds for non-decomposable problems, i.e., problems with intense interactions between knowledge sets such that knowledge sets cannot be separated into sub-problems, e.g., manufacturing a computer processor itself. In such cases, "an actor familiar with a particular technology cannot predictably enhance the value of the product design based solely on the knowledge he or she possesses" (Nickerson and Zenger 2004, 620). In this case 'heuristic search' is best, i.e., "trials are thus selected based on a cognitive map or implicit theory of how knowledge sets and specific design choices relevant to the

[11] For an in-depth analysis of the characteristics of knowledge, see Foray 2004.

problem interact to determine solution performance" (Nickerson and Zenger 2004, 621).

Therefore, when problems are non-decomposable, searching best takes place within the firm, and when they are decomposable the market will be a more efficient machine for organizing searching. In this approach the firm is able to produce valuable knowledge (or, in other words, to solve valuable problems). The language of Nickerson and Zenger's theory is taken from the work of Simon, e.g., the concept of non-decomposable problems was introduced by Simon (1962). The late incorporation of these ideas into TCE is due to the fact that these concepts needed a designation—knowledge—which the TCE of the 1970s was not sufficiently focused on. Thus, the incorporation of Simon's legacy into the theory of TCE took nearly thirty years. From this perspective the move from TCE defining the firm as *an avoider of negatives* towards the view of the firm as *a creator of positives* is not due to the incorporation into TCE of a totally new set of theories or concepts but rather to a more complete assimilation of the Carnegie legacy. Consequently, this transformation of TCE is considered here as its second cognitive turn because defining the economic role of the firm in a positive sense has radically changed the *explanans* of TCE.

It should be noted also that the growing importance of knowledge issues in TCE is related indirectly to the emergence of the new behavioral economics (associated with the work of Amos Tversky and Daniel Kahneman) which deals with various sorts of cognitive biases characteristic of contracting agents (Foss 2001a, 221). One way of mitigating these biases is to use an appropriate organizational form: "[...] organization is not merely a problem [...], but organization is often a solution" (Williamson 1998, 1). A good example is the so-called availability heuristic which states that people tend to overestimate the probabilities of events they have experienced in the past. Consequently, people individually tend to make systematic errors in risk assessments. However, when put together within the framework of a firm, they start to estimate risk in more objective ways thanks to those different individual experiences. Thus, by employing a specific organizational form (the firm), the negative effects of the biases caused by the availability heuristic can be reduced.[12]

This second cognitive turn in TCE also follows the development of so-called cognitive economics, for which:

[12] For more examples, see Foss 2001b.

> Cognition is not only about learning processes in the human brain, but also about external knowledge storage devices, asymmetric distribution of knowledge between individuals and the organization of communication between them (Martens 2004, 7).

Since the concern of cognitive economics is with extending the cognitive capacity of individuals, the emergence of various organizational forms can be treated as the result of "[...] the evolutionary search for ever more cognitive economy" (Martens 2004, 10). Consequently, the need for overcoming the limited cognitive capacity of individuals has come to be seen as an important rationale for the existence of the firm and a significant factor in determining its organizational form.

Last but not least, we should note here that this understanding of a second cognitive turn in TCE is reinforced by the nearly simultaneous growth of the knowledge management (KM) literature. That research agenda takes its roots from Bell's (1973) seminal book *The coming of post-industrial society*, in which Bell argues that the post-industrial society is built upon knowledge rather than things (Hislop 2005, 4). In a similar vein, the role of knowledge in contemporary society and in management practice was described by Peter Drucker: "The basic economic resource [...] is no longer capital, nor natural resources, nor labor [...] It is and will be knowledge" (Drucker 1993, 7).

Knowledge based goods and services have replaced industrial products, and hence the focus of management literature has moved from analyzing production processes towards analyzing knowledge transfer and creation. This becomes evident when we analyze the content of the leading management journals: e.g., in 1990 one can find less than 20 KM articles, but in 1998 there were nearly 170 (Scarbrough and Swan 2001, 6).

Since the role of the firm is to produce and transfer knowledge, the KM literature analyses the organizational structures of firms that facilitate these processes. Interestingly, the main research questions of the KM literature are closely related to those of the KGA approach described above. The claim that knowledge processes can be influenced by governance mechanisms integrates the KM literature and contemporary TCE, and hence reinforces the trend towards theorizing the firm as *a creator of positives*, in particular of knowledge.

THE CONTINUITY OF TCE

Our discussion of the transformation of TCE would be incomplete without some methodological reflection on the continuity between the Williamsonian approach and more recent developments in the TCE literature. Is it possible to claim, for instance, that the KGA approach still lies within the now broadly defined TCE? That question touches upon the issue of what constitutes the very essence of TCE reasoning. Three arguments for continuity seem compelling.

First, the crucial element of TCE's *explanandum* is still the question of how to organize particular transactions in order to achieve the best possible outcomes. Although the second cognitive turn in TCE has transformed TCE's *explanans* (i.e., we now consider more factors responsible for organizational choice), its *explanandum* is relatively untouched.

Second, the TCE transformed by the second cognitive turn still conceptualizes transaction activity as a human undertaking which takes place in a particular institutional framework. In other words, without institutional infrastructure making transactions would be impossible, and therefore the study of institutions matters. The analysis of the interplay between transaction costs and institutions is present in Williamsonian TCE and also in more recent theories, e.g. the modularity approach and KGA. Although transaction costs are conceptualized differently in various TCE branches, the essential meaning of that concept is the same and relates to an elementary idea describing a crucial characteristic feature of human action, namely that exchange is not a zero cost activity. Understanding the economic rationale behind broadly defined costly exchange motivated Coase's 1937 paper, Williamsonian contributions from the 1970s, and is still at the heart of TCE's research agenda. In Lovejoy's terms variations in the meaning of 'transaction costs' result from the fact that it is a "recurrent unit [idea] in many contexts" (Lovejoy 1982, 17), and also from the fact that TCE is such a diversified research program.

Third, it should be stressed that although Lakatos's 'research programmes' methodology has turned out to be of little use in analyzing the issue of overall scientific progress in economics, it can still be a reasonable perspective from which to study the structure of a given scientific program (Hands 2001, 287). Even if we do not treat TCE as a pure scientific research program in the Lakatosian sense, we can identify the hard-core's characteristics that are present in both

Williamsonian TCE and in more recent TCE, i.e., the assumption of bounded rationality, the notion of imperfect information, and the imperative of economizing on transaction costs. Consequently, and in contrast to Groenewegen and Vromen's (1996) opinion, it seems plausible that a more pluralistic theory of economic organization can be built within, and not outside, the domain of TCE. The above-described second cognitive turn in TCE is an important step towards such a theory.

CONCLUSIONS

In a paper aimed at reconstructing the development of TCE, Ronald Coase claims the following: "It is clear to me that Williamson's influence has been immense. In a real sense, transaction cost economics, through his writing and teaching, is his creation" (Coase 1993b, 98). However, as I showed briefly in the introduction, TCE is not only due to the work of Williamson, but also to various attempts to conceptualize the idea of "costly exchange". But Coase is certainly right in underlining the role of Williamson in the rise of TCE and in making its history. It was Williamson who built a theoretical bridge between neoclassical economics and Simon's approach, but he did not offer a complete synthesis of these research traditions.

The explanatory power of Williamsonian TCE lies essentially in the combination of the neoclassical logic of cost minimization (here: of transaction costs) with Simon's emphasis on the effects of bounded rationality, i.e., "[...] it is only because individual human beings are limited in knowledge, foresight, skill, and time that organizations are useful instruments for the achievement of human purpose" (Simon 1957a, 199). Therefore, TCE is situated at the *intersection of economics and organization*, and its contemporary development can be understood as a move from defining the firm as *an avoider of negatives* (i.e., of transaction costs, and other negative effects of bounded rationality) towards viewing the firm as *a creator of positives*. That move is due to the growing interest of economics in knowledge issues.

Interestingly, that process has redirected the attention of TCE back towards Simon's theory. Moreover, TCE has benefitted a lot from the recent developments in the cognitive sciences in which the firm has come to be seen as an important device for extending the cognitive capacity of individual economic agents. TCE may again play a crucial role in transforming modern economics just as it did in the 1970s, and the second cognitive turn in TCE may make that approach a real synthesis of neoclassicism and modern organizational theory (Dugger 1983, 111). Consequently, the second cognitive turn in TCE may in the near future appear as a cognitive turn not only in TCE, but in economics as a whole.

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Łukasz Hardt is an assistant professor at the Department of Economics, University of Warsaw. He also works at the Institute of Economics, at the Polish Academy of Sciences. He is particularly interested in the history of transaction cost economics, institutional economics, and the theory of the firm. In addition, he has written papers analyzing the economic transformation of Poland from the perspective of new institutional economics.

Contact e-mail: <lhardt@wne.uw.edu.pl>

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Tilting at imaginary windmills: a comment on Tyfield

YANN GIRAUD
Duke University

E. ROY WEINTRAUB
Duke University

Abstract: In the inaugural issue of this journal, David Tyfield (2008) used some recent discussions about "meaning finitism" to conclude that the sociology of scientific knowledge (SSK) is an intellectually hopeless basis on which to erect an intelligible study of science. In contrast, the authors show that Tyfield's argument rests on some profound misunderstandings of the SSK. They show that his mischaracterization of SSK is in fact systematic and is based on lines of argument that are at best incoherent.

Keywords: economics of science (ESK), sociology of scientific knowledge (SSK), science studies, meaning finitism

JEL Classification: B40, Z13

In the inaugural issue of this journal, David Tyfield (2008) examined the connection between the economics of scientific knowledge (ESK) and the sociology of scientific knowledge (SSK). He argued that, *pace* Wade Hands (1994), SSK can underwrite the claims of ESK. Indeed, for Tyfield, Hands's argument itself is a bit stronger suggesting that SSK *necessarily* underwrites the claims of ESK. Consequently the weakness, or incoherence, of SSK must do great damage to any attempt to employ economics as explanatory for the development of science.

Tyfield then re-presented SSK as seen by Hands, and recapitulated the reflexivity problems that were claimed by Hands to beset SSK. Tyfield argued that the issues are even more severe than Hands had suggested in 1994 and that, in particular, it is a necessary implication of

SSK that it employ and commit to employing the idea of "meaning finitism". Using some recent discussions about "meaning finitism" as it has appeared in the philosophical literature, Tyfield concluded that SSK is an intellectually hopeless basis on which to erect an intelligible study of science.

In what follows we shall show that Tyfield's argument rests on some profound misunderstandings of the SSK; misunderstandings that amplify Wade Hands's own confusions. We shall show that his mischaracterization of SSK is in fact systematic and is based on lines of argument that themselves are either incoherent or tendentious or both. Furthermore, we shall argue that Tyfield appears unaware of a scholarly literature that provides more than sufficient evidence of the futility of his claims, a literature that opponents of SSK who routinely mischaracterize their opponents' arguments have not engaged.

Tyfield's argument sets out stating that:

> [...] the philosophical problems of SSK are much more profound than the familiar problems of "reflexivity". In particular, finitism is intelligible only if it is false. It follows that SSK is not merely self-refuting, but, insofar as it holds onto finitism, it is unintelligible. If SSK is even to be able to sustain its *own* research program, let alone act as role model for an ESK, it must therefore forsake finitism (Tyfield 2008, 62).

To develop this argument, Tyfield notes that he must work out the nature of SSK. He correctly says that "SSK is the empirical examination of the generation of scientific knowledge as an open-ended and contingent social process, situated in specific socio-historical locations" (p. 63). He goes on to say that:

> The history of science reveals that the development of scientific knowledge is ridden with controversy. The 'facts' can be, and are, interpreted in many different ways. It follows that the 'facts' themselves cannot determine scientific knowledge. SSK instead turns its attention to the causal explanation of how different beliefs come to be believed (p. 64).

He claims that this particular set of ideas developed around 1982, but we note that the literature on this issue has developed quite extensively since then, and has moved far beyond such simplistic notions. For example, Latour (1999) reintroduced ideas of mutual stabilization of beliefs about nature and nature itself—an idea discussed fully by Fleck (1979) as early as 1935—which undermine any argument about a one-way causality that Tyfield seems to believe characterizes SSK. We shall return to this idea of mutual stabilization later.

The key move in Tyfield's paper—a move which we assert can be found in every paper which attempts to claim that SSK is self-refuting—makes its first specific appearance when he writes:

> Starting from the Kuhnian insights into the social relativity of beliefs and the theory-ladenness of observation, and the broader changes in post-positivist (e.g., Quinean) philosophy towards a non-foundational epistemology, SSK argues that whether our beliefs are true or false is entirely inaccessible to us, for we cannot step outside ourselves and our social world in order to compare our beliefs with the world as it is. It follows that there is no ultimate appraisal of scientific knowledge, only the situating of it in *further* scientific understanding of how 'scientific' knowledge is produced and the status of that 'knowledge' (Tyfield 2008, 64).

This paragraph requires scrutiny. Consider the phrase "SSK argues that whether our beliefs are true or false is entirely inaccessible to us, for we cannot step outside ourselves and our social world in order to compare our beliefs with the world as it is". It is important to realize there is no citation provided to ground this claim of Tyfield's, a claim which he regards apparently as self evident to anyone who reads anything about SSK. It is not however self evident, and is in fact nonsensical. SSK *does not* argue that whether our beliefs are true or false is entirely inaccessible to us, because SSK's or the pragmatist's use of true or false is not in fact a matter of "comparing our beliefs with the world as it is". That may be Tyfield's use of true or false, but it is not that of SSK. For us, we have no trouble whatsoever appraising beliefs as true or false, having a very good idea of whether the application of those words to beliefs is coherent.

Beliefs that are true or false for *any* pragmatist concern the work that the beliefs do, the commitments they engender, and the problems they resolve at a particular time and place and community. We have no problem saying that we cannot assess the truth or falsity of string theory, but that string theorists can use those words and have a very clear idea what it is they are agreeing to. We have a belief that it is true that the sun will rise tomorrow. That belief is very useful, and through a long set of contingent processes in which that belief has been useful, we feel very easy saying that it is true that the sun will rise tomorrow. We feel quite comfortable, that is, saying that we have a very clear notion of true and false as applied to these beliefs. Where then is Tyfield's problem located? It is located of course in Tyfield's insistence that we use true and false the way *he* wishes to use true and false. We agree (how could we not?) that whether our beliefs are Tyfield true or false is inaccessible to us, for we cannot step out of ourselves and our world to directly perceive the world as it is. That project of stepping outside ourselves to see the world as it really is—or should we say as it *really-truly is*—is a project in which we have no interest. For someone who is studying how scientists change their beliefs, that which *really-truly is* comes into being simultaneously with the beliefs themselves, that is, beliefs change in the course of doing the work for which having the beliefs is important. The beliefs and the evidence mutually reinforce, mutually stabilize, each other.

This shift in meaning in Tyfield's argument, a shift which moves the discussion from pragmatist meanings to anti-pragmatist meanings, of course entails that the pragmatist meanings are incoherent using the non-pragmatist vocabulary. They are "thus" absurd or self refuting. Anti-pragmatists practice this intellectual sleight of hand to their self-evident delight. That Tyfield is aware of his illegitimate move appears on the very next page (p. 65) in his footnote 10 where he states that "the two communities party to this debate, philosophers and sociologists, tend to use the word 'extension' in two slightly different ways [...] I will be using the term in the philosophical sense." This philosophical *now you see it, now you don't* adorns almost every page of his assault. For example, Tyfield writes that:

> It follows that, *pace* 'rationalist' philosophy of science, neither logic nor the empirical evidence determines the development of science. If this is the case, it follows that something else must determine what scientists believe and how those beliefs change (p. 67).

To assert that for SSK pragmatists, SSK students of the development of science, neither logic nor empirical evidence determines the development of science is simply strange. Who has ever asserted that evidence and logic do not both constrain and shape the development of science? Pragmatists are very comfortable saying that the kind of arguments used, and the empirical evidence used to support various claims, develop *pari passu* with scientific theories. Theories and evidence grow together. The evidence of position of the planets developed with astrologers' theories and astrological argumentation, just as the evidence of planetary movement developed with theories of planetary motion. How could it be otherwise? If you don't know what an egg looks like, if you don't know an egg from a hole in the wall, questions about chickens and eggs make little sense.

The classic example in economics of this misunderstanding is quoted by Tyfield as he reproduces Wade Hands claim that:

> Many of the advocates of the SSK claim to undermine the hegemony of the natural sciences by showing that what is purposed to be objective and 'natural' is neither one of these things, but rather simply a product of a social context in which it is produced. If this is true for all human inquiry, then it must be said for the SSK as well; this makes everything socially/context dependent and thus *relative* (Hands 1994, 92; Tyfield 2008, 69).

Tyfield immediately follows Hands's comment with his own:

> It follows that there would be no grounds, other than social happenstance, for accepting any belief, and this includes SSK *itself*. Hence the "problem of reflexivity" is that if the SSK argument is correct, we have no grounds to accept SSK itself (pp. 69-70).

This argument repeats itself over and over again in Tyfield's paper. For example, he says that:

> […] if we cannot take account of truth or falsity, we have no grounds on which to discriminate 'X' from 'not X' so that we can believe both. As such, the 'truth' and 'falsity' of our beliefs is a necessary condition of the possibility of rational judgement, and without judgement we fall prey to an all-consuming relativism that makes all beliefs equally 'defensible'.
>
> In other words, if we cannot refer to 'truth' or 'falsity' (as per symmetry), we must forsake altogether all use of these concepts, and *this includes tacit presupposition* as well as explicit usage. But this rules out rational judgment and so abandons us to relativism. (p. 72)

Again, here David Tyfield provides no evidence whatsoever that anybody has ever said anything along these lines. There are no citations, no claims that someone said this, or even someone said something that was close to this. All that is presented is a set of statements which are so absurd as to call into question the judgment of anybody who subscribes to such pragmatist views of truth or to any coherence view of the use of words like true or false with respect to beliefs. Quoting David Bloor saying in 1991 that knowledge is “a system of beliefs that a community collectively accepts as knowledge” is hardly something to make one’s jaw drop.

Equally problematic is Tyfield’s using other quotations out of context in an elliptic manner that renders their interpretation difficult for the uninitiated. For example, Tyfield writes in footnote 16 (Tyfield 2008, 73n) that Hands uses the expression ‘throwing oneself out with one’s own bathwater’, while acknowledging that Hands did so not in direct relation to SSK. But he does not then go on to reveal that Hands is actually referring to a neoclassical economics based philosophy of science, which was used by James Wible to argue against the hegemony of neoclassical economics. Linking Hands’s criticism of the neoclassical economics of knowledge (e.g., Wible) to the internal debates that occurred in the field of SSK is at best confusing and at worst misleading. Those internal debates, indeed, are referred to throughout Tyfield’s paper to argue against the consistency of SSK, but the paper itself provides no insight into the content of those debates. Similarly, Tyfield’s inclusion of David Hess’s assertion that “[in SSK studies] sociological theories and (anti) philosophical arguments upstage [its empirical work]” (Hess 1997; Tyfield 2008, 69) is rather jarring when one knows that Hess, in the same book, argues that although he is critical of “the social relativism that characterizes a corner of the social science community”, he is “*more* disturbed by the attackers’ dismissive caricatures and distortions of a huge volume of theory and research” (Hess 1997, 1, emphasis added). Tyfield is thus attacking precisely nothing.

That the claims we are making are unremarkable may be seen from examining the work of scholars who have explored this “self refutation” charge. The best single discussion of this is in Barbara Herrnstein Smith’s (1997) “Chapter 5: Unloading the self-refutation charge”. Smith shows that Tyfield’s kind of critique is as old as Socrates’s examination of Protagoras’s doctrine in *Theaetetus* and that the same string of arguments has been repeatedly used over the course of intellectual history to disembowel any new stream of philosophical innovators such as “the relativist, Hume, the epistemological skeptic, Nietzsche, the perspectivist, and, in our own era, postmodernists such as Kuhn, Feyerabend, Foucault, Derrida, Lyotard, Goodman and Rorty" (Smith 1997, 73-74).

Because the traditional philosopher of science often believes that one's epistemological position also sustains some higher values in society, such as moral judgment and scientific progress, that philosopher must also believe that beating the pragmatist will keep us from sinking into social anarchy, moral relativism, solipsism, or intellectual chaos. It is therefore necessary to ask: against which of those perilous evils does Tyfield wish to inoculate us? What higher purpose legitimates Tyfield's excoriation of SSK? The answer to this question is given at the end of the paper when the author returns to the issue of ESK. Tyfield, recall, believes that SSK necessarily undergirds ESK. He asserts that "ESK, if it is to do anything at all, must be able to offer a critique of how and where the imposition of economic imperatives on scientific research has a detrimental effect on the 'scientific knowledge' thereby produced" (Tyfield 2008, 82). ESK however is economic analysis, not an oppositional stance to modern scientific practices. Nor of course does SSK, or science studies, provide such a critique, at least if the word "critique" is understood as an attempt to valorise the practices of scientists who are engaged in profit-driven research. SSK has no interest in determining what a virginal scientific knowledge—if it ever existed—would look like. Instead, SSK can provide a careful examination of the ways industrial and academic research have evolved and accordingly tell a story about the construction of scientific knowledge that may annoy the non-pragmatic philosopher of science.[1] Tyfield's own annoyance confirms the claim just made.

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[1] We invite the reader interested in these issues to explore Shapin's (2008) new book. This acclaimed volume encourages a fair reading of what recent SSK has to offer, instead of a scandalized reconstruction of its so-called anti-philosophical epistemology.

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Yann Giraud is a postdoctoral fellow at the Center for the History of Political Economy, Duke University. His research focuses on the place of visual language in recent economics.
Contact e-mail: <yann.giraud@duke.edu>

E. Roy Weintraub is a professor of economics at the Economics Department, Duke University. His research has focused on the history of the interconnection of the mathematics and economics communities in the twentieth century.
Contact e-mail: <erw@duke.edu>

HOPE Center website: <www.econ.duke.edu/CHOPE/>

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Raging at imaginary Don-Quixotes: a reply to Giraud and Weintraub

DAVID TYFIELD
Institute for Advanced Studies, Lancaster University

In my article 'The impossibility of finitism: from SSK to ESK?' (Tyfield 2008), I argued that the sociology of scientific knowledge (SSK) is an important, indeed necessary, precursor of an economics of scientific knowledge (ESK), opening the way for the empirical exploration of the impact of economic factors on the production of scientific knowledge. Without SSK's arguments for the irreducible social-situatedness of science, such an ESK would be precluded, as explaining the development of scientific knowledge would be the preserve of an 'internalist' philosophy of science. SSK is thus an invaluable and indispensable contribution to our understanding of the actual scientific process.

I also argued, however, that SSK has some very serious philosophical problems. Most of the literature focuses on the problem of reflexivity. I focused, though, on a problem that I argued is more profound but that has received much less attention, namely 'meaning finitism'. This philosophical position has been increasingly emphasized by SSK, particularly of the Edinburgh School, as its primary philosophical basis. I argued that meaning finitism completely undermines the project of SSK because its explicit pronouncements contradict its necessary conditions of intelligibility, so that it is intelligible only if it is false.

Finally, having repudiated both the anti-SSK 'rationalist' philosophy of science and the anti-philosophical meaning finitism of SSK, I argued that a transcendental analysis of the necessary conditions of intelligibility of meaning-making (including in the scientific process) offers a way out of this problem. Were SSK (and ESK) to embrace this transcendental philosophical analysis, however, it would be recast as an (immanently) critical endeavour, capable of the empirical examination of science (at which it is good) without constantly having to fight rearguard philosophical actions to preserve its own capacity for reasoned judgement (at which it is not good).

I have opened this rejoinder to Giraud and Weintraub's (2009) response to my paper by restating my argument briefly because I hope this shows how far their characterization of my paper is from what it does in fact argue. Giraud and Weintraub's response makes absolutely no mention of the transcendental analysis at the heart of my argument. Nor does it discuss the critique of meaning finitism at any length. Discussion of the two central issues to my paper is thus simply absent. Instead, they caricature my position (Giraud and Weintraub 2009, 53) as simply an all-too-familiar anti-SSK tirade of a recidivist philosophy of science; indeed one with a lineage from time immemorial (pp. 57-58). The hugely positive assessment of SSK and the repudiation of such 'internalist' philosophy of science in my paper are also thereby simply disregarded.

Nevertheless, I am grateful to Giraud and Weintraub for giving me this opportunity to clarify my position. I expect what follows is also unlikely to convince them, but it will at least serve to repudiate some important misreadings of my argument. In only a brief article such as this there is insufficient space to deal exhaustively with all the points raised by their reply. I will therefore proceed directly to the substantive issues their reply raises. I must first, however, briefly rebut a number of the more shrill of their accusations, though others will have to go unanswered.

A major plank of their argument is that I either misquote or do not reference at all central claims of my characterization of SSK. The implication is that the SSK criticized is a straw man, indeed "simply strange" or "absurd" (Giraud and Weintraub 2009, 55-56)—despite my explicit care to appraise SSK on its own terms (Tyfield 2008, 71-72). There are several issues here. First, I must reply regarding the specific allegation of 'sententious' misquotation regarding the 'baby throwing itself out with its own bathwater' analogy (Giraud and Weintraub 2009, 57; referring to Tyfield 2008, 73). As my use of this phrase makes perfectly clear, I am not quoting Hands directly (the phrase is not in quotation marks) but I reference him since the phrase per se is his not mine, albeit in a different context. Lest there be any doubt, here again is what I say: "Hands (1994, 95) uses the phrase, but I note that he is not referring directly to SSK when he does so" (Tyfield 2008, 73, footnote 16). Furthermore, the footnote continues with references to others who have used similar phraseology about SSK itself, so that my use of it is indeed familiar and defensible.

Secondly, that I do not offer references in some sentences regarding characterizations or criticisms of SSK is simply beside the point. On the one hand, my characterization is heavily referenced, so there is plenty of evidence presented for the views attributed to SSK (see *inter alia* Barnes and Bloor 1982; Barnes, et al. 1996; Bloor 1981, 1991, 1997, 1998, 2004), including specific examples of directly contradictory pronouncements from leading SSK proponents (e.g., Tyfield 2008, 76, footnote 19). Arguing that they are absent in some specific sentences is simply to make selective use of the whole paper. On the other, their demand seems to rule out any paraphrasing for the sake of subsequent philosophical appraisal. Yet it is SSK's argument that I am assessing and I see no reason why I should be limited to use its own words to do so.

Ironically, much of Giraud and Weintraub's comments are themselves based on misreadings and non-sequiturs, which seem to evidence a determination to find fault rather than a will to engage. Amongst the most striking examples is the claim that "Tyfield is aware of his illegitimate move" (Giraud and Weintraub 2009, 55) regarding a shift from the 'pragmatist' theory of truth supposedly employed by SSK to the 'anti-pragmatist' one allegedly employed in my argument. In fact, I do not accept this move is illegitimate, as I discuss below. But the evidence adduced for this alleged *mea culpa* is a footnote clarifying the different, and potentially confusing, use of the term 'extension' by philosophers and sociologists, respectively as the set of things covered by a class-term and the act of developing or extending that set. It is hard to see the connection here with the point Giraud and Weintraub are making.

Similarly, dismissing my characterization of SSK as simply absurd also depends upon misquotation and wilful refusal to understand what is being said. For instance, following the quotation of a paragraph regarding the factors involved in the development of science, they suggest that the quotation attributes to SSK some outlandish views about epistemological issues regarding the interaction of evidence and theory (Giraud and Weintraub 2009, 55). Yet the paragraph clearly refers to SSK's (legitimate) repudiation of the belief of 'philosophers of science' that the development of science can be fully explained by an internalist account, i.e., an entirely orthogonal issue, as the sentence immediately following (which they choose not to quote) makes clear: "SSK's solution is that social interests are the relevant determining factor and thus social science can explain the development of science more generally (Barnes 1982, 35; Barnes, et al. 1996, 29)" (Tyfield 2008, 67).

Let us turn now to the substantive points in their reply, of which three are particularly important. These are: (1) that my argument is based primarily on the impatience of a "philosopher of science" (Giraud and Weintraub 2009, 58) with SSK; (2) that this is illegitimate as SSK must be appraised according to its own pragmatist criteria, and thus not doing so presents a straw man that merely finds in SSK faults of its own making; and (3) that the argument as a whole evidences a familiar lack of engagement with SSK's sociological work, rather than philosophical argument, as can be seen in my demand for the illegitimate importation of an evaluative or normative dimension to its descriptive, empirical programme. I will deal with each of these in turn.

First, let me restate that I think SSK, in both its philosophical pronouncements and sociological work, offers exceptionally important and cogent insights into understanding of the development of scientific knowledge. For instance, I whole-heartedly endorse the argument that explaining why scientific controversies pass cannot be conducted on the presumption that the 'true' position prevailed, but depends (overwhelmingly, perhaps) on the entirely contingent consonance of particular positions and social context and the sheer fading into obscurity of those who oppose the emergent dominant paradigm and their findings (e.g., Barnes, et al. 1996, 35). I also gladly concede to Giraud and Weintraub that scientific knowledge is accepted by particular scientists on the basis of pragmatic, socio-historically situated judgement and so must be empirically studied as such.

Nevertheless, from *within* SSK thus, I remain critical of it on two counts. First, its philosophical reflections *undermine* its important sociological programme and, moreover, do so needlessly. Giraud and Weintraub seem to claim that one must be implacably opposed to SSK to see this as the case. Yet this is manifestly contradicted by the heated debate *within* SSK regarding self-refutation and the implications of it (e.g., Pickering 1992). Their insinuation that to be critical of SSK is to be anti-SSK *ex ante* is thus totally bogus. Secondly, I argue that rectifying SSK's needless and needlessly distracting philosophical *aporia* also thereby alters the sociological project slightly, by admitting the normative dimension, always already there, of its subject matter.

These two criticisms, in fact, map almost directly onto Giraud and Weintraub's second and third substantive criticisms, namely the illegitimacy of appraising SSK with non-pragmatist criteria (to use their terminology), and the illegitimacy of demanding a critical edge to SSK respectively. In turning to these issues, though, I will focus specifically on the issue of meaning finitism, not pragmatism as Giraud and Weintraub do, for three reasons: first, my original paper focuses on the former not the latter as the central philosophical position of SSK, *pace* Giraud and Weintraub's suggestion to the contrary (2009, 52-53) that it is my argument that finitism is central to SSK, rather than SSK's own claim that it is so;[1] secondly, shifting to the latter would thus demand a fuller treatment than can be provided in the limited space of a reply; and finally, I would argue in any case that the issues raised by Giraud and Weintraub are subsidiary problems to that of meaning finitism and so can be dealt with substantially the same form of reasoning.

Taking each issue in turn, I readily concede that SSK (or indeed any position) cannot be legitimately appraised except from within, i.e., by way of immanent critique. Otherwise analysis does indeed lead to the problems Giraud and Weintraub indicate regarding straw man fallacies and finding problems that are the result of the evaluating framework itself, not the position being appraised. However, this does not mean that SSK can only be philosophically assessed using pragmatist criteria, for immanent critique also includes comparison of what a position states and what it necessarily presupposes. This is precisely the nature of my argument regarding SSK's problems with meaning finitism; i.e., it involves the assessment of meaning finitism *not* according to some *ex ante*, externally imposed criteria, as Giraud and Weintraub argue, but using concepts that it itself necessarily uses as a condition of its intelligibility.

This form of argument, examining necessary conditions of intelligibility, however, leads to a two-stage critique. The first stage highlights the contradiction between explicit pronouncements and implicit presuppositions, leading to the conclusion that the former must be false. In the case of meaning finitism, as I show in my paper, this leads to the conclusion that this position is intelligible only if it is false because it presupposes intensional, and not merely extensional, meaning—meaning that, both, enables *and constrains* its future use—while explicitly denying such. But insofar as the latter (e.g.,

[1] See, for instance, Barnes, et al. 1996; Bloor 1998; Bloor 2004.

intensionality) is derived by transcendental argument, starting from the pragmatic, socio-historically situated premises demanded of a legitimately non-foundationalist SSK, then SSK no longer needs to choose between the false dilemma of a non-foundationalist extensionalism (use determines meaning) and an *ex ante* intensionalism (meaning determines use). Rather the open-ended non-logically-determined and eminently socio-pragmatic matter of extensionality of meaning is seen to be a *mutual condition of intelligibility* of intensionality. And intensionality is understood here as the possibility of a proposition or term to have a determinate meaning in a given socio-historical context and *not* a fixed, complete and perfect essence.

The conclusion of my argument is thus that the concepts repudiated by SSK (in this case intensionality) are both fundamentally ungrounded, as SSK correctly argues, *and* necessary or inescapable, as it consistently denies, hence its intractable philosophical problems. SSK often comes tantalizingly close to this conclusion itself, only to refute it at the last minute. For instance, Barnes, Bloor, and Henry (1996, 85) state that "there would appear to be no escaping a realist orientation to the world we live in and the ubiquitous conventions of the realist mode of speech". This is incredibly close to my argument, but this conclusion is cast in terms of a particularly intractable and lamentable social 'convention', and one SSK should be on its guard to repudiate. Yet the fact that such 'realist' talk *cannot be avoided* is because it is a necessary condition of intelligibility of discourse itself, not because of social convention identifiable a posteriori. In short, SSK need only admit this problem to be an inescapable philosophical one, and it could preserve its non-foundationalism or pragmatism while forsaking its forlorn attempt to do without that on which it necessarily depends.

This takes us to the final point, namely the suggestion that my paper overlooks empirical work in SSK and is thus harmfully incomplete in its conclusions regarding what can and cannot be done by ESK and SSK. In particular, it is argued that I would aim to have a SSK/ESK that can uncover the deleterious effects of commerce on a pristine "virginal" science (Giraud and Weintraub 2009, 58), while actual SSK work, such as Shapin's (2008), does engage with these issues but yields completely different insights.

Certainly, there is little discussion in my paper of the details of this literature, if only due to constraints of space and the paper's primary focus on a philosophical argument. It is also the case that I would like an ESK that can explore the impact of economic social factors on the production of scientific knowledge, and in ways that go beyond the insights yielded by existing SSK. This is not, however, premised upon an *ex ante* presumption (borne of a 'rationalist' philosophy of science) that 'money' is 'bad' for science—what Mirowski and Van Horn (2005) have called 'Mertonian Toryism'—but the acknowledgement that the goals of commerce and of science, manifest in all their complexity in concrete situations, may often be in conflict; which is hardly controversial. Hence, as I put it in my article, an ESK "should be able to offer a critique of *how and where* the imposition of economic imperatives on scientific research has a detrimental effect on the 'scientific knowledge' thereby produced" (Tyfield 2008, 82, emphasis added). And, perhaps I should add as its flipside: *how and where* it has no such detrimental effect.

On this conception, however, Giraud and Weintraub's citation of Shapin's (2008) new book as the kind of work that shows what SSK *can* do and its incompatibility with the programme I am proposing—to my supposed "annoyance" (Giraud and Weintraub 2009, 58)—can be seen to be quite wrong, for in many respects this is exactly what Shapin's book does. Indeed, Giraud and Weintraub may be pleased (or perhaps disappointed) to know that I read this book (as other work in the field) with great interest. I thoroughly endorse Shapin's statement that "later modern entrepreneurial science is sometimes celebrated and sometimes condemned. […] But […] rarely is it *described* in much detail" (Shapin 2008, 229, original emphasis) and that this forms a good basis for a programme of social scientific work. Similarly, I fully accept Shapin's insistence upon the need to explore the ongoing shift in boundaries between commerce and science in detail, as a social phenomenon that is not fully amenable to "unitary, simple or tidy" linear accounts (Shapin 2008, 13)—from mythical pasts to idealized present—and that such research should be based upon the presumption that "it is better to see the relationship between virtue and the pursuit of knowledge [as one that] has been reconfigured than to assume it has been dispensed with" (Shapin 2008, 17).

However, I do not accept Shapin's explicit protestations (Shapin 2008, 18, 313)—even while I accept them to be perfectly genuine—that his work is thus *purely* descriptive and without any normative conclusions. Certainly, normative commitments cannot legitimately structure the empirical work *ex ante*, but supposedly neutral description of that which is always already value-laden is impossible and will in general serve merely to naturalize the status quo. The only way to avoid this, and thus to keep *open* the relevant normative questions, is to engage with them explicitly. In this respect, Shapin's investigation can be seen to be radically incomplete. He asks effectively: is individual scientific virtue equally prevalent in both academic and industrial settings? Yet the more important question, and the one that keeps open the associated normative issues, is: how has commercializing science changed the science done and which or whose interests does this privilege? Shapin's question describes the problem, while the latter situates it, and *both* are necessary.

Against Giraud and Weintraub's (2009, 57) suggestion that my position is one of a stout defender of the moral purity of science against the relativist barbarians, the transformed and critical SSK I am proposing explores science as a highly contested, social and value-laden process—both after *and before* its current commercialization. In this context, though, SSK has the role of identifying the social forces impacting on the production of science and holding these up for open, participatory debate.

Furthermore, this is not to deny that the complexity of the empirical reality renders such normative judgement difficult. But it does not make it *impossible*, unless one is tacitly assuming that normative judgement must itself always be sweeping and monochrome, rather than detailed and nuanced. Indeed, the conclusion of such investigation is *both* a wholesome disillusionment with grand black-and-white normative judgements, as per reasonable pragmatist scepticism, *and* detailed understanding of the complex interweaving of potentially contradictory normative trends and effects based on acknowledgement of the inescapability of normative judgement on social phenomena, for all its difficulty. It is the latter that SSK's descriptivist posture systematically occludes.

To be sure, this is a more politically engaged form of SSK, but I am by no means alone in arguing for this from *within* science and technology studies (STS) more broadly, where there are increasing calls for STS to engage with political issues, including evaluation of the effects of commercialisation of science on the 'knowledge' produced (e.g., a forthcoming special edition of *Social Studies of Science*). Furthermore, with both grand normative conclusions and denial of normative responsibility ruled out, the conclusion of such enquiry is not crude political slogans but the deepening of the embodied capacity for normative judgement, i.e., a form of 'moral/political education' with potentially significant social repercussions.

In short, in our mutual affirmation of Hess's comment that the "dismissive caricatures and distortions of a huge volume of theory and research" (Hess 1997, 1; as quoted on Giraud and Weintraub 2009, 57) is potentially even more troubling than some of SSK's more relativist excesses, Giraud and Weintraub's and my position are much closer than they evidently care to admit. Nevertheless, their dismissive refusal to engage with the latter, and arguments about it, does little to further the debate. Indeed, such hostile repudiation of even SSK-sympathetic criticism can only deepen the philosophical problems that beset SSK by encouraging the continued refusal even to admit their existence. It is also effectively to block any possibility of moving beyond the sterile and heated debate about the problem of reflexivity, to which Giraud and Weintraub seem committed to drag us back. Conversely, I would argue that the transcendental analysis and critical project I have proposed could lead beyond the long-standing 'dialogue of the deaf' between SSK and philosophy of science to their mutual improvement and benefit. Readers will decide for themselves which path seems more attractive.

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David Tyfield is a research associate at the Institute for Advanced Studies, Lancaster University. His research is on the (global) political economy and philosophy of science and innovation, with a particular focus on climate change and the life sciences.
Contact e-mail: <d.tyfield@lancaster.ac.uk>

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The booming economics-made-fun genre: more than having fun, but less than economics imperialism

JACK J. VROMEN
EIPE, Erasmus University Rotterdam

Abstract: Over the last few years there seems to have been a sharp increase in the number of books that want to spread the news that economics is, or at least can be, fun. This paper sets out to explain in what senses economics is supposed to be fun. In particular, the books in what I will call the economics-made-fun genre will be compared first with papers and books written by economists with the explicit intent of making fun of economics. Subsequently, it will be examined whether or not it makes sense to accuse books in the economics-made-fun genre of economics imperialism, as some commentators have recently done.

Keywords: economics-made-fun, pop economics, freakonomics, economics imperialism, sociology of economics

JEL Classification: A11, A13, A14

In the wake of Levitt and Dubner's best-selling *Freakonomics* (2005) there has been an upsurge in the publication of similar books. Tim Harford's (2005) *The undercover economist* and *The logic of life* (2008), Steven E. Landsburg's (2007) *More sex is safer sex*, Tyler Cowen's (2007) *Discover your inner economist*, Robert Frank's (2007) *The economic naturalist*, and Diana Coyle's (2007) *The soulful science* are cases in point. *Freakonomics* was not the first book in this genre. It was preceded by books like Steven Landsburg's (1993) *Armchair economist*, David Friedman's (1996) *Hidden order*, John Kay's books (such as his 2003 book *The truth about markets*), and Charles Wheelan's (2002) *Naked economics*. But the success of *Freakonomics* surely provided the strongest impetus to the genre.

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Can we really speak of a distinct genre here and, if so, what are its defining and distinguishing features? All the books mentioned above are popularizing books. They all try to reach a broader audience of non-specialists. And most of them want to report on new and sometimes path-breaking research at the frontiers of the discipline of economics. In short, they all want to impart the typical economic way of thinking in general and recent particular developments in economic theorizing to the non-cognoscenti.

Several labels have been proposed for this genre. On Amazon Listmanias, we find 'economics made fun',[1] 'pop economics',[2] and 'cute-o-nomics',[3] as alternative labels for roughly the same sets of books. Since both 'pop economics' and 'cute-o-nomics' have negative, pejorative connotations and I do not want to start my discussion from the outset with such clearly value-laden labels, I opt for the more neutral 'economics made fun' label, and consider all the books set out above as attempts to show to non-economists that doing economics can be fun.

It is not so clear where to draw the boundaries of the economics-made-fun genre. Lately several books have been published that aim specifically at popularizing behavioral economics. Dan Ariely's (2008) *Predictably irrational* is perhaps the clearest example, but Thaler and Sunstein's (2008) *Nudge*, Hallinan's (2009) *Why we make mistakes*, Jonah Lehrer's (2009) *How we decide*, and Peter Ubel's (2009) *Free market madness* can be mentioned here as well. As behavioral economics self-consciously sets itself apart as different from and in several ways critical of standard economic analysis, it can be argued that these books comprise a genre of their own. On the other hand, the books in the economics-made-fun genre mentioned above are typically less critical of standard economic analysis, though they all also do take at least some ideas and insights from behavioral economics on board. For convenience, I will simply assume that the books that specifically popularize behavioral economics do not belong to the economics-made-fun genre.

Once one starts looking more closely and in greater detail into the books that clearly do belong to the economics-made-fun genre, real and profound differences between them meet the eye. For instance, unlike the other books, Cowen's is in the tradition of self-help books: it

[1] See http://www.amazon.com/Economics-Made-Fun/lm/R2FQSXD5EFEA17
[2] See http://www.amazon.com/Pop-Economics/lm/R29R7RXDYXGUQU
[3] See http://www.economist.com/blogs/freeexchange/2007/07/whats_wrong_with_cuteonomics.cfm

contains practical tips for how to live a better life. In contrast, Levitt and Dubner's *Freakonomics* mostly gives a highly readable exposé of "clever" academic economics papers written by the "maverick" economist Stephen D. Levitt and various co-authors. As several commentators have observed, there is not much explicit use of economic analysis in *Freakonomics*. Levitt and Dubner themselves explicitly declare that much in their book is the result of what they call an "honest assessment of the data". By contrast, Frank's (2007) book consists of little more than an informal discussion of the basic principles of economic analysis and how they can be put to use in explaining everyday enigmas.

Yet, despite their differences, I believe books in the economics-made-fun genre all have a few things in common with each other. They all hold that economics can be fun in the following three senses:

1. The basic principles and tools of economic theory are presented in a "light", informal, accessible and entertaining way. Mathematical equations and graphs, with which standard textbooks for introductory economics courses are replete, are conspicuously lacking. The key idea is that the gist of economic principles and their use in explanations of all kinds of phenomena can be taught and understood as well, and perhaps even better and more easily, without invoking esoteric formal language.

Robert Frank especially, in his *The economic naturalist*, is most explicit in promoting what he calls the narrative 'less-is-more' approach to teaching economics. According to the narrative theory of learning, there is no better way to master and remember an idea than to see the idea in action in a catchy story. And the 'less-is-more' approach to learning is based on the insight that to get profound and lasting learning effects, it is better to teach just a few basic principles in economics in verbal form than to try to get the full panoply across with the aid of algebra and graphs.

In short, what is fun here is the way in which the core elements of economic analysis are presented. Economics is not just for autistic nerds. It can be wrapped up in such gripping ways that it also appeals to the most social, literate, and popular guy in high school. Since this sense in which economics can be fun pertains to how it is presented rather than its contents, let's call this sense: 'pimp your economics'.

2. The basic principles and tools of economic theory can be used to explain all kinds of interesting subjects, topics, questions, and phenomena. And, indeed, the scope of the subjects addressed in these books is astoundingly wide, reaching far beyond what are traditionally called economic phenomena. In fact, the subjects tackled are most of the time everyday phenomena that would traditionally be called non-economic. One of the more serious issues discussed in Levitt and Dubner's *Freakonomics*, for example, is the relationship between the legalization of abortion and criminality. Other issues are more trivial and frivolous, such as what sumo wrestlers and school-teachers have in common, and why drug dealers still live with their moms. Issues addressed in other books in the economics-made-fun genre range from why milk cartons have a rectangular shape while cola cans are cylindrical to why more sex is safer sex.

The fun here lies in the sorts of subjects addressed in economic analyses; they are taken to be more exciting and interesting than the supposedly dull or boring issues that are traditionally dealt with in economics. Thus, we could call this sense: 'economics used to tackle really interesting issues'.

3. The basic principles and tools of economic theory can be used to reveal the hidden side of all kinds of phenomena. In books in the economics-made-fun genre it is often argued that economic analysis is needed to look beneath mere appearances and uncover how things really are. "Conventional wisdom" - how things look at the surface - is often derided and taunted. Landsburg especially seems to have great "devilish" fun in debunking popular myths. Contrary to what is commonly believed, for example, Landsburg (1993) provocatively argues that seat belts kill rather than save lives. And, as the title of his book already indicates, Landsburg (2007) argues that more sex is safer sex.

The fun here is with the sorts of insights yielded by economic analysis. Economic analysis allows you to see what is really going on underneath. What is especially supposed to be fun is to tear folk wisdom to pieces. We could call this sense: 'economics reveals the truth they do not want you to know'.

For protagonists of the economics-made-fun genre, the three senses in which economics can be fun are related. They believe it is important to make "thinking as an economist" accessible to a larger audience, because they believe that thinking as an economist often leads to important unorthodox insights into a variety of interesting issues. In principle, what is aimed at could be no more than teaching standard economic theory to economics students in a more juicy, entertaining and engaging way (to bring it in line with the prevailing demands of pop culture, for example). But in fact the further aim is to make the general public more economically literate. The general thrust is that there is a lot to be learned for everyone from using economic principles to explain all kinds of phenomena, and not just phenomena that are traditionally deemed economic. That contributes to an enhanced understanding of the world around us. In short, the overarching aim is to enlighten the general public about the hidden economic side of everyday phenomena.

ECONOMICS-MADE-FUN GENRE *VS* ECONOMISTS-CAN-BE-FUNNY GENRE

The economics-made-fun genre is not to be confused with what could be called the economists-can-be-funny genre.[4] Papers in the economists-can-be-funny genre such as Blinder (1974), Krugman (1978), and Harbaugh (2003) are primarily meant to amuse mostly fellow-economists. Such papers poke fun at serious academic economics papers by parodying them and can be said to provide a healthy dose of self-mockery. In them, economists show some awareness of the limitations, weaknesses, and shortcomings of their own analyses.

Krugman (1978), for example, presents his paper as "[...] a serious analysis of a ridiculous subject, which is, of course, the opposite of what is usual in economics". Apparently, Krugman believes that though the subjects addressed by economics are normally important ones, the analyses of them given in economics should not be taken too seriously. This seems to be almost the opposite of what protagonists of the economics-made-fun genre are arguing. They seem to argue that economic analyses are always serious and that they should be taken (more) seriously by the larger public, but that the subjects addressed by economics have often not been very interesting. They set out to show that economic analyses of more interesting subjects, including (and perhaps even in particular) those that are not traditionally addressed by economics, are insightful and revealing.

That economists can be funny, especially by making fun of their own discipline, is also demonstrated by the world's first stand-up economist, Yoram Bauman.[5] Unlike some of the critiques expressed by practitioners of other social sciences or philosophy, Bauman's tone is not condescending, vitriolic, or scornful, but light, playful and understanding. What is more, Bauman's jokes display a thorough

[4] See http://petermartin.blogspot.com/2009/01/can-economists-be-funny.html
[5] See, for example, http://www.youtube.com/watch?v=YgB6mFmYEcM For more details visit http://www.standupeconomist.com/

understanding of economics and its basic principles.[6] It is perhaps telling that Bauman's performance is especially popular amongst fellow economists. This might strike an outsider as odd and perhaps even as cynical and irresponsible, but, as the numerous economics jokes documented by Clotfelter (1997) show, economists have a long and venerable tradition of making fun of the very principles they themselves use in their work on a daily basis. Unlike some of their critics, many economists see no contradiction between being open and explicit about the limitations and shortcomings of their own discipline and continuing to use its basic principles.

An exchange between Oxoby and Levitt shows in a hilarious way what can happen when these two genres are mixed up. In the University of Calgary Department of Economics Discussion Papers Series, Robert J. Oxoby (2007) published a paper under the title "On the efficiency of AC/DC: Bon Scott versus Brian Johnson". In the paper it is observed that it is difficult to ascertain who in the hard-rock band AC/DC was the better vocalist, Bon Scott or Brian Johnson. Yet, Oxoby argues, some experimental findings suggest that Brian Johnson was the better vocalist. When the AC/DC song "Shoot to thrill" (with Brian Johnson as the vocalist) was played, more efficient outcomes were realized in an ultimatum game experiment than when the AC/DC song "It's a long way to the top" (with Bon Scott as the vocalist) was played. Whereas the offers by the proposers were rejected five times by the respondents when they heard Bon Scott sing, the offers were rejected only three times when they heard Brian Johnson sing. Although Oxoby's paper is relatively short (it is seven pages long, or four pages without references), it has the usual format of an academic economics paper and is replete with standard economics jargon.

When Oxoby's paper was brought to his attention, Levitt's response was: "They grow up to write economics papers like this one, which looks at whether participants in lab experiments get closer to efficient outcomes when exposed to one lead singer of the rock band AC/DC versus another. I hope for this guy's sake he has tenure" (Levitt 2007a). Understandably, Oxoby was not amused by Levitt's denigrating response. In his response to Levitt, Oxoby hastened to make clear that his paper was meant to be a joke. Oxoby seems to be genuinely puzzled that this had not been immediately clear to everyone. He was unpleasantly surprised in particular by Levitt's non-understanding and

[6] See http://www.youtube.com/watch?v=VVp8UGjECt4

denunciation: "I would think that you of all people would recognize a joke when it comes up" (Oxoby 2007). Oxoby's tacit assumption seems to be that Levitt's own specialty is to tell economic jokes. That at least would explain why Oxoby was surprised that Levitt "of all people", failed to recognize Oxoby's paper as a joke. But such an assumption seems to be false. *Freakonomics* is not meant to be a compilation of economic jokes. Far from it; the economic analyses of "freakish phenomena" in *Freakonomics* are meant to be dead serious. It seems that Oxoby mistook Levitt and Dubner's book in the economics-made-fun genre for a book in the economics-can-be-funny genre.

I think there is no doubt that Oxoby meant his paper to be a joke, but Levitt did not realize that. Why not? Of course, it might be that Levitt simply did not pay enough attention to the paper. I think that would leave Levitt with a bit of explaining to do for why he nevertheless thought he could write such condescending lines about it (and, even worse, about Oxoby himself). Alternatively, might it be that Levitt takes all economic analyses, or all papers that superficially have the appearance of a serious economics paper, way too seriously, even if they, like Oxoby's, are meant to be a pastiche of them? It seems Levitt lost a sense (or never developed it in the first place) for distinguishing work that is meant to be taken seriously from work that is only intended to provoke a good laugh—this is what McCusker (2007) suggests. Levitt found Oxoby's paper deficient and wanting on the incorrect presupposition that published work by economists is always meant to be taken seriously.

What is perhaps most puzzling in the Levitt-Oxoby exchange is that Levitt, in his response to Oxoby's response, argues that Oxoby still owes us an explanation or, rather, justification for why he conducted the experiment in the first place: "I still think this leaves Professor Oxoby with a bit of explaining to do as to why they were playing AC/DC as part of an experiment in the first place, however" (Levitt 2007b) Now, isn't that funny? Here is someone who seems to tackle and write publishable papers on whatever lends itself to a clever treatment demanding that someone else justify his choice of subjects. Who is Levitt, with his panoply of "freakish curiosities", to demand a justification from other economists for their choice of non-standard subjects? My point here is not that Levitt's (let's call it a) request to Oxoby is outrageous. I think it is a fair question. What is the point of playing AC/DC as part of an experiment? What sorts of insights did they hope to extract from the experiment? But that Levitt "of all people" is making such a request is really funny.

Oxoby wrongly assumed that Levitt's work was part of the same economists-can-be-funny genre as his own AC/DC paper. But Levitt's work is part of a different, economics-made-fun genre. The misunderstanding appears to have been mutual. Levitt initially seems to have assessed Oxoby's paper on the assumption that Oxoby's paper also belonged to the economics-made-fun genre. Had Oxoby been aware of this, Levitt's dismissive response probably would not have surprised him. What perhaps still would have surprised Oxoby is that Levitt, who after all can be called the master of picking "freakish curiosities", calls on Oxoby to justify his choice of subject. It might be that Levitt considers Oxoby's subject to be not only unimportant but also simply uninteresting. I think that Oxoby would readily agree that the subject of his paper is ridiculous rather than interesting (let alone important). Perhaps the interesting issue here is not why Oxoby wanted to parody serious academic economics papers in the economics-can-be-funny genre, but why it was accepted as a University of Calgary Economics Discussion Paper. It seems Oxoby could earn academic kudos very easily and leisurely in some lost hours in an airport by turning the failed experiments of his grad student into a joke paper. Perhaps this tells us something about prevailing opportunities and incentives in the economics profession, a topic to which I shall return shortly.

THE ECONOMICS-MADE-FUN GENRE IS NOT ECONOMIC IMPERIALISM

The aims of the economics-made-fun genre should not be confused with those of the economists-can-be-funny genre. Whereas books and papers in the economists-can-be-funny genre are meant not to be taken seriously, books in the economics-made-fun genre are meant to be taken very seriously by their authors. If all that the books in the economics-made-fun genre would bring about in their readers were just a jolly bout of laughter or a wry smile, protagonists of the economics-made-fun genre would be deeply disappointed. Their readers are supposed to learn a lot about the hidden side of virtually everything.

In the economics-made-fun genre, economic analysis is used to shed light on "outlandish" phenomena that clearly do not belong to what is traditionally taken to be the economic domain. Furthermore, the insights thus obtained are sometimes compared with and virtually always found superior to the insights obtained in the social sciences that traditionally cover such "outlandish" phenomena. Does this imply, as Rubinstein (2006), and Fine and Milonakis (2009) suggest, that *Freakonomics* (and other books in the economics-made-fun genre) display economics imperialism at work?

Fine and Milonakis argue that *Freakonomics* is economics imperialism driven to the extreme. Fine and Milonakis start by introducing "economics imperialism" in rather neutral terms as "[…] the extension of economic analysis to subject matter beyond its traditional borders" (Fine and Milonakis 2009, 7). Economics imperialism is thus depicted as an outwards pushing movement: by subjecting ever more "outlandish" subjects to economic analysis, economics pushes its borders in an outward direction. *Freakonomics* is depicted by Fine and Milonakis as the crowning achievement of economics imperialism to date, following earlier episodes of what they call old- and new-economics imperialism. Unsurprisingly, they take Gary S. Becker, with his attempts to apply "the economic approach" to a variety of "non-economic" subjects, to be the main spokesman of old-economics imperialism.

Fine and Milonakis identify George A. Akerlof and Joseph Stiglitz as leading protagonists of new-economics imperialism. This might surprise some, since Akerlof's work especially is regarded by many as exactly the opposite of economics imperialism. Instead of using Becker's economic approach to explain phenomena outside the traditional homeland of economics (henceforth "outlandish phenomena"), in much of his work Akerlof tries to amend Becker's economic approach with concepts and insights drawn from other social sciences in order to change and improve the economic analyses of phenomena that fall squarely within economics' traditional homeland. This is acknowledged by Fine and Milonakis. Yet they argue that both Akerlof's and Stiglitz's "information-theoretic" explanations leave basic elements of standard "marginalist" economic analysis intact (such as its commitment to methodological individualism and the assumption that individual agents maximize their own utility under constraints). These same old basic elements are used by Akerlof, Stiglitz, and others to explain ever more "non-economic" phenomena, such as social institutions. Fine and Milonakis consider *Freakonomics* to be the apex of this trend. Books like *Freakonomics* seem to claim that the scope of economic analysis is boundless. As Levitt and Dubner say "[…] no subject, however offbeat, need be beyond its reach" (Levitt and Dubner 2005, 12).

In their book, however, Fine and Milonakis go well beyond the neutral terminology of economists reaching outwards beyond economics' traditional borders by speaking of economists "invading" foreign territory that is already "occupied" by others, "conquering" and "colonializing" their denizens, and "appropriating" and "exploiting" their resources. In calling *Freakonomics* a typical work in academic imperialism, Rubinstein (2006) draws attention primarily to the latter "inwards pulling" tendency in *Freakonomics*: economists are searching for "interesting questions" as "natural resources" that they can exploit. Thus it seems that "economics imperialism" is associated with two opposite movements. Whereas Fine and Milonakis primarily emphasize the expansionist tendency in *Freakonomics* (economists reaching out to conquer other social sciences), Rubinstein stresses that *Freakonomics* reflects a search by economists for "outlandish" subjects that they can appropriate.

Do notions such as "conquering" or "appropriating" aptly and accurately capture what is driving the Economics Made Fun movement? Let us first discuss "appropriating" and then turn to "conquering".

Exploitation and appropriation?

One could argue that at bottom the two opposing movements of "conquering" and "appropriating" are manifestations of the same phenomenon: by pulling in subjects that are traditionally addressed by other social sciences, economics is pushing its boundaries outwards. One could also argue that the latter is instrumental to the former: economics is pushing its boundaries outwards in order to have easy, cheap and continuous access to new resources drawn from abroad. This presupposes that subjects (and issues and phenomena in general) can be appropriated by some discipline in a similar way as natural resources in some territory, such as oil and gas, can be appropriated by some foreign country or company. But are the subjects tackled or addressed by some discipline like that? If economists start tackling "outlandish" phenomena, are other disciplines that traditionally tackled these phenomena thereby denied access to them? It seems not. Unlike natural resources, which are private goods, subjects are more like public goods. Their "use" by the one discipline does not diminish the opportunities for other disciplines to "use" them. Disciplines cannot be dispossessed of their subjects in the same way that countries can be dispossessed of their natural resources.

Furthermore, on closer inspection seemingly clear phrases such as "subjects that traditionally are regarded as falling outside the economic domain" appear to be not at all that clear. Consider subjects such as social institutions, social norms and social structure. At first sight we might be inclined to say that these traditionally belong to the domain of sociology rather than that of economics. On this view, any use of economic analysis by an economist to shed light on them is seen as an extension of the economic domain. But what about firms, for example, and the ways in which they are internally organized? Firms can be seen as institutions and there is undeniably social structure in the ways they are internally organized. Yet most onlookers would say that firms and their behavior belong squarely to the economic domain (or, simply, to the economy).

Our economies are replete with "sociological" (and also "psychological") phenomena. As Simon (1991) once famously remarked, if an extraterrestrial visitor had a look at our economies, its attention would probably be drawn more to production processes within firms than to the exchanges between firms (and between firms and households) in markets. One way to read Coase's (1937) classic (and the work of the new-institutionalist economists such as Williamson that followed) is that by bringing analyses of the nature and boundaries of firms back into economics, Coase put an end to the scandal that (the then prevailing) standard economic theory had not much to say about firms. In other words, much of what is going on in Fine and Milonakis's new-economics imperialism, which they describe as economists appropriating and exploiting phenomena from other disciplines, can also be described as economics regaining economic subjects that they seemed to have lost.

Fine and Milonakis regard not only the explanation by economists of outlandish phenomena (or subjects, or problems), but also the "exploitation" by economists of concepts from other social sciences as economics imperialism (Fine and Milonakis 2009, 123). Thus, apparently, if economists try to accommodate concepts such as "identity" and "fairness" into their analyses in order to enrich and improve them, for Fine and Milonakis this testifies to their economics imperialism. I think Fine and Milonakis conflate two different issues here. One issue is whether using concepts such as appropriating and exploiting does justice to what is going on when economists incorporate concepts from other disciplines into their own analyses. The other issue is to what extent economists are indebted to other disciplines and whether the way in which they accommodate insights drawn from other disciplines does justice to "the real world". Whereas the second issue is a serious and interesting one (more on this below), I think the first issue can be resolved quickly: no, it does not make sense to describe what is going on in terms of appropriation and exploitation. Economists do not steal anything from other social sciences nor abuse them in any other sense by using their concepts.

In fact, this could be called the exact opposite of economics imperialism: instead of economic analysis finding its way into other social sciences, concepts from sociology and psychology find their ways into economic analysis. If one insists on calling this a form of academic imperialism, 'sociology imperialism' or 'psychology imperialism' would be more apt than 'economics imperialism': sociology or psychology have been successful in getting their concepts established in economic analysis. I think it would be even better, however, to abstain from talking of imperialism altogether here. Sociologists and psychologists did not force or impose anything on economists. Economists have voluntarily accommodated concepts from sociology and psychology, whatever specific reasons they might have had for doing so.

Conquering?

I have argued that notions such as "appropriation" and "exploitation" misrepresent the way in which alleged economics imperialists search for subjects and concepts in other social sciences. Is saying that alleged economics imperialists try to conquer other social sciences more accurate? Are these economists driven by the explicit intention to dominate other social sciences, to rule them, or to subject them to economics' hegemony? The image of economic analysis conquering other social sciences might seem most accurate for the first stage of economics imperialism that Fine and Milonakis distinguish, and which they dub old-economics imperialism. Witness Stigler's warlike proclamation: "So economics is an imperial science: it has been aggressive in addressing central problems in a considerable number of neighboring social disciplines and without any invitations" (Stigler 1984, 311). Economists are portrayed here as unsolicited intruders, eager to wipe out non-economic analyses in other social sciences. But it is not clear that Stigler's proclamation is representative even of first stage old-economics imperialism, let alone of later phases of economics imperialism. There is no clear evidence that its protagonists made sustained efforts to promote the spread of economic analysis into other social sciences. There is only some evidence that they made sustained efforts to get economic analyses of "outlandish" subjects accepted in their own discipline.

Lazear (2000) provides a sympathetic overview of the accomplishments of specifically old-economics imperialism. In line with standard economic theory, Lazear argues that in order to tell whether or not it has been successful, "economics imperialism" should be subjected to the market test (Lazear 2000, 104). Its success should be measured in particular in terms of the increase in the market share of economic analyses in other social sciences and in terms of how many economists have replaced non-economists (have forced non-economists out of business, in Lazear's own terms) in the other social sciences. This seems to make sense: what should be looked into is whether economic analyses have been used more often in leading journals in other social sciences and whether economists have increasingly taken the positions of non-economists (in faculties and departments of other social sciences, for example). However, even if it turned out that there has been an increase on both counts, this by itself would of course not testify to the success of economics imperialism per se. An increase need not be due to the deliberate efforts of economists and especially of economics imperialism's protagonists to increase the market share of economic analysis and of economists in other social sciences. Practitioners of other disciplines could have come to the conclusion that economic analysis is useful for them independently of such efforts. For example, Paul Glimcher, Michael Dorris, and Hannah Bayer's (2005) version of neuroeconomics seems to be a case in point (see, Ross 2008 and Vromen 2007). Nevertheless, if the market tests showed a clear increase in the market shares of economic analysis and of economists in other disciplines, the data would at least be consistent with the hypothesis that the protagonists of economics imperialism have succeeded in what they are after.

As it happens, however, Lazear has not conducted either market test (nor has anyone else, as far as I know). Instead, Lazear discusses many examples of papers published in economics journals in which economists have used economic analysis to shed light on "outlandish" subjects. With only a few exceptions, most papers mentioned in Lazear's references are published in prestigious economics journals such as the

American Economic Review, the *Journal of Political Economy* and the *Quarterly Journal of Economics*. Thus, instead of measuring changes in the market share of economic analyses in non-economics academic journals, Lazear seems to be reporting mainly on the increased market share of economic analyses of “outlandish” subjects in leading economics journals. Strictly speaking, the only thing Lazear shows is that economics imperialism has been successful in economics. There has become more room for, and acceptance of, this sort of work in the profession of economics. Becker’s type of work might have been controversial among economists even at the time he was awarded the Nobel Prize, but it seems it has become less controversial ever since.

Mutual disdain?

Saying that clear evidence is lacking that economics imperialists are driven by the desire to conquer other social sciences is not to say, of course, that they would not be delighted if the market share of economic analysis and of economists in other social sciences were to increase. To be sure, proponents of the economics-made-fun genre occasionally express dissatisfaction with, and sometimes even disdain and contempt for other social sciences. In *Freakonomics* there is quite some sneering at “experts”, which include, according to Levitt and Dubner, practitioners of other social sciences who mostly repeat “conventional wisdom” based on moralistic wishful thinking rather than an honest assessment of the data. Fine and Milonakis argue that the typical attitude of economists towards other social sciences can be characterized as parasitic, arrogant, ignorant and contemptuous (Fine and Milonakis 2009, 122-126). Though this might be a bit harsh and overblown, I think Fine and Milonakis are right that the average economist does not hold the scientific achievements and credentials of other social sciences in high esteem.

Disdain for other social sciences does not seem to be restricted to economists, however. The disdain that economists feel towards other social sciences seems to be mutual. Practitioners of other social sciences often loath or poke fun at the narrow-mindedness of economics as a discipline. The economic conception of humans and their behavior in particular—the infamous *homo economicus*—is taken to be a grotesque caricature and simplification of how people of flesh and blood really are and really behave.

The subfield of economic sociology seems to be especially interesting in this respect. Much of what is currently going on in economic sociology arguably takes issue with and wants to compensate for the narrow-mindedness of economic analysis in economics' own traditional homeland. In fact, many take economic sociology to be an antidote to economics imperialism. In particular, the economic conception of humans and their behavior is taken to be "under-socialized". Mark Granovetter's (1985) plea for the explicit recognition of the social embeddedness of persons and their actions is a well-known case in point.

Many economists feel that their discipline (and especially standard economic analysis) is badly misrepresented in the (economic) sociologists' critique of economics. This feeling is not restricted to economists who believe there is nothing wrong with standard economic analysis. It is shared by some economists who are open and even sympathetic to the suggestion that standard economic analysis has its limits and shortcomings and that economic analysis can be enriched and improved by bringing in concepts, ideas and insights from (economic) sociology. Gibbons (2005) notes that the typical reaction of economists to the critique of economic sociologists is to point out that they get economics wrong. Even economists who could readily agree that the basic thrust of a critique is compelling tend to concentrate on how economic analysis is misrepresented in the critique rather than on the constructive suggestions it makes for how to enrich and improve economic analysis.

Combating the dismal science image

The economics-made-fun literature similarly seems to be more a protest against what is taken to be a misinformed and unfair image of their own discipline than an attempt to conquer and rule over other social sciences. Economics-made-fun economics first and foremost wants to enlighten the general public about the breadth and power of economists' analysis, and practitioners of other social sciences seem to be part of their intended audience. Their primary "collective" concern thus seems to be to boost the public image of economics. They seem to be fed up with the dreadful image of "the dismal science" that in their opinion still haunts economics. They believe economics never was such a dismal science in the first place, and, given all kinds of new developments in economic theorizing, this characterization is more inapt than ever.

This is most clearly visible in Diana Coyle's (2007) *The soulful science*. Coyle sets out to show that current economics, and especially cutting edge research at the frontiers of the discipline, is not at all like the dismal science that many still take economics to be. As she puts it succinctly: "The popular unpopularity of economics rests on perceptions that are twenty or thirty years out of date and were always a bit of a caricature anyway" (Coyle 2007, 2). What current economics has to say about the problems of economic development is discussed in the first part. In the second part of the book Coyle turns to recent developments in micro-economics. One of the things Coyle want to show here is that leading economists have moved far away from the models of selfish, calculating individuals that many onlookers think still populate economic analysis.

Coyle thus confines her attention to economic analyses of subjects that are traditionally considered to be key economic subjects. In other economics-made-fun books economic analyses of "outlandish" phenomena take center stage. Yet they also aim to inform a large audience about what economic analysis really is and what economists really do nowadays. And, indeed, the number of A-list publications in economics journals on "outlandish" subjects does seem to have increased over the last decade. It seems economics imperialism has become even more successful in economics since Lazear published his overview. Whence this increase? In an interesting article published in *The New Republic* (2007), Noam Scheiber suggests that it is due to the prevailing incentive structure in the economics profession. Especially for young economists starting out, writing a clever paper on a "freakish" subject that no other economist has touched allows academic kudos to be earned more easily and quickly than trying to bring a big, important issue that the brightest minds in the profession have already worked on a tiny bit closer to full resolution. Scheiber reports that grad students are actively discouraged by their supervisors from working on problems they cannot solve in one month. Of course, working on a "freakish" subject only pays if editors of economics journals are willing to accept them for publication. Since an increasing number of influential mainstream economists have openly confessed that they find "everyday enigmas" more interesting and exciting subjects than "boring" or "dull" traditional economic ones such as budget deficits and exchange rates, this condition seems to be met.

On his blog, Gregory Mankiw writes that he is not worried that *Freakonomics* type work will drive out work on the big important issues in economics:

> All research programs run into diminishing returns; eventually, all the cleverness in finding natural experiments and off-beat identification will seem less clever than it did at first. Moreover, the profession has a healthy enough set of incentives that people will keep coming back to the big questions, as long as they think they can make progress on them (Mankiw 2007).

But if all research programs face diminishing returns in due time, this must also apply to work done on big and important issues. In fact, working on big and important issues has arguably already run into diminishing returns and new scholars know it. So I think it is questionable that the prevailing incentives structure is as healthy as Mankiw takes it to be.

The picture of economics that emerges here is one of an introverted rather than extraverted discipline. Its practitioners do not step out of economics to disseminate economic analysis in other disciplines or to pursue a career outside economics. Economists rather turn to outlandish phenomena because that is where they see the best opportunities to further their own career within their profession. Insofar as there is a collective concern driving the economics-made-fun genre, it is to correct and boost the public image of economics as a discipline. In short, rather than showing an interest in invading and conquering other disciplines, economists do not seem to show individual or collective interest in affecting other disciplines. The technical term for this is 'non-tuism', aptly coined by the economist Wicksteed to describe the disinterest of agents in the interests of those they interact with characteristic of purely economic relations. If this indifference makes the economics-made-fun genre look even worse than its portrait as the apex of economics imperialism, so be it.

IS HOMO ECONOMICUS STILL AMONG US?

Contrary to what Coyle argues, several commentators seem to maintain that current economics is still wedded to the view that economic agents are selfish, calculating individuals. Although they do not deny that economic analysis underwent several changes in the process starting from old-economics imperialism and culminating in *Freakonomics*, Fine and Milonakis argue that the economic analysis in *Freakonomics* is still committed to the view that individuals pursue their self-interest, and that they do so in an instrumentally rational optimizing way (Fine and Milonakis 2009, 107, 110). Ariel Rubinstein (2006, 1) seems to have something similar in mind when he writes: “This worldview seeks a simple explanation for the behavior of human beings that is consistent with their aspirations to attain a goal, attributing high importance to money and status and low importance to moral values.” In a similar vein, Stephen A. Marglin (2008) argues in his *The dismal science: how thinking like an economist undermines community* that economists still assume that economic agents are obsessively engaged in ‘cold’ rational calculations to figure out what serves their own interest best. There is room in standard economic theory for neither intuition and ‘hot’ emotion nor duties, obligations, and other other-regarding concerns.

Who is right? Coyle, who argues that economics has long left behind the stage in which it was assumed that the only thing economic agents have on their minds is the conscious pursuit of their own interest? Or Fine and Milonakis, Rubinstein, and Marglin, who argue that the fictitious worlds of economists are still populated by such monomaniacal economic men? Let us have a closer look at two books in the economics-made-fun genre - Levitt and Dubner’s *Freakonomics* and Frank’s *The economic naturalist* - to see how people and their behavior are depicted.

As John DiNardo (2007) observes, there is not much economics in Levitt and Dubner’s *Freakonomics*. The little economics there is in the book can be summarized by “people respond to incentives”. This is a mantra that is repeated many times in the book, often to denounce the “conventional wisdom” voiced by experts. Economists traditionally focus on economic or, more specifically, monetary incentives. Raising or lowering prices by raising or lowering taxes is perhaps the best known example. In *Freakonomics*, however, Levitt and Dubner argue that there are two other “flavors” of incentive besides the economic one. Social incentives relate to the (alleged) fact that people do not want to be seen by others to be doing things that are deemed wrong or bad in the society or community they are part of. They do not want to feel the shame that the disapproval by others induces. Moral incentives relate to the (alleged) fact that people do not want to do things they themselves consider wrong. People want to avoid the pangs of guilt that they feel if they nevertheless do things that they take to be immoral. Levitt and Dubner argue that it is wrong to assume that economic incentives alone will always determine how people behave. Sometimes people seem to respond more strongly to social and moral incentives.

This at least is one way to read their discussion of Paul Feldman's bagel business. While Paul Feldman was still the head of the public research group of the US Navy (from 1962 to 1984) he started to make a habit of bringing in bagels for his colleagues. To recoup the costs, Feldman placed cash baskets with a sign with the suggested price. The collection rate was roughly 95%. After his research institute fell under new management in 1984, Feldman decided to leave and to make a living by selling bagels to companies in a similar way as he had done before. After a while, the collection rate began to fall slowly to some 87%. Levitt and Dubner attribute this to the fact that before, when he still worked in the same office, his presence deterred theft. Once Feldman was no longer present at the companies that he brought the bagels to, the social incentive for employees of the companies to avoid Feldman's disapproval (by duly and honestly paying the price for their bagels) ceased to exist. In the new situation, with this social incentive no longer in place and economic incentives weakly pointing in the opposite direction of more widespread cheating, only moral incentives could have prevented the remaining 87% of the employees from cheating. More generally, in the absence of social and moral incentives, Feldman's collection rates would have been much lower than they actually were.

What to make of these figures? It seems Levitt and Dubner are a bit undecided. On the one hand, it seems they want to stress that people tend to cheat whenever the stakes prompt them to do so and that in this case Feldman was the victim. After all, a decrease in the collection rate from 95% to 87% means a 160% increase in theft. It seems they also want to emphasize that social incentives (e.g. that people do not want to be observed cheating) are powerful in preventing such a large increase from occurring. But in the end Levitt and Dubner note that it cannot be denied that even in the absence of such powerful social incentives at least 87% still refrained from cheating. They observe that this seems to prove Adam Smith right: people seem to be innately disposed to act honestly. People are generally good even without enforcement.

It is not clear whether Levitt and Dubner believe that the analysis of such moral behavior falls squarely within the purview of economics.

Their discussion of moral incentives suggests that they do think so. As argued above, Levitt and Dubner seem to argue that only moral incentives can explain why, even when not paying would not be observed by others, the vast majority of Feldman's clients continued to pay the indicated price for their bagels. At the end of their discussion of Feldman's bagel business, Levitt and Dubner argue however that "[...] the story of Feldman's bagel business lies at the very intersection of morality and economics" (Levitt and Dubner, 2005, 46). This suggests that the domain of moral behavior and the domain of economic behavior overlap only partly and hence that only part of moral behavior is amenable to economic analysis.

At any rate, it is clear that when Levitt and Dubner argue that people respond to incentives, they are not implying that these are only monetary incentives. Levitt and Dubner recognize that there are lots of things people do not do because they do not want to be ashamed of themselves (social incentives) or because they do not want to feel guilty (moral incentives). We can find a similarly broad understanding of the sorts of "costs" and "benefits" that might go into individual decision-making in Robert Frank's *The economic naturalist*. Frank's book is a collection of narratives (mostly composed by his students) in which basic economic explanatory principles are used to explain everyday enigmas. One such basic explanatory principle stands out from the rest as the mother of all economic ideas, Frank argues, and that is the cost-benefit principle (Frank 2007, 10). On closer inspection, if there is one thing that becomes clear from the various ways in which the cost-benefit principle is used as an explanatory principle, it is its flexibility and generality. In its most straightforward use "costs" and "benefits" of course refer to monetary magnitudes. But "costs" and "benefits" can also be used, and actually are used in Frank's book, to refer to psychic satisfactions and dissatisfactions of various kinds.

Consider for example Frank's discussion of why women's clothes always button from the left, while men's clothes always button from the right (Frank 2007, 26-28). What is paradoxical or enigmatic about this phenomenon is that most men and women are right-handed. For right-handed people buttoning shirts from the right is easier than buttoning them from the left. So at first sight cost-benefit considerations would seem to favor buttoning from the right as the "universal" norm for both men and women. Why then do women's clothes button from the left? Frank's answer is that the social norm that women's clothes button from the left was already established in the seventeenth century. Ever since, it has been unattractive for individual women to buy and wear right-buttoning clothes for basically two reasons. The first reason is practical: as women had already grown accustomed to left-buttoning clothes, it would have taken them time and effort to develop new skills and habits to switch to right-buttoning clothes. The second reason is social: given the prevailing norm of wearing left-buttoning clothes, women found it socially awkward to appear in public wearing right-buttoning "men's" clothes. Manufacturers of women's clothes either correctly anticipated that they would not sell many right-buttoning clothes or found out to their dismay that there was no market for the right-buttoning clothes they produced.

What Frank is arguing here is that the very existence of some social norm generates costs for people if they were to deviate from them. This might prevent them from doing what they would have done in the absence of the norm. This is similar to how Levitt and Dubner conceive of the working of social incentives. Note that Frank's "economic" explanation seems to be not unlike standard sociological ("structuralist") explanations of individual behavior: people tend to conform to prevailing social norms because they tend to seek the social approval of others (and try to avoid their social disapproval). In fact, all Frank and his students seem to be doing here is garbing such a standard sociological explanation in a new economic dress. That they are doing this seems to escape their attention. Neither Frank nor his students display any awareness that sociologists have been giving such explanations for ages.

Frank is famous for his own earlier work on emotions as commitment devices (Frank 1988). The key idea is that emotions such as guilt (what Levitt and Dubner call a "moral incentive") could have evolved not despite but precisely because they limit the choice space from which people choose. If some people cannot bring themselves to cheat or defect in commitment problems because their emotional dispositions prevent them from doing so, then that might allow like-spirited people to selectively interact only with them (and so avoid being exploited by other-spirited, more opportunistic types). The cost-benefit principle is invoked here to explain how emotions could have evolved: thanks to their "handicap", people with particular emotions could have reaped benefits that are out of reach to opportunists. In his new book, Frank emphasizes that this use of the cost-benefit principle does not imply that emotionally committed people consciously invoke cost-benefit considerations. On the contrary:

> [...] an emotional commitment to one's spouse is valuable in the coldly rational cost-benefit calculus because it promotes fitness-enhancing investments. But note the ironic twist. These commitments work best when they deflect people from thinking explicitly about their spousal relationships in cost-benefit terms (Frank 2007, 195).

Thus economic explanations in terms of costs and benefits can be given of behavior that is not the product of conscious cost-benefit calculations.

We can conclude with Coyle, and pace Fine and Milonakis, Rubinstein and Marglin, that, appearances notwithstanding, economic analysis, as it is promoted in the economics-made-fun movement, is wedded neither to the view that agents pursue their own interests, nor to the view that agents engage (in a "coldly" rational way) in instrumental reasoning in order to attain their goals. There is room for feelings of guilt, commitments, and duties even in the economic analyses promoted by those who hold that people respond to incentives and that the cost-benefit principle is a powerful explanatory principle that can be used across the board. Those who argue to the contrary seem to underestimate the flexibility and elasticity of current economic analysis. As Herbert Gintis (2007; 2009) for example argues, (expected) utility theory and game theory are more like a language, in that they allow for the expression of many different assertions, than like a substantive theory making specific determinate assertions about the real world. And, indeed, this is exactly how Gintis himself and his co-authors (such as Samuel Bowles) use these theories.

ARE NON-ECONOMIC FACTORS DONE JUSTICE TO?

Observing that current economic analysis is very flexible and elastic does not imply, of course, that specific economic analyses of specific "outlandish" phenomena contribute a lot to our understanding of them. One might rightly ask what is gained by garbing sociological explanations in a new "economic" dress, for example. Is our understanding of why women tend to conform to the norm of wearing left-buttoning clothes enhanced (or deepened) by saying that this is less costly for them than switching to right-buttoning clothes instead of saying that it is the prospect of being subjected to social disapproval that prevents them from switching to right-buttoning clothes? It rather seems to be the other way around: the "original" sociological explanation seems to be more informative than its translation into economic parlance. If this is what Fine and Milonakis mean when they argue that economic analyses of "the social" are often parasitic on work already done in other social sciences, then they are on to something real and important. But when they interpret "parasitic" in terms of exploitation and acquisition, I think they are overstating their case. As I argued above, in paraphrasing explanations that are originally given in other social sciences, these other social sciences are not thereby dispossessed by economics. More importantly, I think that the flexibility and elasticity of current economic analyses raises interesting and important issues that warrant further discussion. But I fail to see how discussing such work in terms of economics imperialism helps to bring these issues closer to a satisfactory resolution.

Observing that current economic analysis is very flexible and elastic does not imply either that specific economic analyses of specific "outlandish" phenomena are on the right track. Consider once again Levitt and Dubner's discussion of incentives and how human behavior responds to them. Levitt and Dubner argue that an incentive is simply a means of urging people to do more of one thing and less of another. Although there are some incentives that come naturally, Levitt and Dubner note that most incentives that we know of have been invented by people such as economists and politicians. Taxes and subsidies, as paradigm economic incentives, are a clear case in point. Taxes are negative incentives that, if introduced correctly, act like the proverbial stick by deterring people from doing certain things that they otherwise would have done. Subsidies are positive incentives that, if introduced correctly, act like the proverbial carrot by inducing people to do certain things they otherwise would not have done. Taxes and subsidies are artificially created rewards and punishments that change the pay-offs that agents face in their external, objective environment.

Are social and moral incentives also like that? Consider social incentives first. Sometimes it seems Levitt and Dubner argue that shame is a (and perhaps even the) social incentive. Like taxes, shame (or perhaps rather the prospect of being ashamed) might prevent people from doing things they otherwise would have done. But unlike taxes, shame itself does not seem to be something in the external objective environment of people. Rather, shame is something internal to people. What is external to people are the conditions or circumstances that might make people feel ashamed. Thus, what might make a big difference is whether or not people might be observed and caught by others for example in the act of cheating. As in Feldman's bagel example, the very presence of some particular person might act as an effective deterrent against cheating. So if we insist that an incentive be something objective external to the agent, the presence of people who can watch the agent's deeds rather than the agent's shame might better qualify as a social incentive.

What about moral incentives? Sometimes it seems Levitt and Dubner argue that guilt is the moral incentive. And again it is easy to see how guilt (or perhaps the anticipation of it) can act like shame and taxes in preventing people from doing things they otherwise would have done. But again, unlike taxes and like shame, guilt is something internal rather than external to agents. As Levitt and Dubner rightly note, guilt seems to be unlike shame, however, in that its occurrence is independent of whether there are other people around who can observe the agent's behavior. If people do things they deem morally wrong, they feel guilty no matter whether they are (or can be) observed. That is not to say, of course, that the inducement of guilt in people is independent altogether of the agent's objective external environment. As Levitt and Dubner rightly observe, whether or not people feel guilty might depend on the information that is provided to them. People might start feeling guilty about buying cigarettes on the black market, for example, if the government discloses the information that terrorists raise money by selling black-market cigarettes. But it does not make sense to call the provision of such information a moral incentive, I think.

Levitt and Dubner also discuss the interesting case of the Israeli day-care centers in which the introduction of a small ($3) fine for parents who picked up their children late paradoxically led to an increase rather than a decrease in the number of late-comers. Levitt and Dubner argue that the introduction of the fine meant that a moral incentive (i.e., the guilt that parents were supposed to feel when they came late) was substituted by an economic incentive (i.e., the $3 penalty): "For just a few dollars each day, parents could buy off their guilt" (Levitt and Dubner 2005, 19). On the basis of just the few lines they devote to this case, it is not so clear what exactly changed according to Levitt and Dubner. Is it that after the introduction of the fine, late-coming parents are assumed not to feel any guilt anymore? This is what Levitt and Dubner seem to suggest when they write that moral incentives are substituted by economic incentives. Or is it that parents are assumed to still feel a bit guilty, but that they came to think that by paying $3 they could fully redeem their guilt to the day-care center and its employees? This is what Levitt and Dubner suggest when they write that the smallness of the fine signaled to the parents that late-coming was not such a big problem for the day-care center after all (so that they did not need to feel very guilty when they were late). Either way, casting the discussion in terms of moral incentives does not really contribute to its clarity.

What Levitt and Dubner's discussion of the Israeli day-care centers does make clear is that Levitt and Dubner believe there are other ways for people to put their guilty feelings to rest than by simply refraining from doing the things they deem morally wrong. Levitt and Dubner foster the impression that in the end, whether or not people's feelings of guilt prevent them from doing things they consider to be morally wrong depends on economic incentives after all. Indeed, one of the major themes in their book is that just about everyone cheats if the stakes are right (Levitt and Dubner 2005, 20). And although Levitt and Dubner allow for the possibility that "the stakes" include social factors (notably whether or not people can be observed and caught in the act of cheating), they tend to concentrate on the standard economic ones. Everything has its price, as the familiar economic saying goes, whether it be the revenues one forgoes by buying this pair of shoes rather than another or whether it be the revenues one forgoes by not plundering one's mom's purse. The assumption is that for everyone there is a point (for one person the purse should contain at least €1,000; for another at least €10,000; and for yet another perhaps at least €10,000,000) at which the temptation to plunder the purse becomes irresistible and at which moral scruples are overcome.

Thus the take-away message of *Freakonomics* about human behavior, and particularly about different kinds of incentives and how they affect human behavior, is not very clear. On the one hand, Levitt and Dubner recognize that even in the presence of countervailing economic and social incentives, moral "incentives" might be strong enough for people to refrain from cheating (as in the case of Feldman's clients). How exactly moral "incentives" are supposed to do this is, as we saw, also not very clear. At best it is not worked out. At worst, it is simply confused.

On the other hand, the general assumption seems to be that if the economic gain of cheating is high enough, nothing will stop people from cheating. It is just that the economic gains must be higher if people know there is a fair chance that cheaters will be caught and shamed or if people have strong moral reservations against cheating.

One might rightly wonder whether moral feelings and moral considerations are done justice when attempts such as Levitt and Dubner's are made to squeeze them into the standard terminology of economic analysis. This seems to be a legitimate concern of those who, like Fine and Milonakis, criticize *Freakonomics* for its extreme economics imperialism. But, to repeat, discussing this concern in terms of economics imperialism and its alleged attendant attributes, such as exploitation and appropriation, does not help a jot. In a sense, such discussions badly distort what is questionable about such attempts.

CONCLUSION

Books in the economics-made-fun genre should not be mistaken for papers and books in the economists-can-be-funny genre. Papers and books in the latter genre are not meant to be taken seriously. As parodies of serious academic economics papers, they are meant to make fun of economics in a light and non-condescending way. These papers and books engage in a mild form of self-mockery that is intended to amuse or entertain primarily fellow economists. By contrast, writers and protagonists of the economics-made-fun genre want their work to be taken very seriously. They think their books show that economic analysis can uncover the hidden side of all kinds of interesting phenomena. The intended audience is not so much fellow economists as those who have not yet been initiated into "thinking as an economist". The fun here is: first, with the accessible and entertaining way in which the basic economic principles are explained; second, with the recognition of the breadth of the scope of economic analysis; and third, with the sort of contrarian insights that economic analyses yield.

The books in the economics-made-fun genre want to spread the message that economic analysis is general enough to address all kinds of phenomena that are traditionally considered to be foreign to the economic domain. They furthermore want to convey that economic analyses of such "outlandish" phenomena tend to produce insights that run counter not only to conventional wisdom but also to the insights produced in other social sciences. Does this imply that books in the economics-made-fun genre practice, or at least reflect, economics imperialism? In this paper I have argued that this is not the case. There is no doubt that the set of phenomena made amenable to economic analysis include phenomena that were traditionally covered by other social sciences. It is also true that the economists involved often seem to believe that their discipline is superior (especially in terms of analytical rigor) to other social sciences. Yet, this does not imply that terms such as “invading”, “conquering”, “appropriation”, and the like, which are often used to characterize “economics imperialism”, are apt or accurate here.

Proponents of the economics-made-fun genre show no special interest in influencing what is going on in other sciences. They do not seem to be interested in “imposing” their approach on practitioners of other disciplines. Nor do they seem to want to enter other social sciences to take over the positions of their current practitioners and make a career there. Instead, they seem to be more concerned “collectively” about the unflattering and (in their opinion) unfair image of the dismal science that still haunts their discipline. They want to show that this image is blatantly at odds with economics as it is practiced nowadays. And “individually”, it seems that young economists in particular believe they can best boost their own careers in the economics profession by tackling “outlandish” subjects. As such, the books in the economics-made-fun genre reflect the prevailing incentive structure within the economics profession and the changing perceptions of leading “mainstream economists” (especially in their roles as editors of economics journals) about what sort of work (and papers) in economics are interesting rather than a desire to invade and conquer other disciplines.

Finally, and perhaps most importantly, “economics imperialism” suggests that the intellectual transfer of ideas, concepts, insights and the like between economics and other social sciences is a one way street: that economists bring their approach and basic explanatory principles to bear on subjects that are traditionally deemed non-economic but there is no transfer in the reverse direction. This belies the fact that concepts and insights developed in other social sciences have started to find their way into economic analysis. All the work in the economics-made-fun genre reflects this reverse transfer of concepts and insights from other social sciences into economic analysis, though admittedly to various degrees. One might argue, of course, that to date this reverse influence of other social sciences on economics has been very small and also that in accommodating concepts and insights from other social sciences economics has badly distorted them. These are important issues that deserve serious further discussion. But discussing them under the heading of "economics imperialism" impedes rather than helps their informed and satisfactory resolution.

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Jack J. Vromen is professor of theoretical philosophy, dean of international affairs, and academic director of the Erasmus Institute for Philosophy and Economics (EIPE), at Erasmus University Rotterdam. His research interests are in the philosophy of economics, with an emphasis on conceptual and meta-theoretical aspects of the relation between evolutionary and economic theorizing.
Contact e-mail: <vromen@fwb.eur.nl>
Website: <www.eur.nl/fw/contact/medewerkers/vromen/>

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Cambridge social ontology: an interview with Tony Lawson

TONY LAWSON (born in Minehead, UK) is a trained mathematician located in the Faculty of Economics at Cambridge University. His work spreads over various fields, but it focuses primarily in the philosophy of social sciences, in particular: social ontology. Amongst his publications are the Routledge monographs *Economics and reality* (1997) and *Reorienting economics* (2003). Numerous journal symposia and publications by others have been devoted to his work, most recently Edward Fullbrook's *Ontology and economics: Tony Lawson and his critics* (2009).

Lawson's various activities over the last twenty five years include founding and chairing the Cambridge Realist Workshop and the Cambridge Social Ontology Group and serving as the director of the Cambridge Centre for Gender Studies. He is an editor of the *Cambridge Journal of Economics* and a member and trustee of the associated Cambridge Political Economy Society. Outside Cambridge, Lawson is a joint founder of the European Association for Evolutionary Political Economy and a founding member and trustee of the Centre for Critical Realism. He also is the primary instigator of the International Association for Critical Realism. He sits on the editorial boards of numerous international journals including *Feminist Economics.*

EJPE is very pleased to present this interview with Tony Lawson in which he discusses his work on various issues including social ontology and critical realism in economics, along with the differences that he perceives between his position and those of Uskali Mäki and Nancy Cartwright. We had the opportunity to sit down and talk with Lawson about all theses issues following his presentation this past spring at the research seminar series at the Erasmus Institute for Philosophy and Economics (EIPE), in Rotterdam.

EJPE'S NOTE: This interview was conducted by Clemens Hirsch and C. Tyler DesRoches. Hirsch is a PhD candidate at the Erasmus Institute for Philosophy and Economics (EIPE) at Erasmus University Rotterdam. DesRoches is a PhD student in the Philosophy Department at the University of British Columbia, and a co-editor of the *Erasmus Journal for Philosophy and Economics*.

EJPE: ***Perhaps you can begin by providing us with some background. As far as we know, you are a mathematician by training. How did you become interested in philosophical issues regarding economics, and in realism in particular?***

TONY LAWSON: Yes, you are right; my training is mathematics, pure mathematics. The intermediate step was politics, student politics. I became involved in student politics in London. I became quite active. But I found the jargon of economists a barrier to constructive discussion. So instead of taking up a PhD place in mathematics as I had intended, I pursued economics at the graduate level.

Once in an economics faculty—I studied for a Master's degree at the LSE—I was immediately struck by the use of formalistic models which seemed to me to be very silly. These models were advanced, then as now, by people who, I think it is fair to say, are rather pedestrian in their approach to, and often very poor at, mathematics, though seemingly in awe of it, or perhaps in awe of mathematicians.

I asked numerous economists: why are you pursuing formalism if doing so seems to force you to resort to making all these irrelevant assumptions? What is the point? What is the value of it all? The only reasonable response I received was that it was hoped that the models would improve with time. Economists then, as now, seemed to be mostly unaware that there are limits to the uses of any specific form of mathematics. It was at this point, in effect, that I started to become interested in ontology, though I did not know the term. But I became very aware of the gap that often existed between the world-view presupposed by the methods used, and our best accounts of, or certainly my own intuitions about, the nature of social reality.

I moved on to Cambridge to study for a PhD. This was the mid-1970s. Here I discussed these sorts of issues with fellow research students in particular. I remember that my concerns about the irrelevance of formalistic methods were met at one point by the assertion that contemporary thinking reveals that all methods fail to facilitate insight into social reality anyway. So formalism was on par with all other methods. I was then encouraged to read more Wittgenstein and 'up-to-date' post-modernist philosophy.

In the course of these discussions I came to recognise that the position I held was that of a philosophical realist. I was never convinced that I should give up on the idea that there is a world out there and that we do get to know it under some descriptions. I decided to spend a year or so trying to work out whether the orientation I adopted really was so old fashioned, and whether that mattered. I wanted to explore the limitations of my intuitions. As I say, I was not convinced by any argument I found against realism. But a process that I had anticipated constituting a one-year project turned out to be an aspect of my activities that lasted for the rest of my life—so far.

Was this around the time you came across Roy Bhaskar's work?

No, not at all. That came many years later. I produced stuff criticising economics from an explicitly realist perspective for ten years or so before coming across Roy. At some point, I discovered that a number of us were making similar or anyway related critiques of current social scientific practice, but situated in different disciplines. Margaret Archer was doing it in sociology; Andrew Sayer in human geography, and so on. Roy was doing a similar thing in philosophy and had the philosophical language. Eventually, we all sort of came together picking up especially on Bhaskar's philosophical language—and the rest of his contribution, of course.

Before discovering Bhaskar's realism, which account of realism were you most drawn to?

I don't know. I didn't really know where I was headed. I just read anyone and everyone. I read quite a bit of Aristotle, Marx, Hegel, Kant, Hume, Whitehead—and many others. I also read people like Bas van Fraassen. I remember trying to make sense of his version of realism as an aid to finding the most charitable interpretation I could of what econometricians were doing. Ultimately, though, I found I had to provide my own.

My concern has long been ontological realism. But I did not get to ontology by way of reading texts on ontology or philosophical realism. Without using the category, I was focusing on ontological issues from very early on. As I said, when I first came into economics at the LSE, my basic concern was that the methods we were taught presupposed a world of a sort very different to the one in which we actually seem to live. I was asking why aren't we looking at the nature of the phenomena to begin with, or anyway at some stage in the analytical process, and I was questioning whether our methods are appropriate to the sort of reality being addressed. In that sense ontology was always my concern.

You are commonly perceived as a 'critical realist'—by many, as the leading critical realist in economics. However, in your talk yesterday it was surprising to hear that you seem to be distancing yourself from critical realism. Is this perception correct?

No, I do not at all distance myself from critical realism. What I stress is that my primary concern is ontology, a form of study, not any set of results. Critical realism, if always evolving, remains the best self-consciously ontological account or theory of which I am aware, but my primary interest is ontology itself, the study of the structure of the nature of reality. If you were to convince me that critical realism, or an aspect of it, is critically flawed, and showed me something more sustainable, I would be very happy about it. It is not specific results that matter to me as much as relevance. And of specific relevance to social understanding at this point, I remain convinced, is ontology. My project is characterised by a turn to ontology in social theory as an explicit undertaking. This is what I have been doing since the late 1970s.

So I am actually very happy to be perceived as a critical realist. It is not at all a misinterpretation. But it is important to see this project as multifaceted and continuously evolving. And it is also variously interpreted. You mentioned critical realism in terms of economics, but critical realism has now taken on so many forms in so many disciplines. The emphasis and presentation vary depending on where you go. So incidentally does its reception. Critical realism in some disciplines, say in human geography, is almost mainstream. It is big too in sociology and critical management studies.

I am lucky enough to be invited to give talks to groups in various disciplines and communities, and in some places I am treated like this is where it is all happening; in other places though I am treated like I am doing something really subversive. Of course, given the dire state of modern economics subversion here is indeed the goal. Some like to represent critical realism as comprising various different turns: the dialectical turn, the spiritual turn, and so on. I do not find this especially helpful. The point though is that the more that this variety occurs, and the more my own thinking evolves, the more I find that clarity is best served by elaborating precisely what I am saying at any given point rather than arguing that the results achieved are part of critical realism.

You emphasise that your main concern is ontology as a form of study. How do you see the relationship between ontology and methodology in economics, broadly speaking?

Of course it depends on what we mean by methodology. If you mean the study of methods, all methods have their ontological presuppositions. So it is possible to examine the methods of economists and others, and a lot of my time has been spent doing that, to uncover the *kind*, I emphasise 'kind', of reality they are in effect presupposing.

If instead we elaborate a general social ontology, an account of social reality—should that be possible—this can inform substantive theory and choice of method. But it does not directly support any specific theory or method. To go from ontology to theory or method requires additional empirical assessments. Any two people agreeing on a particular ontological conception can differ in their additional empirical claims. But ontological insight helps avoid inappropriate reductionist stances and aids explanatory and ethical work. This is a very long story.[1]

Does the use of a hammer presuppose a nail? I do not know if all methods presuppose a strict ontology.

That is right, they do not. That is why I emphasise the *kind* of reality presupposed by given methods, and the like. The hammer does not presuppose a nail in particular, but, *qua* hammer, it does presuppose something that needs to be met with a specific kind of force, and, if the intention is not to break the object, then it presupposes something that can withstand the sort of force that can be exerted with a hammer. Certainly, if I say I urgently need a hammer, you can infer that the immediate task before me, i.e., the task for which I am intending to use it, is not to write a book, cut the hedge, clean the window, and so forth.

Your question gets at an important point. There is no isomorphism between ontological claims and either method or substantive theory. It is important to be clear—I do try to be. Thus I argue the sorts of formalistic methods that mainstream economists insist upon presuppose the occurrence of event regularities. But the latter is a *kind* of reality; I am not too specific. Regularities can be actual or fictitious, stochastic or deterministic, and so on. In addition, when I argue that economists tend to construct theories in terms of isolated systems of atoms, I usually insist that, although this conception tends to be

[1] Though see chapters 2, 4, and 5, of my *Reorienting economics* (Lawson 2003).

adopted as one that guarantees an event regularity formulation, it is not a necessary condition. An event regularity could come about purely by chance, underpinned by a different causal mechanism on each occasion. Of course, the latter is unlikely and economists do hope to be more systematic in their theorising. But whilst a construction in terms of isolated systems of atoms is only sufficient, not necessary, to guarantee an event regularity, I do observe that a posteriori this is how economists mostly, in fact, proceed.

You often stress that your argument is an ontological one. However, you have also emphasised the importance of explanation as an epistemic goal in economics (Lawson 2003, chapter 4). Can you elaborate on the implications that ontology has for explanatory goals in economics?

Ontology per se cannot be expected to provide any necessary implications in terms of a precise explanatory procedure. But it can provide insight. For example, the ontological conception I defend finds that reality, natural and social, is structured: there are different ontological levels. It follows that the phenomena at any one level may be caused by, and so warrant explanation in terms of, phenomena lying at a deeper level. Certainly, it cautions against assuming a priori that all causes lie at the surface, that events are caused only by other events. But what exactly is the case in any context requires investigation. An orientation, though, is indicated. Ontological results point to the sorts of conditions that methods must be designed to be consistent with. I have argued many times, for example, that methods that presuppose closed systems are unlikely to be generally useful for helping economists understand open systems.

Having said all that, I have spent some time elaborating a dialectical approach that can be called contrast explanation, or, as I prefer, the method of explaining critical contrasts. Why? I have done so, in part at least, as a strategic move. An initial response to my setting out the ontological conception I defend was the suggestion that, because reality is portrayed as so complex, all method is limited and must knowingly distort. Therefore, it was frequently concluded, mathematical modelling in economics is no less relevant than any other approach. Of course it does not follow that just because reality is complex our analyses of it must knowingly distort. But in emphasising this I felt that the onus was on me to indicate examples of explanatory method that does not knowingly distort under the conditions in which we live, or as described by the ontological conception I defend, and to give some illustrations. This I have done using the method in question (see, for example, Lawson 2009a). As it happens I have found that this dialectical approach, if abstractly formulated, seems to encompass most other explanatory procedures as special cases. But that is a discovery, not a requirement. So I have engaged in explanatory illustration. But I have always emphasised that it is merely illustration. Of course that has not prevented some critics from mistakenly, and perhaps wilfully, interpreting me otherwise.

Many economists claim to be interested only in the predictive success of their theories and models, and not in establishing a 'deeper level' causal explanation. It seems that you are trying to re-direct economists away from prediction and towards epistemic activities they may not be interested in. Is this the case?

Yes, well this is where the analysis of social reality leads me. It is not an a priori orientation that I adopt. It is not that I am somehow against prediction. I think that if forty years of econometrics has revealed anything of value to us it is that you cannot very often make successful predictions of the sort that economists seek. Despite the claims of some econometricians, most results they achieve are pretty useless. Anyone can run millions of regressions with a set of data and report a result that seems to pass all tests—though the fact that millions of regressions are run means that most of the conditions of the tests are violated. But even with such results we find that as soon as new data come along the previously reported results or models typically break-down. What I am saying is that no matter how interested in successful prediction some economists may be, this interest does not make it feasible. Even so, I believe that though we cannot obtain what so many economists clearly want, we can nevertheless often get what we need; at least this is so if ultimately the underlying goal is to provide insight of a sort that enables us to contribute to making the world a better place, which I suspect it ought to be.

I have been concerned both to explain the predictive failure of economics and to come up with a conception that enables us to see exactly what we can achieve analytically. As it happens I find that we can achieve rather a lot, which ordinary people do every day anyway. Fundamentally I believe that we can make our own history. We can grasp the structures of reality and we—the community at large—can intentionally transform them in part according to our own goals. The future is not predetermined. So as I say we can make our own history. My arguments have driven me to this conclusion, and it is a conclusion that I am happy with. What more do we need—other than wisdom, of course, to direct our history making?

Actually, let me add that I believe the emphasis on prediction *in a world that is clearly open*, is ultimately an aberrant form of behaviour that itself requires an explanation, probably a psychological one. In fact I am quite susceptible to the suggestion that, in many cases, the over-concern with prediction is something of a coping mechanism resulting from earlier traumas in life. But that is another story.

On several occasions you are warning against an 'epistemic fallacy' in methodological debates in economics by which you mean "the view that questions about being can always be reduced to questions about our knowledge (of being), that matters of ontology can always be translated into epistemological terms" (Lawson 2003, 111). How would you respond to an argument that your focus on ontology, might fall victim to committing an 'ontological fallacy', i.e., that it tends to reduce questions of epistemology to questions of ontology?

Well, I hope not. This takes us back to your question about the hammer. Both reductions are to be avoided. It is in order to reduce the risk of the ontological fallacy that I often go on and on about the importance of *not* interpreting substantive theories or methods as critical realist ones (see, for example, Fullbrook 2009, chapter 4). Any two individuals starting from a shared ontological conception can end up with a different theory of phenomenon *X* or find themselves investigating it in different ways. Also substantive theories held as true at a moment in time can be revised in due course, with new experiences, without necessarily revising the ontological conception informing the analysis. All such possibilities depend on avoiding the ontological fallacy.

An important conceptual distinction in your account of ontology is the one between "philosophical ontology" on the one hand and "scientific ontology" on the other (Lawson 2004). Can you elaborate on this distinction?

Yes, though it should be noted that I do revise these concepts all of the time. But briefly, I have tended to use the category philosophical ontology to refer to the practice of seeking to uncover shared properties of phenomena of a given domain, whilst I use the category scientific ontology to explore the specifics of a phenomenon in a domain. Thus if we focus on the social domain, under the heading of philosophical ontology, I have tended to argue that social phenomena are, for example, all produced, reproduced, or transformed through practice, and are inherently relational, structured, emergent, and meaningful, etc. Under the heading of scientific ontology I have explored the differentiating features of money, gender, institutions, technology, and so forth.

Philosophical ontology is an enterprise that many philosophers of science reject. How do you see it as a justifiable enterprise?

It is not just philosophical ontology that gets rejected. Pretty much any form of social ontology is rejected by many. And there is a strand of twentieth-century philosophy, inspired by Kant, and associated with the likes of Carnap, Putnam, and Strawson, that goes further. This strand conceives all ontology as properly concerned not with any external world in itself but only with human concepts, languages, or systems of beliefs. According to defenders of this position, the most that can be undertaken is a study of the presuppositions, or ontological commitments, of specific theories or systems of belief, an activity termed 'internal metaphysics'.

But I guess you are thinking of the later Quine, or those perhaps influenced by him, who are prepared to accept certain theoretical claims as reliable and so commit to the reliability of the posited ontology as well. However, such reliability is attributed only to some very special forms of reasoning, and in effect is confined to parts of natural science. The presumption here is that it is only our best natural scientific theories that are successful in providing insight. And because these theories are about specific causal mechanisms and the like the insight provided relates only to the subject matter of what I am calling (natural) scientific ontology.

If we are forced to start from substantive theories regarded as reliable, then Quine and the others seem to be right. Certainly, the substantive theories of social science are mostly contested. And in the case of economics they are mostly simply irrelevant. However, in seeking reliable, and recognised-as-reliable, entry points we are not constrained to consider, with Quine, merely the content of theories. We can, for example, just as legitimately commence from any feature of experience regarded as adequate to the relevant domain of reality, including those concerning human practices. I myself have certainly used every day and scientific practices as entry points for ontological analyses.

And if philosophical ontology aims, as it does, at generalised insights we can seek reliable conceptions of human practices and so forth that too are reasonably generalised. We can do this starting both with practices whose (generalised) conditions of possibility are the subject of (non-social) natural ontology, and equally with practices whose conditions of possibility are the subject of social ontology.

This is a long story, set out for example in chapter 2 of *Reorienting economics* (Lawson 2003), or in a position paper downloadable from the Cambridge Social Ontology Group website (Lawson 2004). What I have just said, though, should indicate why, in contrast to many philosophers, I do indeed believe justified (non-dogmatic and non-transcendent) philosophical ontology to be possible. Indeed, it is something I take myself to have been doing.

In a recent article your position on the scope of ontology has been compared to that of Nancy Cartwright (Pratten 2007). Another recent article compares your realism to Uskali Mäki's work (Hodge 2008). What common ground and divergences do you see between your position and those held by Cartwright and Mäki?

I am certainly an admirer of the contributions of these two. We are all realists and we all—Mäki, Cartwright, and I—self-consciously present ourselves as such. The most obvious research-guiding commonality, perhaps, is that we do all look at the ontological presuppositions of economics or economists. Cartwright has questioned what the world would be like for econometrics to work, and Mäki has looked into the presuppositions of Austrian economists amongst numerous others. I have looked at mainstream modelling, Keynes, Veblen, and Hayek and others in this regard. So, yes, a common ground is an interest in examining ontological presuppositions.

Where we part company, I believe, is that I want to go much further. I guess I would see their work as primarily analytical and my own as more critically constructive or dialectical. My goal is less the clarification of what economists are doing and presupposing as seeking to change the orientation of modern economics. Or perhaps I am just more overt in the latter. So I come across as far more critical. Indeed I am. Specifically, I have been much more prepared than the other two to criticise the ontological presuppositions of economists—at least publically. I think Mäki is probably the most guarded. I think too he is the least critical, at least of the state of modern economics. Cartwright can be critical. I find her to be more forthcoming in presentations than when she comes to writing things up for publication. Probably most people are, myself included. But the difference between Cartwright's presentational and written styles seems more significant. Maybe this is a sensible strategy.

As you have noted, a central part of my work is philosophical and scientific social ontology. This of course relates to what I have just said. My goal is to reorient social theory. I seek to develop an explicit account of the nature of social reality. The other two do not seem to go there too much. I do not think they put forward theories of the constitution of society—or at least not very often. On these issues, I think that my own stuff connects more closely with that of the likes of John Searle. But where there are overlaps of concerns in the contributions of myself, Mäki, and Cartwright, I am not sure there is that much difference in the sorts of positions taken.

One feature of Mäki's work that I am not overly convinced by, but which he seems to value, is his method of theoretical isolation (Mäki 1992). If he is advocating it as a method for social scientific research, I doubt it will be found to have much relevance—for reasons I discuss in *Economics and reality* (Lawson 1997). But if he is just saying that the most charitable way of interpreting mainstream economists is that they are acting on this method, then fine. Sometimes, though, he seems to imply more. Otherwise there is not too much to divide us, I think, in terms of results.

The big differences are our goals and orientations. But these of course do shape the scope and nature of the projects pursued. For example, I cannot get enthused by Mäki's concern to see what can be justified in contemporary formalistic modelling endeavours. The insights, where they exist, seem so obvious, circumscribed, and tagged on anyway. So our actual contributions do end up being very different, which is probably good. I for one, though, would be happy if there was more communication between us all, though somehow I doubt it will happen.

One apparent difference between you and Mäki regarding ontology is the distinction between 'bottom-up' and 'top-down' approaches to ontological theorising. Mäki regards his own approach as the former and there is evidence to suggest that he would likely describe yours as the latter (Mäki 2005; Hands 2001). Do you agree? How do you see your approach?

I do think Mäki gets this very wrong. He places in opposition what he calls 'bottom-up' and 'top-down' approaches—the former developing philosophical insight by starting from concrete economic analyses, the latter imposing onto economics philosophical injunctions determined outside of economics—and, as you note, he associates the former with himself and the latter with me. He is wrong, and possibly mischievous, in the way he characterises me, and I think it is misleading, or unhelpful, to present the options in such a dichotomous fashion.

As I view things, anyway, a real difference between Mäki and me is that he is far less, or less openly, critical of the state and practices of modern economics, as I noted just now. And this bears on our research strategies. In seeking to draw philosophical insight from modern economics Mäki seems more inclined to accept mainstream economic contributions as largely successful, or anyway uncritically. I certainly do not think we can accept mainstream contributions as successful, and so I proceed somewhat differently.

In my own stuff, as I earlier touched on when discussing the possibility of social ontology, I have preferred to start out from the everyday practices of lay people. These include, for example, those practices in which lay people negotiate: markets, institutions, and ever present social relations. These practices, I believe, are (and are recognised as being) reasonably successful. Thus I have questioned what is presupposed by the widespread everyday practices of all of us, what the social world must be like given them.[2]

So if there is a difference here it is that Mäki more often starts out from mainstream academic economic analyses accepted rather uncritically, whilst I prefer to start from those everyday practices widely regarded as successful. It seems to me, though, that the two approaches are equally 'bottom-up', just different.

But, as I say, I also think the dichotomy of 'bottom-up' and 'top-down' is not too helpful. There is no harm, and often great value, in

[2] See, especially, chapter 2 of *Reorienting economics* (Lawson 2003).

examining the contributions of other disciplines, which I also do. Nor must borrowing or abducting insights from one field to another be harmful or misleading. Problems only arise if the results of one discipline are imposed onto a second discipline. If instead the question is posed: does this insight from that field, say philosophy or biology or wherever, have any relevance to economics, then, as long as the theorist is prepared, if appropriate, to modify any such insight to meet the conditions of the target domain, I do not see the problem. Is it such a problem to examine whether it is possible that the study of Darwinian evolutionary processes can yield insights for social analysis? As it happens my own answer to this latter question is given in *Reorienting economics* (Lawson 2003, chapters 5, and 10). And is it necessarily problematic to ask similar questions concerning whether insights from the philosophy of natural science have relevance for the successful development of social science?

So the distinction that Mäki draws between his approach and mine is not right; nor do I believe it is especially helpful. I am pleased you asked the question. It gives me the opportunity to express a view on the matter in this forum where Mäki is clearly influential. Perhaps Mäki will think I misrepresent him in turn. I hope I do not. But if so I hope he replies.

Judging from your work, and also from what you have been saying so far, it is very clear that you reject mainstream economics. Is this a wholesale rejection or are there elements that you think should be retained?

It is a wholesale rejection. Yes! But let me quickly elaborate. What I take to be essential to mainstream economics is the *insistence* that methods of mathematical modelling be everywhere and always employed in economic analysis. I emphasise the word 'insistence'. It is this insistence that I reject wholesale. I do not, of course, oppose economists using or experimenting with mathematical methods, though I am pessimistic about the likelihood of much insight being so gained. But I am opposed to the *insistence* that we must all use these, and only these, methods, that the use of these methods constitutes proper economics, that employment and promotion be restricted to those who use only mathematical models, that only modelling methods be taught to students, and so on. This though is unfortunately the current state of economics. The mainstream dominates.

Let me add that I am of course very happy for advocates of the mainstream *insistence* on mathematical method to defend their case. But currently the method is imposed without argument and in the face of repeated explanatory failure. So yes, if I can emphasise that by the mainstream I mean the *insistence* on methods of mathematical modelling, mine is a wholesale rejection of this mainstream.

One of your main arguments against mainstream economics is that deductive-mathematical models do not accurately represent the targeted social phenomena of interest (Lawson 1997, chapters 2, 8; Lawson 2003, chapter 1). In a broader context this argument seems related to the long-lasting debate in economic methodology about the realism of assumptions of theories and models, originating from Friedman's famous essay (Friedman 1953). Can you discuss your position in this debate?

Okay, let me elaborate my position in a series of steps.

First, a starting point of my position is the widely recognised long history of failure of mathematical-deductivist modelling in economics. These methods presuppose event regularities or correlations. It has been found that these sorts of regularities rarely occur in the social realm.

Second, an additional starting point is that mathematical models are typically found to be formulated in ways that are acknowledged, even by their formulators, as being wildly unrealistic.

Third, mathematical economists, many of whom seem endlessly optimistic that success will eventually be achieved, persevere with their modelling endeavours and so seek theories that are consistent with, that guarantee, event regularity formulations. The way this is typically achieved is by their implicitly constructing theories in terms of isolated atoms. By atoms I do not mean something small. I mean factors that have the same effect, if triggered, whatever the context. It is this assumption of atomism that guarantees that if the factor is triggered—this triggering is the first event—the same outcome, the second event, always follows, so long as nothing interferes. It is the assumption of system isolation that guarantees that nothing does interfere.

Fourth, I defend a conception of social reality—a social ontology—as an emergent realm that is: highly interrelated, with each phenomenon being constituted in relation to everything else; intrinsically dynamic or processual, being continually reproduced or transformed through practice; structured; and characterised by meaning, values, and much else.

Fifth, the failures and lack of realisticness of many economic contributions that constitute my starting points are easily explained if the ontological conception I defend is at all correct. For social reality is found not to comprise parts that are isolated, for more or less everything seems to be constituted in relation to other things. And components cannot be treated as atomistic or stable, for each is being continually transformed.

Hence, in producing theories couched in terms of isolated atoms that are quite at odds with social reality, modellers are actually compelled to make substantive claims that are wildly unrealistic. And because social reality does not conform to systems of isolated atoms, there is no guarantee that event regularities of the sort pursued will occur. Indeed, they are found not to. This is a long story set out, for example, in *Reorienting economics* (Lawson 2003, chapter 1). But the above contains the gist of my critique of the modern mainstream *insistence* on methods of mathematical modelling.

Now, sixth, Friedman enters this scene arguing that all we need to do is predict successfully, that this can be done even without realistic theories, and that unrealistic theories are to be preferred to realistic ones, essentially because they can usually be more parsimonious.

The first thing to note about this response is that Friedman is attempting to turn inevitable failure into a virtue. In the context of economic modelling, the need to produce formulations in terms of systems of isolated atoms, where these are not characteristic of social reality, means that unrealistic formulations are more or less unavoidable. Arguing that they are to be preferred to realistic ones in this context belies the fact that there is not a choice.

What amazed me about the initial responses to Friedman by numerous philosophers and others is that they mostly took the form: prediction is not enough, we need explanation too. Rarely, if ever, was it pointed out that because the social world is open, we cannot have successful prediction anyway.

So my own response to Friedman's intervention is that it was mostly an irrelevancy, but one that has been opportunistically grasped by some as a supposed defence of the profusion of unrealistic assumptions in economics. This would work if successful prediction were possible. But usually it is not.

Strangely enough, perhaps, if we could have successful prediction I might be inclined to side with Friedman. If spontaneous event regularities were ubiquitous, then we could indeed use them for predictive purposes irrespective of the theory of underlying causal mechanisms associated with them. And if they were spontaneous and ubiquitous we might not be able to identify underlying causes anyway. It is our ability to manipulate the latter, or the failure of supposed certain regularities, which is often essential to uncovering underlying causes.

Anyway, I think I have said enough on this. Friedman's intervention was based on the error of supposing we can predict successfully, and is now often opportunistically referred to as a supposed justification for unrealistic models in a context in which realistic models are not a viable option and successful prediction is not achieved either.

Incidentally, Friedman's position is often advanced as an alternative to realism. That is just a mistake. Friedman is a realist about events and models, and even causal mechanisms. He has to be to assess, as he does, that certain formulations of them can be unrealistic. Only a realist can coherently claim to be, or that others are, wrong, or unrealistic, or right, or realistic, etc. It never was a realist versus non-realist debate. Every position is realist. I do not know anyone in economics who is not a realist. Some of us, though, are explicit about it. That is a major difference.

What about Deirdre McCloskey?

McCloskey is a realist about rhetoric! She is a realist about the economic profession. She is a realist about econometrics… Realism is inescapable! The question is always not whether someone is a realist, but what form that person's realism takes.

Contrary to your rejection of mainstream economics you seem to hold a quite favourable stance towards heterodox economics in general. Can you be specific? Which branches of heterodox economics do you regard as particularly promising and how do you see the role of your ontological account in relation to the various heterodox schools of thought?

I can be critical about everyone. But I think my dominant orientation, and natural inclination is to be inclusive in all walks in life, and I am very critical of the mainstream because it is exclusive. It insists that only people doing just mathematical modelling should be admitted to the economics academy. As you know, my background is mathematics, I can do the maths. So I do not feel excluded in principle even by them; I just do not like them to exclude everyone who does not want to do maths.

So yes, most of my arguments directed towards heterodox economics are concerned with identifying commonalities. I think the heterodox groups implicitly share an ontological conception broadly along the lines I outlined a few minutes ago. These groups are differentiated from each other in focusing on different aspects. So I see them basically as divisions of labour in the same overall project, which I truly think we are.

I feel positive about aspects of most of the traditional heterodox groups. This is especially true of feminist economics, but also of old institutionalism, post Keynesianism, Marxian economics, and even Austrian economics—which seems to surprise and dismay some people. In recent years, I have probably taken most from the feminists.

But I can certainly be critical even in the context of the traditional heterodox groupings. I mean, we find all sorts of funny things going on at heterodox economics conferences. There are people there who still think that theorems are the most important thing. They are just more tolerant than the mainstream in the sense that they do not try to make everyone else do theorems. Others think that econometrics is necessary to applied work. Most strangely, perhaps, there are those that seem to think that a switch from linear to non-linear forms of mathematical modelling represents some kind of advance in terms of realisticness. Worst of all, there are those, overlapping with some of those already mentioned, who apparently believe that, so long as conclusions already thought to be correct are reached, it does not matter what methods or assumptions are employed. And there are post-modernists who think we cannot say anything much about anything. I can be critical, but I put the emphasis more on unification and commonality.

Probably the feature of the heterodox traditions of which I feel the most critical is a lack of willingness on the part of some to fundamentally question the founding contributors. People identify themselves with a certain tradition and are very resistant to anything that challenges views that they associate with their figureheads. Relatedly, there can be too much arguing from the authority of the figureheads. Any is too much. And, perhaps even worse, there is quite a lot of trying to pretend that all recent insights, including ontological ones, were first formulated by these founders, when it is often very obvious that such is not the case. Probably the worst of these latter tendencies are manifest in the contributions of some of those who associate with Keynes or Veblen.

What impact do you think critical realism has had so far on economics, mainstream or heterodox, and on philosophical issues pertaining to the discipline?

I do not know. I am not sure it is for me to say. Perhaps let me make one claim. I think that it has contributed significantly to the fact that ontology is now explicitly a part of the ongoing conversation. It is not really apparent in the mainstream discourse, but it is pretty much evident everywhere else in economics. This is so even amongst those methodologists who seem reluctant to criticise mainstream economics and seemingly have little interest in heterodox economics or in changing the state of modern economics. It is true even of those methodologists, mostly a subset of the latter, who apparently feel uneasy about critical realism, including those who like to pretend to themselves, or to the world, that it is not really there.

In Edward Fullbrook's introduction to his recent book (Fullbrook 2009) he points out how, at least up to the mid-1990s, the term 'ontology' almost never figured in economic methodology, or indeed anywhere else in economics. Now that has all changed. And I think those contributing to critical realism can take a good deal of the credit for this—or blame, if you feel negative about the situation.

In your view, what role does or should economics play in society? Maybe you can also discuss the role of economics, as you see it, in the current financial crisis?

My views on all this are long and complex. In brief, I think the goal of economics, and indeed social sciences more widely, should be to uncover or identify the conditions that get in the way of a society based on generalised human flourishing. Of course we are all different and everything changes, so this is a complex story. It requires lots of ontology.

The role that economics has in fact played in the current crisis, certainly academic economics, is basically a passive but negative one. By getting on with their mathematical modelling activities, as they do, economists are mostly being irrelevant, but are diverting resources that could be used to provide insight. And worse still, irrelevant mathematical models of the sort economists produce, have been used by investment bankers and other speculators and, perhaps most worryingly of all, by rating agencies. But this is a long story. I have set out some of my views on this in a paper that appears in the *Cambridge Journal of Economics* (Lawson 2009b).

Earlier in our discussion of Cartwright and Mäki you expressed a wish for more dialogue. Indeed, it seems to be the case that there is relatively little contact between critical realists and other economic methodologists who, one might argue, hold similar positions. Why do you think this is so?

I do not know if what you say is entirely true. I mean there are different forms of contact. For example, we have this workshop in Cambridge and almost everybody I can think of in economic methodology has been invited, and most have turned up.

Speaking personally, it is true that, because my engagement with methodology is in large part motivated to change things, I perhaps share more with heterodox economists interested in methodology than with the economic methodologists who do methodology more for its own sake. I think I am quite active in heterodox circles. But I am happy to engage with anyone.

As a rather boring practical matter, it is the case that I get very little money for travel. Mostly I go where people invite me and throw in the travel costs. I thus interact with whoever invites me. This also means I miss most of the big conferences. But at this moment it does mean that I am able to interact with you lot here at Rotterdam. I am sorry Mäki has moved on to Helsinki.

Of course, I interact with everyone in publications. See for example Edward Fullbrook's (2009) latest volume, or past issues of Journal of Economic Methodology (2004, vol. 11, issue 3), Feminist Economics (2003, vol. 9, issue 1), Review of Social Economy (1998, vol. 56, issue 3), Economia (1997, vol. 1, issue 2), or the volume by Fleetwood (1999), and such like. Indeed, I think I probably engage in written debate with other methodologists as much as anyone. Am I so wrong in thinking that?

Perhaps the previous question was somewhat lacking in precision. Let me try again. It seems that there is, potentially, considerable common ground between your work on ontology in economics on the one hand and work on social ontology by philosophers such as Margaret

Gilbert, John Searle, and Raimo Tuomela on the other. You briefly mentioned Searle a few moments ago but in your published work there seems to be little interaction with these authors. Do you think there is a conflict between this work on social ontology and your ontological account?

It is certainly the case that I do not have much contact with academic philosophers, less perhaps than I should. This is mainly because I find them, by and large, to be overly analytical, more concerned with being thought to be clever than with addressing matters about the way the world is, which is my interest. By and large I find the best philosophy, or anyway that which connects most with my own interests, is done outside philosophy departments. But John Searle is fundamentally interested in the way the world is, as are the others you mention. Indeed, Searle's work on the constitution of society is ignored by many philosophers precisely because it is insufficiently like their conception of proper analytic philosophy. Searle's contributions, I think, like those of critical realism, are much more influential amongst natural and social scientists than amongst philosophers.

Actually, I did take up an invitation to visit Searle and his ontology group in Berkeley last summer, for about five weeks. In fact I went twice, because I was also earlier invited by Searle to give a talk at his bi-annual Collective Intentionality Conference, which incidentally also featured Tuomela and Gilbert. It was a very fruitful experience for me. No, I do not see a big conflict in our projects, certainly not between mine and Searle's. Searle actually thinks that we agree on just about everything. I am not so sure, but he well may be right. Certainly we agree on rather a lot. And we are very, very similar in our mentalities and orientation. Searle is very ready to speak or write his mind on anything, to say things as he sees them, no matter what the consequences in terms of unpopularity within his own discipline. I think I try to do the same.

In terms of our projects we spent a lot of time comparing notes. In fact, I led a seminar contrasting the two projects. The chief difference between us, I believe, is not the positions we sustain, but how we get there. Searle has kind of built on his theorising of language, the mind, and so forth. At all stages he has been concerned that his theories are consistent with our best conceptions in the natural sciences. The latter have acted as an explicit control on his thinking. In contrast, I have tended to start from conceptions of generalised social practices, and asked what the social world must be like for them to occur. But as I say, the results we reach turn out to be very similar. The categories used are sometimes different, and we present our results differently. We may differ on issues like emergence; I appear to defend a stronger form than Searle does. But actually even here the difference seems mostly to disappear once we unpack some of the terminology. So no, I do not think there is any significant conflict. None anyway that seems irreconcilable. We actually discussed the idea of joint work when we met. But I doubt we will ever find the time or opportunity to fit it in.

You already mentioned the Cambridge Social Ontology Group. The group has been going strong for years now. Can you describe what it is and how it came about?

Yes! First though it is important to distinguish the Ontology Group from the Realist Workshop. Many conflate the two. The latter is a weekly Monday night seminar, open to all. It started twenty years ago with a group of PhD students, each working with me on philosophical issues, who wanted to meet with each other and discuss philosophical matters. We met one Monday night. The session was successful, so at the end of the night we arranged to meet the following Monday. That led to us meeting again on the Monday following that one. And we are still going twenty years later. That is the Realist Workshop.

When we started out the Realist Workshop it was a very informal, organically developing sort of endeavour. Those who came kind of grew up together and helped each other in their research, and so forth. After about ten years or so the Realist Workshop had changed. It was still meeting on Monday nights, but it was no longer this organic group we started with where we read each others' papers. Many of the original attendees had left Cambridge to gain academic employment. And the emphasis had become less personal. It had become more another type of performance.

People come from around the world, famous people are coming in and give their talks, Nobel Memorial Prize winners like Amartya Sen or whoever. Each talk, though, is understandably usually unconnected with that of the previous week, and the audience can vary from week to week as well. It has remained a wonderful intellectual event. But en route we lost that organic character we had in the beginning. We lost the idea of developing our ideas together as a group.

That is why about ten years ago I set up the Ontology Group. It is a smaller group—of about fifteen people. The idea is that the same people show up each time, and there is continuity in the discussion from meeting to meeting. What we do there is basically discuss topics in ontology. The structure is variable. A topic can last for an hour, or for a term and more. We spent about a term discussing the nature of gender, even longer discussing the nature of rules. We have even discussed the nature of econometrics. As I say people are expected to come to each meeting and the discussion progresses. That is the point of the Ontology Group.

Now the Realist Workshop and the Ontology Group are both oriented more to questions than to answers, though we seek answers. In the Ontology Group in particular we explore limitations of our shared beliefs. Sometimes it almost feels like a confessional. We question and re-question everything, not least the things we defend quite strongly in public. And we do laugh a lot. We continually criticise ourselves. We also go round and round in dialectical circles, trying to make sure that everything is coherent with everything else, following every criticism and change in understanding—though we rarely succeed. No one feels the need to be protective about anything. Everyone's ego is left outside the room. It is very enjoyable and rewarding. The meetings are supposed to last two hours but usually they go on longer. When we are really keen or excited we fit in additional meetings at night times in pubs, or we may meet over vacations. As I say it is basically an ontology talk shop. But everyone involved seems to get a lot out of it.

Finally, how do you see the future development of critical realism? Where do you think it is headed? Perhaps you can also tell us a bit about your own research plans?

I do not know about the future of critical realism. Throughout the disciplines it is quite healthy. It has become a big movement now. Once a year there is the conference of the International Association for Critical Realism, an organisation I effectively set up about ten years ago, and all the disciplines are there: sociology, politics, anthropology, all the natural sciences, and all the arts too, and the humanities. Everyone is there. It is doing well.

But I do not know what the future holds. There is clearly an awful lot of ontological work still to be done—of course, there always will be. And critical realism is branching out in different directions. There might come a point where the label has outlived its usefulness. As I think I mentioned earlier, there are people who call themselves this or that sort of critical realist. There are all sorts of turns and there are people within critical realism who do not like this turn or that turn or another turn. So, it is much more heterogeneous than it may seem to be. The future is open. Who knows where it will all lead?

As for myself, I am working on questions like: what is the nature of money? What is the nature of this? What is the nature of that? I am also working on a theory of society that extends, but in some significant ways is quite different from, my earlier account. It is slightly more substantive, and in some ways more naturalistic, than what I have done before, and I do not know whether other critical realists will find it appealing. But that is something for the future.

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Tony Lawson's Website:
<www.econ.cam.ac.uk/faculty/lawson/>

Cambridge Social Ontology Group Website:
<www.csog.group.cam.ac.uk/>

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Review of N. Emrah Aydinonat's *The invisible hand in economics: how economists explain unintended social consequences.* London/New York: Routledge, 2008, 272 pp.

MARK BLAUG
Amsterdam School of Economics

This is a splendid book about a controversial concept in economics, the notion that there may be unintended benevolent *social* consequences of actions undertaken by individuals for entirely *private* reasons and that these consequences are not merely benevolent but are capable of producing an order that appears to be designed although it is actually the product of spontaneous action. I say it is a controversial concept in economics, but in reality it has been only too eagerly adopted by economists and domesticated as part and parcel of modern general equilibrium theory à la Arrow and Debreu.

The word but not the concept of the invisible hand was of course invented by Adam Smith who had an inkling, but only an inkling, of the modern version of the doctrine in the form of the First Fundamental Theorem of welfare economics, namely that every competitive equilibrium achieves a Pareto-optimal allocation of resources (Blaug, 2008). Emrah Aydinonat follows Emma Rothschild in sorting out the checkered role of Adam Smith in the genesis of the Invisible-Hand Doctrine, carefully distinguishing Smith's *process* interpretation from the modern *end-state* interpretation of the final result of the invisible hand (Aydinonat 2008, 68-81, 88-91).

He further explores the role of the Invisible-Hand Doctrine in economics with a critical discussion of Menger's much praised use of it in explaining the emergence of money, showing that while the origin of commodity money may be the unintended social consequence of private action, it is doubtful that fiat money is likewise the unintended consequence of dispersed private action (pp. 27-48); in any case, Menger never fully explicated the mechanism for the spontaneous co-ordination of individual money holding. Menger's story is depicted as a possible explanation of the emergence of money, but by no means a complete or even a fully convincing account.

Similarly, Thomas Schelling's checker-board model of racial segregation of urban housing as a result of the mild preference of some citizens for living near people like themselves is discussed as a paradigmatic example of an invisible-hand explanation of a social phenomena, which nevertheless is only a partial explanation of the phenomena of urban segregation falling well short of a total explanation (pp. 50-97). This may well be the fault of all models in social science, a subject the author explores in two interesting chapters on the philosophy of science (pp. 119-134), including the role of game theory in modelling behaviour in economics (pp. 149-169).

This brings us back to the fundamental distinction between *end-state* models and *process* models. According to *end-state* models, we are told a great deal about the nature of equilibrium once we have reached it, but almost nothing except hand-waving about how we actually reach it. The same is true of many invisible-hand explanations, such as those of Menger on the origin of money, Schelling on residential segregation, and game-theoretic explanations of Nash equilibria (pp. 159-164). Even when all players have common knowledge of each other's rationality and even when their beliefs are consistently aligned, there are always multiple Nash equilibria in any indefinitely repeated game—this is so well known that it has been called a folk theorem of game theory. What it means is that to explain how individuals select their optimal strategies in social interactions, we have to go outside game theory, and that of course is one of Emrah Aydinonat's messages in these final chapters on the philosophy of science.

This is a book that cannot fail to provoke thoughtful reactions from its readers about the potentialities of explanation in economics.

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Mark Blaug has made valuable contributions across a whole range of fields, including history of economics, methodology of economics, and economics of education and the arts. He is affiliated to the Amsterdam School of Economics, and honorary member of EIPE, Erasmus University.
Contact e-mail: <m.blaug@gmail.com>

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Review of David D. Raphael's *The impartial spectator: Adam Smith's moral philosophy*. Oxford: Oxford University Press, 2007, 143 pp.

NEVEN LEDDY
Simon Fraser University

This book is the summation of 40 years of David Raphael's engagement with Adam Smith. *The impartial spectator* represents a significant development in that engagement, though there is not a great deal of new material in this volume. Raphael's greatest scholarly contributions have often come in his editorial work, beginning in 1948 with his critical edition of Richard Price's *Review of the principal questions in morals*. His 1976 edition of Adam Smith's *Theory of moral sentiments* (*TMS*) included a hugely influential introduction, co-written with his Glasgow University colleague Alexander L. Macfie. The great success of that edition, since taken up by the Liberty Fund (1982), was to demonstrate the importance of reading Smith through the six editions of that work. It is difficult to underestimate the importance of Raphael's approach to Smith's *Theory of moral sentiments*, which is restated and further developed in this book.

This volume can be most profitably read as Raphael's settling of accounts with the world of Smith scholarship, and an attempt to synthesize his scattered comments on Smith in various articles over the years, with additional reflections on the themes of greatest interest to him. In so doing Raphael touches on many of the central debates in Smith scholarship over the past four decades. As such, this book provides a valuable insight into the reception and interpretation of Adam Smith from the gestation of the Glasgow Edition of his works (1976-1987) through to the present.

Unfortunately, the apparatus of this monograph is a weakness: both the index and the bibliography are slight. Raphael's references to his own edited selections of Hume, Kames, Hutcheson, and Shaftesbury from *British moralists* (1969) is perhaps understandable, but adds to the impression that his scholarship is less than entirely up to date. In this, Raphael has not lived up to the high standards of his own editorial work.

Raphael's argument is that in 1759 Smith set himself the task of explaining both moral judgement and the character of virtue. According to Raphael, he not only failed at the second task, but he did not even notice his failure until the 1780s, and then sought to correct it in the sixth edition in 1790 by adding a new Part VI 'Of the character of virtue'. This new sixth part too, according to Raphael, is a failure—at least in contrast to Smith's success in developing a theory of moral judgement. Raphael's identification of Smith's failings in the discussion of the character of virtue in the 1790 edition dates back to his 1992 essay, which was initially developed in response to criticism of his stoicization thesis by Lawrence Dickey. The stoicization thesis is the straightforward argument that in the final edition of *TMS*, Smith gave far more attention to the stoic tradition than to any other, which reflected his own increasingly stoic outlook—an interpretation that was challenged by Dickey through the concept of prudence. At the time, Raphael grudgingly acknowledged that there was a problem with his initial presentation of prudence as stoic in 1976, but he ascribed that problem to a mistake on Smith's part. In the present volume he develops on Smith's failure, and obliquely reasserts his view of prudence as essentially stoic, a claim he had abandoned in 1992. His treatment of the impartial spectator as stoic "in context" remains unchanged from 1976. Raphael is equally determined to defend his view of Smith's moral philosophy as a marriage of Christian benevolence and stoic self-command, and Smith himself as a sceptical deist.

In the first chapter, he tips his hat to his former student T. D. Campbell (Raphael 2007, 4), retraces his own tracks on certain occasions, and abandons certain claims from the 1976 introduction, all the while retaining his emphasis on working through the editions. Raphael disavows the 1976 claim that the extensive revision to the 1790 sixth edition constitutes a "new book", which he attributes to an exaggeration on the part of Alexander L. Macfie (p. 5). His foremost claim in this chapter is that *TMS* is a largely descriptive work, in which he follows closely on Campbell, but then further argues that this was not Smith's advertised intent. On this reading, the new material on the character of virtue in Part VI of the sixth edition in 1790 was an attempt to provide a normative theory of virtue.

In the next chapter he offers an interpretation of approval and sympathy in *TMS* through Hume's comments on the first edition, which is an elaboration of his twice-published piece "Adam Smith and 'the infection of David Hume's society'" (1969, 1976). More significantly for current Smith research is his development of 'spontaneous sympathy', distinct from the common reading of Smith's sympathy as an imaginative process. Raphael begins by prioritizing imagination over sympathy in Smith's system, arguing this is not a conscious process:

> An explicit exercise of the imagination is certainly part of Smith's account of moral judgment. In that context imagining oneself in someone else's place is more pervasive than the actual experience of sympathy (Raphael 2007, 13).

He goes on to distinguish between approval and sympathy:

> The identity view (of approval and sympathy as synonymous) is in any event far-fetched, while the causal connection seems a reasonable account of the psychological explanation that Smith has in mind. I conclude that the two statements of identity are a rhetorical lapse, intended to emphasize the necessity of the connection between sympathy and approval (p. 18).

In effect, Raphael offers a corrected version, not of his own scholarship, but of Smith's. Raphael then explains that Smith's argument was worked out in response to Hume's objection to the first edition of *TMS,* regarding sympathy with tragedy. In the third chapter Raphael offers an insightful critique of Smith's theory of sympathy with motive and consequence:

> We have to conclude that Smith's portrayal of the role of sympathy in judgments of propriety is unduly limited. He represents it as sympathy with motive alone, instead of including also sympathy with intended or probable consequences (p. 25).

This process, where critique is employed as a form of rehabilitation is common to the book as a whole, giving the impression that Raphael is keen to see a corrected Smithian sympathy take its rightful place in contemporary philosophical discourse.

STOICISM AND THE IMPARTIAL SPECTATOR

Raphael's stoicization thesis has endured over 30 years of sustained fire, and is here deployed in its final streamlined version, having previously been updated in 1992. The Dawes Hicks lecture on philosophy of 1972 is acknowledged as the initial source of chapters 4 to 6, though Raphael passes over another version published in *Essays on Adam Smith* (1975), which offered only slight revisions to that initial presentation. This is, in fact, the third published version of that lecture and consequently invites certain tedium on the part of the faithful reader. By the same token, however, it is in the details of the revisions that the significance of this book emerges. If Raphael taught us to interrogate Smith's *TMS* through the successive editions of this work, the same approach applied to Raphael's work yields some interesting insights into his influential stoic reading of Smith.

Raphael repeats the view that "Humanity and self-command together constitute for Smith 'the perfection of human nature', a combination of Christian and stoic virtue" (p. 34). Likewise, Raphael rolls out the association of the impartial spectator to stoicism and self-command in the context of its introduction: "he first spoke of the 'impartial' spectator when describing the stoic virtue of self-command, which he placed on a par with the Christian virtue of love" (p. 40). In 1992 Raphael had argued that Smith himself was mistaken in this presentation of prudence as incorporating self-command—in 2007, he further suggests that the entire undertaking of the new sixth part of the 1790 edition is a failure, and in so doing he further marginalizes prudence. Moreover, having ceded a certain amount of territory to Dickey on this point in 1992, he here attempts to make the claim in a different way, suggesting that prudence can be reduced to "living according to nature", that is, in the most common definition, living a stoic life.

PSYCHOLOGY AND THEOLOGY

In the seventh chapter, Raphael returns to the issue of descriptive and normative elements in Smith's system, this time in a theological context:

> Is the end result Smith's own view, or is he simply showing how the conventional view (of people generally, reflected in rationalist philosophy) comes about, without implying that he himself shares it? […] This would mean that Smith is a theoretical sceptic and a pragmatic conformist (p. 53).

Raphael's point is that there is a link between conscience and prudential self-interest, alluded to by Butler, and that this posed three problems for Smith. Of the first, Raphael concludes that:

> He calls prudence a virtue, that is, a possible object of positive moral judgement; but he does not write of prudence as itself a form of moral judgement. I conclude that we must reject as faulty Smith's first philosophical argument for the thesis that moral rules are laws of God (Raphael 2007, 61).

> The [second] argument is based on self-interest and assumes that an appeal to this motive is the best, or the most likely, way to induce us to obey the rules of morality. No doubt it is effective for people who are ready to accept the underlying theological doctrine (p. 61).

Smith's third argument is of unintended consequences, which Raphael calls the economic case. He then suggests a paradox where moral sentiments do not line up with the economic case for moral rules:

> Both the economic tendencies and the common moral sentiments are products of nature, so that nature is inconsistent. Smith does not seem to be worried about this (p. 62).

Into this inconsistency, Raphael injects Smith's purported theism as a solution, which seems to be an instrumental use of the text. I would suggest instead that there is something of a three dimensional paradox in Smith: that he approaches these problems on different planes of explanation, moving from one to another (from the economic to the psychological in this instance) when it suits his purpose. Raphael's insistence on finding a "solution" to this technique seems to me inappropriate and unnecessary.

This theistic and explicitly anti-materialist reading of Smith is further developed in chapters 8, 9, and 11. In chapter 8 this takes the form of a marginalization of prudence and an emphasis on a tandem of Christian benevolence and stoic self-command. This interpretation carries over into chapter 9 "The cardinal virtues" where prudence is discussed in economic and political contexts, as inferior and superior prudence respectively—a distinction added in the sixth edition. Raphael further argues "that Smith never was a practical atheist" (p. 79), based on Smith's final position regarding universal benevolence in the sixth edition. Chapter 11 presents a refined claim regarding Smith's religiosity: that Smith gradually abandoned Christianity, but remained a theist. With reference to *TMS* II.ii.3.12, Raphael explains:

> The text of the first edition, in its specific reference to atonement as part of revealed doctrine, implies acceptance of specific Christian

> belief as well as of natural religion. […] The sixth edition seems to have abandoned it [revealed doctrine] altogether (Raphael 2007, 98).

This amounts to a kind of a process of elimination by which Raphael concludes that Smith was a theist. (The primacy or even the incorporation of prudence in the life of virtue by Smith would have suggested "practical atheism" according to Raphael.) In the face of textual evidence of Smith's waning faith in the divinity of Christ and the Christian worldview, however, Raphael refines his interpretation into mitigated scepticism—and on that point he suggests that Smith was somehow cowed by Hume's ghost (p. 100). In short, Raphael's theistic reading of Smith is predicated on the marginalization of the virtue of prudence in the earlier chapters; the result is that these equally tenuous claims become interdependent.

While I find neither of these interpretations particularly convincing, Raphael's presentation of them as parts of one unified case at the very least makes his argument clear. Previously, in his various writings, it has not always been obvious why he placed such importance on denying the Epicurean flavour of prudence in the teeth of much criticism. His theological presentation of Smith is likewise much more accessible in the present version. As a result, this book will serve as a more effective entry into Smith scholarship than either the introduction to the 1976 Glasgow Edition of the *TMS*, or any one essay of Raphael's. In this case the whole is certainly more than the sum of its parts, and for that Raphael should be pleased, and Smith scholars grateful.

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Neven Leddy teaches in the departments of history and humanities at Simon Fraser University. He is the author of articles on Adam Smith's cultural and intellectual context in the *Adam Smith Review* and *Epicurus in the Enlightenment* (Studies on Voltaire and the eighteenth century, *SVEC* 2009: 12). His current research focuses on Scottish interaction with the Francophone world in Geneva, Paris, and Montreal.
Contact e-mail: <NBLeddy@gmail.com>

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Review of Bart Engelen's *Rationality and institutions: on the normative implications of rational choice theory.* Saarbrucken: VDM Verlag, 2008. 276 pp.

SHAUN P. HARGREAVES HEAP
University of East Anglia

The motivating thought behind this volume is that "if one wants to "make up the rules" of the game of life, one has to start from a realistic view of its players" (Engelen 2008, 2). This may seem self evident but it is an idea that rarely achieves the prominence which it deserves—not least in methodological debates. For example, much ink has been spilled around Friedman's famous 1953 essay that brought a form of positivism/behaviourism to economics, but comparatively little around this point. Yet it is really rather important.

If a theory is to be used normatively to provide, say, policy advice, then it has to be realistic in important respects. Otherwise the theory cannot hope to provide advice about how to change *our* world: its advice will apply to some 'other' world (i.e., the one that is in the relevant respects captured by that theory). To be specific, in mainstream economics, guidance is usually based on the Pareto criteria: that is, a policy intervention is warranted when economic theory predicts that some people will become better-off through that intervention without making any worse-off. The judgement about what makes people better or worse off is quite precise for this purpose. It comes from mainstream's theory of what makes people act (i.e., the assumption of individual rationality): people are better-off when they better satisfy their preferences. If it turns out that people are not rational in the sense of being instrumental preference satisfiers, then the theory can still be useful in predicting people's actions (because they could act 'as if' so motivated) but the theory cannot tell us when and if they are better off. The latter depends on the theory of individual rationality being in important respects descriptively accurate on the matter of how we act and how such action affects our well being.

This is why Bart Engelen is right to begin his book with a discussion of individual rationality in economics.

One might think that if a theory failed this realism test (and so was providing advice for some other world and not our own), then that would prove a terrible handicap. There are two defences that are sometimes deployed to avoid this conclusion. The first, potentially respectable one, is to argue that the theory is providing advice on what should be done if we are to live up to whatever alternative (or ideal) version of life is encoded in the theory. I say respectable in the sense that the theory would still be doing some work in guiding us with respect to what to do even if only in a sort of hortatory way. Nevertheless it would, if this was the case, still require some supplement in the form of a set of arguments around what made this alternative world ideal. For some of the reasons that are related to what Engelen sets out in his discussion of the rational choice model of preference satisfaction, it is difficult to come up with convincing buttresses of this kind (I say more on this below when discussing the difficulties of evaluating the activity of preference satisfaction if that is all one does). The second defence is that theories need *not* aspire to guide: it is enough that they predict (or explain) what happens in the world. Personally, I am unpersuaded by this as I cannot imagine how knowledge of the social world could ever be separated from acting in it.

This, in turn, is why Engelen's arguments, in the first part of this book, are also important, because he finds the economic rational choice model to be descriptively wanting and this is a problem for the reasons I have just sketched.

In the second part of the book, Engelen gives a tour of the dominant economic instrumental conception of rationality in chapter 2, and contrasts this with an alternative expressive notion of rationality in chapter 3. The economic model for this purpose is characterised as maximising in the sense that one satisfies best one's preferences and this depends in the usual way on having a coherent set of preferences (i.e., so that it is meaningful to talk about satisfying them best); in addition, these preferences are taken to be exogenous and egotistical.

The key difficulties with this model come from what behavioural economics tells us about individual behaviour. In particular, there are the various anomalies with respect to belief formation and inconsistencies in the notion of a preference that have been identified in the laboratory, and there is copious evidence that people have 'other regarding' or 'process' preferences. Here, less is made of this first group of findings than of the second. In particular, Engelen focuses on whether these non-egotistical preferences can be accommodated by the model. In his view, they cannot and this is largely because of the difficulty that, to put the point compactly, the self has in being selfless. That the glow from being selfless has to be unintended is another way of expressing this; or, to make a bridge to what comes in the next chapter, actions acquire motivating force when they are other regarding because they mean something and not because they have consequences.

This is a line of argument that I have also tried to make (and so I can't help but be sympathetic). Nevertheless, while I think the argument is based on a genuine distinction between types of motives to action that is important, I have become less persuaded that the concept of a preference is not sufficiently elastic to accommodate other-regarding or process-oriented actions (or alternatively that the concept of a consequence cannot be expanded to include the meaning of an action). This elasticity comes at a cost, however, and this is where the genuine distinction between the types of motive resurfaces, albeit in the language of preferences. Preferences acquire a two tier structure and they can no longer be taken to be exogenous as they are socially embedded and activated. Psychologists have yet another way of developing the same insight which has filtered through to some parts of economics: there are two types of reason, 'extrinsic' and 'intrinsic', and what is interesting is the dynamic between the two as when 'intrinsic' reason is 'crowded-in' or 'crowded-out'. In much the same way, Engelen concludes the chapter on expressive reason by arguing that the two types of reason are complementary: one cannot be reduced to the other, and the important task of social science is to decide when and where which type of reason is guiding action.

The next chapter gives an illustration of how some of these ideas can be set to work in unravelling the paradox of voting (chapter 4). There are two parts to this paradox. The first is why people vote when they can have virtually no effect on the outcome. This is sometimes answered by arguing that people find voting pleasurable but if this was the only reason, then there would still be no explanation of why people vote for the particular person that they do. This is the second part of the paradox. Engelen argues that people vote because it is expressively rational. Voting offers the opportunity to say something about oneself precisely because it cannot be construed as an instrumentally rational action, and it is because one is saying something about oneself that one can explain the choice of who one votes for. 'Voting is like cheering' is the helpful way this is put: it is done largely to express support and there is no real expectation that bleating in this way, for example from the stands in a football match, will have any effect on the outcome.

This is persuasive and it is nicely argued. It also seems to me to be generaliseable. The general point is that it is precisely when instrumental reason cannot give clear guidance to action that there is scope to express something through action. Otherwise the meaning of an action would be obscure even if one intended to express something, say moral, through action because it could equally be construed as selfishly instrumental. Voting fits the bill well because there is apparently no clear selfishly instrumentally rational interpretation. Some care is required here, though. It is not that it makes *no* sense to vote in all circumstances because, if nobody else votes, then one would influence the outcome by voting. Formally, the paradox is really bound up with the fact that this is a game where there are no *pure* strategy Nash equilibria. There is, of course, a mixed strategy Nash equilibrium, although I have never seen any attempt to explain voting by appealing to this solution concept even though it would formally resolve the so-called paradox. This must be because mixed strategy equilibria are so implausible that they could not be generally taken as supplying a reason for instrumentally rational agents to vote. The clearer class of games where instrumental reason fails to guide (and this is the point about generalisation) are games with multiple Nash equilibria and these would, therefore, be the circumstances where there is scope to instantiate (unambiguously) the norms which enable action to become symbolic.

The last part of the book turns explicitly to how the different conceptions of rationality construe the institutions of the market, state and communities. Buchanan's constitutionalism is used as the exemplar of what happens when you build institutions around the economic model of rationality. Thus chapter 5 provides a quick sketch of a set of familiar theses that: a) connect freedom with efficiency, b) turn Rawlsian and other collective choices into a choice over rules rather than outcomes, c) make unanimity in these matters all important, and which d) are alert to the drift to a wasteful form of big government through rent seeking and the like.

What seems so obviously wrong with this account is the way that politics becomes no more than the pursuit of self interest by another name. What has been lost is any sense that the political (and other non-market) arenas are spaces where ideas and argument (about how to live and how to organise society) are tested and agreements are reached; and that having an institutional space where this goes on is very important.

What makes such a public space important is the way that, by their nature, arguments in these arenas have to be impartial because arguments can never be persuasive if they appear self evidently self serving. This, in turn, connects with the alternative expressive conception of rationality. 'People are concerned with a sense of self-worth' is the way that I would put it. They reflect on what they do and they like to find their actions worthy. The standards for such reflections can, however, *never* be purely personal, otherwise they could be self-serving and the judgement of worthiness in such cases would lose its psychological edge. This is why communities, groups and the political institutions of collective decision making are so crucial: they potentially provide the standards that are external to any single individual and to do so they have to be governed by a different currency (e.g., intrinsic reason, if we shift to the language of psychology).

Let me put it differently. If you are solely a preference satisfier, how could you know that the pursuit of preference satisfaction was a worthy activity if you lived in a society where the political and other social non-market institutions were merely another set of arenas where individuals pursued their preferences? Of course one can naturalise the pursuit of preference satisfaction and so deny that there is an issue, but this would be to fly in the face of much of what we know psychologically about humans. What cannot be done is to appeal to a meta-preference that we have to act in this way. Preferences (including the 'meta' ones) are just that: they do not provide reasons for action. And if we do, indeed, care about whether we have good reasons for action, then we need a shared external space where those reasons are manifest (i.e., where they are debated, discussed, and tested) because purely private ones will not do the trick.

Chapter 6 makes these points systematically. Institutions should be designed in the knowledge of how people behave: they should recognise the role of communities as the locus for judgements about intrinsic value, the role of the state in mediating between communities, and the dependence of the market on norms of pro-sociality and trust which can all too easily be crowded-out. That is, the main challenge thus lies in "developing an institutional structure such that states, markets and communities are mutually enhancing" (Bowles and Gintis 2002, F431; quoted by Engelen 2008, 234).

This quote from Bowles and Gintis is what concludes this part of the book. I agree completely with it, but it also flags up an earlier discussion of Bowles and Gintis in this chapter that fits less comfortably with the rest of the book. Let me just sketch how Bowles and Gintis are used earlier in this chapter. They are important in Engelen's view because they supply an evolutionary account of pro-social behaviour within a group which depends on the existence of some competition between groups. This is why the conception of citizenship and the institution of the state are potentially important in the way sketched above: they are the mechanisms through which we escape the evolutionary legacy of group conflict without, if we are clever, losing the incentives to behave pro-socially that come from identification with groups.

I actually think this is an important and interesting argument which needs to be incorporated in the design of our institutions. Nevertheless, evolutionary arguments are not obviously helpful when accounting for why we are expressively rational in the sense that Engelen and I use the term. This is because evolutionary arguments can be constructed to explain the origin of pro-social *behaviours* but this is not the same as explaining why we come to attach symbolic significance to those behaviours. That is, it does not explain why we think such behaviours might be right, honourable, just, and so forth: this is the symbolic realm where expressive reason roams. I would not want to exclude the possibility of an evolutionary element in the explanation of this human faculty, but I cannot help but feel that the approach of current evolutionary arguments seems strangely to overlook the old methodological lessons that found behaviourism wanting. The analysis of behaviour is simply not enough.

This grumble at the end should not detract from the fact that this is a good book. It is exceptionally well written and its argument ranges across big literatures to draw important conclusions for the institutions of social life. What else can one ask for?

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Shaun P. Hargreaves Heap is professor of economics at the University of East Anglia, UK. His current research focuses through experiments on the social and emotional aspects of decision making and on the measurement of diversity in the media.
Contact e-mail: <s.hargreavesheap@uea.ac.uk>

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Review of *Equilibrium in economics: scope and limits*, edited by Valeria Mosini. London/New York: Routledge, 2007, 322 pp.

DOUGLAS W. MACKENZIE
The U. S. Coast Guard Academy

Many scholars have already focused on the use of equilibrium concepts in economics. Consequently, we must consider the extent to which new books on equilibrium improve our understanding of this subject. *Equilibrium in economics* does much to explain the historical development and scholarly value of equilibrium concepts in the natural and social sciences and contains much historical detail for a book of its size.

Chapter 4 by William Dixon and David Wilson should be of great interest this year, as it examines concepts from Smith's *Theory of moral sentiments* 250 years after their publication, but it would be engaging even without the usual interest generated by anniversaries. The idea that Ken Arrow and Gerard Debreu completed Smith's model with general equilibrium analysis always lacked plausibility. It is also important to realize (as argued convincingly in this chapter) that the modern use of *homo economicus* as a representative agent hearkens back to Thomas Hobbes rather than to Smith. But this use of Hobbes's *homo economicus* is even more important than indicated in chapter 4. Modern economists have exported equilibrium concepts to political science, via public choice theory. The Hobbesian assumptions in public choice were not made simply by chance: early public choice theorists were explicit about the influence of Hobbes on their work, and Hobbesian issues remain relevant in public choice theory today.[1] Thus, Hobbesian behavioral assumptions have found their way back into political science through economics.

Adam Smith was not the only one who rejected Hobbes's view of human nature. In chapter 5, Richard van den Berg examines the work of Achilles Nicholas Isnard, published in 1781. Isnard is largely unknown to modern economists, yet many of his ideas are familiar. Isnard combined mathematical modeling of markets with discussion of how

[1] See Deirdre McCloskey's (2006) *Inaugural James Buchanan Lecture*.

conscious calculation and virtuous habits lead to desirable outcomes. Richard van den Berg notes subtle differences between Isnard and Adam Smith. Further work on Isnard might link to Hayek, as he was influenced by the Scottish Enlightenment and critical of French Rationalism but has perhaps more in common with Isnard than with Smith. Isnard's mixing of conscious calculation and habits in a world where "entrepreneurs do a better job directing economic activity than a central 'administrator' could ever do" does seem consistent with Hayek's concept of spontaneous order.

Chapter 5 suggests that Isnard did more than anticipate Walras. Chapter 1 (by Ivor Grattan-Guinness) examines how Walras, among others, drew analogies between equilibrium in mechanics and market equilibrium. The idea that Walras viewed market equilibrium in mechanical terms is not new, but there is much detail to explore. Grattan-Guinness argues that the influence of mechanics on the work of late nineteenth century neoclassical economists has been overstated. This is notable because, as mentioned in several chapters of this text, many of the early neoclassical equilibrium theorists had engineering backgrounds and one might expect that mechanics would strongly influence economic theorizing by engineers. It is also the case that the Austrians who initially developed the non-mechanical version of marginal value theory were all educated in law.[2] Yet we must not assume too much regarding the influence of anyone's educational background on any subsequent scholarly work.

Chapters 2 and 3 show how equilibrium concepts moved back and forth between economics and chemistry or biology. For example, economics influenced the La-Chatelier-Braun principle in chemistry, and Paul Samuelson in turn used this equilibrium concept in his highly influential *Foundations of economic analysis*. Chapter 6 reveals the depth of Cournot's work on political, in contrast to Walras's purely mechanical approach to economics.

While the history of science is interesting, the real strength of this book is in its critique of modern economics. Chapters 7, 12, and 13 contain strong critiques of modern equilibrium analysis. Tony Lawson draws a sharp distinction between the examination of real social structures and the fictitious nature of formalistic equilibrium modeling. Alan Freeman argues that neoclassical economists transformed the

[2] Carl Menger, Eugen Bohm-Bawerk, Friedrich A. Hayek, and Ludwig von Mises all studied law in Austria.

equilibrium concept they borrowed from physicists, and used it in a religious fashion. Andy Denis attributes the failure of modern equilibrium analysis to its denial of the micro-macro divide. Denis prefers dialectics to static equilibrium analysis and macro analysis leads to dialectics because macro models entail equilibration, but not any final equilibrium.

Chapters 8, 9, and 11 take more balanced and pragmatic views of equilibrium analysis in economics. Warren Samuels seeks to refocus attention away from the stability and uniqueness of equilibrium, and towards equilibration and dis-equilibration. As one might expect, Victoria Chick finds great merit in Keynes's use of equilibrium in particular. Roger Backhouse advises caution in reacting to some of the more harsh condemnations of equilibrium economics, on the grounds that the term equilibrium has many meanings.

While *Equilibrium in economics* is highly informative, it did leave some areas of interest unexplored. There is little mention of the early twentieth century Stockholm School in this book. Wicksell is mentioned briefly on page 30, but there is surely more to say about the ideas and influence of this great economist. Someone might also have written at length on Lindahl and Cassell. The use of equilibrium concepts by Swedish economists is worthy of attention on its own. Furthermore, the connections between the Stockholm School and Keynes, and also the Austrians, might have been worth exploring in this volume. For that matter, this book has little to say about Austrians other than Hayek. Ludwig Lachmann and Mario Rizzo have advanced thoughtful critiques of equilibrium economics, but readers of *Equilibrium in economics* must look elsewhere to learn about these ideas. The work by Henry Davenport and Frank Knight might also have been worth more attention. There is also much to be written on the use of equilibrium models in public choice theory and new institutional economics. Both public choice and new institutional economics started as low-tech real world orientated research programs. Yet over time both of these programs adopted high-tech equilibrium analysis. Discussion of the ideas mentioned in this paragraph would lengthen this book, however *Equilibrium in economics* is not overly long in its present form, and the inclusion of a wider range of perspectives would better inform its readers. Furthermore, a wider range of perspectives might also have drawn a wider audience.

Equilibrium in economics will prompt its readers to undertake an important task: the critical evaluation of the equilibrium concept as used throughout the history of economics. We should think critically about equilibrium because, as one of the authors of this volume notes, economics is in an unhealthy state because of the way we use equilibrium. The problem with equilibrium is not merely that many economists use this concept badly, but that many of us use it thoughtlessly. The difficulty in addressing the thoughtless use of equilibrium concepts by many economists is that it is exactly this practice which leads many economists to ignore the type of essays contained within *Equilibrium in economics*.

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Douglas W. MacKenzie is a visiting assistant professor of economics at The U. S. Coast Guard Academy. His main research interests are on the theory of the firm, public choice, market equilibrium, and the history of the interwar years.
Contact e-mail: <dmackenz_2000@yahoo.com>

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Review of Benjamin Balak's *McCloskey's rhetoric: discourse ethics in economics*. New York: Routledge, 2006, 142 pp.

DANIEL VARGAS GÓMEZ
EIPE, Erasmus University Rotterdam

Benjamin Balak presents the reader with a concise yet densely philosophical narrative in which rhetorical analysis in economics, as it follows from Deirdre McCloskey, is explained, expanded, criticized, and related to ethics. The endeavor is Herculean, and so is the task of the reader who decides to follow him through a series of intricate paths, which go from the Vienna circle to deconstruction, and from Plato to Feyerabend. Throughout the journey, the reader is prompted to realize the importance—for economic science—of McCloskey's criticism, and the potential that rhetorical analysis has for future work throughout the field.[1]

Balak commences his work by introducing, without any warning to the reader, a series of philosophical authors and the 'method' to be followed. Within a few pages the reader is told that deconstruction, drawing from Derrida and Culler specifically, will be used profusely as a way to shed light on McCloskey's criticism of philosophy and methodology in economics. To the uninitiated in methodological and epistemological matters (economists for the most part, I presume) this can, surely, sound frightening, for they are certainly entering unknown territory. The relation between economics and these philosophical ideas may seem anything but straightforward. However, Balak makes a constant effort to relate both disciplines, and to stay in constant conversation with the economist just as much as with his philosophically inclined readers.

He begins with a delineation of McCloskey's criticism, promising to focus mainly on McCloskey's (1994) *Knowledge and persuasion in economics*. Her criticism, the reader is told, can be understood as having three main targets: 1) social engineering in economics: where successful

[1] McCloskey's criticism and development of 'the rhetoric of economics' began with her wildly acclaimed eponymous paper in the *Journal of Economic Literature* (1983). In 1985, she published a book under the same title, collecting together and expanding her rhetorical project.

prediction is taken as a sign of true knowledge and thus of the power to change outcomes through policies; 2) blackboard economics: empirically empty and thus always risking irrelevance; and 3) the confusion between statistical and (real) economic significance: statistics alone cannot answer the question of "how big is big?" or the need for economists to focus on welfare questions that spring from statistical discussion, like: how many people will be worse or better off?

Balak's exposition of McCloskey's critique is clear and illuminating. This section of the book (chapter 2) is a great introduction for those who are not familiar with McCloskey's writings, or have had difficulties understanding her ideas in a coherent and methodical way.

After a serious exploration of McCloskey's criticism the reader is embarked in a difficult philosophical conversation (chapter 3): the exchange between Uskali Mäki and Deirdre McCloskey on the rhetoric of economics project.[2] There is a clarifying exposition of arguments and misunderstandings, but, particularly, a critical reading of Mäki's "dry" analytical reconstruction, as well as of McCloskey's lack of involvement and even paternalism in her reproach. In short, Mäki is portrayed as following the Platonic gesture that has characterized western philosophy throughout history: foundationalism as the basis for any methodological discussion. McCloskey's idea of rhetoric, the reader must bear in mind, does not claim to belong to methodological discussions in a conventional way. She is not interested in positing a set of epistemological rules, but, on the contrary, to denounce the strictness and sterility of traditional methodology. In turn, "the rhetoric" is proposed as a coherence theory of truth, without any axiomatic core to be found (Balak 2006, 54-58). Mäki's criticism fails to engage in a profound reading of McCloskey's anti-foundationalist position as a result of his poor understanding of the postmodern-like ideas that populate her writing. The analytical-continental split comes frequently to mind throughout the exchange and may lead the reader, from time to time, to see both views as simply incommensurable.

Surprisingly enough, there is a point that Balak concedes to Mäki, with respect to the moral constraint that seems to support McCloskey's idea of a "rhetorically conscious" community. The fact that "the rhetoric" is a bricolage that rests on persuasion, argument, and

[2] Balak focuses mainly on Mäki's "Diagnosing McCloskey" (1995) and McCloskey's reply: "Modern epistemology against analytic philosophy: a reply to Mäki" (1995). Both articles can be found in the same issue of the *Journal of Economic Literature*.

conversation (Balak 2006, 51), and not a *proper* methodology begs the question of normativity: for how is the scientific effort to be directed if the task of choosing between all theories that are built into a coherent system is a *rhetorically persuasive* one? Mäki, says Balak, wants to solve the matter by constraining the set of beliefs that form a coherence theory of truth (McCloskey's) based on their plausibility. Plausibility would thus become the criteria of selection to him.

Balak argues in favor of McCloskey's reply though. He eagerly wants to show that Mäki seems to condition the answer to come, hence avoiding the deeper elements of the disagreement (mainly over epistemological issues). Briefly, Balak argues that McCloskey does not adhere to a "theory of truth T" (universal, complete, and so on), because she's trying to avoid metaphysics, while Mäki is trying to "push" his. She's not making the naïve claim that there is no truth, but that all truths are socially constructed (even those that Mäki would favor).

McCloskey belongs within a 'postmodern' view of philosophy and one cannot engage with her properly unless one understands where she's coming from. In keeping with his original intention, in chapter 4 Balak begins a "Proof" of this claim, as a way to inundate the dry land left after the Mäki-McCloskey exchange. He uses Foucault in order to stress that knowledge is not exterior or alien to the social context from which it emerges. They are, in actuality, different sides of the same coin. The importance of language is also stressed and a hurried rendition of the Searle-Derrida debate is presented. The notions of context, iterability, and a criticism of speech-act theory's view of intentionality are *all* main points in this chapter. All of them pose a difficult reading, filled with questions. The argument here is quite dense, I should add, and will ask an effort on behalf of the reader, which may take him to engage with the original texts.[3]

Chapter 5 turns to a clarification of McCloskey's take on ethics. Ethics is important, Balak tells us, because it relates to McCloskey's attempt to delimit the conversation, thus creating an ethics of conversation (pp. 105-106). Concretely, ethics plays an important role in economic science. Having an understanding of the ethical commitments involved in the application of economic theory, via economic policy, is part of what the economist should be able to talk about.

[3] Derrida's essay, which originated the debate, together with a summary of Searle's response to it, and Derrida's response to Searle's response can be found in Derrida's *Limited Inc* (1988).

Balak ends his take on McCloskey's ethical perspective by talking about an understanding of ethical rules as following an evolutionary process, which includes historical as well as biological determination. The discussion goes from Veblen to Vernon Smith, and back to Nietzsche. This last part is more for the reader interested in an understanding of ethics including historical as well as biological aspects, but it is not a necessary consequence following from McCloskey's ideas.

The book ends (in McCloskey's fashion) with the *peroratio*: an analysis of the ethical commitments behind knowledge production, where McCloskey's take on ethics becomes useful. The ethical standpoint of McCloskey, says Balak, comes precisely as a consequence of her epistemological perspective: given that truth is contextual, coming always from a perspective, it is *ethical* to root for pluralism and tolerance within the scientific conversation. The rhetorical analysis of economics leads in fact, he concludes, to a *new pragmatism* (p. 121).

All in all, the book serves the purpose of effectively introducing the reader into the ideas underlying McCloskey's rhetorical project. Balak's analysis is well supported with quotations and references. The analysis of the Mäki-McCloskey debate is interesting as well for various reasons, including its illustration of the difficulties that emerge in actual conversations between scientists, which the author addresses carefully. Those with only a casual interest in McCloskey's work on rhetoric may find parts of this book too demanding, but those with a more systematic interest will appreciate its depth and sophistication.

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Daniel Vargas Gómez is currently a research master student at the Erasmus Institute for Philosophy and Economics, Erasmus University Rotterdam. His MA thesis "Producing postmodern economics: an interpretation of Deirdre McCloskey's rhetoric of economics" focuses on the constructive articulation of elements of rhetorical analysis, hermeneutics, and deconstruction, within the construction of a contemporary economic conversation.
Contact e-mail: <312662dv@eur.nl>

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PHD THESIS SUMMARY: The political economy of urban reconstruction, development, and planning.

EMILY C. SCHAEFFER*
PhD in economics, April 2009
George Mason University

Public economic planning is a pervasive part of the social space of political economy. In times of crisis, public officials in American government agencies at the local and national level increasingly use the vocabulary of economic planning. My PhD dissertation is a collection of papers representing a pointed critique of urban economic planning by public officials in times of crisis and prosperity. I explore John Stuart Mill's argument in his *Principles of political economy* (1848) concerning the market mechanism (compared to rational central planning) through which economies experience remarkable recovery in the wake of devastation:

> This perpetual consumption and reproduction of capital affords the explanation of what has so often excited wonder, the great rapidity with which countries recover from a state of devastation; the disappearance, in a short time, of all traces of the mischiefs done by earthquakes, floods, hurricanes, and the ravages of war (Mill 1848, 74-75).

The papers are rooted in the classical liberal philosophies of David Hume and Adam Smith, and stem from the scholarship on planning of the economists Friedrich A. Hayek (1935; 1937; 1945; 1952) and Ludwig von Mises (1920; 1922; 1949). Particularly following a crisis, demands for planning tend to be especially strong and persist in the name of economic and social development. At the core of the planning paradigm is the idea that public planning can produce results superior to the spontaneous outcomes of markets. My dissertation explores this idea by using a theory of politics as exchange and draws on the fields of

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philosophy and sociology to sharpen the understanding of economic planning.

Traditional economic approaches to planning and development use comparative statics, in which the modeler stands outside the system and policies are enacted "on top" of a market structure. In other words, political planning is predicated on the idea that rational construction of outcomes can be substituted for market exchange. The alternative framework herein explains how the knowledge coordinating properties of the price system produces emergent outcomes superior to those constructed by public planners. Neither one single mind, nor group of minds possesses the cognitive ability to design and coordinate a system of such complexity. In fact, attempts to implement public planning set in motion an endogenous and non-constructivist order as well, producing patterns of exchange based on distorted relative prices that do not accurately reflect underlying scarcities.

My framework posits that the same components of human agency are present in markets and politics. Adam Smith's argument for the propensity of humans to "truck, barter, and exchange" does not wane when individuals move from institutional contexts of private to public life (or contexts of strong private property rights to weak or absent private property rights). However, the manifestations of these propensities do change. Unless under situations of unanimous voting, political exchange involves traders that do not bear the full cost of the public action. Individuals in the marketplace are constrained to actions that satisfy both the immediate wants of his exchange partner (leaving both parties better off) and increasing total wealth.

Satisfaction of this condition in private exchange serves as the foundation for the superior epistemic properties of market generated coordination. As prices emerge through the exchange of private property, local knowledge is communicated to relevant actors. For example: when a hurricane destroys a town, the price of lumber rises dramatically. People thousands of miles away may not know why the price of lumber is higher, but they are "told" by the price to conserve their use of lumber because it is valued relatively more in the disaster-hit area. Similarly, someone outside of the disaster area may see that the price of lumber is triple its normal price. Lured by exchange for profit, the individual will buy near and sell dear. The price system transmits the knowledge of relative prices to agents throughout the system, thus communicating the incentives to bring about a more coordinated state of affairs. Profit and loss accounting provides the necessary discipline to ensure that the information traveling through the system reflects the underlying relative scarcities, and is therefore accurate knowledge. I argue that public exchange lacks such epistemic virtues and thus results in an inferior kind of coordination when compared to a market generated order.

The first paper of my dissertation is entitled, "Earth, wind, and fire! Federalism and incentives in natural disaster response". Following catastrophic natural disasters, the benefits of centralization include the ability to amass resources quickly and a unique ability to overcome externality problems. Through a comparative analysis of three large-scale natural disasters, I find that citizens' groups led a more efficient response than that led by a federal agency specializing in disaster recovery. Lacking effective mechanisms for communicating the relevant knowledge and the weak incentives of federal officials, the advantages of centralization failed to provide an adequate recovery due to an inability to harness the local knowledge of time and place.

The second paper of the dissertation, "The role of public and private bureaucracy in urban natural disaster response" considers how a city rebounds from natural disasters when there is no federal agency involvement. I explain how a private bureaucracy functioned following the Chicago Fires of 1871. My findings suggest that the operation of an effectively monopolistic group of private citizens provided a relatively superior recovery services when compared to public bureaucracy.

Finally, the third paper, "Mixed-income development housing: what's left in neighborhood economic planning?" addresses localized mixed-income housing policies that attempt to correct the effects of concentrated poverty with government planning that involves human capital investment. Political planning attempts to subvert the workings of this decentralized price mechanism with a hierarchical structure of decision-making. Lacking the epistemic properties of market orders, political planning devolves into political exchange between interest groups. Using ethnographic case study analysis of the St. Thomas/River Garden Development, I examine the planning process from a political exchange perspective and explain how the outcomes of policy deviate from the intended or "planned" outcomes.

In sum, *The political economy of urban reconstruction, development, and planning* is an attempt to transplant a popular framework of political and market exchange to the local planning level, where urban resilience and decay can be effectively explained.

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Emily C. Schaeffer obtained her PhD in economics from George Mason University in April 2009. The dissertation committee included Professor Peter J. Boettke (George Mason University) as the committee chair, Peter T. Leeson (George Mason University), and Virgil H. Storr (Mercatus Center, George Mason University).
Contact e-mail: <Eschaeff@gmu.edu>
Website: <www.Emily-Schaeffer.com>

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PHD THESIS SUMMARY: The market's place in the provision of goods.

RUTGER CLAASSEN
PhD in philosophy, May 2008
University of Utrecht

Which goods should we be able to buy and sell on the market and, alternatively, which goods should remain sheltered from the market? For many goods in modern societies, this has proven to be a thorny question. Moreover, as I argue in the introductory chapter, it is a question that cannot be answered by way of a theoretical shortcut, that is, by attributing certain general values (or disvalues) to the market and inferring from these general attributes that the market is (or is not) the best institution to govern the provision of a specific good. Rather, we need a framework for making decisions at the level of these specific goods.

In the first part of this work, three theoretical building blocks are proposed to frame the way we should handle the market question. First, a social theory is formulated which treats the market as one out of five main socio-economic modes of provision, the others being public provision, professional provision, informal provision and self-provision. Second, in choosing the best institutional framework for a specific good, it is argued that we should not restrict ourselves to the option of implementing one of these modes of provision only. Special attention should be paid to the option of 'institutional pluralism', i.e., instituting a market and a non-market alternative simultaneously for the same good. Both, the attractions and the limits of such an institutional pluralism are discussed. Third, in making institutional choices about these modes of provision, we need a normative theory providing the moral criteria to guide our choices. Here a capability theory is proposed, which consists of three central principles, parallel to a distinction between three types of capabilities: those that are 'immoral', those that are 'morally required' and those that are merely 'morally permissible'. The principles prescribe avoiding the realization of the immoral type of capabilities and promoting the realization of morally required and permissible capabilities.

In the second part of this work, this threefold framework is applied to three specific goods. The first good is security, i.e., protection against criminal threats. The second good is media, i.e., mass communication by media such as television, radio, the press, and the like. The third good is care, i.e., caring activities provided on a structural basis to people in a position of dependency, such as care for children, chronically ill and elderly people. For each of these goods it is argued—on the basis of a capability analysis—that the most appropriate institutional framework is institutional pluralism. In the case of security this is a pluralism of market and public provision, in the case of media a pluralism of market and professional provision, and in the case of care a pluralism of market and informal provision.

In the final chapter, I investigate the conditions that have to be fulfilled for these institutionally pluralist arrangements to be stable, that is, to have a robust chance of survival. The main challenge to stability comes from the dynamic toward capital accumulation that is inherent in contemporary capitalism, and if left unchecked requires an ongoing conversion of non-market practices into markets. Two solutions are proposed to counter this threat: a general working time reduction which reduces capital accumulation and a strategy of publicly funded investment of accumulated capital in non-market parts of institutionally pluralist practices.

Rutger Claassen obtained his PhD in philosophy from Utrecht University, The Netherlands, in May 2008. He was supervised by Marcus Düwell (Utrecht University) and Elizabeth Anderson (University of Michigan). He currently works as a political philosopher in the Political Science Department, University of Leiden.
Contact e-mail: <claassenrjg@fsw.leidenuniv.nl>

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PHD THESIS SUMMARY:
Democracy-as-fairness: justice, equal chances and lotteries.

BEN SAUNDERS
DPhil in politics, June 2008
University of Oxford

My dissertation lies primarily in the area of normative political philosophy, yet draws from sources as diverse as economics (the desiderata of social choice), moral philosophy (fairness in choosing between groups of people), and ancient history (use of lotteries in Athenian democracy) to challenge the widespread acceptance of majority rule.

I argue that democracy is legitimate as a solution to co-ordination problems, but that it is only acceptable to all if it gives each at least a chance of getting at least some of what they want (chapter 1). Adopting a contractualist approach to the justification of decision procedures, I reject two popular arguments for majority rule. Firstly, it need not produce good outcomes, since for example it does not take account of intensities (chapter 2). Secondly, it is not necessarily fair to all parties: unless all have some chance of ending up in the winning coalition, those excluded have no reason to accept the process as fair (chapter 3).

These arguments do not show that majority rule is always illegitimate, but they do suggest that there are some circumstances—such as when there is a permanent minority—where it is inappropriate. Here, we need some procedure that respects minorities. Chapters 4 and 5 develop one such proposal, termed 'lottery voting', in which a single vote is randomly selected to determine the outcome. Consequently, each individual is equally likely to be decisive (with a probability of 1/n) while the chance of each option winning is proportional to its level of support amongst voters. I consider various possibilities for the implementation of such a procedure in small group decision-making, including how it may be combined with judicial review or time limits on decisions, and rebut objections based on the possibility of extreme minorities winning (suggesting that this could be avoided by institutional checks, but may be unlikely given that voting behaviour is endogenous to the system and a system where any vote could win encourages responsible voting).

The final two chapters (chapters 6 and 7) evaluate lottery voting against certain normative requirements commonly employed as axioms in the literature on social choice. Firstly, I evaluate it against the necessary and sufficient conditions of simple majority rule identified by May: decisiveness, anonymity, neutrality, and positive responsiveness. This comparison is complicated, since May assumes a deterministic rather than probabilistic procedure, but I argue that lottery voting meets analogues of his conditions that share the same intuitive appeal: it always produces a decision, it treats all voters and options equally, and voting for an option always favours it (which, I note, removes any incentives for strategic voting). I then proceed to compare lottery voting to Arrow's axiomatic conditions: collective rationality, universal domain, Pareto, independence of irrelevant alternatives, and non-dictatorship. Again, there are some difficulties because democracy is here understood as a pure procedure for settling conflicts of individual interests, rather than as a system for computing a single collective will or interests. Nonetheless, I observe that lottery voting will always respect unanimous preferences; while a random dictatorship is normatively unproblematic (there is no individual who always gets his or her way, regardless of others' preferences).

Chapter 7 is devoted to rationality, and argues that no decision procedure is inherently rational or irrational: what matters is the rationality of individual agents adopting it, which is a condition of my contractualist approach. Just as it may be rational for two individuals to settle a disagreement by tossing a coin, so it may be rational for a larger group to settle disagreements by agreeing to accept a randomly-drawn vote.

If democracy is understood as citizen sovereignty and political equality, then the possibility of lottery voting shows that it does not logically require majority rule. Whether the members of society should prefer lottery voting to majority rule or vice versa seems to rest on the conditions that they face (e.g., whether there are permanent minorities), but the mere possibility of an alternative discredits some arguments for majority rule and shows that we need to justify that procedure separately from democracy. Moreover, lottery voting has a number of benefits, aside from giving minorities some chance of victory, because it removes incentives for strategic voting and makes it easy to use weighted voting (if desired).

Ben Saunders obtained his DPhil from the University of Oxford in June 2008. He was supervised by David Miller (University of Oxford), and examined by David Estlund (Brown University) and Adam Swift (University of Oxford). He is currently departmental lecturer in philosophy at the University of Oxford, and working on a book based on the thesis.
Contact e-mail: <ben.saunders@philosophy.ox.ac.uk>

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PHD THESIS SUMMARY: The use of knowledge in comparative economics.

ADAM G. MARTIN
PhD in economics, May 2009
George Mason University

For most of the twentieth century, comparative economics asked one question: capitalism or socialism? The particulars of the question took many forms, both: positive and normative, from stark contrasts to shaded nuances (e.g., 'the mixed economy'), but the central theme was always planning versus markets. By contrast, the 'new comparative economics' (e.g., Djankov, et al. 2003) draws on diverse sub-disciplines such as law and economics, public choice, and new institutional economics. What makes these comparative exercises 'economics' is their method, not their question.

Contemporary comparative economics, though focused broadly on issues of economic development, is not merely concerned with the proximate causes of the generation of wealth or the production and distribution of goods and services. Rather, it uses rational choice as a constant across institutional contexts to illuminate how different institutions engender different outcomes. Consequently, the capitalism-socialism debate no longer constrains the comparative discussion. Any sort of comparison is fair game: democracy versus dictatorship, common law versus civil law, and even cross-institutional comparisons such as democracy versus markets.

The move to cross-institutional–rational-choice analysis is a large step forward in comparative economics. An insistence on behavioral symmetry—that, absent specified selection mechanisms, the same sorts of agents populate different institutional settings—creates a common grammar in which genuine, non-orthogonal comparisons can be made (as against, for example, comparing neoclassical perfect competition with a Leontieff input-output table). And in contrast with the institutionally antiseptic models of mid-century economics, models by which the profession judged the market socialists as the winners of the economic calculation debate, purely technocratic matters no longer dominate. The importance of property rights, governance, and even culture are now widely recognized. But though the conclusions of the market socialists are no longer in fashion, the new comparative economics has inherited their faulty economic anthropology. It was that faulty model of agency that made the arguments made by Austrian economists Mises and Hayek in the calculation debate fall on deaf ears.

The organizing theme of my dissertation is a simple question: what would the new comparative economics look like if its economic anthropology were more Austrian? Rehashing the calculation debate is not my aim; I take it as given that the Austrians were correct. Rather, I build on the insights gleaned from that debate in order to fill in some of the gaps in the new comparative literature. Doing so requires identifying the unique characteristics of Austrian agents in contrast to market socialist (neoclassical) agents.

Like their neoclassical cousins, Austrian agents confront scarcity and therefore exercise instrumental choice. However, in contrast with traditional rational choice theory, Austrian agents must first subjectively construct (imagine) the opportunity set from which they choose. I identify this condition of ignorance (or non-givenness) of opportunity sets with Frank Knight's concept of uncertainty (Knight 1921). Though Knight is not an Austrian, in the midst of his debate with the market socialists Hayek invokes uncertainty as important to understanding how his opponents were misusing the equilibrium construct (Hayek 1937). Uncertainty, a condition of individual agents, is the microfoundation of (macro-level) Hayekian knowledge problems.

Just as scarcity necessitates choice, uncertainty necessitates imagination. The Austrian literature identifies this additional aspect of agency with entrepreneurship (Kirzner 1982), which is not unique to markets but can take place in any institutional context. This tweak to the standard economic anthropology furnishes the comparative economist with new tools for understanding how institutional contexts differ while keeping behavioral symmetry intact. As shown in figure 1 below, when uncertainty is added to scarcity as a problem facing purposive agents, institutions must not only align incentives to settle disputes over interpersonal resource and rent allocation, but also must generate knowledge to coordinate the potentially divergent expectations of multitudinous agents.

The first essay argues that, as pure rational choice explanations are applied to different social spheres, the possibility of inefficiency—unrealized gains from trade—is progressively excised from economic analysis. The only dollar bills that will not be picked up are those that are not perceived. Inefficiency presupposes Hayekian knowledge problems. Comparing how and how well institutions generate knowledge of potential gains from trade allows for substantive cross-institutional analysis not predicated on universal efficiency or institutional convergence.

Figure 1: The Microfoundations of Comparative Economics

Praxeological Condition	Economic Anthropology	Social Dilemma	Institutional Function
Scarcity →	Choice →	Allocation →	Incentive Alignment
Uncertainty →	Imagination →	Coordination →	Knowledge Generation

The second essay engages in just such a comparative analysis in terms of the environmental feedback that agents receive in evaluating the conjectures on which their actions are based. I argue that in tight feedback environments, agents' mental models converge to perfect substitutes, while in loose feedback environments mental models matter more. Ancient Greeks who believed in fire *daemons* responded much the same way to getting burned as do modern, thermodynamically savvy physicists, but ancient healers—operating in a looser feedback environment with particular mental models—acted very differently from contemporary doctors. I then argue that, in terms of the interpersonal coordination of plans that constitutes social order, markets as an institutional environment offer tighter feedback than democratic polities. Markets enable economic calculation to coordinate activity. Democracy relies on speech acts that communicate intersubjectively valid criteria for evaluating proposals. Ideas about the nature of social order will thus be of significantly greater causal import in the operation of democratic politics than in the operation of markets. Errors made by market actors, like those made by children touching hot stoves, are self-correcting. Democratic error correction, by contrast, requires bringing better ideas to bear.

The final essay is more methodological than theoretical, addressing the relationship between Austrian economics and the critical realist project. The argument explores what I dub the "Austrian Paradox": the peculiar fact that Austrian analysis is rational choice theoretic and yet embraces emergence, open-ended processes, and other salient features of heterodox social ontology. The addition of uncertainty to rational choice allows Austrians to capture the most persuasive elements of mainstream and heterodox economics. Uncertainty serves as the microfoundation for the panoply of heterodox concerns, while a strict Misesian commitment to rational choice is necessary for critical realism's own ontology of social structures (including institutions) to make any sense.

This dissertation is one part of a larger, long-term project applying Knightian uncertainty to modern political economy. Uncertainty as a tool of analysis has traditionally been limited to the study of markets, firms, and entrepreneurship narrowly defined. The purpose of the project is to ask what modern economics would look like if uncertainty were injected into the study of law and economics, public choice, economic development, and institutional economics. My hope is that uncertainty as I have defined it—open opportunity sets—will be a good candidate for introducing traditionally heterodox and Austrian concerns into mainstream analysis since it does not ask economists to abandon rational choice theory.

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Adam G. Martin received his PhD in economics from George Mason University in May 2009. Peter J. Boettke (George Mason University) served as his dissertation chair. He is currently a research fellow at the Center for the History of Political Economy, Duke University. In September 2009, he will begin a post-doctoral fellowship at the Development Research Institute, New York University.

Contact e-mail: <amartini@gmu.edu>

Website: <www.adamgmartin.com>

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PHD THESIS SUMMARY: Kahneman and Tversky and the making of behavioral economics.

FLORIS HEUKELOM
PhD in economics, May 2009
Amsterdam School of Economics

The twofold aim of this thesis is to understand Daniel Kahneman's and Amos Tversky's research, and to understand how this research has altered economics in fundamental ways. I frame my historical analysis in terms of Peter Galison's disunity concept. Galison uses the notion of the disunity of science to capture the idea that sciences and scientific practices may be separate and different, but at the same time be communicating and mutually influencing each other.

I start by discussing the work of the mathematical psychologists and behavioral decision researchers at the University of Michigan in the 1950s and 1960s. I argue that the key to understanding mathematical psychology and behavioral decision research is to see that, although largely separated and focused on different questions, both presumed the same two-sided understanding of psychology. In order to measure, one needed a sound theory of the measurement instrument, which was the human decision maker.

This double understanding of psychology as using a measurement instrument to investigate that same measurement instrument became problematic when it turned out that the measurement instrument did not behave as it should. That was the problem Tversky struggled with. Tversky had to choose between declaring the experimental results invalid and saying that the received theory of the measurement instrument was incorrect.

Kahneman came to the rescue by suggesting that the human decision maker systematically and predictably deviates from how it should behave. Thus, the experimental results could be accepted, while at the same time the axioms of the measurement theory could be maintained. It did, however, give psychology the new task of investigating how and when human decision makers deviate from how they should behave. That new task was the basis of Kahneman and Tversky's collaborative research of the 1970s.

Tversky was educated at and received his PhD in the early 1960s from the University of Michigan under the supervision of Clyde Coombs and Ward Edwards. Tversky's research embodied the synthesis of mathematical psychology and behavioral decision research. Towards the late 1960s, however, Tversky increasingly struggled with the tension between Leonard Savage's a priori axioms of decision theory and the behavioral deviations he observed in his experiments. Kahneman, for his part, came from a very different background. Strongly influenced by his experience as a psychologist in the Israeli army, Kahneman's different research interests focused on humans' cognitive mistakes. Kahneman showed that despite the fact that we think we do cognitively quite well in the course of our daily lives; in fact, we constantly make systematic cognitive mistakes.

In 1969 Kahneman and Tversky started their long and fruitful collaboration. I discuss Kahneman and Tversky's research of the 1970s and show how in 1979 their research culminated in prospect theory, a theory which describes actual human decision behavior as a systematic deviation from the normative rules. Kahneman and Tversky considered prospect theory applicable to both economists' and psychologists' use of expected utility theory. The paper was published in *Econometrica* and argued that cognitive psychology and economics were unified in one field of behavioral science.

Subsequently, I investigate how economists responded to Kahneman and Tversky's understanding of experimental violations of expected utility theory and their descriptive alternative, prospect theory. I argue that there were two main responses, each with their own history. Experimental economists such as Vernon Smith corroborated and accepted the experimental results, but rejected all preference theories as a solution, including expected utility theory *and* prospect theory. In addition, experimental economists inferred that the experimental deviations further emphasized the importance of the market as the mechanism that over time drives the economy to a rational equilibrium.

Financial economists, such as Richard Thaler, also accepted the experimental results, but instead they took it as proof of the observed irrationalities in financial markets. In addition, financial economists hailed Kahneman and Tversky and prospect theory as being the most important, if not the only claimant to a solution to the problem. The use of prospect theory in financial economics led to the new field of behavioral finance. The reason for prospect theory's swift success was that it offered financial economists an elegant way out of their problems. The normative-descriptive distinction ensured that traditional neoclassical models could be maintained as the normative theory, while at the same time it offered a descriptive alternative that was only slightly different from previously-used theories and hence easy to learn by economists.

In the late 1980s and early 1990s, Thaler also started applying the behavioral finance approach to problems outside the field of financial economics. The new field grew quickly and in 1994 it was officially termed *behavioral economics*. Once the traditional economic theories were saved in the normative realm and new theories could be developed under the rubric of descriptive theory, a surge of exploration ensued. Gradually the labels of normative and descriptive were replaced by full rationality and bounded rationality, which in turn allowed the behavioral economists to develop their own view of economic policy advice under the label of paternalism. These developments contributed to the gradual emergence of behavioral economics as a stable and clearly defined mainstream economic program. As a result, it also brought to the fore how behavioral economists saw their program as being different from other economic programs and disciplines. Behavioral economists began to distinguish their program, in particular from psychology and experimental economics.

The history discussed in this thesis shows how economists have actively used psychology to redefine economics. The flow of theories, methods, and experimental results from psychology to economics was not a neutral process that left these theories, methods and experimental results unaffected. Instead, they lost some of their psychological connotations and gained new economic connotations. What is particularly illustrative in this regard are the two cases of experimental and behavioral economics, which both added different new economic connotations to the theories, methods, and experimental results drawn from psychology to redefine economics in their own ways. Thus, as I argue in this final chapter, this thesis not only shows that the theories, methods, and experimental results that travelled from psychology to economic have not been stable entities, but it also shows that the definition of economics has not been constant. Therefore, the history of economics and psychology can only be understood by recognizing economics and psychology as disunified cultures.

Floris Heukelom obtained his PhD from the Amsterdam School of Economics in May 2009, under the supervision of John B. Davis (Amsterdam School of Economics) and Harro Maas (Amsterdam School of Economics). The author is currently assistant professor of economics at Radboud University Nijmegen, The Netherlands.
Contact e-mail: <F.Heukelom@fm.ru.nl>