

ECONOMIC EPISTEMOLOGY: HOPES AND HORRORS

1. The dismal queen and the science wars

The cultural and epistemic status of science is under attack. Social and cultural studies of science are widely perceived to offer evidence and arguments in support of an anti-science campaign. They portray science as a mundane social endeavour, akin to religion and politics, with no privileged access to truthful information about the (socially unconstructed) real world. Science is under threat and needs defence. Old philosophical legitimations have lost their bite. Alarm bells ring, new troops have to be mobilised. Call economics, the good old friend of the status quo depicting it as a generally beneficial social order while accommodating a rather mundane picture of human behaviour. In contrast to constructivist and relativist sociology of scientific knowledge, economic accounts of science seek to provide a rigorous defence of the cultural and epistemic legitimacy of science by accommodating plausible elements in the sociological accounts and by embedding them in invisible-hand arguments about the functioning of some market-like structure within science. Viewed through economic spectacles, science re-emerges from the ashes as stronger and more beautiful than ever. A spectator raises an innocent question: is economics itself strong and beautiful enough to offer such alleviating services? In order to examine the emerging issue of disciplinary credibility, we need to look at economics itself more closely, and we need to address traditional issues in the philosophy of science as well as less traditional issues of reflexivity. We will see that the above caricature concerning the role of economics in the science wars calls for heavy qualifications if not wholesale rejection (no comment here on the caricatured role of the SSK).

Let us then try a less dramatic and less streamlined start, one that is initially more neutral with respect to the science wars, and more focused on issues that are endogenous to epistemology

and science studies. Knowledge is a product of social processes. Therefore, epistemology has to be socialised. No matter what degree of agreement there is on this claim, it does not yet fix anything about *how* to socialise epistemology. An obvious first step is to suggest that answering this question requires consulting the relevant sciences for their contributions to the understanding of the social aspects of cognition. Just as the "naturalisation" of epistemology may be taken to require being informed about the advances of neuro-sciences, its "socialisation" would require consulting the social sciences. But again, this imposes very weak constraints on how to proceed in the socialisation project. The multiplicity of possible theoretical resources that could be mobilised is almost endless. There are many social sciences, all of them internally heterogeneous, each with a variety of rival and complementary theories, methods, and styles of research. Not only do we have varieties of general social theory, but there are specialised fields, schools, traditions, and theoretical frameworks within political science, sociology, anthropology, social psychology, legal studies, socio-linguistics, management research, cultural studies, communication studies – and economics. On what grounds are we to choose from among such options?

In choosing the theoretical resources for the socialisation of epistemology, one is naturally constrained by one's background beliefs and preferences. If one thinks highly of science, economics may appear attractive for numerous reasons: economics offers tools for building an effective pro-science account, one that rectifies the epistemic rationality and objectivity of science; economics itself is a rigorous and reliable scientific discipline with theoretical and explanatory powers superior to those of other social sciences; and given that economics presents itself as the science that deals with issues

of scarcity, choice, and trade-off situations, it has the much needed capacities for systematically dealing with pressing issues in science policy in the era of tightening financial constraints. This optimistic picture depicts a science-based and science-friendly balance between science as a subject of social epistemology, economics as the scientific resource for socialising the epistemology of science, and contemporary science policy.

None of this will convince those with different background beliefs and preferences. Definitely for those with anti-science sentiments, but also for many of those with moderately sceptical attitudes, such a picture appears to tell a horror story based on a self-contained and self-celebrating scientism. If science in general is suspect, then economics must be at least as much so, and there is no justification for setting out to defend a suspect subject by appealing to a suspect tool.

Indeed, economics is a contested discipline, inciting conflicting attitudes of assent and admiration, suspicion and hostility. Perceptions tend to be polarised between seeing it as a scientific success story and as a culturally dangerous failure. Economics is the “queen of the social sciences” for some, while others view it as the “dismal science” in one or more senses of this expression (see Mäki 2002). Where many see technical rigor, theoretical insight and empirical success, others find empirically empty intellectual toy games played by mathematically minded technicians or politically minded ideologues. Many are convinced that economics is our best guide to understanding the conditions of social co-ordination, efficient use of resources, and people’s wellbeing, but others are worried about the increasing adoption of its suspiciously narrow and distorting worldviews as part of the questionable cultural trend of economisation, marketisation, monetisation and commodification of our social lives at large. The “dismal queen” is able to trigger hopes as well as horrors. (See e.g. Mayer 1993; Reder 1999; Colander 1991)

A related observation that helps put economics on the intellectual map points out its expansionist tendencies. Economic concepts and principles and styles of inquiry are increasingly employed in the study of phenomena that lie beyond the boundaries of the traditional domains of economics. This has changed disciplines such

as political science, sociology, and law. Indeed, economics is an expansionist discipline, disrespectful for traditional disciplinary boundaries. Philosophy is not innocent either, it is on the list of disciplines increasingly influenced by economic ideas. This has been obvious in areas such as political and moral philosophy, and nowadays no less so in epistemology and philosophy of science. The “economisation” of philosophy is just a special case of its “scientisation”. All of this is a matter of hopes of scientific unification for some, horrors of intellectual imperialism for others.

The epistemological adaptations of economic ideas are not of a recent origin, but it is only recently that the use of economics as a resource for social epistemology and philosophy of science has become a growing industry, as witnessed by various bodies of work, such as Bartley (1982), Goldman (1999), Goldman and Shaked (1991), Kitcher (1991, 1993), Luetge (2004), Rescher (1989, 1996), Strevens (2003), Zamora (1999, 2002a, 2002b), and others (for overviews, see Hands 1997, 2001). Unsurprisingly, these developments have been accompanied by a series of criticisms (e.g. Hands 1995, 1997; Mirowski 1996, 1997; Roorda 1997; Sent 1997; Ylikoski 1995).

I find this body of literature fascinating and challenging for many reasons, including the fact that there is a surprising (and inspiring) gap in it: the absence of a systematic examination of economics itself as a resource for socialising epistemology. One would expect to see consultations of the vast literature in the philosophy and social studies of economics as part of such an examination. This expectation is reinforced by the recognition of the deep and extensive controversies over the status of economics as a scientific discipline, and over the economisation of our culture more generally. Indeed, one would expect to find some discussion on how to set out to provide a normative assessment and justification of the use of this or that economic theory (or more generally the dominant economic style of modelling) as a resource in the philosophy of knowledge and science. The challenge is to assess the credibility of economics.

The obvious way to go in looking for such a normative account is based on the recognition that in order to assess the credentials of *economic*

epistemology we need to appeal to the *epistemology of economics* – including the *social and economic epistemology of economics*. If one wants to be justified in choosing to view *science as an economy*, this implies a call for gauging *economics as a science* – including *economics as an economy*. Intriguing issues of normativity and reflexivity emerge. While the focus here is on the “economisation” of epistemology, it is clear that analogical issues arise in other branches of science-informed epistemology.

Before dealing with these questions in more detail, I will briefly point out the heterogeneity and flexibility of economics as a source of ideas, with variable implications for the economic epistemology of science. While many models in economic epistemology invoke something like the invisible hand with apparently science-friendly implications, economics is in no way predetermined to yield those implications only: it has a rich set of theoretical resources with flexible uses. Some of the horrors alleviated, some of the hopes circumscribed.

2. Economic epistemology and the plurality of invisible hands

Economics may enter epistemological investigations in a number of ways, but only some of them are directly relevant to social epistemology (the issue of whether economics has a deep enough notion of the social is ignored here). Many of those ways that are relevant, are expected to contribute to social epistemology conceived as the study of the reliability of various social mechanisms and processes in the generation of knowledge. Perhaps the most striking contribution of economics to social epistemology is the adaptation to another sphere of one of the most important insights of economic thought since Bernard Mandeville and Adam Smith. The key social mechanism we find in economics is that of the generous invisible hand: individuals in interaction with one another may bring about beneficial aggregate consequences without aiming at those consequences. The paradigm of this idea envisages individuals driven by their self-interest in the market for goods and services, and their interactions bringing about the maximum possible social welfare for all as an unintended

consequence: private vices, public virtues. This is a powerful notion that economic epistemologists may use in seeking to accommodate both the traditional epistemic virtues of science and many of the plausible insights offered by the sociology of scientific knowledge.

This is probably why economic epistemology appears to many as decisively pro-science, telling a story of epistemically under-motivated (or unmotivated) behaviour with an epistemically happy end. The contrast with traditional philosophical images and recent SSK portrayals of science is often perceived along the following radically simplifying lines. Traditional philosophy of science: private epistemic virtues → public epistemic virtues. SSK: private epistemic vices → public epistemic vices. Economic epistemology: private epistemic vices → public epistemic virtues.

The invisible-hand argument about science may permit individual scientists to pursue non-epistemic goals, even predominantly. The outcomes of collective interactions between the individuals may be epistemically honourable, by virtue of the functioning of certain co-ordination mechanisms within the (market-like) institutions of science. These then are indeed thought to be among the social mechanisms that reliably channel scientists' activities such that knowledge and its advancement will be attained as an outcome. While Bartley (1982) and Hull (1988) make rather straightforward use of a strong notion of invisible hand in the scientific context, philosophers like Kitcher and Goldman are more qualified in their accounts.

Goldman and Shaked (1991) examine the consequences of scientists pursuing their professional interests for truth-acquisition in science. They assume that scientists act like sellers and buyers in a market, exchanging scientific products – even though the “analogy with the marketplace is imperfect” (31). Using various assumptions about scientists' behaviour they build models that show that there is no incompatibility between credit-motivation and truth-acquisition, and that under certain conditions, credit-motivation fosters truth-acquisition (even though it does not serve truth-acquisition quite as well as truth-motivation). The authors are explicit about their results running counter to the presumption attributed to the SSK that individual pursuits of

reputation and credibility undermine hopes of attaining truths about the world.

Kitcher's account is more complex. In a series of formal models in Chapter 8 of *The Advancement of Science*, he explores various social arrangements under which credit-seeking, authority, and competition have varying epistemic consequences. Inspired by evolutionary theory, he stresses the importance of cognitive diversity for epistemic progress, and examines the conditions of such diversity in a community of scientists. The picture that emerges is that an epistemically successful community must have a substantial proportion of "sullied" agents as its members (those who are driven by considerations of credibility and priority). A community with only epistemically pure (knowledge-motivated) agents or with only epistemically sullied agents will not succeed in ensuring sufficient degrees of cognitive diversity that is required for progress in capturing significant truths about the world. The general conclusion in Kitcher's words:

The very factors that are frequently thought of as interfering with the (epistemically well-designed) pursuit of science – the thirst for fame and fortune, for example – might actually play a constructive role in our community epistemic projects, enabling us, as a group, to do far better than we would have done had we behaved as independent epistemically pure individuals. (Kitcher 1993, p. 351)

Such conclusions, and the models that suggest them, tend to bolster the perception of economics having been called for help to restore the image of science as an epistemically successful objective and rational endeavour. But the contents of economics is in no way pre-programmed to yield only such pro-science services in the science wars. In addition to accounts in terms of a generous invisible-hand, economics also offers a store of invisible-backhand accounts. In both, the outcomes are construed as unintended consequences of self-seeking individual behaviour. In one set of arguments, those outcomes are desirable, in the other they are undesirable. With small adjustments in the assumptions of models, the conclusions will be of one kind or of the other. Examples could be multiplied, but here I will

briefly mention just a couple of examples of recent theorising that combine individual rational self-seeking behaviour and collective outcomes that are suboptimal. In both cases, the epistemological preference for diversity is violated.

Herd behaviour involving "informational cascades" conflicts with cognitive diversity (see e.g. Bannerjee 1992; Bikhchandani, Hirshleifer, and Welch 1992). Economic models of herd behaviour assume that individuals are rational but imperfectly informed. Individuals infer to the missing information by making observations about other individuals' behaviour, supposing others have relevant private information that they themselves don't. By assuming that individuals make sequential decisions and base their decisions on the observed behaviour of previous individuals in the sequence, the models show that this results in a convergence on one single line of belief and action. Conformity and imitation occur without any group pressure, coercion, or sanctions. Mechanisms of positive feedback are causally sufficient for the phenomenon to arise: my joining the herd encourages you to join encourages her to join ... The result is that anybody's behaviour becomes decreasingly valuable to others as a source of reliable information. The likelihood that nobody chooses the correct option will become non-negligible. A famous quotation tells the same: "Let them alone: they be blind leaders of the blind. And if the blind lead the blind, both shall fall into the ditch." [Matthew 15: 14]

No doubt similar mechanisms are in operation also in scientific interactions. Imperfectly informed scientists imitate other scientists driven by the belief that those others are in possession of information they don't have. In such situations, there is no cognitive diversity and no epistemically generous invisible hand in operation to support the collective discovery of truth. This has immediate implications for the role of epistemic trust in social epistemology. Knowledge acquisition of the kind we recognise in our scientific and other lives would be impossible without epistemic trust, but trust comes with a risk. The trustworthiness of any cognitive agent is not easy to judge, also because complete sincerity can go along with complete error as the notion of informational cascades indicates.

Models of network externalities and path dependency add more social contents to these considerations. Again, individual agents in interaction are assumed to rationally seek their self-interest. Accidental and perhaps very small historical events may put in motion a process that takes a direction that is dependent on the historical path itself, rather than on any equilibrating negative feedback mechanism set to generate a collectively optimal outcome. Network externalities provide the mediating positive feedback mechanism that generates a cumulative process: similar choices by a growing number of individuals create mutually beneficial networks between them, and individuals make those similar choices in order to be able to join those networks. The bad news is that those outcomes may be collectively suboptimal. Even worse, there may be a lock-in such that by individually rational behaviour alone, there is no way to get away from those suboptimal equilibria. The major examples of path-dependent processes mediated by network externalities and resulting in suboptimal outcomes come from technology (such as the QWERTY keyboard, VCR video system, and PC computers; see David 1985; Arthur 1989).

Again, it is obvious that similar mechanisms may be in operation in science. An accidental historical event and a suitable self-fortifying mechanism may give direction to research for long periods of time. One after another, individual researchers join the crowd, motivated by the forthcoming network externalities: sharing a research agenda and standards of judgement, citing others so as to get cited oneself, having an audience and being recognised as a serious scientist, having access to publication outlets and funding, and so forth. Once a network start forming in a cumulative fashion, it is not rational for self-seeking individuals not to join it. But in analogy with technological trajectories, path-dependent processes in science may result in epistemically suboptimal outcomes. For long periods, science may be stuck to wrong tracks. Naturally, modelling such a scenario requires a set of definite assumptions that can be challenged and changed.

The upshot is that economics is sufficiently heterogeneous and flexible to offer resources for

deriving almost any conclusions about the epistemic successfulness of science. Economic epistemology seems intrinsically neither pro- or anti-science. Hopes and horrors qualified.

3. What economic models do: From actual to possible science

The question arises as to the nature and functions of economic models, whether in their traditional domains or transmuted into accounts about science. A moment's reflection on the function of models will reveal an obvious but under-emphasised feature of (much of) economic epistemology. An implication of this feature is worth noting: The move from social-constructivist accounts to economic accounts of science has not only been a matter of moving to different answers concerning the status of science, but this shift in answers may have required a shift in the questions asked. In considering this possibility, we shall also get a sharper view of the precise contents of those answers.

So what is this feature of economic models? What do models deliver? One service that many theoretical models in economics provide is to envisage possibilities. They help answer explanatory questions of the form: how could an actual phenomenon P have possibly come about? The model does not tell us how P actually did come about, but rather sketches a possible scenario in which P could in principle come about. The functionalist variation suggests that P possibly occurs due to its capacity to serve some useful function. If P is not actually in place (or if one is agnostic about whether it is), the model may help us identify mechanisms and other conditions that have the capacity to bring it about. Or, in the functionalist mode, if P were in place, it would serve a useful function. This is to say theoretical models in economics characteristically offer how-possibly or why-possibly explanations. They describe what happens or is the case in the model world rather than in the actual real world. Supposing this is also a feature of models in economic epistemology, and supposing that many social-constructivist accounts of science are concerned with science as it is actually practised, it follows that the economic accounts are not to be

understood as direct rivals to those constructivist accounts. Their services are limited. They answer different questions that many would find more modest. Hopes need to be adjusted accordingly.

The affinities between the styles of reasoning in theoretical economics and in analytic philosophy of science and knowledge are perhaps close enough to have made the adoption of economic models smooth and easy for philosophers. But the issue of justification is pressing. The models in economic epistemology are supposed to be science-based models about science. Just as when dealing with *models in science* ordinary philosophical issues arise concerning how those models relate to the real world, similar issues arise regarding *models in economic epistemology*. These are the standard issues of realism and antirealism, reference and truth, structure and functions of models, role of idealisations and simplifications, explanatory mode and power. The models of economic epistemology are philosophically puzzling: how do we get from the models to the world – from science as it is portrayed in models to science as it is practised in reality? Economic epistemology should put these higher order issues on the philosophical agenda.

Suppose a given model in economic epistemology says there is a possible social world in which science is so organised that relatively reliable truth acquisition or epistemic progress takes place. But is there a passage from the possible to the actual? Many uses of several popular arguments in contemporary philosophical debates about science would require this move, such as inference to the best explanation, pessimistic induction, actual scientific progress as truth approximation, and indeed many arguments that seek to demarcate instances of science from instances of non-science: they are mostly presented as being concerned with the actual mechanisms of actual science. In case philosophy of science is not supposed to make any claims about actual science, the move is not needed, but then its agenda is to be understood differently. It would only pass judgements of the form, "If science were organized so and so, it would be relatively reliable in the pursuit of knowledge". This sort of counterfactual epistemology could be cast so as to connect with the practical purposes

of institutional design: the task would be to design the academic institutions of science so as to maximise the likelihood of successful attainment of knowledge, and it would be up to social epistemology to pass recommendations about optimal design. That this opportunity may not be available yet is suggested by Kitcher who very perceptively wants to "caution against overinterpreting my results: although my analyses reveal neglected epistemic possibilities, they are far too idealized to enable us to be confident in reaching conclusions about practical strategies for (say) funding research. *Perhaps* that can come later." (1993, 305; original emphasis)

Economic epistemologists indicate awareness of all this. In Kitcher's Chapter 8, a new model is characteristically introduced by "Let us consider a community..." where this community is defined in terms of a number of explicit and implicit assumptions of the various properties of the members of the community and their relationships. To examine that community is to examine a model. And to examine a model is to examine a possible scenario. Kitcher is explicit about having sacrificed realism in pursuing precision in his models. "My toy scientists do not behave like real scientists, and my toy communities are not real communities." (1993, 305) Yet, that sacrifice comes in degrees and can be diminished. Just as often happens in economic modelling, Kitcher moves from simpler models that exclude important factors to more complex models that incorporate them, by relaxing some of the idealising assumptions in the former – and he explicitly says this is what he does (1993, 357ff). The series of models in Kitcher's Chapter 8 often aims at examining what happens when this or that idealising assumption is relaxed (such as moving from communities of homogeneously sullied or pure agents to mixed communities). Just as in economic modelling generally, only a few such steps are taken, and the modified models still describe abstract possibilities in idealised model worlds.

The wording used by Goldman and Shaked indicates similar awareness of the nature of their intellectual exercise: "In this paper we *explore the assumption* that scientists ... engage in activities primarily in the attempt to advance their professional interests. ... We do not claim that

professional success is the sole motive of scientists ... it is worth considering *what its ramifications would be* for the truth-getting propensities of science" (1991, 31; emphases added, except 'is'). They set out to demonstrate that there is "no necessary incompatibility between the goal of professional success and the promotion of truth acquisition" (32; emphasis added). Characteristic commentaries are attached to the exercise, such as saying that an otherwise inaccurate assumption (such as the assumption that earning credit is a matter of changing other agents' subjective probabilities) is adopted as "a tolerable first approximation" (34), and that for the sake of simplicity certain real factors (such as critical speech acts) are neglected (38). There are many more such assumptions and commentaries (see Sent 1997).

Based on examinations of the properties of models, one can make precise claims about what happens or is the case in this or that model, under these or those assumed conditions. Claims about what is the case in a model can then be taken to imply claims about what is possibly the case in the real world (provided the model meets some conditions of *de re* possibility). But no claims are implied by these exercises about what is actually the case in the real world. We should add that even the possibility claims are far from evident as suggested by the serious criticisms that have been levelled against Kitcher's and Goldman's models (Roorda 1997; Sent 1997).

How exactly do such economic models contribute to social epistemology, conceived as the study of the reliability of various social practices and mechanisms in the generation of knowledge? This is hard to tell as the authors of those models have not articulated any systematic accounts of these models. Many economic models can be viewed as theoretical laboratories in which (thought) experiments are conducted, and mechanisms are isolated. The claim is then sometimes made that those same mechanisms are in operation also outside the theoretical laboratory of the model (see e.g. Sugden 2002; Mäki 2005). Likewise, one might view the models of economic epistemology as describing economic mechanisms in science, accompanied by the claim that roughly similar mechanisms are in operation in real-world science; or else those

mechanisms are not yet there but can be designed and then implemented in real-world science. An attractive further possibility would be to suggest that the various models isolate different mechanisms that conjoin, and sometimes conflict, in generating actual outcomes of collective interactive behaviour of scientists, but none of them may dominate the actual composite process. But it is not clear that any of these possibilities is actually entertained in (at least some important parts of) economic epistemology. At any rate they are not systematically articulated and defended.

So what might economic epistemology manage to deliver – and where does it stand in contemporary debates over the epistemic and cultural status of science? It seems the strongest claim its models can be used to make in this context is this: individual actions motivated by non-epistemic goals and subjected to social pressures do not necessarily undermine the capacity of science to attain knowledge and to make epistemic progress; or, it is possible for science to exhibit the traditional epistemic virtues even though individual scientists are not epistemically virtuous. What the arguments in support of these claims might at most manage to accomplish is to block any rival arguments that straightforwardly infer from private vices to public vices, from epistemically non-pure individual aspirations to the failure of science to attain knowledge and make epistemic progress. No doubt this would be a remarkable contribution to the controversies over the status of science. But it is a limited contribution. No claim has been made or implied or supported regarding science as it is actually practised. These possibility claims of economic epistemology are compatible with claims to the effect that actual science is a politically and commercially corrupt epistemic failure. Hopes and horrors, both.

4. Issues of credibility and reflexivity: Epistemology (including economic epistemology) of economics

Whatever claims about science are made using economic models, none of them should convince those parties in the science wars who are inclined not to think highly of science in the first place.

Those claims about science are made in terms of (social) scientific theories and models. If science is found dubious, then by implication any science-based epistemology about science is also viewed as dubious (and an economics-based epistemology might be viewed as particularly dubious). The question is about the credibility and justification of the scientific resource that is used for socialising epistemology. If economics is used as such a resource, how are we to judge its credibility? This is a challenge that economic epistemology must meet (just as it has to be met when using sociology as a resource). The chronic debate over the credentials of economics as a scientific discipline must be performed also in relation to the economic accounts of science. Economic epistemology implies a call for the epistemology of economics. (Cf. Mirowski 1996; Hands 1997; Mäki 1999.)

Recent work in the philosophy of economics has argued that economics does not satisfy certain criteria of scientificity: its theories are unfalsifiable; its research programmes do not exhibit Lakatosian progress; and the discipline does not generate predictive progress. Others reject such criteria of demarcation, or reject the relevance of the issue of demarcation itself. Most commentators are concerned about how the very simple theoretical models based on radically false idealising assumptions should be assessed; some adopt an instrumentalist conception, while others argue for a realist interpretation of such models, subject to some further conditions. Views are divided as to what those models are able to deliver. Is there a sense in which they might be true? Do they conceivably explain, and if so, what and how? Do they predict? It is also not clear what exactly is being accomplished when a theoretical model is associated with an economic experiment or with an econometric model.

The key issue can be recast as an issue in social epistemology: Does the industrial organisation and the associated incentive structure of the discipline of economics support serious fact-finding research or mathematical games with fictional toy models? Is economic inquiry so organised that it functions as a reliable source of knowledge about important social mechanisms? This gives rise to reflexivity issues in the economic epistemology of economics.

Given the heterogeneity of economics, there are many possible ways to go in doing economics of economics. Zamora (2002b) applies his constitutional economics approach that examines the emergence of scientific norms and the way they shape the goals and behaviour of economists. It is well known that the norms governing economic inquiry are extremely severe, allowing for little variation or flexibility in the style of research performance. Zamora argues that the academic norms setting the incentives in economic research are such that accepting a model as a good one does not imply accepting the model as (even approximately) correct. Goodness of a model is often more a matter of its builder exhibiting a mastery of mathematical skills (370-371). The originator of constitutional political economy, Nobel Laureate James Buchanan makes a similar observation, without an explicit economic argument: "Persons who choose to become economists, at the century's end, are those who are attracted by the analytical properties of the models manipulated rather than by the success or failure of such models in offering improved understanding of economic reality" (1999, 9). It is notable that while Buchanan here makes a straightforward empirical claim, Zamora, having developed an explicit economic argument, avoids making such empirical claims about the actual situation in economics: "only the conceptual possibility of epistemic inefficiencies is indicated" (372).

My reflexivity argument in terms of transaction cost economics of economics can similarly be construed as examining a space of possibilities (Mäki 1999; see also 1993). Using different theoretical tools, the argument reaches conclusions similar to those of Zamora. The thrust of the argument is that economics itself implies that the most cost-efficient type of economics deals with mathematical fictions on the blackboard rather than complex real-world issues. This is a broad sense of cost-efficiency, in line with the spirit of social epistemology: transaction costs are genuinely social costs, they are the costs of an institutional arrangement. The general idea is that the most transaction-cost efficient arrangement will be selected for a given purpose. In the intellectual and academic context, transaction costs include the costs of search, communication,

measurement and monitoring of academic performance and intellectual quality. These costs are incurred as we look for a paper to read, a publication outlet for our own paper, someone to hire for an academic job; as we measure and monitor the performance of our peers, a journal, a university department; as we engage in the activities of assessment, criticism, utilisation, awarding, sanctioning, collaboration, debate, networking. These costs will be lower in case research is strictly rule governed, highly standardised and formal instead of informal, only loosely defined and obscured by real-world complexities. There is more complexity in the argument, based on various simplifying assumptions, but so are all typical economic arguments. The point is that fiction-oriented economics modelling possibilities is a more transaction cost efficient institutional regime than fact-oriented economics reaching out to the complex actualities. If broad cost-efficiency were the determining factor, then the best of economics should not be relied upon as a source of knowledge about the world. And social epistemology should not rely on the best of economics in case it were interested in acquiring truthful information about actual scientific practice.

Hopes and horrors are becoming increasingly informed and qualified. It should be noted, however, that thanks to the feature we have identified in section 3, theoretical economics is self-consoling. The above exercises in the economic epistemology of economics only envisage possibilities, and that is truly harmless compared to making well established empirical claims about economics. There is room for other models that portray economics as possibly epistemically successful.

5. Further reflexivity issues: Description as construction

The final sentences of *The Advancement of Science* proclaim: "The philosophers have ignored the social context of science. The point, however, is to change it." (Kitcher 1993, 391) In general, it is easy to agree. But there is a version of this idea that might be less agreeable.

Consider the recent discussion on whether economics students are more selfish and whether

this is due to their studies of economics. Frank, Gilovich, and Regan (1993) set out to investigate "whether exposure to the self-interest model commonly used in economics alters the extent to which people behave in self-interested ways" (159). They examined charitable donations and prisoners dilemma experiments, and reported the finding that economics students were less likely to co-operate and more likely to defect and free-ride than others. Among the possible explanations of this behaviour they mention that due to the exposure of students to economic models that assume self-interested behaviour on the part of agents, they are more likely to construe the situations in which they are put in self-interested terms; and they are more likely to expect other agents to defect, which fortifies their own inclination to so behave.

A recent Presidential Address to the Econometric Society suggests that a prominent game theorist may have been influenced by these findings: "I am a micro economics teacher. I am part of a big 'machine' which I suspect not only influence the world but even is brainwashing students to think in a way which I do not particularly like." (Rubinstein 2004, 16) He shares his doubts about whether economic "toy models" are able to inform us about the world but believes that nevertheless "models can have an enormous influence on the real world, not by providing advice or by predicting the future, but rather by influencing culture, that is, the collection of ideas and conventions which people believe in and which influence the way they reason and act" (22).

Similar reflexivity issues arise for economic epistemology. What if exposure to economic models of science were to make an impact on scientists' behaviour just as they may have influenced the behaviour of economics students? What if those models could distort the ideal proportions between the sullied and pure agents in Kitcher's model worlds by triggering excessive proportions of sullied behaviour in the actual world? What if the design and imposition of market-like competitive structures on science will not only channel some pre-given behavioural dispositions of scientists, but will also shape those dispositions to the detriment of science? Horror for all.

It is generally believed that rational self-interested choice explains better in situations where the agents are acting under the pressure of competitive markets compared to situations in which agents are not subjected to such pressures. For example, rational choice models better explain the behaviour of business firms than that of households. This is because the selection mechanisms of competitive markets force firms to maximise for survival, while this pressure is mostly absent in the case of households. This might be taken to suggest that in the scientific context, perhaps not all agents are *intrinsically* either sullied or pure, but rather potentially either way (perhaps with stronger and weaker potentials). It is the circumstances that fortify and trigger and actualise those potentials. Indeed, what if the relevant distinction is between sullied and non-sullied behavioural disposition (rather than pre-given character or ultimate preference), and what if publicly describing science in market-like terms and imposing on it market-like structures will fortify and trigger increasing proportions of sullied dispositions? This is a different image from that of Kitcher: in his account, individual motives are given and only channelled by social institutions. On the image suggested here, some such motives may be weak and dormant but reinforced and awakened by social institutions and situations. My speculative provocation suggests that while models like those of Kitcher may indeed have the potential for changing the social context of

science, they may also have self-destructive potential. This is because they may have science-destructive potential. Horror throughout.

This calls for a final consolation. If describing science in economic terms were to make a difference for what is being described, wouldn't the same hold for describing it in, say, sociological terms? And how do we tell which construction-by-description is more science-friendly? After all, economic and sociological accounts share much of the relevant vocabulary, terms such as credit, reputation, recognition, priority, authority. And one may want to indicate that economics is more than avaricious egotism in competitive markets (while lamenting that this perspective may still dominate the disciplinary culture). The range of theoretical resources available in economics is increasing in richness as it is making serious attempts to model situations that contain co-operation, norms, values, trust, reciprocity, fairness, emotions and moral motivation. Economics is a controversial discipline, but it is also a heterogeneous and evolving resource for social epistemology.

In order to exploit its resources in a responsible and more deeply science-friendly manner, economics has to be philosophically monitored. Reciprocity suggests, and is supported by the speculations (of possibility) in this essay, that philosophy itself must be scientifically (and philosophically) monitored.

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