



NEW PHILOSOPHY OF THE SOCIAL SCIENCES
CONTRIBUTED PAPERS (ABSTRACTS)

Deliberation and Commitment

Sabine A. Döring | The University of Manchester | mail@sabinedoering.de

With the consequentialist's view, the emotions are a nonrational source of practical reason by constituting goals for action. What if, then, they interfere with the reasoned pursuit of these goals? Can emotional choices be rational? This is the claim of Robert H. Frank, whose evolutionary economic theory has recently gained some popularity among philosophers. Frank's so-called commitment model is my concern in this paper. I explain its formal version which is meant to demonstrate that 'honest' individuals can prosper in the material world. It is those individuals who, according to Frank, regularly make rational emotional choices so as to solve 'commitment problems'. This is claimed to be due to their specific genetic predisposition. I offer two interrelated arguments against the commitment model. First, I argue that Frank does not succeed in bridging the gap between deliberation and evolution. His model rather is exemplary of a research strategy which does not address the question of how the two processes may be combined coherently. Frank applies what, in inversion of Elliott Sober's 'heuristic of personification', I am calling the 'heuristic of biologisation'. He draws upon the isomorphism between deliberation and evolution consisting in that both are optimising processes. But the respective optimality criteria can come apart. I point out that this is so in the one-shot Prisoner's Dilemma. Frank's prime example of a commitment problem provides a counter-example to the method he applies, and his classification of choices is decisionist and inconsistent. This in turn undermines his account of practical rationality and morality. Opposing Frank's ambitious normative claims, I show, secondly, that the commitment model does not rationalise emotional choices, leave alone establish their morality. Instead, Frank's adaptive strategy leads to a paradox, which emerges precisely because the gap between deliberation and evolution remains unbridged.

Agent-based simulation, generative science, and its explanatory claims

Till Grüne-Yanoff | Royal Institute of Technology, Stockholm | till.grune@infra.kth.se

Social scientists have claimed that by ‘generating’ a social phenomenon, agent-based computational simulations explain it – according to the motto ‘If you didn’t grow it, you didn’t explain its emergence’. Starting from this claim, this paper discusses the explanatory potential of ‘generation’ – i.e. the reproduction of a social event or property with the tools of computational simulation, based on symbolically represented autonomous, interacting agents. It concludes that generation is neither necessary nor sufficient for causal explanation, but necessary and under certain circumstances sufficient for constitutional explanation.

Simulation allows imitating one process by another process.² Simulations are also often associated with empirical techniques, like ‘laboratories’, ‘culture dish approaches’, or ‘experiments’. If taken literally, this association presupposes that simulation can produce empirical data about real phenomena. However, in contrast to an empirical experiment *involving* the real process, a simulation model represents a process that is *supposed* to imitate the target process. The association presupposes, therefore, that the model has the adequate structure to imitate the real process of interest. This is the question of *structural validity* of the model.

Structural validity of a model is not about accuracy *per se*, but about validity *for* a purpose. Explanations that pursue different purposes need to be distinguished, as validity conditions they impose on models may differ. *Causal explanation* explains an event by citing its predominant cause. *Constitutional explanation* explains a system’s property by showing how the system’s elements have properties that instantiate it.

Take the putative causal explanation of a pre-historical settlement’s dynamics with the agentbased Artificial Anasazi model. This model simulates the life, procreation and death of households based on potential harvests in Long House Valley, Arizona. Potential harvest per hectare was theoretically reconstructed from paleo-environmental data from 382 to 1400 A.D. Household agents attributes were ‘derived from ethnographic and biological anthropological studies of historic Pueblo groups and other subsistence agriculturalists throughout the world’. Agents’ behavioral rules were modeled as optimization behavior under very limited information. The simulation ‘generated’ a population dynamic that qualitatively mimicked the archeological record, but differed quantitatively.

There are good reasons to not deem this a causal explanation. First, the study does not take into account alternative causes besides harvest variations. Second, it does not investigate similar settlements in the area that started from different initial conditions in order to control for alternative causes. Finally, all it is willing to concede when it fails to generate the demise of the settlement around 1300 AD is that the studied cause may not be a *full* explanation, and that institutional causes are needed to ‘fill in’.

Instead, the study insists that it identified the major contributing causes of the settlement dynamic. This puts considerable pressure on the validity of the model. To be explanatory in this way, the model has both to capture the real initial conditions and the genuine process. The evidential resources for such a validation are, however, limited. Direct observation is not feasible. Transfer of direct observation from still existing small-scale societies requires a similarity comparison, which would beg the question. Experimental observation can

support some of the individual behavioral rules, but it is difficult to replicate adaptive and interactive effects in experiments. Derivation from theories in the social sciences is problematic, either because theories are not well-confirmed or because *ceteris paribus* conditions are too narrow to allow valid deduction. Nor will abduction be a strong argument for any particular configuration, given its many degrees of freedom. In the light of these deficiencies, generation seems insufficient for causal explanation.

Many generativists, of course, acknowledge this insufficiency. They suggest instead that generation is an important step in causal explanation, which ‘enriches our understanding of fundamental processes that may appear in a variety of applications’. But to what extent does generation actually add to our understanding? Take for example the explanation that the drop in crime in the US in the 1990s was caused by the legalization of abortion with *Roe v. Wade*. The authors only sketch a causal mechanism between performed abortions and crime rate, citing no more than *on average* relationships. They then identify a strong statistical correlation between the legalization of abortion in different states and the drop in crime rate per capita, controlling for a variety of factors that influence crime. What would we add to this explanation, if we generated the crime drop in an agent-based model? Obviously, there are many possible ways of generating such a phenomenon. A simulation that would have generated it in only *one* way would have given a biased and misinforming account. A simulation generating it in every possible way, however, would provide a great deal of information irrelevant to the *explanandum*. Hence, and contrary to the generativist motto, generation is not necessary for causal explanation.

These conclusions, however, do not imply that simulation does not have any explanatory function. Social scientists often ask what it is for a system to have a certain wealth distribution, segregation pattern, price equilibrium, norm, etc. In these kinds of *constitutional explanations*, simulation plays a crucial role.

The concept of generation is neutral with respect to these two concepts of explanation. It may denote a simulation of a causal process, or otherwise a simulation of a family of processes where a system property is instantiated by the capacities of its elements. The surprising feature is that processes are involved at all here. In standard examples of constitutive explanations, no process is involved. To explain why a certain gas has temperature T , e.g., requires only citing the average kinetic energy of its molecules. Similarly, to explain why an economy is in price equilibrium— in standard General Equilibrium theory — only requires pointing out the deliberation and cognition capacities of the economy’s members. Agentbased simulations pursue the same explanatory goal, but investigate ‘the extent to which learning, institutions, and evolutionary forces might substitute for the high degree of individual rationality assumed in standard economic theories’, and to show ‘how little [individual rationality] is enough to generate the macro equilibrium’. The focus here still is on the instantiation of the macro property through the agents’ capacities – even though it is now clear that the agents capacities are so limited that they will realise the macro property only through some process of repeated calculation, intuition, selection or other. The explanation, however, does not ask for that process. It is enough to show that the macro property is instantiated by the capacities of the agents in a family of processes. Understood thus as instantiation, generation is obviously necessary for constitutive explanation.

Finally, with the processes only playing a subsidiary role, much less pressure is put on the validation of a model for constitutive than for causal explanation. Indeed, all that is required for the model's validation is that the agents' property attributions are justified. This, even though no small task, can in principle be satisfied through extended direct observation and through experiment. Under the right validation conditions, generation is therefore also sufficient for constitutive explanations.

The Evolution of Norms in Economics: taking the Linguistic Turn seriously

Tilman Hertz | tilman.hertz@univ.u-3mrs.fr

Norms, especially ethical ones, occupy a special place in economics. They have to be taken into account because they can influence the economic decision making process in ways which go against the predictions of economic theory. In order to get a better understanding of economic agents decision process, a good understanding of norms is strongly desirable. This paper proposes a model for the emergence and evolution of ethical norms. For this it uses evolutionary game theory. The particularity of the model is that it takes account of the Linguistic Turn, initiated by Wittgenstein [1953] and Heidegger [1927]. The Linguistic Turn is a movement which forcefully puts into question the characteristics of the economic agent as depicted by traditional orthodox economics. What it does is to inverse the tradition: most «intentional acts» are not brought about by mental deliberation but by unconscious rule following. Integrating this picture of the economic agent in evolutionary game theory means the following: for strategy choice agents display bounded (in terms of «rule») rationality, strong behavioural inertia and there is a heavy reliance upon past experience rather than rational expectations as a determinant for strategy choice. According to the model, social and ethical norms arise then out of the repeated game theoretic interaction of self interested agents, notably when they (due to the above characteristics) generate an account of common knowledge which enables them to settle on the cooperation-cooperation outcome in a Prisoners Dilemma. The epistemic environment in which agents find themselves allows then for such norms to evolve slowly through the population (via offspring) and to finally be internalized. Once internalized, agents prove to be behaviourally *determined* to «play» such norms: they will be followed unconsciously. The model has the advantage of being able to explain some puzzling things highlighted by experimental economics like the ones in one shot ultimatum games highlighted for example by Güth, Schmittberger and Schwarze [1982], Thaler [1988] or Marwell and Ames [1981] without questioning the fundamental assumption of economics, notably that an agent is motivated by self-interest. In those games, player one has to divide an amount of money between him and player two. Player two can accept or refuse (in which case the deal does not take place). The optimal thing to do would be for player 1 to propose a division of 99/1 and for player two to accept it. However, player 1 often proposes 50/50 which is not, at first sight understandable from a self interested point of view. According to our model however, agents are behaviourally determined by a “fairness” norm; norm which has its origin in a repeated game theoretic interaction of self-interested agents and which evolved through the population to become a rule which, once internalized, is applied unconsciously.

Kahneman and Tversky and the origin of behavioral economics

Floris Heukelom | University of Amsterdam, History and Methodology of Economics Group + Max Planck Institute for Human Development Berlin, ABC Group | F.Heukelom@uva.nl

Kahneman and Tversky and their behavioral economics stand in a long tradition of applying mathematics to human behavior. In the seventeenth century, attempts to describe rational behavior in mathematical terms run into problems with the formulation of the St. Petersburg paradox. Bernoulli's celebrated solution to use utility instead of money marks the beginning of expected utility theory (EUT). Bernoulli's work is taken up by psychophysics which in turn plays an important role in the making of modern economics. In the 1940s von Neumann and Morgenstern throw away Bernoulli and psychophysics and redefine utility in monetary terms. Relying on this utility definition and on von Neumann and Morgenstern's axiomatic constraints of the individual preference relation, Friedman and Savage attempt to continue Bernoulli's research. When this fails economics and psychology go separate ways. Economics employs Friedman's positive-normative distinction; psychology uses Savage's normative descriptive distinction. Using psychophysics Kahneman and Tversky broaden the normativedescriptive distinction and argue with increasing strength for a descriptive theory of rational behavior. Behavioral economics is founded upon the export of Tversky and Kahneman's program to economics. However, two different branches of research can be observed. The psychological branch in behavioral economics continues Kahneman and Tversky's search for a descriptive theory of rational behavior and extends the normative-descriptive distinction with a prescriptive part. The economic branch takes Tversky and Kahneman's work as a falsification of the positive theory. It argues that economics should take account of the psychological critique but stick to rigorous mathematics and Friedman's positive-normative distinction.

Saving Realism for Economics

Frank Hindriks | University of Groningen | F.A.Hindriks@rug.nl

Many economic theories contain assumptions that are generally regarded as false. Because of this, economists often take those theories themselves to be false, and frequently go on – attached as they are to their theories – to conclude that the idea that one should pursue true theories in economics is misplaced. This provides a challenge for realist interpretations of economics, as a concern for truth is one of the prime characteristics of realism. In the face of this challenge, two strategies for saving realism for economics have been proposed. These are what I will call 'the truth-of-paraphrase strategy' and the 'significant-truth strategy'. The former was proposed by Musgrave (1981), and has been developed further by Mäki (2000) and Hindriks (2005). The latter figures prominently in Mäki's work (including his 1992). Both strategies allow for non-negligible falsehoods, which compromises the realist ideal of true theories. I will argue that these strategies can be replaced by two other strategies that come closer to the realist ideal in that they do not require us to make such a compromise. These are what I will call 'the ultimate-truth strategy' and 'the counterfactual truth strategy'. I illustrate these strategies using examples both from physics and economics, which support the idea that (at least) two strategies for saving realism for economics are needed. It is argued that these strategies provide for a

defence of a kind of realism that is as strong as economics as it is actually practiced can support. The two strategies proposed in this paper help us to see that there is more truth in economics than meets the eye.¹

Musgrave (1981) recognized that a (realist) concern for truth is compatible with accepting theories that contain false assumptions. The way to appreciate this, he argued, is to consider the roles such assumptions play or the functions they have in (the development of) economic theories. Consider an assumption *A* according to which some factor *F*, for instance an imbalance in the government's budget is absent, or Newton's assumption that there is only one planet that orbits the sun. Such an assumption may be imposed because factor *F* has negligible effects on the phenomenon under investigation. Alternatively, assumption *A* may be imposed because it plays a heuristic role in the development of a theory that yields more precise predictions than the current theory that contains *A*. Musgrave's contention, which was developed further by Mäki (2000), was that when paraphrased in terms of considerations such as negligibility and heuristic value in the ways just presented, assumptions better be true. This is the truth-of-paraphrase strategy.

As it stands, this strategy for defending false assumptions faces the following problem. The paraphrases are meta-statements that are not part of the theory. This raises the question how their truth is supposed to justify the falsity of the assumptions of the theory. The first thing to note is that the realist does not need to insist on the whole truth and nothing but the truth. Instead, she can settle for theories that are descriptively incomplete and only approximately true. If a factor *F* has negligible effects relative to this goal, the falsity of the assumption that *F* is absent is not problematic for the realist. With respect to false assumptions that are imposed for their heuristic value, matters are more complex. In Hindriks (2006) I have argued that the immediate reason for imposing such assumptions is usually a concern for tractability. Doing so may prove to be conducive to the development of theories that do not anymore rely on such tractability assumptions. In other words, such assumptions may turn out to have heuristic value. As a consequence, the false assumptions are part of a theory only temporarily. This amendment of the truth-of-paraphrase strategy is what I call 'the ultimate-truth strategy', as in this context the economist is concerned with developing theories the ultimate versions of which are true, or at least approximately so. It is illustrated in terms of the measurement methods of the mark-up ratio of price over marginal cost that Hall (1986) and Roeger (1995) have developed starting from Solow's method for measuring productivity growth.

The problem from which the ultimate-truth strategy suffers when considered as a general strategy for saving realism for economics is that it only works for assumptions that are taken to be or at least hoped to be temporary. In fact, it fails with respect to one of the examples Musgrave uses, Galileo's vacuum assumption. Whereas it is plausible to say that Newton hoped that at some point astronomers could do without the one-planet assumption,

¹ Two other strategies for defending realism should be mentioned here. First, Hacking (1983), Cartwright (1983), and Giere (1988) have developed entity realism. The arguments presented below suggest that this kind of realism is not as ambitious as a realism concerning economics could be. Second, the members of the Poznan school, in particular Nowak (1989), subscribe to a form of realism that is more ambitious than the strategies proposed in this paper. Their insistence on concretization as a prerequisite for truth ascription makes realism for economics impossible and does not fit with the role that isolations and abstractions play in scientific practice.

such a claim is problematic when made about Galileo. He thought, and – in effect – we still think that he got to the heart of the mechanism underlying the process of falling bodies. Mäki (2004, 25) suggests that idealizing assumptions such as Galileo’s vacuum assumption are made ‘to create imagined conditions in which the influence of a number of potentially relevant factors is removed or neutralized in order to examine the impact of another selected set of factors’. He goes on to argue that conceived of as ‘a claim about the way in which the force of gravity influences free falling bodies, regardless of whether the conditions in which they fall are ideal ones’ might be true (ibid.). And this may hold also for assumptions that play a similar role in economic models ‘even though the idealizing assumptions that effect the isolation are false’ (ibid.). What we gain is a significant truth about free falling bodies. And we should focus on that truth, even though its appreciation depends on a theory that contains a false assumption. This is the significant-truth strategy.

Just as the previous one, this strategy for defending false assumptions admits too much to the opponent of the realist. A stronger version of realism can be defended by conceiving of the role of the assumptions under discussion in a slightly different way. The point to appreciate is that claims such as ‘There is no air-resistance’, which are usually false, can figure in conditionals that, when evaluated as a whole, may well turn out to be always true. In the case of our guiding example, the claim would be this: If there were no air-resistance, the rate at which a body would fall would equal $\frac{1}{2}gt^2$. So, instead of as falsehoods, the relevant idealizations should be regarded as the antecedents of true counterfactuals (cf. Niiniluoto 1989). For this reason, I call this alternative strategy ‘the counterfactual-truth strategy’. This perspective on Galileo’s vacuum assumption does more justice to his theory as well as to subsequent developments than both the perspectives of Musgrave and Mäki. It enables us to explain why physicists after Galileo put so much effort into determining the value of the constant in Galileo’s equation, and why in doing so they proceeded on the assumption that air-resistance often has a non-negligible effect (cf. Schliesser 2005). Because of this, it is more natural to say that the theory expresses a truth, albeit an incomplete one, rather than a significant truth at the expense of a false claim. The impression that the theory involves falsities is due to this incompleteness. This counterfactual-truth strategy is illustrated in terms of the Modigliani-Miller theorem about financing firms.

Both the truth-of-paraphrase strategy and the significant-truth strategy grant too large a role to falsehoods in economic theories. The ultimate-truth and the counterfactual-truth strategies fare better in this regard. Apart from doing more justice to the realist’s concern for truth, these strategies fit better with scientific practice.

Complementarity and explanatory pluralism

Caterina Marchionni | Erasmus Institute for the Philosophy and Economics | marchionni@fwb.eur.nl

In recent years the discussion over the legitimacy of different forms of explanation in the social sciences has flourished, and a variety of forms of pluralism in matters of explanation have been proposed. Often key to arguments in favor of explanatory pluralism is the idea that different forms of explanation are *complementary* rather than competing, and thus that were we to dispense with one or another, valuable explanatory information would be lost.

What precisely it means for two (or more) forms of explanation to be complementary however is rarely spelled out.

Advocating explanatory pluralism on the basis of an unspecified idea of complementarity might lead to conflate complementarity with mere compatibility. Although there may be reasons to favor a plurality of compatible (forms of) explanations (as, in some cases, there also are for holding pluralism about incompatible explanations), the conditions under which this plurality is held to be desirable need not be, and in most cases will not be, the same as those that make a plurality of complementary explanations desirable.

My aim in this paper is to contribute to recent discussions on explanatory pluralism in the social sciences by proposing a relatively simple and attractive thesis of what it means for (forms of) explanations to be complementary (for a similar move cf. de Regt 2006).

[C] Complementary (forms of) explanations of a phenomenon or event e provide information about mutually exclusive portions of the ideal explanatory text for e .

The notion of ideal explanatory text is Peter Railton's (1981), and in this context it is to be understood as providing the true complete explanation of all facts about e .

Thesis [C] allows us to qualify as complementary only explanations that are compatible since portions of the ideal explanatory text, and hence correct information about them, cannot by definition be incompatible. Second, complementary explanations will only be explanations that provide information about (different aspects of) the same phenomenon or event e . It is at the very least uninteresting to attribute complementarity to an explanation of why my pen fell and one of why dinosaurs got extinct, even though the two explanations might be compatible. Third, [C] permits to rule out cases in which an explanation is either parasitic on another or is contained by it. Although on [C] these cases do not qualify as complementary, this does not imply that the explanations in question are rival or competing. This leaves room for forms of pluralism about compatible explanations, even in those cases in which pluralism about incompatible explanations is deemed unacceptable.

In order to make [C] applicable to actual (forms of) explanations, what are mutually exclusive portions of the ideal explanatory text for e needs to be made specific, and this largely depends on what (forms of) explanations we are considering. Rather than attempting to develop my own specification of it, I consult proposals advocating explanatory pluralism about social-level and individual-level explanations in social science, which, more or less explicitly, claim those forms of explanations to be complementary.

I consider a selection of proposals—ranging from those exclusively based on metaphysical arguments (e.g. Jackson and Pettit 1992) to those that stress the variety of pragmatic interests (e.g. van Bouwel and Weber 2002)—and see whether, and how, a suitable specification of what would count as mutually exclusive portions of the ideal explanatory text can be derived from them so as to flesh [C] out. I show that metaphysically-grounded proposals aimed at proving the irreducibility of the social to the individual, are more likely to be successful in establishing genuine complementarity between social-level and individual level explanations. This does not mean however that other approaches fail to promote pluralism about social-level and individual-level explanations; instead in most cases, these approaches should be reinterpreted (and assessed) as offering reasons for favoring a plurality of compatible (forms of) explanations.

How to tear down some economic walls made of ontological bricks. A response to Dasgupta

Armando Menéndez Viso | University of Exeter | A.Menendez-Viso@exeter.ac.uk

In a recent paper², Partha Dasgupta defended economists against the accusation of ignoring ethics in their work, by asserting that “modern economics is built on broad ethical foundations”, which were “constructed over five decades ago” and now are “regarded to be a settled matter”. He claimed that disputes within economics are never about values, but about facts. As it is well known, there are a number of authors (Nussbaum and Sen among them) who try to argue exactly the opposite. The first aim of this paper will be to show that such a debate is simply futile, since there is no true opposition between facts and values. Its second goal will be to draft some of the remarkable consequences for theorists and policy makers derived from discarding that opposition.

Economics, like all scientific disciplines, constitutes a reductive enterprise, since it tries to reduce (both subjugating and cutting down) a certain sort of phenomena to simpler elements –these being explanatory principles, axioms, or models of any kind. Throughout its history, economics has taken interests, utility, or preferences as its primary components, and its conclusions have varied depending on the nature of these elementary particles. Thus, Mandevillian assumptions led to conclusions, which differed significantly from those stemmed from, say, marginalist presuppositions. However, if anything, economics has always been the science of assigning values to human products (be these goods, commodities, services, activities, or whatever); i.e., the art of commensuration, of translating to a numeraire, of ordering. How could it be free, or above, or aside of values? Even the latest environmental or development economic theories (like those proposed by, among others, Daly, Costanza, Brown, the Ehrlichs, or Dasgupta himself) try to value things like Nature, inclusive wealth or social worth, which means dealing with values. But dealing with values does not imply leaving facts aside.

That facts are not independent of values (and vice-versa) has already been said too many times; what this paper will defend (sharing some of the arguments given by Putnam, but reaching different conclusions) is that facts and values are not opposites in any sense. Moreover, the use of the term ‘values’ leads to the construction of artificial barriers in the field of economics, the most limiting of which is the pretended gap between what is and what should be done, between the world and its transformation. Declaring that there are things known as values, separated from other kind of things called facts, condemns us either to renounce to search for political guidance on reality, or to incur constantly into the naturalistic fallacy. It will show that insisting on that distinction appears to be pretty absurd, given the historical, economic origin of the contemporary concept of values.

The dichotomy between facts and values is as misleading as the opposition invoked by Dasgupta between constituents and determinants. Constituents are too close to essences, and essences fill the economic realm with closed entities. There are alternative ontological materials to build up economic knowledge on a more open field. Doxa/episteme,

² Dasgupta, P. (2005), Why Do Economists Analyse and Why: Facts or Values?, *Economics and Philosophy*, 21, 221-278.

objective/subjective, speculative/empirical and other similar pairs will be presented as more suitable than what is usually intended by the false dichotomy between facts and values.

Some controversial consequences can be derived from the denial of the opposition between facts and values:

a) Value conflicts can (and should) be resolved by appealing to facts, and facts continue to be established through evaluation. Discussions about facts are of the same kind as those about values.

b) There is room for public participation in economics. Paraphrasing von Mises, it could be said that scientific debates in economics do not differ from lay discussions in their subject or tools, but only in their skilfulness and caution.

c) Saying that we will agree on what to do if only we were able to know all facts, is nothing but naïve Socratism. Theoretical conflicts do not arise from values or facts, but from the way both are connected. These conflicts can only be resolved through practice.

d) Economics, when practised as political arithmetic, is a pure experimental discipline – almost nothing but a guide for socio-economic experimentation. That is the reason why Dasgupta can affirm that economic discussions are always about facts: the disputes invoked are mostly debates about the plausible result of the economic experiment, which is the application of a given policy measure.

e) Once subjects are omitted, aggregation becomes a false problem. Aggregated preferences are informative (and performative) only for hypothetical aggregated agents. Like the mean person, aggregated results have no reality, except by chance. And, if they had it, they could hardly entail any recommendations for policy makers.

f) Economics, like other social disciplines, is a moral enterprise and a scientific venture at the same time.

Mediating Between Causes and Probability: the Use of Graphical Models in Econometrics

Alessio Moneta | Max Planck Institute of Economics, Evolutionary Economics Group, Jena (Germany) | moneta@econ.mpg.de.

This paper examines the relationship between causes and probability in the particular context of econometrics and discusses a methodological framework to address the problem of causal inference in econometrics. There are two conceptions of econometrics that have marked its history. The first one considers causes to be something that economic theory must provide and that statistical methods must measure. The second conception considers economic theory to be a not very reliable source of causal knowledge and opens the possibility of inferring causes from statistical properties of the data alone. The first conception was advocated by some exponents of the Cowles Commission during 1950s and is still largely accepted by econometricians who use structural models or calibration methods. The second conception was formalized by Granger's (1969) tests of causality, which are still very popular in nowadays econometrics.

I will argue that these conceptions can be interpreted as two opposite solutions to the problem of under-determination of theoretical causal relations by statistical data (usually named by econometricians the problem of identification). While the risk of the first approach is the commitment to an apriorist strategy, the second approach is hampered by difficulties which are typical of the probabilistic theories of causality, as many studies in the philosophy of science have shown. Econometrics offers a particular and clear example of the general problem of causal inference. I will argue that the general problem of causal inference can be solved only by delicately mediating between background knowledge and statistical properties of the data. The method for this careful handling is in large measure dependent upon the discipline considered.

With respect to macro-econometrics, graphical models, that is the methods for causal inference developed by Pearl (2000) and Spirtes, Glymour and Scheines (2000), can be very useful for this important task of mediating between probabilistic and causal knowledge. Indeed, graphical models permit to take into account the maximum amount of probabilistic information (partial correlations of all possible orders), which can be used to exclude false causal relations. Partial correlations, however, are never sufficient to isolate the unique true causal relations, except in very exceptional circumstances. Indeed, background knowledge has to be incorporated and this approach permits a very efficient use of background causal knowledge. This will be demonstrated by presenting some examples taken from time series models.

Fellow-feeling and Fellow-reasoning: Is Rational Choice Theory Incomplete?

Michiru Nagatsu | The University of Exeter | m.nagatsu@ex.ac.uk

In a recent paper (2002), Robert Sugden points out the conceptual inability of rational choice theory (RCT hereafter) to accommodate an important positive (i.e. causally relevant) relation between people's affective mental states which he calls *fellow-feeling*. My paper argues that a specific kind of fellow-feeling, which I call *fellow-reasoning*, is constitutive of the conceptual framework of RCT. The argument hinges on the fact that some kind of *rationality assumptions* is indispensable in RCT, and that these assumptions presuppose the possibility of the theorist's entering into or going along with actors' reasoning. In other words, preferences are not merely "whatever [an individual] takes to be choice-relevant reasons, all things considered, or as the psychological dispositions that prompt her to make whatever choices she makes," (Sugden 2002, p.66), but rather something which *requires* us (both the actor and the theorist) to choose one action rather than another.

The paper also aims to show that the widespread hermeneutic criticism against RCT is misplaced. According to this criticism, RCT is incomplete as an explanation of human actions in that it does not enable us to understand empathically the 'real' mental states of others, which are allegedly different from those ascribed tentatively by RCT for the predictive purposes. Although some applications of RCT unduly underestimate verbal evidence (e.g. interviews, questionnaires, documents, etc.), however, this does not mean that RCT is committed to instrumentalism, being only concerned about the theory's predictive success. On the contrary, RCT presupposes the very possibility of our knowing actors' perceptions whenever it empirically specifies the choice situations actors are facing: after all, preferences must be about *something*, and that something is only specifiable by

referring to actors' perceptions. Moreover, RCT, by way of assuming rationality in actors' reasoning (implicitly or explicitly), enables us to talk about actors' reasoning by analogy with our own reasoning. Thus there is no incompleteness in RCT: it enables us to *predict, explain and understand* people's choices. For a fruitful discussion, this fact should be fully recognised by both critics and advocates of RCT.

Philosophy and the Social Psychology of Sociologists

Hauke Riesch | University College London | h.riesch@ucl.ac.uk

'It's all nonsense, all of it. I'm fed up being labelled a positivist by people who wouldn't know positivism if they had it in their soup.' (Head of a research funding body). [quoted in Taylor 2002]

Having rightly concluded that philosophy was of some importance to the sociological enterprise, sociologists (and I am one) have used the discipline much as the military might use a guided missile. Safely fired in the conviction that it will seek and destroy, the soldier need know little of the missile's true workings and consequences. [Tudor 1982]

Almost every introductory text on the philosophy of social science includes disclaimers that terms such as 'positivism', 'reductionism' and 'social constructivism' have many different meanings and usages among both philosophers and social scientists.

In this talk I want to first propose a discourse analysis inspired study on the sociologists' use of philosophy and philosophical concepts, focusing mainly on the very controversial terms positivism, reductionism and constructivism. Although I intend to present some preliminary results from a search of social science literature (such as ESRC grants, as obtained from www.esrcsocietytoday.ac.uk), I want to focus on some of the implications that the sociologist's use of philosophy can have on the practice of sociology and the philosophy of sociology themselves.

As the above quote was meant to illustrate by Taylor, the concept of positivism is being used by social scientists to negotiate a disciplinary identity that sets them apart from other disciplines and methodologies, and I suspect similar things are going on with other philosophical terms. In this respect the social sciences are of course no different to the natural sciences (as I try to show elsewhere in my research). But I intend to argue that for the case of at least the qualitative social sciences, this trend to enlist philosophy to fight interdisciplinary wars has been much worse, and through the issues of reflexivity can have disturbing consequences for the philosopher, as well as for the budding sociologist who worries about her methodology and the status of the knowledge she hopes to produce.

In the qualitative social sciences 'constructivism' and an opposition to 'reductionism' and 'positivism' is these days taken very much for granted. At the same time, because there are so many differences in the precise definition of these terms, the usual presentation of them comes down to the lowest common denominator, and we end up being presented by a straw-man positivism (or reductionism) that it is very hard for anyone to agree with. This has then the result that qualitative sociologists end up shaking their head at the perceived pig-headedness of economists and the like, who they perceive to be irredeemable positivists. These people in turn, like the research head quoted above, learn to be irritated by what they perceive as the unfair misuse of philosophical terms against them.

The immediate inspiration for this talk is my experience of going to postgraduate qualitative sociology conferences in the UK and being struck by an overwhelming feeling of epistemological insecurity among the delegates. Young sociologists with obvious positivist leanings, for example, struggle to formulate their views in a coherent way, because they are simply unaware of how the debate has developed, and what the terms in which they are debated mean to different people.

At the same time, people I spoke to agreed that they were so confused about the epistemological status of the results that sociology produces that they felt the whole discipline was in a confidence crisis. In part, it was suggested that this can be demonstrated by the over-excessive emphasis the funding bodies place on research and teaching in methodologies, not because of too much philosophical preoccupation (because there is not much philosophy in methodology courses), but presumably because they feel they need to be seen to take their scientific aspirations seriously.

This almost desperate identity construction within and between the social sciences, with philosophical and methodological issues being used to threaten, insult, cajole and to set boundaries so much more fiercely than in the natural sciences, demonstrates that the institutional self-consciousness of sociology has not gone away since the ‘physics envy’ days; it merely manifests itself in another form.

Tudor, Andrew (1984): *Beyond empiricism: Philosophy of Science in Sociology*. London Routledge

Taylor, Chris (2002): *The CBN Consultation Exercise: Stakeholder Report* Cardiff University School of Social Science. Available at <http://www.tlrp.org/dspace/retrieve/5/stakeholderreport.pdf>

Abstraction, idealization, and the policy relevance of economic theories

Menno Rol | University of Groningen | m.e.g.m.rol@rug.nl

Idealization in scientific reasoning is often seen as a procedure by which, deliberately, falsity is allowed in theories. The description of ideal objects or ideal properties of real objects is taken to be useful in science, but it is not always clear what it is that false propositions are supposed to accomplish in a truth-seeking business. One answer - familiar to economists - is that unrealistic assumptions do not matter as long as our theories have predictive power. This has been loosely interpreted as ‘instrumentalism’. However, Milton Friedman’s celebrated 1953 thesis goes further: ‘*the more significant the theory, the more unrealistic the assumptions*’ (‘The Methodology of Positive Economics’, p.14). As to this alleged significance, it has been noted that Friedman’s attitude to economics seems to be more realist than instrumentalist *viz.* by Dan Hammond, Uskali Mäki, and others. Friedman believed, for instance, that a well confirmed economic theory could reveal ‘*manifestations of a more fundamental and relatively simple structure*’ in economic reality’ (ibidem, p.33). Meanwhile, it is easy to find many jokes on the internet that mock economics as a social science: its ubiquitous use of unrealistic assumptions is believed to make economics unsuitable for policy advice. Economics seemingly is not about actual economic reality.

Among the many aspects of idealization generally overlooked in this judgement, two are interesting for my point. The first aspect has something to do with what Mäki has noted earlier: that horizontal isolations form a procedure completely different from vertical isolations. The former leave the level of abstraction untouched and the latter increases the level of abstraction. In Mäki's view, vertical isolation is abstraction. Analogously, I propose to define idealization in such a way that it is the same as horizontal isolation. The second aspect is that idealization can be explicated as the use of a *true* counterfactual with a *false* antecedent. In other words, the fact that falsity is somehow inserted in theories that idealize the object of study, the actual propositions by which the idealization takes place need not be false at all.

I shall illustrate both aspects of idealization with an example from physics. The Ideal Gas Law and Boyles Law are respective idealizations of the van der Waals Law. It will be shown that the idealizational procedures involved can be repeated at a higher level of abstraction than that of the laws as we know these from physics textbooks. Thus, idealization and abstraction can be seen as relatively independent methods of reasoning, the one to be carried out with or without the other at the same time.

The lessons for the policy relevance of science – and of economics in particular – are that the use of ideal models does not necessarily imply a total lack of external validity, although for policy objectives the idealizational inferences that scientists propose do tend to present obstacles for serious policy proposals. The more idealized a theory, the more dissimilarities there are between the actual world and the hypothetical worlds of the extension of this theory. On the other hand, abstraction in theorising increases policy relevance of theories, rather than that it is decreased. This is because the more abstract a theory, the more likely it is that the actual social world – where policy makers try to intervene – belongs to the extension of this theory.

I shall also deal with one very problematic aspect of the common use of *ceteris paribus* clauses in economics, which challenges the analysis I shall present: that such clauses tend to have an imprecise reference. Without the precise reference of a clause, it is impossible to judge the external validity of theories hedged by this clause. The further distinction between *ante explicationem* and *post explicationem* types of abstraction is to help deal with these cases.

If time allows for it, I shall also illustrate my claims by means of a case. It shows how I believe economic-theoretical approaches of the infamous inefficiency of health care systems should be understood. The inadequate policy interventions frequently undertaken to correct the inefficiencies are based on an idealized and henceforth mistaken view of what sort of market we are concerned with.

The ‘Materials’ of Experimental Economics: technological versus behavioral experiments

Ana C. Santos | ISCTE, Portugal + Dinâmia, Portugal + EUR, The Netherlands | anacsantos@iscte.pt

The epistemic value conferred to by the participation of the ‘material world’ in the experimental process of knowledge production is consensual. This is no different in

experimental economics. However, the scrutiny of the epistemic role of the ‘materials’ of economics is still incipient. The present paper is meant as a contribution to this inquiry. Two categories of experiments are identified according to the differentiated role of the ‘materials’ of economics. The *technological experiments* produce knowledge of how to design economic institutions for specific purposes. The crucial ‘material’ of these experiments is the institution that organizes the interactions of the experimental participants. The *behavioral experiments* produce knowledge of individual behavior in various decisional and interactive contexts. The crucial ‘material’ of these experiments is the agency of the experimental participants. Even though the same ‘materials’ are present in both kinds of experiments, the institution and the experimental subjects play different roles in them. In the technological experiments economists manipulate economic institutions in order to learn about its characteristics. The experimental participants are instrumental to that end. They simply allow testing the robustness of the institutions. In the behavioral experiments the experimenters manipulate the institution in order to learn how it influences and shapes subjects’ behavior. Whereas the focus of technological experiments is the relationship between the institutional set-up and the performance of the economic system, the focus of behavioral experiments is the relation between the institutional context and human behavior. Notwithstanding the methodological and the epistemological differences between the two kinds of experiments, they can be complementary tools. The behavioral experiments can open the *black boxes* of the technological institutions that produce stable outcomes. The FCC spectrum auctions and the ultimatum game experiments illustrate the technological and the behavioral experiments, respectively. General policy implications are also drawn for each kind of experiment.

Realism about Groups

Dr Paul Sheehy | Richmond Upon Thames College | psheehy@richmond-utcoll.ac.uk

In both the formal discourses of the social sciences and our everyday talk we refer to social groups in our descriptions and explanations of the social world – the domain of human interaction. The truth conditions of many propositions about the social world depend upon the existential or referential status of groups such as nations, peoples, classes, communities, teams, tribes and families. In our everyday talk, and in the descriptive and explanatory discourses of the social sciences, a proper understanding of what is said - of what we mean - turns on how we are to treat references to social groups. Any account of the nature and aims of the social sciences possesses an ontological commitment. That is, such an account must render pellucid and consistent its understanding of the metaphysical status of social groups. In this paper the primary aim is to motivate the case for a form of realism about groups, ontological holism. This is the thesis that regards social groups as composite material particulars capable of standing in their own right in causal and explanatory relations. Social groups are among the entities over which we quantify in the set of our best descriptions and explanations of the social world. At a high level of taxonomic categorisation groups feature alongside kinds such as organisms and artefacts.

Opposed to realism is ontological individualism. It is individuals and their relations which enjoy ontological and explanatory priority. Groups can be shown to be identical to sets or mereological sums of individuals or person-stages, mere fictions or reductively analysed

out of social scientific discourse. The truths about groups are held to be expressible, without loss, as truths about individuals. The core issue between ontological individualism and holism is ultimately the modality of the ineliminable role of groups in our forms of discourse.

Social groups cannot, though, be identified with sets, aggregates or mereological sums of individuals. For such a strategy of identification fails to account for the survival of a group through membership change or the possibility that a group (the very same group) could have had a different membership. More positively I explain that reference to social groups is ineliminable in our everyday and formal social scientific discourse. Social groups are objects in their own right, which can not be reductively analysed out of those descriptions and explanations. In sketching this claim I judge ineliminability from our best theoretical model to be the hallmark of realism. Therefore the apparently ineliminable role of groups in our discourse provides a *prima facie* reason for taking them to be objects capable of standing in causal and explanatory relations with other things in the world. Furthermore, the realist thesis may be extended to the claim that *social group* can play the role of a natural kind term within social scientific discourse.

A naturalist approach to the social sciences maintains that there is a substantive continuity between the natural and social sciences, even if that continuity is no longer located in a common methodology or in a shared commitment to universal exceptionless laws. The identification of causal regularities within a particular domain of enquiry may be one of the ways in which the natural and social sciences are importantly continuous. Holism can – and individualism cannot – explain the role groups considered in their own right appear to play in fields such as social theory, history and economics. If the arguments for holism succeed, then the role of groups in bringing about or influencing certain states and events is to be articulated in terms of the group as such being an entity capable of standing in causal relations. Alongside individuals groups are among the relata of a naturalised social science.

Market and Desert.

Obdulia Torres | University of Copenhagen | omtorres@hum.ku.dk

Over the last century, defences of the market system have abounded, both deontological and consequentialist. The deontological foundation affirms that the market is the best stage on which to realize particular values or moral principles, such as liberty and sovereignty, which are regarded as essential. The consequentialist defences assert that the distributions that result from market processes, even though unequal, provide everyone, both the best and the worst situated, with the best possible consequences, and that the latter (i.e. the worst situated) would be better off in an economic market distribution than in an egalitarian or need-satisfaction distribution. The force of the argument falls on the invigorating effect that incentives have on the economy, since they guarantee efficiency and economic growth. One of the consequentialist defences affirms that the distributions resulting from market interchange are fair since they fall under the desert principle. From this perspective, individuals will receive distributively according to what they have produced. In this sense, there is a positive correlation between what every individual contributes to the social product and what he receives from society; the underlying idea is that “you reap what you sow”

To confer moral values on distributions that result from the market process entails the danger that nothing more is needed, that there are no redistributions capable of improving the situation, since the new redistributions would distort the justice of the obtained outcome, where everyone enjoys according to his desert. From this perspective, it could be argued that the political arrangements of the welfare state are morally illegitimate, as they redistribute wealth, for example by requiring tax payments which are given out to the worst situated. The basic point which I am concerned with is that, regardless of whether the desert argument is suitable as a wealth redistribution criterion, the market does not reward individuals according to desert and, therefore, this cannot be used to justify morally the distributive shares that result from market interchanges. Secondly, I wonder if the desert principle, as it is understood in the economic field, matches up with our moral intuitions about “desert” means. As an additional outcome, it is affirmed that desert, even having a role in distributive justice principles, cannot be the only concept that governs distribution, since its use drives a wedge between the encouragement of aggregated aims and its distribution. The hypothesis that I suggest is that the moral foundation of the market has a major influence on the individual’s perceptions about what is a fair outcome at both a distributive and a redistributive level.

The analysis that I do is restricted by the following points: when we affirm that the market does not reward individuals according to their desert we refer to a theory framework and this theory framework is obviously the neoclassical theory of general equilibrium. This is so because it is in this structure that the foundation is framed, taking marginal productivity theory as its instrument. Secondly, there are two questions that we have to keep separate. One of them is: would it be desirable to have a world where distributive rewards are arranged according to the desert principle? The other question would be: is the market that world? I want to show that independently of the answer to the first question, the market does not reward according to desert. I will only briefly comment on a matter related to the first question. Finally, I will focus almost exclusively on the individual’s income distribution, especially incomes earned in the market. The question that I will attempt to answer is: to what extent can wage differences be justified by a desert argument? Normally, it is supposed that redistributions by political mechanisms fulfil the need satisfaction criterion, not the desert criterion. Therefore, incomes obtained by political means are beyond the scope of this paper.