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The International Network  
for Economic Method

## VI INEM CONFERENCE

Madrid, 12-13 September 2008

— Book of abstracts —

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**M. Klaes (Keele University), The End of Constructivism in Economics**

The notion of frames has found wide application across the humanities, social sciences and beyond. This paper will approach framing effects in the context of the recent emergence of a neo-behavioural paradigm in economics and finance (Kahneman, Slovic, Tversky). For much of the 20th century, modern economics has been focused on an abstract conception of individual choice, based on instrumental rationality and atomistic individualism. The historical roots of this perspective are manifold, but can be usefully approached in terms of Pareto's distinction between 'logical' and 'non-logical' action, which in his framework gives rise to a division of labour between economics and sociology which in many ways has remained formative for much of the ongoing 'boundary work' between these two disciplines. Within economics, explanatory primacy is given to the logical action paradigm, while phenomena arising from non-logical action are relegated to the domain of residual categories.

Economic neo-behaviourism has focused on this residual with a view to opening it up towards accounts of departures from logical action that nevertheless qualify as 'economic'. A key category here has been the notion of bounded rationality but the scope of these endeavours is wider, encompassing a new psychology of economics in which framing effects have been receiving renewed attention. Experimentally however, it is a curious feature of framing effects that it is difficult to restrict them to the psychological level only since the social context of a choice situation too affects outcomes in systematic ways. This is typically addressed by making some acknowledgement of the possibility of 'social' framing effects. The paper investigates to what extent taking these framing effects serious is compatible with atomistic individualism. Drawing a distinction between essential and non-essential social framing, it will be argued that framing effects that are essential call for a broadening of neo-behavioural economics towards a more broadly developed economic institutionalism.

At the same time however, the paper concludes that such broadening is unlikely to occur. Conceptually, this can be made plausible with reference to the Paretian division of labour between economics and sociology. Empirically, such conjecture finds corroboration in the direction that the 'construction of preference' approach in economic behaviourism is currently taking.

**S. Deichsel (Universität Bremen), Friedman and Laudan setting the stage for a pragmatic turn in theory appraisal**

My talk will propose a pragmatic way out of the rivalry between postmodern relativists and their more positivistic enemies by focusing on the economic elements of Friedman's and Laudan's methodologies.

Milton Friedman's famous defence of unrealistic assumptions is by far the most influential methodological statement in economics and many economists still use its arguments when they defend themselves against attacks referring to the lack of realism of economic theories. On the other hand, most methodologists and philosophers of science have harshly criticised Friedman's essay as inconsistent, wrong and misleading. First, by presenting arguments for a pragmatic reinterpretation of Friedman's essay, I will show why

most of the philosophical critique misses the point. While a more pragmatic reading has been proposed by some authors (e.g. McCloskey 1983, p.485; Hausman 1992, p.162.), it was actually performed very rarely (Mayer 1993 and Schröder 2004 are exceptions). However, there are several reasons why a pragmatic reading should fit well to the text: First, Friedman wrote his essay for economists with the aim of preventing them from caring too much about the reality of their basic assumptions; he did not write it for philosophers. Second, Friedman is taking problem-orientation seriously, much more seriously than Popper did: Even false predictions are only a shortcoming of a theory if they occur within the domain of phenomena the theory is intended to explain. Third, Friedman does neither argue that the assumptions of a theory do not matter, nor that unrealistic assumptions lead to significant theories – he merely wants to stress that a productive methodological discussion should care about the adequacy of assumptions, not about their realism. His defence of the use of highly unrealistic assumptions is straightforward for economists, because it employs economic reasoning: Friedman sees highly realistic assumptions as a cost factor that would not help very much improving economic explanations.

Larry Laudan's work fits perfectly to this pragmatic view of science presented by the reinterpretation of Friedman's essay. Of course, Laudan, as a philosopher of science, goes deeper than Friedman and shows how many problems in philosophy of science such as incommensurability, theory-ladenness and underdetermination can be overcome by a pragmatic and problem-oriented approach. Laudan argues that theories do not have to be completely translatable for making a theory-shift rational: New problems require new theories that are better than their predecessors in solving the new problem. The modern economic analysis of science is the logical next step of a pragmatic methodology like Laudan's. I will show that it is nothing more than a consequent elaboration of the problem-centred philosophy of science found in Friedman's and Laudan's work. Somewhat astonishingly, the economic analysis of science is very well suited for the critical discussion of economic models, even if it is difficult to use this approach for evaluating the usefulness of very fundamental research. This, however, is a weakness shared with any other approach in philosophy of science I know of.

**Friday, 12 | 10.30-12.00 | 1B. METAPHYSICS OF THE SOCIAL**

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**J. W. Lindemans (K.U. Leuven), Methodological Individualism as an Ontogenetic Approach to Social Order.**

F.A. Hayek supported methodological individualism as well as an evolutionary approach to social theory, which includes economics. It has often been noticed that there might be a conflict between both views. G.M. Hodgson (1993) adds a new topic for discussion to this debate. He argues that methodological individualism implies an ‘ontogenetic approach’ to social order, while a truly evolutionary approach should be ‘phylogenetic’. After discussing Hodgson’s claims, I will consider B. Caldwell’s responses and assess the arguments of both sides.

According to Hayek’s methodological individualism, the sole task of social theory consists in reconstructing the social order from the beliefs, preferences and rules guiding individual action. In this way, the market order is the unintended result of millions of people acting on the basis of their subjective beliefs and preferences within the framework of the rules of property and contact.

Hodgson claims that Hayek’s methodological individualism commits him to an ‘ontogenetic approach’ to social order. In biology, ‘ontogeny’ is the predetermined development of an individual organism from a given set of genes, while ‘phylogeny’ is the evolution of a species, which presupposes variety in the gene pool. In social theory, an ontogenetic approach would start from fixed elements, and so does methodological individualism: individuals, beliefs, preferences and rules are given. However, according to Hodgson, a truly evolutionary approach to social theory must give a phylogenetic explanation of the social order, with variable beliefs, preferences and rules. Furthermore, Hodgson argues that Hayek’s evolutionary approach conflicts with his classical liberalism. ‘Structural pluralism’ should supplement ‘the pluralism of individuals and entrepreneurs’ and replace the dogmatic non-interventionism of ‘totalitarian liberalism’. Hayek’s refusal to accept this political conclusion is just another proof of his ontogenetic spirit.

Caldwell (2001) replies that Hayek was not a mainstream methodological individualist. Even though, for methodological reasons, preferences should be taken as ‘given’, they are not necessarily ‘fixed’. Furthermore, Hayek explicitly recognized the difference between ontogeny and phylogeny and rejected the ontogenetic view of social order because of its ‘historicist’ tendency.

Nevertheless, Hodgson (2004) remains convinced of his claim. And Caldwell (2004) concludes that they simply disagree on this issue. He argues that, according to Hayek, there are multiple sources of variety in a market system: new knowledge is constantly being discovered within a market system.

This is certainly true, but maybe Caldwell fails to acknowledge here that there is a difference between variety within and variety of systems of rules. Hence, I agree with Hodgson that Hayek’s methodological individualism commits him to a kind of social ontogeny. However, this does not mean that Hayek neglects phylogeny. Methodological individualism is only a part of the story. The earlier Hayek was indeed wrong in claiming that the sole task of the social sciences consists in the reconstruction of social order from the beliefs, preferences and rules guiding individual action. However, Hayek’s theory of cultural evolution is truly about the phylogeny of social order: groups accidentally following rules that bring about a prosperous order will outcompete other groups. Hence, the ontogenetic approach of methodological individualism and the phylogenetic evolutionary

approach are complementary rather than conflicting. The former gives a ‘proximate’ explanation of the functioning of the social order, while the latter gives an ‘ultimate’ explanation of the origin of the social order. Hodgson recognizes that ontogenetic and phylogenetic explanations are complementary; it is not clear why he would not recognize this in Hayek’s case. I agree with Caldwell that Hodgson makes out a case against mainstream economists rather than against Hayek. Furthermore, both explanations reinforce each other: the fact that certain rules bring about a certain order of actions (ontogeny) explains why these rules persist once they emerge (phylogeny).

It becomes even more complex because ontogenetic mechanisms can make use of evolutionary mechanisms and start phylogeny on a new level. This is what the market system does according to Hayek: new knowledge evolves within the market system, which is what Caldwell emphasized. Hodgson claims that Hayek is ambiguous on the question whether the market is a product or a cause of evolution. But it is both. Evolution occurs on multiple levels: on the level of the constitutive rules of the market system, and on the level of knowledge within the market system. Therefore, ontogeny and phylogeny become relative concepts.

### **G. Papadopoulos (Jan Van Eyck Academie & Erasmus University Rotterdam), Money and Collective Intentionality**

John Searle’s work on social ontology and the introduction of collective intentionality as the constitutive attitude of social facts provided a framework for a fruitful re-examination of the debate between ontological individualism and collectivism. One of his usual examples in his exposition of social ontology was money.

Money has been the focal point of a series of debates on ontology also in economics. There are two competing research traditions; the commodity and the claim theory of money and much of their differences can be explained on the grounds of their competing ontological conceptions of the economy and of economic action. The former shares the individualistic framework that characterises neoclassical analysis, while the latter allows an explanatory role for the state and for social institutions.

Searle himself indicated the relevance of his work on ontology for economics on a recent article making again explicit references on money as well as an indirect contribution on the recent debate on the possibility of a money-less society. Still, his examples are often simplifying the phenomenon of money and his presentation falls short of the sophistication of the analysis of monetary economics.

The aim of his paper is to investigate the relevance of the Searlian framework for the ontology of money. The first step will be to analyse the accuracy of his examples and then extend the analysis of money in terms of collective intentionality against the background of the debate between the commodity and the claim theories of money. The main issues of the theoretical debate on money, i.e. its emergence, its persistence and its evolution will be revisited using the same framework.

The issue of the ontology of money has been recently addressed in further important contributions. Among them Geoffrey Ingham (2004) and Uskali Mäki (2005) make direct reference to the work of John Searle. Their comments and their criticisms will be also evaluated in the course of the argument.

**J. Turk (Slovenian Acad. of Sciences and Arts), Economics and social reality: getting traction in the world**

This article critically examines the methods used in economics by contrasting them with the tools used in experimental physics. I argue that economics is superficially empiricist in that it confines itself to the comparison of data and models, whereas experimental physics is realist in that it fully takes into account how the data is produced from and is ultimately traceable back to underlying physical quantities. The thorough treatment of the relationship between the data and the underlying physical world it makes tangible is where physics gets its traction in the world. Data plays an intermediary role between what is out there and knowledge about what is out there. Before data can be used to compare higher-level theories to what happens in the world, a lower level understanding of how the data relates to the world is necessary. Thus in experimental physics, while data is indeed used to compare models to what really exists in the physical world, the comparison of models to data is incomplete without a thorough prior understanding of how well the data represent the underlying physical quantities of interest. I argue that economics needs to better understand the relationship between the reality of the social world and data that can make that social world tangible. Then economics will have some real traction in the world.

Economics should not be done like physics, biology or any other natural science. Rather I use the comparison with physics to argue that economics must be done as a social science with a set of methods tuned to the subject matter of economic activity as it occurs in human societies. The heart of the argument is that because of the nature of the data used, mainstream economics has little traction in the world. Instead, borrowing from realist social theory, I describe how narratives can have traction in the social world to the extent that they convey the internal conversations of key actors operational at the time under study as a real causal mechanism driving economic and social change. In other words, if, as Margaret Archer suggests, the internal conversation acts as a real causal mechanism mediating between structures and agency in the social world, then we need data that makes that internal conversation tangible. Once we have access to what is actually happening in the social world, we can perhaps compile knowledge from the data thus obtained or compare that data to various theories. The important point is not only that theories at higher level depend on lower level social theories of the relationship between the reality of the social world and how to make that social world tangible through data, but that models or knowledge based on data have to be traceable back through the data to the social world from whence that data comes.

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**Friday, 12 | 10.30-12.00 | 1C. BEHAVIOURAL ECONOMICS**


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**J. Daemen (Erasmus University Rotterdam), Ketchup Economics: The Methodology of Finance**

Back in 1985 Larry Summers employed the metaphor of “ketchup economics” to describe the relationship between economics and finance. He distinguished two groups of researchers: general economists who study the ketchup market as part of the broader economic system and so-called “ketchup economists”, located in the Department of Ketchup where they receive much higher salaries than do general economists. General economists focus on fundamental determinants of price and quantity of ketchup, the various supply and demand factors, and try to explain price fluctuations by examining various types of data and using models. Ketchup economists, on the other hand, reject this approach and its results. They point out that the aggregate data, used by general economists, are almost meaningless accounting entities which are not even accurately measurable in the first place. Instead they focus on studying the hard observable data of ketchup transaction prices and possible excess opportunities in the market. The lack thereof and the resulting efficiency of the ketchup market is regarded as the best established fact in empirical economics by ketchup economists.

There have been some other contributions on the similarities and dissimilarities between economics and finance. Gibbons (1987) points at the common interests in explaining certain phenomena and the sharing of methods -econometric methods in particular- and data. Ross (1987, 2005) states that economics and finance have differing methodological perspectives: whereas in economics in general the equilibrium concept takes center stage, finance evolves around the no-arbitrage principle. Campbell (1994) combines both arguments. The stories mentioned above are to a large extent a claim about methodology: the same phenomenon is approached from another angle with different methods.

But while there exists a large literature today on the methodology, and more general philosophy, of economics, there is no such thing in finance. There are papers and chapters on method and ways of doing research, including the viability of certain statistical procedures, the proper use of data, and how (un)realistic certain assumptions are, but these are usually confined to a technical treatment. Which least squares calculation is most appropriate? What distribution of returns fits best?

There would appear to be space for a more thorough, fundamental treatment of the methodology of finance. A first step would be to investigate the ways of argumentation in finance. This paper is an empirical attempt in that direction. The approach is two-pronged: the two leading journals in finance, the *Journal of Finance* and the *Journal of Financial Economics*, will be examined through the years and a selection of the most famous and most cited articles in finance (e.g. Black & Scholes, 1973; Fama, 1970; Merton, 1973; Sharpe, 1964) will be scrutinized as well. Attention will be paid to the structure of the writings, writing style, and testing issues such as the use of statistics and data-mining.

The goal is to gain insight in the ways of argumentation in finance and to see if there are recognizable patterns to be found. Additionally, an attempt will be made to infer methodological and philosophical inclinations from the findings and evaluate these along the lines of the broader literature on the methodology and philosophy of economics.

### **M. Nagatsu (U. of Exeter), Behavioural economics beyond ‘Ptolemaic Science’**

In his recent book, Don Ross (2005) characterizes behavioural economics as a ‘Ptolemaic science’ –which (he says) is not meant to be insulting. Improving the accuracy of a theory is an important part of a normal science. Rather, his claim is that this articulation process has reached a point at which epistemic returns from the effort to improve the accuracy of the theory are diminishing. For example, in the case of individual decision making, economists have been trying to explain away an anomaly called preference reversal phenomenon by ingenious parameterizations and(/or) representations of utility functions (generalized expected utility theory, regret theory, etc.) And yet none of these models are successful in explaining all the empirical data in a satisfactory way. For Ross, even psychological approaches (e.g. the compatibility hypothesis) are not promising pathways towards a scientific revolution, since the ideas behind this programme are not theoretically well-motivated. I will argue that, in categorizing economists and psychologists in the same ‘Ptolemaic’ camp, Ross is overlooking an important difference between the two approaches. That is, while psychologists explicitly ask whether different measurement methods yield the same result in measuring preferences (‘detection robustness’ in Jim Woodward’s term), this question tends to be ignored in the economics literature. Emphasis on robustness seems to be a methodological virtue that economists should learn from psychologists, independently of theoretical motivations.

I will further explore this lesson in the case of the explanations of another famous anomaly in interactive decision making, i.e. pro-social behaviour in a set of experimental games (e.g. ultimatum games, dictator games, public goods games, etc.). Contrary to the standard game theory’s prediction, it turns out that people do not always behave so as to maximize their material gains in these experiments. I propose a useful categorization of different models that have been developed to explain this phenomenon: (i) preference-measuring approach (e.g. Fehr et al. 2003); (ii) preference-eliciting approach (e.g. Binmore 2006); and (iii) preference-triggering approach (e.g. Bicchieri 2006). Several interesting points can be made by looking at the differences between these approaches: (i) can be criticized as not having learnt the importance of detection robustness, while (ii) and (iii) take full account of the fragility of preferences in both experimental and ecological settings; the relation between (ii) and (iii) is a little more complicated, but can be characterized by differences in practical motivations (market vs. social engineering) and theoretical motivations (evolution vs. cognition). I argue that, because of these differences, robustness analysis alone is insufficient to evaluate the two approaches. Based on this finding, I will motivate the two approaches equally so that they can lead us beyond the phase of ‘Ptolemaic science.’

### **A. Santos (Dinâmia-ISCTE), Behavioral experiments: social preferences or social norms?**

Economics experiments produce knowledge about socioeconomic institutions and the motivational factors underlying human behavior. Market experiments, the technological experiments of economics, are particularly suited to learn how to design market mechanisms that are to accomplish pre-determined aggregate results. Game theory and individual decision making experiments, the behavioral experiments of economics, are the economics experiments that allow the making of inferences about the motivational factors underlying human behavior (Santos 2007).

The paper focuses on a particular set of game theory experiments – the ultimatum game, the prisoners’ dilemma, the public goods game – that depict social situations that involve going against narrow self-interest for the attainment of a joint gain. Even though self-

interest dictates actions generating a situation that is worse for all, individuals manifest pro-social behavior in these experiments. The social preferences explanation (SP) takes this behavior as an indication of stable social preferences. The social norms explanation (SN) explanation, in turn, takes these behavioral patterns as an indication of social norms which are not necessarily aligned with individuals' preferences. According to the SP interpretation pro-social behavior indicate that the individual prefers an outcome that is best for all to an outcome that benefits him only. The SN explanation interprets this behavior as the expression of a code of behavior that dictates that individuals should choose the course of action that benefits (or does not harm) the other(s).

The major difference between these interpretations is that while the SP version presupposes a direct link between individual preferences, the actions of the individuals and resulting outcomes, the SN version allows dissociating behavior from motivations and from resulting outcomes. On this view, a given action may be compatible with different motivational factors (selfish and other-regarding) and may be explained by other reasons than the resulting consequences (e.g. the sense of doing the right thing). The SP interpretation invariably interprets pro-social behavior as individual actions that improve individual welfare because individuals possess social preferences.

These interpretations convey two different views of the experiments. The SP explanation takes behavioral experiments as measuring tools of social preferences, which are manifested whenever there is a conflict of interests. The SN explanation takes the experiment as an identification device of social norms, which strongly depend on the context of interaction. The paper scrutinises these explanations from the viewpoint of the demands it raises upon behavioral experiments, either as tools for the measurement of social preferences or for the identification of social norms. It shows that the SP explanation impose more stringent conditions because it requires the design of experiments that allow imputing preferences to observed behavior. It requires the design of experiments that create social situations in which experimental subjects reveal their underlying preferences through choices that bring about intended outcomes.

**Friday, 12 | 12.30-14.00 | 2A. EVIDENCE IN ECONOMICS**

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Symposium organized by  
J. Reiss (EUR) & D. Teira (UNED)

The aim of this panel is to discuss the concept of evidence in economics combining historical epistemology with a general philosophy of science view. Harro Maas and Damien Fennell will discuss well established and yet contested concepts of evidence in past and current economics. We will organise the debate around the divergences that arise between these various uses of evidence and discuss in what sense economics can be evidence-based.

**Harro Maas (University of Amsterdam), Armchair Observation in Nineteenth Century Political Economy**

From the days of Ricardo until well after the Second World War, economists were typically considered the kind of scientist who relied too much on self-evident principles contrived in the comfort of the armchair. Rather than joining the chorus of those who condemn and condemned this practice, this paper will trace its particularities back to the days when political economists for the first time styled their subject as a separate science, in the first decades of the nineteenth century in Great Britain. Explicitly framed in opposition to speculative moral and political systems, political economists generalized the paradoxical experiences of an emerging market economy into common-sense principles that served as reality checks in the social realm. This generalization was achieved by using fictitious (counterfactual) exemplary cases as demonstration device, exhibit, or 'bottled phenomena'. Thus, armchair observation bears a family resemblance to what we now refer to as the method of thought experiments. Armchair observations were made in the confines of one's study from notes on histories, travel journals, governmental reports, or statistical materials such as the census. But they were seldom a purely private affair. They were discussed in correspondence; they resulted from disputations on the pressing issues of the day at dinner tables in country houses, college common rooms, or the Political Economy Club.

**Damien Fennell (LSE), Evidence in Econometrics**

In this paper I ask in what sense (if any) is structural econometrics evidence-based? Here the idea is to bring different concepts of evidence and see what (if anything) they can tell us about how we get knowledge using structural econometrics. This is perhaps surprising given econometrics' position as the most sophisticated and developed branch of empirical economics. However, its very complexity often acts as a barrier for asking the simple question of just what structural econometrics tells us and what the status of its claims are. Identifiability, for example, is an issue which is philosophically rich and where there is a fine line between using data to parameterised an assumed model, and one where different models are tested, and potentially rejected from the data. This paper uses the concept of evidence as a way of asking this question of what structural econometrics tells us. The concept of evidence has recently received renewed attention, in philosophy, policy and medicine. By juxtaposing it with structural econometrics the hope is to bring fresh light on the issue. The paper concludes that econometrics provides evidence that rest on strong background, theoretical assumptions. As a result, causal inference in econometrics is evidence based in a weak and contingent sense. However, the paper also argues that this

does not undermine the usefulness of econometrics , rather it shows that its claims must be suitably qualified and related to context to be of most use.

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**Friday, 12 | 12.30-14.00 | 2B. METAPHYSICS OF THE INDIVIDUAL**


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**B. Boulu (University of Lyon), Approaches to the concept of identity in Economics: an application to the economic analysis of the firm**

The aim of this paper is to elicit the implications that arise from applying different approaches to the concept of identity on the questions of identification, multiple-selves and individuation within the economic analysis of the firm.

These three issues refer respectively to the economic impact of individuals' concern for the way they view themselves in a particular economic context, the economic outcomes that result from the multiplicity of social identities, and the ones that result from how individuals distinguish with each others. Application of the concept of individuation inherently falsifies results delivered by the identification approach: whereas the theory of individuation separates economic agents working within the same firm, identification explains how individuals' preferences can be affected by specific economic contexts.

The concept of identity, which has been previously used by psychologists, sociologists and law scholars (Hill 2007), has been first introduced into economics by Amartya Sen (1977) who used the concept as a theoretical support for his explanation to the conditions of rational choice. This conceptualization gained audience among other economists (Kirman and Teschl 2004) following the work of George Akerlof and Rachel Kranton (2000) who incorporated identity into formal models. The contribution of John B. Davis (2003) further exposed its relevance to economic analysis. This renewed interest in the modification of the representation of the economic agent and the different ways it has been applied raises theoretical and methodological issues.

On one hand, Akerlof and Kranton (2000) focus on social identity in a utility maximization framework and strongly emphasise the theoretical features that arise from social psychology, such as the role of social categories and the role of cognitive dissonance anxiety. Sen, on the other hand, developed the commitment approach, which highlights self-scrutinizing behaviour and the role of social groups over social categories. Davis (2007) develops a personal identity framework that addresses issues related to how individuals arrange their multiple social identities.

An appropriate approach to the use of the concept of identity may significantly improve theoretical formalization of the nature of the firm, its boundaries and internal organization, which constitute the three major issues of the economics of the firm. The analysis of firms' internal conflicts and firm's costs' structure as well as the description of the firm as an uncertainty reduction device, the determinant of the make or buy decision and path dependency can be revised by a different account of the economic agent. Applying different accounts of the economic agent can revise the analysis of firms' internal conflicts and costs structure, as well as the description of the firm as an uncertainty reduction device, the determinant of the "make or buy" decision and path dependency. In light of this, I overview methodological implications, notably experimental ones.

## **D. Gindis (University of Lyon 2), From Fictions and Aggregates to Real Entities in the Theory of the Firm**

The paper argues that the two dominant economic perspectives on the firm, namely the ‘nexus of contracts’ (Jensen and Meckling, 1976) and the ‘collection of assets’ (Grossman and Hart, 1986) views, are variations on the same theme. These are ‘fictionalist’ and ‘aggregationist’ positions that rely on one of two moves: they either deny the existence of the firm by regarding it as a legal fiction and/or a shorthand form of expression, or they reduce the firm to an aggregate of its parts, be these contracts, individual owners of resources or nonhuman assets. In both cases, firms and similar social entities are said to be ‘nothing but’ aggregates of these parts. Dissatisfaction with these views has prompted a search for new foundations for the theory of the firm (Blair, 1999; Zingales, 2000).

Although rarely acknowledged by economists, both views are modern revivals of old theories of the corporation that have been recurring in a cyclical fashion for many centuries (Avi-Yonah, 2005). ‘Fiction theory’, which dominated Roman law and medieval debates, regards corporations as simply names or imaginary legal persons that are nothing more than the individuals composing them. ‘Aggregate theory’, popular in the second half of the nineteenth century, is a variant of fiction theory that holds that corporations are simply aggregates of natural persons, usually shareholders. However, examination of the legal literature reveals that an alternative ‘real entity theory’ dominated debates from roughly 1900 to 1930. On this view, the corporation is neither a fiction nor an aggregate but a non-reducible real entity. Interestingly, Blair (1999) suggests that this forgotten view can provide new foundations for the theory of the firm.

Three generations of entity theorists are distinguished in order to counter a common criticism that entity theory relies on dubious organicist and vitalist claims. Such claims, made by the early generation of entity theorists, were rejected by later entity theorists who provided other foundations for real entity theory. This paper identifies these useful insights and discusses the ontological issues involved in the ascription of reality to an entity such as the firm. There is clearly more to revisiting the entity theory of the past than the exploration of the history of a legal idea. Reviving real entity theory is particularly relevant in the context of the rising literature in economic and social ontology (Lawson, 1997; Weissman, 2000; Mäki, 2001; Davis, 2003; Hodgson, 2004). Indeed, the main dividing lines between both past and present theories are ‘fiction vs. reality’ and ‘aggregate vs. entity’, and Iwai (1999) appositely speaks of a debate between ‘corporate realists’ and ‘corporate nominalists’.

The paper is organized in five sections. Section 2 discusses the main economic theories of the firm and establishes that these views are about fictions and aggregates. Section 3 reconsiders the merits of the short-lived entity theory, underlining the genuine insights in its later versions. These aspects are assessed in section 4 that clarifies the proposition that the firm is a real entity. Section 5 concludes that the firm is undeniably a real entity, which cannot be explained away or otherwise reduced.

**J. Davis (U. Amsterdam & Marquette U.), Psychology's recent challenge to economics: Rationality and the individual**

This paper distinguishes two different lines of thinking in the postwar psychology field of Behavioral Decision Research, one associated with prospect theory and one associated with preference reversals, and argues that they make two different critiques of standard rationality thinking in economics. Standard rationality theory is taken to be an expression of the atomistic individual conception in economics, and so these two different critiques of rationality in economics are associated with two different sets of implications for the conception of the individual in economics. I argue that neither line of thinking overcomes the problems their critiques of rationality generate for thinking about the individual. Prospect theory's critique leads to multiple selves problems and the preference reversals critique eliminates any basis for conceptualizing individuals. These problems concerning the understanding of individuals are then argued to be inherited by behavioral economics and experimental economics market research program, the former as a product of prospect theory and the latter as influenced by the preference reversals literature.

**Friday, 12 | 12.30-14.00 | 2C. SOCIOLOGY AND SOCIAL EPISTEMOLOGY****T. Duppe (Erasmus University Rotterdam), The Professionalization of the Commentary of Economics**

Since the mid 1970s economics experienced the emergence of a professional commentary (associations, journals, qualifications). Broadly conceived, this commentary is organized along two paradigms, the history and the philosophy of economics. Other branches such as the sociology of economics are located between these two depending on their inclination to either theorize on economics or concretize it historically.

Rather than arguing programmatically about these two genres, I question the professional ethos of commentary, i.e. the relation to one's audience, in a historical fashion. I depart from the claim that the professionalization of both history and philosophy of economics was historically conditioned by the formalist revolution of the 1950s and 1960s since it marked the preliminary end of the battles between theory and history in economics. The core of economics at the end of the 1960s was both a-historical and philosophically disengaged. Only within this historical situation one could perceive the need and opportunity of a separate commentary on economics. The formalist revolution achieved the occupational closure of economics that disclosed the very object of commentary.

The critical question to be posed is whether this historical condition determined or even constrained the professional ethos of historians and philosophers of economics. I distinguish two different ways of relating to economists in order to assess the professionalization of commentary. The first is a critical attitude aiming at the renewal of historical and philosophical reflections by means of pointing to the symptoms of its deprivation (such as the historical debunking of the narratives of progress from classical to neoclassical economics, or the philosophical charge of a gap between actual and implicit scientific standards, the empirical shortcomings of formal theories etc.). The second attitude is less the critique but the symptom of the historical situation of economics in that it substitutes historical and philosophical reflections of economists (such as a preservative approach to history or the philosophical re-justification of the lack of empirical reference). Such ethos results in the occupational closure of commentary, leaving the occupational closure of economics untouched.

On such ground I examine the history of the professionalization of commentary since the 1970s. The sources of my analysis are mainly programmatic writings that shaped the discursive identity of the commentary (as e.g. first issue articles, debates as on rhetoric of economics (1995), 2000 symposium on historiography etc.). I consider two generations of commentators separately. The first (associated with names such as Hutchison, Blaug, Boland, Coats, Heilbroner etc.), trained in economics before the formalist revolution, shared much of the general discontent of the 1970s that gave rise to the heterodoxy of economics. For them, being in the immediate impression of the formalist revolution, history and philosophy were not yet separate. The second generation of the 1980s and 1990s (associated with names such as Weintraub, Hands, Mäki, Mirowski, associations like the HES, ESHET, INEM, HISRECO, Journals and also qualifications) describe the actual professionalization. Does the hypothesis apply that this development was a move from the first to the second ethos?

I conclude that even if the self-understanding of the commentary always has been critical, with the professionalization the awareness of its historical finite conditions was weakened. Therewith it became more difficult to reflect on the unintended consequences of a separate commentary on economics. Politically speaking, rather than advancing the

professionalization, historians and philosophers should join forces within the institutions of economics.

### **H. Holm (Lund University), On Anonymity Policies in Peer Review Processes**

The scientific publication process plays a crucial role in modern society by clarifying what the scientific community regards as “good” or “accepted” science. In addition, scientific publication is very often used as a measure of success in the academic meritocracy, which means that a considerable amount of incentives (and hence efforts) are linked to it. Studies of various aspects of the scientific publication process are therefore urgent and will hopefully improve our understanding of how different scientific disciplines evolve.

Almost all ranked international economic research journals rely on some form of peer review process (PRP) that may differ with respect to the degree of anonymity between the author and the reviewer. This paper analyses these anonymity policies both theoretically and empirically. The various anonymity policies have different pros and cons and this paper focuses on four effects. First, a(n) (double blind) anonymity policy may mitigate (or facilitate) discrimination (e.g., papers by female authors or authors from unknown universities are discriminated against). Second, anonymity may mitigate or simplify corruption. Without anonymity an author might be able to bribe a reviewer. On the other hand, with a (single-blind) anonymity policy, a reviewer may reveal to an author that he currently reviews her paper (or has done so in the past) and demands (or expects) something in return. Third, anonymity may obstruct unwanted negative reciprocity. For instance, without the anonymity shield a junior reviewer may be unwilling to be frank in the evaluation of a senior researcher’s paper, since the latter may in the case of a negative review have the power to punish the former. Finally, an anonymity shield may facilitate theft of ideas and research results.

The present paper provides empirical data on the anonymity policies of the major economic research journals and finds that the two most important anonymity policies are the so called “single blind” and the “double blind” policy. The paper also provides empirical findings indicating the difficulty of providing an anonymity shield to an author in an environment where referees have access to Internet.

A number of conclusions regarding the potential effects of the present anonymity policies in the economics journals are drawn. One conclusion is that, although the double blind anonymity policy is used by some important journals, it has virtually no comparative advantage to the single blind policy in practice. Another conclusion is that economic journals are highly homogenous with respect to anonymity policies and that there seems to be considerable room for experimentation with more open review processes.

### **S. Leppälä (Turku School of Economics), An Epistemological Perspective on Knowledge Transfers**

With social sharing of knowledge the aggregate knowledge base can grow without everyone being forced to discover all the facts individually (Goldman 1999, 103). Various specialized fields, such as organizational learning and geography of innovation, have therefore focused on the circumstances conducive to knowledge transfers or spillovers. This paper, however, starts with the object of these processes, knowledge. What are the conditions, imposed by the properties of knowledge as a justified true belief, that successful social sharing of knowledge requires?

Knowledge transfer can be said to happen in between two sequences, where first individual *i* knows a proposition *p* and individual *j* doesn't, and after the transfer both *i* and *j* know that *p*. Due to nonrivalry of knowledge it is possible that *j* becomes to know *p* without *i* being forced to give up this knowledge. Yet knowledge is not always a pure public good, since to some degree it is excludable. Without telepathy one does not have a direct access to others' mental states, and deliberate communication in form of information or other actions, by which the knowledge gets communicated unintentionally, are required.

From the perspective of social epistemology, such communication requires more than availability of information. The truth condition of the belief is unchanged in the process, but it is required that also *j* justifiably believes that *p*. Firstly, *j* must believe that the information received from *i* is true, which highlights the role of trust in social sharing of knowledge. In addition, while the belief needs not at this stage be justified, it at least needs to be consistent with other beliefs held by *j* in order to be accepted. Secondly, applying the traditional definition of knowledge, also *j* needs to be able to justify her belief that *p*. Thus, *j* needs to have another belief(s) that justifies the received one or such justification needs to be provided by *i* in addition to the target proposition. Both of these options highlight the fact that additional knowledge needs to be possessed to learn new things, stressing the cumulative character of knowledge. The implications given by different theories of justification, foundationalism and coherentism, will be examined in the paper. Also the issue of whether the knowledge is shared deliberately or not will be examined. While the paper will concentrate on propositional knowledge, we will also examine the implications of procedural knowledge ("know-how" (Ryle 1949) or "tacit knowledge" (Polanyi 1958)), since its importance is stressed in organization and innovation studies.

The intended contribution of the paper is to show that the social sharing of knowledge, a hot topic in economics and other social sciences, could benefit from insights given by social epistemology. Simple availability of information is not sufficient for knowledge transfers, the information needs also to be believed. And even dissemination of beliefs, even if true ones, does not accumulate social knowledge base, if the beliefs are not justified. Social epistemology helps to analyze when these conditions are met and what is required to meet them.

**Friday, 12 | 15.00-16.30 | 3A. EVOLUTION AND HAYEK****F. Guala (U. Exeter & San Raffaele U.), What Is Evolutionary Game Theory Trying to Accomplish?**

The influence of naturalism has been growing steadily in many areas of philosophy, including social and political theory. This paper is concerned with one specific form of naturalism blending evolutionary theory with game-theoretic modelling, which promises to deliver important insights in the structure of norms that govern our society (or social contract, for short). The primary goal of this programme is to explain the emergence of norms from the repeated interaction of individuals, without appealing to any higher, pre-existing ethical principle. The evolutionary programme promises to deliver a purely naturalistic contractarian theory, in other words. Evolutionary game theorists like Brian Skyrms and Ken Binmore however have not been content to highlight the explanatory potential of their models, but have also stressed repeatedly the relevance of their work for normative social philosophy. They have argued, in particular, that understanding the origins of social norms and moral dispositions will help us to avoid advocating utopian reforms with no chance to succeed (but much potential for disaster).

I argue that such promises are not only premature, but that they cannot be fulfilled even in principle. The promise of policy relevance is based on a fundamental confusion between the explanatory and the prescriptive function of evolutionary game theory. Prescriptive claims must be based on a full understanding of our moral dispositions and of the social norms that govern our behaviour. Most demonstrations of the evolution of fairness in contrast provide at best partial invisible-hand explanations of the emergence of moral norms, and partial models cannot impose constraints on intervention and policy.

Part of the problem is that human behaviour as observed in experiments and in the field seems too varied and complex to be captured by a single model (or set of models). When evolutionary game theorists try to provide a full account of morality, they typically try to reduce such complexity by heavily reinterpreting the empirical evidence. I argue that this strategy is highly dubious, and effectively amounts to revising what we know quite well (and potentially can know better – by collecting more data) in the light of what we don't know very much about (conjectures about the environments in which human morality may have possibly evolved). I conclude that an understanding of our moral dispositions is more likely to be provided by empirical studies in social psychology, experimental economics, and anthropology. Although evolutionary models can do a useful service by providing tentative explanations of how such norms and dispositions have evolved, they cannot by themselves set independent constraints on policy-making nor prompt radical reinterpretations of the empirical evidence.

**C. Chelini (University of Torino), The cultural evolution of a rule of conduct: Hayek's epistemology and current evolutionary theories in biological anthropology**

In this paper I would like to endorse the claim that Hayek's theory of knowledge is compatible with modern evolutionary theories about cultural transmission in biological anthropology. In Hayek's writings, the emergence of norms and the importance of imitation and social learning are related to a process of cultural evolution and spread of rules through the mechanism of the market that enables economic agents to use the

knowledge dispersed and fragmented among idiosyncratic individuals. In particular, he explains how our institutions are shaped by a process that considers the role of our cultural heritage: what is learned is passed on both at an individual and a societal level and rules of behaviour are characterized by a feedback process, an adaptation and continuous adjustment with the surrounding environment. In Hayek's epistemology, the most important problem that economic theory should try to solve is not the allocation of scarce resources but the distribution and utilization of fragmented knowledge. In the first section I'll explain the process of norms formation following Hayek's contribution and stressing a cognitive economic perspective that is focused on the limits of human rationality. Then, the core of the paper will develop in details the question about how the kind of methodological individualism that Hayek endorses and the process of cultural group selection can be sustained at the same time. I'll account for how the process of cultural group selection can explain not only the emergence of cooperative behaviour in prisoner dilemma type situations, but also the emergence of self-enforcing institutions like in pure coordination games. In doing this I'll use theories about cultural evolution and the emergence of conventions in modern biological anthropology. I'll also give an explanation about the debate on the role of group selection in the biological realm and why group selection normally doesn't work, while cultural group selection can be viable. Finally, I want to address the question about what the implications in economic theory are, given that this strong comparison among Hayek's epistemology and modern evolutionary theories holds.

### **G. Marques (UBA & UNLZ), A revision of Hayek's view on holistic concepts**

The third generation of Austrian economists (particularly, Mises and Hayek) deeply appreciated orthodox economic theory. But they were critical of the type and contents of its categories. They believed that classical economists had identified most of the relevant economic concepts, like prices, rent, interest rates, time preference, etc., i.e. the ones considered by agents themselves when making economic decisions. As far as these considerations guide economic actions, economic processes must be elucidated in these terms. In this sense, the classical procedure truly conforms to a methodological individualistic approach, and it was judged perfectly admissible. In spite of this, the classical approach was flawed because the relevant concepts were deemed to have an universal and objective nature. This aggregative and not subjectivist procedure brought economics to a halt. We therefore can say that, from the Austrian point of view, classical aggregations met the requirements of methodological individualism, but failed to incorporate subjectivism.

Classical aggregation should not be confused with the type of aggregation that predominated in macroeconomic theory, which generated concepts that were only imaginary constructions, like "aggregate velocity of circulation of money" or "aggregate capital", considered void of contents by the fathers of Austrian economics, and do not form part of the agents' decision making process. These concepts were considered irrelevant (and even dangerous) for economic analysis, and mistaken from an epistemological point of view. While classical aggregation provided an inadequate treatment of adequate concepts, macroeconomic aggregations provided an inadequate treatment of inadequate concepts. To avoid confusions it will be useful to distinguish between two radically different forms of aggregation, which may be designated as universalism and holism. According to the Austrians, universalism may be reformulated, but holism has to be rejected.

Following Mises and Hayek, most Austrian economists claimed that modern economic theory must follow (and improve) the work of classical economists, keeping the

individualistic approach and categories, but giving up its universalism and incorporating a subjectivist view. Besides, most of them rejected macroeconomics, because they believed that subjectivism and methodological individualism were incompatible with holistic categories.

Today, things are changing, and some Austrians hold a more open minded approach to macroeconomics and are making substantial efforts to incorporate some holistic concepts into economic analysis. In spite of that, the historical prejudice against this kind of concepts due to their supposed commitment to methodological holism, may hinder this progressive movement. If correct, the arguments provided in this article may help pave the way for a more unbiased view of macroeconomics and for further incorporation of holistic concepts into Austrian economics.

Particularly, in this paper I will argue in favor of the following thesis:

- 1) A full commitment to subjectivism blurs Hayek's sharp distinction between "constitutive" and "speculative" concepts, allowing holistic categories, and particularly, macroeconomic aggregates, be "constitutive" in Hayek's sense;
- 2) From an Austrian point of view holistic concepts are unacceptable for explaining the objective consequences of economic actions, but, as long as individual decisions should be explained by constitutive concepts, macroeconomic aggregates could be incorporated for explaining economic decisions and actions;
- 3) Rejecting holistic aggregates imposes severe restrictions on Austrian's treatment of expectations, forcing them to consider only a subset of those that are really at work in the economic process. The Austrian view of intertemporal coordination may be also in danger.
- 4) On a more methodological level, holism as a type of aggregation should not be confused with holism as a methodological approach. Thus, even if methodological holism were incompatible with methodological individualism, holistic macroeconomic aggregations are perfectly compatible with both methodological individualism and subjectivism.

Friday, 12 | 15.00-16.30 | 3B. ETHICS

**P. Dietsch (Université de Montreal), Conceptualising Distributive Equality**

The notion of equality as an economic and political ideal is ambiguous. There is widespread agreement on the fact that individuals are owed equal consideration of their interests. However, the fact that defenders of theories as different as utilitarianism, libertarianism, liberal egalitarianism, or a capability approach agree on this point shows that its theoretical commitments are minimal. Once we ask the question what equal consideration means in terms of the distribution of various kinds of social advantages, the consensus evaporates. Not all theorists attribute any value at all to distributive equality, but for those who do, the currency of justice in the realm of which they call for an equal distribution varies. Amartya Sen's question "Equality of what?" has received many different answers.

This paper aims to shift the focus of our attention away from the question "Equality of what?" to the justification of distributive equality in the first place. My argument is divided into two parts. The first, negative part of the argument uses the analysis of equality in economic theory to suggest that its appeal rests on a controversial anonymity assumption: Suppose you are asked to evaluate two distributions of income among three individuals (A, B, C); distribution  $X = (10,20,30)$ , distribution  $Y = (20,10,30)$ , with the numbers representing units of income. A condition of anonymity requires you to be indifferent between the two distributions. Your judgement with respect to a given distribution has to be invariant to permutations of the members of the population within the distribution. In other words, there are no distributively relevant differences between people at all. This is implausible. In practice, the information that individual A in distribution X above gambled part of his holdings away at the casino while individual C worked hard for his larger holdings does influence our judgement about the justice of the resulting distribution. It turns out that distributive equality is no more than a default position.

The second, positive part of the paper examines what we can learn from these insights for thinking about distributive equality in economic and political philosophy. Political philosophers for the most part do not, or at least no longer, make an anonymity assumption. Liberal egalitarians, for instance, want the distribution of social advantage to be sensitive to the choices individuals make. In this case, the distributive outcome, in terms of income for example, will necessarily be one of inequality rather than of equality. Against this background, characterising theories of this kind as providing an answer to the question "Equality of what?" strikes me as misleading. Instead, I suggest it is useful to think of theories of justice as depicting and regulating a mapping. A theory of justice maps relative shares in the distribution of a currency of justice (the output), like income, onto individual characteristics (the input). We want this mapping to abstract from some individual characteristics like race or sex, yet at the same time we want differences in some other traits to be reflected in the distribution of income.

### **A. Menéndez Viso (U. Oviedo), The place of Ethics in Economics**

During the last few years, an intense controversy about the role of values in economics has taken place, mainly in the pages of the *Review of Political Economy and Economics and Philosophy*. The main figures in the dispute are Partha Dasgupta (2005), Martha Nussbaum (2003), Hilary Putnam (2002, 2003, 2007) and Vivian Walsh (2003, 2007). The two confronting positions in this controversy can be sketched as follows:

- a) Values and facts are entangled, therefore economics has to deal inescapably with both, therefore economics and ethics go hand in hand (which is the position defended by Putnam, Walsh, and Nussbaum).
- b) Even if the distinction between facts and values is not clear-cut, they can be distinguished, economics is about facts, and therefore it does not depend on ethics (which is the position held by Dasgupta).

These theses about the role of values in economics entail some others about the epistemic status of the discipline:

- a) Economics' content depends in part upon the moral assumptions of economists (i.e., the contents of the discipline are not completely settled by the economy). Mainstream economics lacks a human face, which has to be restored by introducing explicit ethical concerns in it.
- b) Economics' contents are as objective as they should be; i.e., they are not defined (mainly) by the moral assumptions of economists, but by the empirical phenomena the discipline tries to explain.

It will be shown that these consequences do not follow immediately from the respective definitions of the fact/value dichotomy that apparently underpin them. To derive them it is necessary to assume a number of controversial theses about the meaning of 'value' and 'fact', and their role in both ethics and economics. This work does not intend to take sides in the debate, but rather to shed some light on the roots and terms of the discussion. For there are a number of debates entangled in the contemporary discussion around values and facts, which need to be separated: the relations between ethics and economics, the ultimate ends of economics, economists' motivations and drives, and the role of values in economics. The confusion of these questions derives from a persistent ontological approach to values, which leads to their reification and cannot be avoided by proposing their entanglement with facts. The way out of the subjective dead end to which this ontological viewpoint dooms us, is precisely the abandonment of any ontological conception of values and facts and, consequently, the rejection of their dichotomic relation. Freed from the ontological approach, the relation between facts and values, and between ethics and economics, loses its problematic character. Both can and should be distinguished but, contrary to what some authors claim, economics has never lost its ethical face; and, more importantly, economics has a place at the core of ethical debates.

### **A. Witztum (London Met. University), Positive Ethics and the Science of Economics: An Enduring Robbins's Fallacy**

The basic principles of Robbins's *Essay on the Nature and Significance of Economics Science* are still present in most textbooks in Economics. We thus face a combined problem of historical and contemporary nature. On the historical front, the Austrian association attributed to Robbins, sometimes, hides the originality of Robbins's work in combining Austrian and some Lausanneian principles even though a similar line of reasoning was attributed to his main Austrian inspiration—von Wieser. In this context, Robbins's assertion concerning ethics-economics relationship has three main difficulties. Firstly, the presumption of means-ends analysis which is oblivious of the ends people seek to promote is not as neutral as it appears. Robbins (as does Wicksteed) chooses to ignore the ends by focusing on cost minimisation. This, implicitly (though not inherently), suggests another end—wealth, or means, maximisation-- which by no means can be considered as ethically neutral. Secondly, there is an implicit assumption that whatever the ends people seek to promote, there will always be a co-ordinated outcome to their actions. As competitive prices are the means to achieve waste minimisation (through proper pricing of opportunity costs), the assumed co-ordination must be that of general equilibrium and thus, co-operative based co-ordination must be excluded. Thirdly, Robbins demands that the postulates of economics be based on empirically recognised introspection. His Means-End agent is clearly opportunistic regardless of his objectives. This suggests opportunism which may not be so universally compatible with all possible ends. In the face of mounting evidence (current) can we still uphold these Robbinsian principles of economics?

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**Friday, 12 | 15.00-16.30 | 3C. MODELS AND ISOLATION**


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**R. Crespo (U. Austral), Nancy Cartwright, Millian and/or Aristotelian?**

Nancy Cartwright combines elements from Aristotle, with others of Mill, and modern thinkers, as Elizabeth Anscombe and Ian Hacking, among others. This paper will address some tensions present in Cartwright's thinking. It will propose that those problems might be overcome by a greater reliance on Aristotle and Anscombe's thought. These tensions, the paper will argue, obey in part to her reliance on Mill.

Cartwright understands scientific explanation in terms of stable causes, which she calls "capacities" or "natures". Cartwright's program aims at defining what capacities are (ontology), how they are known (epistemology), and how we use them.

Cartwright opposes Hume's reduction of causality to regularity of mere associations. Cartwright also opposes covering-law supporters because they do not consider causes; they merely include the so-"probed" singular case within a general covering law. Otherwise, Cartwright agrees with Mill's proposal of "tendencies" which she identifies with her "capacities". According to Cartwright, Mill's tendencies are not tendencies of events but tendency factors or stable real causes. These tendencies or capacities may give rise to Cartwright's so called "nomological machines", stable configurations of components with determinate capacities properly shielded and repeatedly running.

Cartwright's proposal has been criticized in different ways. Margaret Morrison highlights first, a tension between the singular and universal aspects of capacities. Cartwright looks for a concept stronger and more general than Humean laws but she puts singular causes first. Second, Morrison also sees in Cartwright a conflict between her empiricism and capacities. For singular capacities to be stronger than Humean general laws supposes a metaphysical commitment that makes verification unnecessary. How could a person be empiricist and metaphysic at the same time? Finally, Morrison considers that Mill's tendencies differ in important way from Cartwright's capacities. Another point of criticism is her "local realism", a disunified view of sciences. A final criticism is her skepticism about the possibility of explanation in social sciences.

The paper will first analyze the Cartwright – Mill connection, then the Cartwright – Anscombe and finally the Cartwright – Aristotle connection. The tensions mentioned above will be solved by Cartwright's arguments, based in Anscombe and Aristotle, and by complementary Aristotelian elements. The paper will also argue for the skepticism of Cartwright concerning a successful explanation in natural and (mostly) in social sciences.

The problem of her interpretation of Mill will be tackled in the first section about the Cartwright – Mill connection. The problem of the tension singularism-universalism, will appear in the section about the Cartwright – Anscombe connection. The tension empiricism-metaphysics, and the problem of her disunified view of science, in the section about the Cartwright – Aristotle connection. This last section will also include an Aristotelian account and development of the statements of Cartwright concerning social sciences. Thus, the conclusion will be that there is still much we can learn from Aristotle with respect to economics and social theory.

## **T. Grüne-Yanoff (University of Helsinki), Are economic models tools for theoretical isolation?**

Uskali Mäki's isolation account has considerably altered the old realism of assumption debate in economics. By distinguishing between realism about models from 'realisticness' of the model assumptions and conclusions, he argues that models can be true without having either true assumptions or true conclusions. This paper criticizes his account on three levels.

First, economic modeling typically does not involve an isolating process. As research papers and interviews of modelers show, they do not commence from any specific target system, but create their model in a playful and spontaneous process that is guided only by their desire to investigate a 'problem' and by vague intuitions how this 'problem' could come about. Further, pace Mäki's analogy of models with experiments, information about the causal structure of actual economic situations is typically too scarce to allow for control, manipulation and hence isolation to be applied.

Second, economic models do not only nullify, neutralize or purify a certain aspect of the world, as Mäki claims – they also contribute, specify and pollute. Economic modeling tools have important influences on what is modeled and how it is modeled. They provide an environment in which causal relations can play out. Typically, this environment is presented as a mathematical structure. Properties like tractability, derivability, solvability and comprehensibility are very important for the use of such structures. But these properties in turn influence what is modeled and how it is modeled. Far from providing a neutral environment for the isolation of causal relations, economic models strongly influence the way these relations play out.

Third, pace Mäki, models are not appraised according to the adequacy of their isolations. On the one hand, to determine the relative adequacy of isolating models would require an appropriately modular ontology of the economic world. Such an ontology, however, is highly problematic and is not part and parcel of contemporary economics. On the other hand, even if such an ontology was accepted, the question would still remain how the adequacy of an isolating model could be determined. It would require, as Mäki admits, that we had an account of partial resemblance between model and an appropriately specified causal factor of the world. No such account exists in economics, and the comparison between models and the real world makes not use of such a notion, not even implicitly.

I therefore conclude that Mäki's isolation account, although it reinvigorated the realism of assumption debate, fails to solve it. Economic models are not tools for isolation, and modeling is not isolating.

## **A. Lehtinen (University of Helsinki), Normative isolations in economics**

Most discussion on assumptions in economic methodology concentrates on discussing various unrealistic assumptions and their different roles in positive economic theories. Thus far there has been much less discussion on various assumptions in normative economic theory and institutional design. In this paper, I will distinguish an assumption-type that is widely used in such exercises, and try to see how its role differs from some common types of unrealistic assumptions in positive theories. They may be labelled 'normative isolations'. They are assumptions that isolate a domain of consideration from other domains rather than a causal influence or a process from other influences. An

example may be useful here. The literature on optimal taxation commonly assumes that the various alternative tax instruments collect the same amount of money, the purpose of the exercise being to determine which tax instrument generates the least welfare loss in terms of distorting incentives. The assumption that the amount of money to be collected is to be the same does not isolate any causal influences, and it is not a construction in Sugden's sense either. Nevertheless, it makes sense to say that the purpose of such assumptions is to isolate something, namely, the welfare-effect issue from the budget-size issue.

These assumptions are typically eminently realistic in the sense that it is difficult to imagine why anyone would want to argue that they ought not to be made. In our example, if we want to study the relative distorting effects of different tax instruments, it is clearly necessary to assume that the amount of money to be collected is the same for the various instruments because without this assumption it would not be possible to compare two different instruments in the first place. It is equally clear that if the government makes a decision to implement a change in the tax policy, this will usually have the effect of altering the amount of taxes collected. However, it does not make sense to say that the assumption isolates (or abstracts from) the causal processes that bring about the change in the amount of taxes collected. This is because the assumption enables the normative comparisons between tax instruments, and the results of such normative comparisons are independent of the possible changes in the amounts of tax that the policy change generates. In other words, the model of institutional design has something to say about which tax instrument generates the best outcomes and assuming that the amount of taxes collected is the same with each instrument allows for explicitly formulating a policy recommendation - without the assumption the result would be tainted with irrelevant considerations. It is in this sense that the assumption isolates the normative judgment from other normative judgments. I thus propose to call such assumptions theoretical or normative isolations because they isolate a field of normative consideration from another.

Another example is the idea of trying to isolate considerations concerning allocative efficiency from those concerning income distribution. (Here the received view is that since the compensation criteria fail, it is generally not possible to distinguish these two from each other.)

**Saturday, 13 | 10.30-12.00 | 4A. INVISIBLE HANDS**

Symposium organized by N. Emrah Aydinonat (Ankara University)

The invisible hand is one of the most popular concepts in economics and many economists provide invisible-hand explanations of unintended social phenomena. Yet the invisible hand is a mystified concept. It is used in many different ways and with very different meanings. This makes the task of understanding the merits and demerits of invisible-hand explanations difficult. The aim of the present session is to contribute to the demystification of the invisible hand and reach a more enlightened view of invisible-hand explanations. The session is organised around the following basic question: What is the nature and value of invisible-hand explanations? The first paper of the session sets the ground by presenting a historical account of the invisible hand and its relation to ‘unintended consequences’, ‘spontaneous order’ and ‘conjectural history’. It depicts the invisible hand as a metaphor that embodies a certain explanatory strategy which emerged from the Scots’ desire to create an empirically grounded science of man that applies the Newtonian experimental method to moral matters (Craig Smith). In a similar vein, the second paper takes it that the concept invisible hand has a certain explanatory strategy attached to it and investigates the value of invisible-hand explanations. It argues that a valuable IHE is a partial potential theoretical explanation that explicates a possible way in which some real mechanisms interact and may bring about the unintended social phenomena under examination (N. Emrah Aydinonat). The third paper takes on the conjectural character of invisible-hand explanations, characterizes them as how-possibly explanations and articulates some ways in which such explanations might make a contribution to our explanatory understanding (Petri Ylikoski). Finally, the last paper elaborates a general account of invisible-hand mechanisms and of modelling such mechanisms. It shows that modelling invisible-hand mechanisms meets scientific realist canons (Uskali Mäki). Overall, the proposed session presents a historically and philosophically enlightened view on the concept of invisible hand and on models that provide invisible-hand explanations. The session particularly emphasises and examines the role and function of invisible-hand mechanisms in invisible-hand explanations.

**C. Smith (U. St. Andrews), The Scottish Enlightenment, the Invisible Hand and the Science of Man**

The metaphor of an invisible hand is perhaps the most famous and contested phrase to emerge from Adam Smith’s writings. This paper seeks to outline how arguments regarding unintended consequences, and what later became known as spontaneous order, pervade the work of Smith and the Scottish Enlightenment. It then seeks to relate this observation to the Scots’ desire to create an empirically grounded science of man that applies the Newtonian experimental method to moral matters. This theoretical preoccupation leads to the celebrated ‘conjectural history’ that typified Eighteenth Century Scottish historical writing. I’ll argue that the Scots’ use of invisible-hand explanations reflects not only the historical context of the increasing complexity of the commercial society of the Eighteenth Century, but that it also reflects their intellectual context through a dissatisfaction with prior modes of political and economic thinking. The desire to produce a science of man leads to a preference for explanation in terms of generalizations drawn from experience and the evidence of history, and this in turn provides ample evidence that many social institutions are the product of the unintended consequences of human social interaction.

Drawing on the work of Adam Smith, David Hume and Adam Ferguson, I'll demonstrate how this observation became the core feature of the new science of political economy. While using the examples of its application to the historical evolution of morality (Smith) and government (Hume) to shed light on its place in the development of the social sciences. The main contention of the paper will be that the recognition of the existence of unintentionally generated social orders shaped the Scottish Enlightenment's understanding of the nature and scope of social science. Moreover, I'll argue that modern theoretical social science must place orders that arise from unintended consequences at the heart of its investigations because they remain, almost by definition, the most genuinely 'social' phenomena that we encounter.

#### **N. Emrah Aydinonat (Ankara University), Value of Invisible-Hand Explanations**

The aim of the present paper is to clarify the way in which invisible-hand explanations (IHEs) work and explain when they are valuable. It is sometimes argued that invisible-hand explanations are valuable independently from their truth for they explicate the process that may have brought about the social phenomenon at hand (Ullmann-Margalit 1978). This suggests that IHEs may be valuable even if they are false, or even if they do not get the facts right. On the other hand, many people undervalue IHEs because they think that they are false. The present paper argues that both views are wrong about the relation between the truth of an IHE and its value. It is particularly argued that (i) the value of IHEs depend on the truth of their statements concerning the explanatory mechanisms, and (ii) IHEs may be valuable even if they contain false statements concerning particular exemplifications of the intended explanandum phenomenon. Well known examples of IHEs are theoretical explanations that explicate the interaction of some of the real mechanisms that may bring about unintended social phenomena. That is they contain true statements concerning these mechanisms. Nevertheless, these explanations contain false statements about some of the important (and indeed explanatory) facts concerning particular exemplifications of their explanandum phenomenon. The paper solves this apparent puzzle by way of showing that a valuable IHE is a partial potential theoretical explanation that explicates a possible way in which some real mechanisms interact and may bring about the unintended social phenomena under examination. The main individual mechanisms (i.e., which are deemed to be explanatory) depicted by a valuable IHE should be real, yet the process which is formed by the interaction of these mechanisms need not represent a particular process. Moreover, the mechanisms depicted by IHEs need not be the 'major' causes of particular exemplifications of the intended explanandum phenomenon. IHEs are valuable only when they show how a certain social phenomenon may be brought about by the interactions of some real individual mechanisms. They are valuable when they explicate possible ways in which some real mechanisms may interact.

#### **P. Ylikoski (U. Helsinki), Evaluating How Possibly Explanations**

Invisible hand explanations are often based on sketchy accounts of 'social mechanisms'. Even when these sketches are spelled out as formal models, they remain highly abstract and their intended explananda heavily stylized. These features make it difficult to assess the explanatory adequacy and relevance of these accounts. On the one hand, these explanations seem to fail to capture the richness of empirical reality, but on the other hand, some people judge these accounts to be primary examples of explanatory excellence – they are said to provide understanding of the essential features of the empirical phenomenon.

As these explanations often fail to be factually accurate, even the most successful ones are regarded as 'how possibly -explanations' (HPE). In this paper I will articulate some ways in which HPE's might make a contribution to our explanatory understanding. First, the HPE's are often judged unfairly: they intended explanandum is often much narrower than the standard of success advocated by the critics. All explanations explain specific aspects of the explanandum phenomenon, and the failures of the factual accuracy might be irrelevant from the point of view of the explanatory dependence the simplified model attempts to capture. In cases like these we could actually be wrong in labelling the model as a HPE. Another source of the explanatory value of HPE's is the comparative nature of explanatory evaluation: the value of an explanation is not judged simply by comparing it with the explanandum, but by comparing it to its alternatives. For this reason, the exploration of the space of possible explanations has explanatory input; if we do not know about the range of alternatives, our judgments about the explanatory contribution of a model are less secure. And if we are in position to rule out some of these possibilities as a consequence of our exploration, the remaining alternative gain in explanatory value. However, the value of exploring of the space of possible mechanisms is not limited to particular applications: the sometimes models provide more general explanatory understanding. The intended applications of HPE's are often misunderstood: even if the abstract model explains only a very specific aspect of the phenomenon, this aspect might be more general than the original phenomenon. For example, it is a fundamental mistake to assume that Schelling's checkerboard model is only about racial preferences in residential segregation. The example used by Schelling is only an illustration, and it could be that it just happens to be a badly chosen illustration. The model expands our explanatory toolbox, and it might prove to be useful in some new context. The explanatory input here is not an explanation of some particular phenomenon, but an expansion in our ability to construct explanations. Finally, models spelling out HSE's are sometimes misjudged by evaluating them individually. When models are illuminating, they come in clusters. The understanding they provide does not reside within the direct representative relation between the model and its target, but in the insight the differences in slightly different models provide via robustness analysis.

#### **U. Mäki (Academy of Finland), Modelling the invisible hand: Scientific realism in action**

Scientific realism subscribes to the weak thesis that science is or should be in the business of tracking truths about science-independent reality (Mäki 2005). Characteristically, a stronger view is suggested on which science is needed for accomplishing this task since important parts of that reality are not apparent. Invisible-hand mechanisms are among such non-apparent parts of the real world. Economics is one of the social sciences that is preoccupied with modelling various invisiblehand mechanisms (Aydinonat 2008, Mäki 2008). The paper elaborates a general account of [a] invisible-hand mechanisms in terms of non-apparent unintended consequences of intentional and non-intentional action (Mäki 1991) and of [b] modelling such mechanisms, while showing [c] how modelling invisible-hand mechanisms meets scientific realist canons (Mäki 1990).

**Saturday, 13 | 10.30-12.00 | 4B. INSTITUTIONALISM****J. C. Caldas (CES) & A. Costa (ISCTE), Claiming Deliberation for Institutional Economics**

The distinction between neoclassical economics and institutionalism is often misconceived in terms of an opposition between a “science of choice” (neoclassical economics) and an account of behaviour guided by instincts, habits or customs (institutional economics). The perspective of institutional economics thus conveyed is that of a theory purporting a very mechanistic, robot-like, view of the human agent.

In this paper, this dichotomy is vehemently rejected with two arguments. Firstly, neoclassic economics is not really a science of choice. The conception of rational choice of neoclassical economic theory depicts choice as the resolution of an optimization problem subject to restrictions. In this conception, choice pertains only to the selection of the best means to achieve fixed ends (thus excluding reflexion about the ends of action), and is always determined by the maximization of a scalar function representing the individuals’ preferences. Following others, we argue that in these circumstances individuals do not actually choose, they merely pick an option dictated by the resolution of the maximization problem given the set of alternatives and the restrictions.

Secondly, behaviour in institutional economics is not mere stimulus-response. Notwithstanding the fact that institutional economics gives a great emphasis to habit in human conduct, this does not mean that it excludes autonomy, volition or rationality. At the root of the above misconception, as shown by Hodgson (2004), is a notion of habit as non-reflective, stimulus-response behaviour, presently prevailing among economists, but foreign to the broader understanding of habit in institutional economics. In fact, habit in the institutionalist tradition, namely Veblen’s – which draws on the pragmatist view of Charles Sanders Peirce, William James and John Dewey – is not only compatible with reflection and deliberation, but a precondition for all thought. Rather than viewing habit-driven human conduct just in terms of a stimulus response behaviour, the pragmatist view of Peirce, James and Dewey, among others (Hands, 2001; Hodgson, 2004; Lawlor, 2006; Mirowski, 1987; Twomey, 1998), upholds that individuals are capable of recognizing and acting upon their habits. In the pragmatist perspective, there is obviously room for volition and rationality.

In claiming for institutional economics a concept of deliberation compatible with habit, we firstly address the notion of habit within the pragmatist institutionalist tradition with the aim of clarifying this concept and disentangling it from the current misconceptions.

We further argue that institutionalism needs an elaborated theory of deliberation. Intending to contribute to this development in institutionalism we turn once again to the pragmatist literature, namely John Dewey's *Human Nature and Conduct* (1922) in search for foundations. In fact, Dewey’s approach, by advancing a perspective on action open to imagination and creativity (Joas, 1996), highlights a conception of deliberation and choice which radically departs from the neoclassical conception of rational choice. In particular, Dewey sets a distinction between mere calculus (the choice of ‘rational’ choice) and deliberation, where deliberation is conceived as a process, in which means and ends are simultaneously explored and eventually discovered. Finally, we highlight some of the implications of deliberation thus conceived, namely in terms of collective action and institutional change.

### **C. Hirsch (Erasmus University Rotterdam), The Explanatory Boundaries of Rational Choice Theory in New Institutional Economics: The Case of Transaction Cost Economics**

Many commentators have distinguished between two different interpretations of Rational Choice Theory. On the one hand there is a 'psychological' reading, interpreting the theory as making claims about psychological processes. Adherents of this stance often draw on rich empirical evidence to argue that the actual behaviour of individuals systematically violates the axioms of rationality. On this basis they often propose replacing rational choice theory with more descriptively realistic conceptions of human behaviour such as bounded rationality. On the other hand defenders of traditional rational choice theory typically reject this criticism by arguing that the axioms of the theory have to be understood to serve the purpose of securing its 'coherence' to allow for drawing inferences between the preferences of agents and their behaviour and for making predictions.

While experimental and behavioural economists develop their research program on the basis of the former conception of rationality, neoclassical economists adopt the latter conception. In New Institutional Economics, however, it is not always clear which interpretation of rationality is employed. Admittedly, New Institutional Economists frequently point out their difference from the neoclassical mainstream by stressing that they are addressing different questions, but then again they often seem to continue to hold a 'coherence' account of rationality.

An interesting case in which both readings of rationality are entangled is Oliver Williamson's account of Transaction Cost Economics. In his work Williamson explicitly refers to Herbert Simon's account of bounded rationality. However, it seems that much of the core of Williamson's account can be reconstructed within the boundaries of the 'coherence' interpretation of rationality. Moreover, it appears that some problems of inconsistency in Williamson's later work are created precisely by the attempt to integrate psychological elements which are alien to traditional rational choice theory.

Disentangling the two notions of rationality in Williamson's account has implications for New Institutional Economics more generally. It brings up at least two related methodological questions. First, it raises the question of how traditional rational choice theory is suited for the purpose of studying institutions. Second, there is the question of how different theoretical traditions can be combined in institutionalist research without creating methodological inconsistencies.

In respect of the first question I will argue that traditional rational choice accounts have made successful contributions to explain the static and the constraining aspects of 'well-structured' institutions, but that they are less suited to explain dynamic aspects of institutions in which agents face few constraints on exerting agency.

Concerning the second question I will argue against an eclectic merging of different theoretical traditions and in favour of a methodologically informed pluralism in the sense of the exploitation of distinct methodological resources in order to address a particular institutional phenomenon. The latter practice allows for the back-tracing of partial explanatory successes and failures to certain methods and conditions of their application and thus allow for a more directed development and refinement of theories and models. To substantiate this claim I will use Transaction Cost Economics as an example to show that the introduction of notions of bounded rationality and the corresponding shift of the

explanatory weight from the constraints of a situation to the deliberation of the individual agent decreases rather than increases the explanatory power of a theory.

**W. Milberg (The New School for Social Research) & P. Spiegler (U. Massachusetts), The Taming of Institutions in Economics: The Rise and Methodology of The New, New Institutionalism**

A new wave of institutional economics has emerged, narrower in its conception of institutions than earlier institutionalisms, but more ambitious in its predictive power. Led by the work of Daron Acemoglu -- but with a growing number of practitioners in fields as diverse as economic growth, economic development and economic history -- this "new, new institutionalism" (NNIE) uses rigorous rational choice mathematical models in an attempt to capture the important role of institutions in the explanation of grand historical trends in power, politics and long-term economic change. Although this work has appeared in the top journals of the profession and in the books of its top university presses, it has received surprisingly little recognition in the wider economic community for its achievements. This paper provides a first attempt at methodological assessment of the NNIE. Although linked to older forms of institutionalism by its concern with the nature and functioning of institutions, the NNIE's central dependence upon highly reductive mathematical models marks a significant methodological departure from the Old Institutional Economics (OIE) of Veblen, Ayres and Commons, and the New Institutional Economics (NIE) of Coase, North and Williamson. The promise of such an approach is enormous, but so are the perils— while it has the potential to deliver on both the OIE's desire for a robust understanding of deeply socially embedded institutions as well as the NIE's desire for analytical rigor, it also faces the peril of creating overly-reductive models that are incapable of capturing the nuances of the social reality they are intended to represent. In the first part of the paper, we look to the history of institutional economics to argue that the NNIE is methodologically distinct from past institutionalisms. We briefly discuss the reasons for the emergence of the NNIE as the outcome of the methodological failings of the new growth and trade theories that emerged in the 1970s and as a result of the numerous current political debates related to the status of property rights protection. With the NNIE, institutions are no longer a separate or elusive aspect of social life, but just another feature that can be modeled with the standard tools of optimization. We introduce a new, four-step process by which the connection between model and data is created, involving delimiting, naming, solving and interpreting. This decomposition of the NNIE methodology reveals a narrowing of the conception of what constitutes an institution, a reduction of the degree of embeddedness of institutions and a greater distancing of the analysis from operational status. We conclude that while the NNIE tames the role of even the grandest institutions, in the sense of confining them to rational choice arrangements and explaining their role in a deterministic fashion, such a taming of institutions shows precisely why a richer institutional economics would be needed to address issues of economic, political and cultural change—especially over long periods of time.

**Saturday, 13 | 10.30-12.00 | 4C. METHODOLOGY****L. Boland (Simon Fraser University), On the economist's view of inductive reasoning**

If one reads celebrated philosophy literature concerning induction or “inductive logic”, one gets a very different impression than one would get by reading what economists say about induction. Part of the difference might be explained by noting that even David Hume struggled to deal with induction. Of course, he demonstrated that the usual notion of “induction by simple enumeration” was a logical impossibility. Even Francis Bacon, the Seventeenth-Century advocate of inductive method, understood the limits of “induction by simple enumeration”. Bacon thought there was a better way to do induction – by simply arranging observational data such that the right explanatory hypothesis would be obvious. But so far nobody has come up with a method that would do so. Yet Hume maintained that people do induction all the time – in their heads, so to speak. So, what do the economists associated with the Santa Fe Institute mean by induction and “inductive reasoning” (e.g., Arthur, AER 1994)? What do economists in general mean by induction? Surely, they do not mean “induction by simple enumeration”. This paper will provide answers to these questions and then show why there is little in common between what these economists and most philosophers and historians of science understand as induction.

**A. García de la Sienra (U. Veracruzana), Suppes' economics and its methodology**

For no less than fifty years, Patrick Suppes has produced not only a very influential methodology for the empirical sciences, economics included, but also has made interesting contributions to economic theory in which his important methodology of science is found in operation. The present paper intends to make a revision of Suppes' contributions to economics and its methodology in their systematic interaction.

Beginning with a classification of those works which can be considered as contributions to economics, or to scientific methodology, I proceed to discuss whether Suppes' contributions to economics can be deemed as contributions to received economic theories or —at least in some cases— provide one or several new paradigms for the field. This will involve a reconstruction of his economic theories in his own methodological terms.

Suppes himself has classified his own research in eight fields: (1) foundations of physics; (2) theory of measurement; (3) decision theory; (4) foundations of probability, and causality; (5) foundations of psychology; (6) philosophy of language; (7) education and computers; and (8) philosophy and science. It is clear that at least the theory of measurement, decision theory, foundations of probability, and causality, foundations of psychology, and philosophy and science —i.e. at least five of these fields— contain contributions to economics and/or economic methodology. The challenge is of course to provide a compact but complete and systematic view of these contributions.

The challenge is great, if we take in consideration the fact that only on methodology, probability, and measurement Suppes has produced about 120 papers. To this should be added those papers in psychology relevant to his economics (books apart). Fortunately, Suppes is willing to provide some advise on selecting among these only those which contain the “essence” of his work on the field. The bibliography of the paper will certainly

contain the list of all of Suppes' works that have made contributions to economics and/or economic methodology,

Among his most recent publications on economics and/or economic methodology, perhaps the most important single item is "Rationality, Habits and Freedom" (2001). In this paper Suppes unites some of his views on psychology and measurement to provide a stochastic theory of consumer behavior, and proposes entropy as a measure of what he calls 'market freedom'. In this paper some of Suppes' methodological views can be seen at work, including his conceptions on experimental economics. My paper will focus initially on this item, but of course Suppes has said much more on economics or methodology than what this paper expresses. A hypothesis leading my work is that Suppes has a full-fledged theory of consumer behavior that presents itself as an alternative to classical demand theory as based upon the usual notion of preference. In Suppes' view the psychological concept of association plays an important part, but this doesn't mean the choices of the agent are entirely arbitrary: there is some sort of law, albeit stochastic, regulating the choices. One question, among the many that can be addressed to Suppes' economics is whether a sort of stochastic version of general equilibrium can be defined for agents characterized in these terms. This and other questions will be considered in the paper.

#### **F. de Sousa (Technical University of Lisbon), Old Hopes and New Avenues: Transdisciplinarity and Economic Methodology**

Although scientific research has become highly interdisciplinary, this attitude is still regarded with some suspicion by 'mainstream' economists. Despite having been in great difficulties under severe attacks for almost a century, the neoclassical view remains highly dominating and 'heterodox' economics, including a multitude of approaches, have not succeeded in dethroning the canons of neo-classicism. There have been attempts to broaden the conventional assumption of rationality from inward the context of neo-classical theory, but this quest has, naturally, been more strident from the part of those seeking for alternative frameworks.

In the early nineties, in the United States and Europe, economists which included Nobel laureates have issued a plea for pluralism in economics. Implicit in the concept of pluralism is an openness regarding the visions of the discipline, its ontological commitments, the adoption of alternative methodological frameworks and heuristic tools. But the concept of pluralism also involves an openness towards other scientific fields and the acknowledgement that economics can benefit from, and contribute to, research being carried out in those fields.

The aim of this paper is to assess cross-disciplinary research aiming at clarifying mechanisms involved in decision making and focusing particularly in the way preferences are shaped by evolutionary processes. The interest in exploring this theme has tremendously increased in recent years. The need for an interdisciplinary collaboration is generally acknowledged, but remains the road less travelled by economists. It was championed by Simon, who laid the basis for the development of new fields of scientific inquiry. It was also encouraged by research networks whose foundation can be traced back to Kenneth Arrow and Amartya Sen.

Walking on their footsteps, major projects aiming at promoting transdisciplinary research were carried out throughout the last decade. They have drawn on ideas from economics, evolutionary biology, anthropology and psychology. Their goal was not to articulate unified theories, but to gain insights about cognitive processes used by humans to arrive at their

behavioural repertoire, and at formulating and testing analytical models of behaviour and cooperation. As a methodological strategy, experimental economics played a key role in these programs that bounded disciplines analytically by relying on a common lexicon of game theory and a consistent behavioral methodology. One of the main assumptions they were based upon is an extended concept of bounded rationality taking into account cognitive tools, emotions and cultural contexts. Indeed, there is evidence that cultural and biological evolutionary processes can lead to mental models of problem solving accounting for the diversity of patterns of reciprocity and cooperation found in social groups.

While surveying these experiments and assessing their conclusions, our goal is twofold. We wish to highlight their contribution for the formulation of valuable heuristic tools capable of clarifying problems that haunt our discipline where the economic agent is conceived as a homo behaviouralis, while asking ourselves if the theoretical limitations of these programs compromise their potential for providing a basis for the reconstruction of economics.

## Saturday, 13 | 10.30-12.00 | 4D. RATIONALITY

**C. Hoewel (Catholic U. of Argentina), Endogenizing Preferences: A Personalist Approach**

Many behavioral experiments are showing evidence that challenges the main assumptions and predictive ability of neoclassical economics. Although this evidence is mainly a result of a cooperative relation between psychology and economics, it can also be enriched by some developments that have taken place during the last decades in the field of philosophy. In this paper I will try to introduce the reader into what I think is the main contribution of the personalist philosophy of Paul Ricoeur to the specific problem of endogenizing preferences.

In the first part, the paper will describe Ricoeur's hermeneutical phenomenology of human action. This phenomenology shows the triadic structure of the human person composed by what Ricoeur calls self-esteem, solicitude towards the other and living within just institutions. According to Ricoeur, a personal action cannot be described in a naturalistic way, merely as a fact that follows from a series of predetermined causes previously contained in the agent and therefore reduced to the attributive proposition "S is P." While a semantics of human action reveals the individual dimension of action, its pragmatic dimension demonstrates its triadic dimension that shows the structure "A does X in the Y circumstances."

The second part of the paper connects the Ricoeurian triadic dimension of human action to some of the results of behavioral economics. The dimension of self-esteem, in the first place, reveals the ambiguous character of self-motivated actions. It implies, on the one hand, a self-interested conduct assumed as a necessary consequence of a series of explainable, consistent and stable causes (called needs, desires or preferences by rational choice models) that follow an individual fixed identity that Ricoeur calls *idem* identity. But, on the other hand, the self-esteem dimension also shows actions like promises, compromises, initiatives and other intentional actions –well demonstrated by numerous experimental games - by which the agent affirms herself as a self but not in an autistic way but as a person (*ipse* identity) capable of intermingling her intentions within the transsubjective events of the world.

In the second place, the second dimension of Ricoeur's triad is solicitude to others, also very close to behavioral economics, especially in what it has to do with the reciprocity principle. In Ricoeur's view, human action is conceived only as interaction, under innumerable forms that go from cooperation to competition and conflict. But a real interaction is always, according to him, a movement of the self towards the other that responds to the call of the self from the other, and not a movement towards a complete alterity. That is why the basic model to understand interaction is always reciprocity.

Finally, the dimension of living within just institutions, is born from the fact that there are relations with the other that do not consent to be built on the model of reciprocity but have to be based on the idea of justice. The other, in this case, is the one that is in front of me but without face, it is the each one of just distribution. However, this institutional dimension is at the same time endogenous to personal and interpersonal interactions. Indeed, institutional rules are previous to each of the individual agents, taken individually and also in the interaction relationship. Therefore, as it is also being shown by behavioral

economics, the term institution has not to be considered in a political, juridical or moral way but in the sense of a teleology that regulates an action.

The third and final part of the paper will try to show the possible advantages of using Ricoeur's concept of person to endogenize preferences in relation to other concepts, such as individual, self, subject, community or society. While these other concepts lead to monistic or dialectical explanations of preferences, Ricoeur's personalism helps to understand their heterogeneous origin and variability based on the triadic dimension of the human person.

### **J. Kuehnelt (NYU), Explaining and Justifying Norms: Why Does Economic Theory Only Refer to One Type of Individuality?**

Economists often focus on a descriptive analysis, which tries to specify the benefits of certain norms, while philosophers are usually more interested in a normative project that is supposed to legitimize norms convincingly. Still, the results of both projects strongly depend on the model of individuality that is presupposed. Firstly, I would like to argue that reductionist models of individuality which refer either to egoism or to altruism are not convincing for many contemporary societies. Secondly, I would like to suggest a modified Humean position that is capable of explaining both egoism and altruism.

The traditional understanding of Hobbesian theories is compatible with the classical model of homo oeconomicus. Only instrumental rationality and basic egoistic interests are presupposed not only to explain the benefits of norms but also to justify them. Consequently, even a moral sceptic is supposed to be convinced of the gains of certain legal norms. However, many scholars criticise the descriptive statement that individuals always act as benefit maximizing egoists. Furthermore, not only James Buchanan criticised the normative project of contract theorists, as they try to show the potential gains of cooperation relative to an unregulated state of nature in which everybody is similarly powerful. If the inequality of individuals and their egoism is taken seriously, it becomes clear that the distribution of power influences the results of contract theories: At worst, given a strong inequality of power, even a slavery state would have to be called legitimate, as both masters and slaves could not obtain a better result relative to their power. However, this result strongly depends on the assumption that all or most individuals are in fact benefit maximizing egoists.

David Hume argues that individuals are not only rational egoists but also have sympathy towards others. In opposition to similar approaches, for instance John Rawls idea of fairness, Hume's position is more attractive, as he is capable of explaining altruism within a minimalistic theory. He provides a better model of individual behaviour that avoids the often criticised reductionism to egoism within the model of homo oeconomicus and he provides a justification of certain norms which are closer to our moral and legal intuitions. However, the difficulty arises that not everybody shares this sympathy towards others; as at least some individuals will not turn down the opportunity of a sufficiently beneficial free ride. Furthermore, the moral sceptic will reject Hume's justification of norms, as he will deny having this sympathy and the corresponding altruistic interests. This rejection should be taken seriously as the proposed norms are only sufficiently beneficial for everybody if all individuals share his minimalistic understanding of sympathy and altruism.

In the second part of my presentation I will argue that Hobbesians as well as Humeans can be criticised as reductionists because they argue relative to one type of individuality; Hobbes refers to egoism and Hume assumes that everybody has a certain amount of

sympathy towards others. This problem becomes clearer relative to recent empirical research, for instance, the work of Colin Camerer and Ernst Fehr, as they suggest that both types of individuality exist in contemporary societies. In this context, I will outline a minimal understanding of moderate altruism which is close to Hume's conception. Furthermore, I will argue that a modified Humean model can also explain egoism as a lack of sympathy. This more abstract model is able to integrate both types of individuals, and hence could be the basis for a better description of individual behaviour and for a more convincing justification of norms. At least some benefits of this perspective will be sketched at the end of the presentation

### **J. Zamora (UNED), Towards a normative naturalist theory of rationality**

In this paper, a framework for the study of knowledge and action is proposed, mainly inspired by some approaches to pragmatics. Combining the Sellarsian metaphor of rationality as the ability of being a player in the space of reasons, with David Lewis' reading of Wittgensteinian language games as governed by a set of scorekeeping rules, and with the pragma-dialectical theory of argumentation, I conceptualise deliberations (both individual and collective) as processes governed by inferential norms, in which each speaker becomes committed to some claims because of the assertions she has made, because of the happening of some other publicly observable facts (including the assertions of some other people), or because of the way the relevant inferential rules govern how some previous commitments give place to others. The obligations and entitlements a speaker has in a certain moment constitute her deontic score, which not only includes the propositions she must or can assert, but also the actions she must or is allowed to perform.

Figure 1 depicts the basic elements of this framework. An agent's epistemic state is described by two elements: her deontic score (i.e, the claims and actions she is committed or entitled to; or, more exactly, an indication of how much committed or entitled she is to each possible relevant claim or action; these claims include not only factual statements, but also judgments of preference or value), plus her psychological state (i.e., her beliefs and desires, which don't need to coincide with her commitments). An essential element of one agent epistemic state consists in the representation of other agent's scores, allowing public deliberation and collective action as something deriving from each actor 'keeping the score' of others. Epistemic states can change both by their internal dynamics or by the influence of external events, though their two constitutive elements change according to their own laws: your deontic score evolves according to the inferential norms you are subjected to, but your beliefs and desires change according to psychological regularities or tendencies, which lack any normative content. The distinction between these two planes, the deontic and the psychological, is essential for a normative epistemology, for only the former is under our voluntary control, and, no less importantly, can be subjected to public checking. In particular, it may happen that a process of deliberation starts with your beliefs nicely aligned with your doxastic commitments, but it ends in a conclusion you simply don't believe, even if you fail to identify some wrong step in your reasoning. The same misalignment can exist between your practical commitments and your desires. The final element in the figure shows that some epistemic states will be followed by actions (including verbal actions), not in a deterministic way, but according to some statistical law that links the degree of commitment you have in performing an action, and the probability of your performing it. Lastly, any action can constitute an external event for other agents, and, of course, actions can trigger, due to natural processes of causation, other external events that will affect the epistemic states of the agents.

This schema also allows to describe collective agents (with their own deontic scores kept by other actors, but without their own psychological states) and institutions. By the latter I mean a particular set of interrelated inferential norms connecting the normative scores of a group of agents. An epistemic institution is one whose output is mainly constituted by doxastic commitments (more than by practical obligations). Some authors (e.g., Kusch, Faulkner) have defended a communitarian view according to which epistemic practices only make sense within groups or communities. The opinion defended here is that humans can only master a rich system of rules by learning them from others, but this is only possible thanks to the individual's ability to interpret the behaviour of others according to some innate cognitive patterns; you cannot see the inferential norms your neighbours are following: the only thing you can do is to guess what these norms are; this proceeds by trial and error, not till when the actual inferential norms you master happen to be identical with those of your neighbours, but only till the moment when no one has any further motivation to revise them, and this can happen in a social state in which each individual has some slightly different interpretation of the inferential norms from the one other members of the group may have. The game-theoretic notion of equilibrium can illuminate this.

The view of 'knowledge' that derives from this description of epistemic practices is just the set of claims an individual or a group is committed to, by the working of the inferential norms they have, and of the events and actions that have affected the evolution of their epistemic states. This is what the individual or group take as knowledge. The philosophical question is how to assess this knowledge and the epistemic practices supporting it. An epistemic institution will tend to produce doxastic commitments with some properties rather than ones lacking those properties; we can call implicit epistemic values those goals that an epistemic institution seems to be promoting. Of course, the agents can also have themselves an explicit view of which ones are the goals they are promoting. A normative assessment of the set of claims attained by those agents could consist, then, in showing what other claims would have been better according to their own standards (and the same can be made, at a higher level, to assess a group's epistemic norms). Furthermore, it can be discussed whether the goals of a group are valuable or not according to our own view of what is valuable (cf. Zamora Bonilla, 'Science as a persuasion game', *Episteme*, 2006).

Lastly, this approach is also a naturalist one in the sense that a schema like that of fig.1 can be interpreted as a blueprint for producing empirical models about individual and social facts, models that can be subjected to empirical testing. So, the approach allows to generate a scientific research programme in the social and cognitive sciences, starting from ideas derived from philosophical theories. It is also shown how the approach can be shed light on some recent debates in epistemology, as the connection between the 'morally guided' (e.g., epistemic agency, virtue epistemology) and the 'rational choice' (e.g., Bayesianism, game theoretic) approaches to epistemology, the question of epistemic progress and relativism, the paradoxes of rule following, the theory of rational action, the nature of social norms, and the ontological status of social groups.

## Saturday, 13 | 12.30-14.00 | 5A. ECONOMETRICS

**A. Moneta (Max Planck I. of Economics), Evidence for Causal Inference in Econometric Models**

According to an established tradition in econometrics and in the philosophy of science, in order to learn causal relationships in a non-experimental setting one needs both some background of causal knowledge and an appropriate statistical model of the data. Hoover (2007) has recently argued that these two requirements should be carefully distinguished and that the establishment of a probability model does not require causal presuppositions. In this paper I elaborate some these about the kind of evidence used to draw causal relations in econometric models which qualify Hoover's statement.

First, this statement is true in the sense that, for the sake of establishing a probability model, one does not need to know in which directions causal relations operate (if present). For example, knowing that some causal directions are not possible (e.g. cycles) is extremely useful, and in some cases necessary, to infer causal structures from evidence of statistical associations, but this knowledge is not needed in order to build a probabilistic model, and in particular in order to test conditional independence among the variables. Furthermore, this type of knowledge does not give any contribution to a better specification of the probabilistic model.

The situation is quite different when we consider background knowledge about the "mode" in which causal relations operate. A crucial mode in which causal relations operate is determined by the functional form which describes these relations. Knowing the functional form, I shall argue, is extremely relevant to test features of the probabilistic model. For instance, knowing that the functional form of the causal relations within a group of variables is linear permits the researcher to use straightforward statistics to test conditional independence relations among them. Another important causal presupposition is knowledge about the level of aggregation in which causal relations operate. For instance, knowing that causal relations operate at an individual level and that the observed variables are aggregate (e.g. sums or averages) of individual variables brings some important consequences for the specification of the probabilistic model. I will argue that the role played by these two presuppositions, that regard the mode in which causal operations operate, has often been neglected in the econometric literature.

My final thesis is that these presuppositions are not per se necessary to build a probabilistic model, although they give in general a significant contribution to better specify it. Referring to the literature on nonparametric statistics, I will show how, in case of lack of knowledge about the functional form, one can build, under certain constraints, probabilistic models allowing causal inference.

**A. Spanos (Virginia Tech), Theory Testing in Economics and the Error Statistical Perspective**

The paper discusses the problem of *theory testing* in economics and argues that addressing the problem requires understanding the nature and changing roles of theory and data in the development of economic thought, along side of the shifting philosophies of science which explicitly or implicitly find their way into economic theorizing and econometric practice.

It is argued that the agonizingly slow progress in the theory-data confrontation is primarily due to the fact that economic theory has consistently been afforded a dominating role over data. Indeed, one can make a strong case that economic theory has neither being properly tested nor has its development been curbed by real-world data. Attempts to redress the balance and give data a more substantial role in theory testing were frustrated by several inveterate problems, including: (i) the huge gap between economic theories and the available observational data, (ii) the difficulty of assessing when a fitted model ‘accounts for the regularities in the data’, and (iii) the problem of relating the statistical and substantive subject matter information.

It is argued that these problems can be better understood when placed in the context of a broader modeling framework which brings out the need to deal adequately with three interrelated modeling stages:

(A) *from theory to testable hypotheses*: fashioning an abstract and idealized theory  $\mathbf{T}$  into hypotheses or claims  $h$  which are testable in terms of the available data  $\mathbf{z}_0 := (\mathbf{z}_1, \mathbf{z}_2, \dots, \mathbf{z}_n)$ , often observational (non-experimental),

(B) *from raw data to reliable evidence*: transforming a finite and incomplete set of raw data  $\mathbf{z}_0$  – containing uncertainties, impurities and noise – into reliable ‘evidence’  $\mathbf{e}$  pertinent for theory appraisal,

(C) *confronting hypotheses with evidence*: relating  $h$  and  $\mathbf{e}$ , in a statistically apposite way, to assess whether  $\mathbf{e}$  provides evidence for or against  $h$ .

Link in (A) proved to be particularly challenging in economics because of (i). Link (B) brings out the problem in (ii) as an example of establishing the reliability of the information contained in the data independently of the theory in question. The link in (C) relates to (iii) and raises broader issues that have to do with how statistical inferences pertain to substantive information; the last link that gives rise to ‘learning from data’.

The primary objective of this paper is to make a case that the *error statistical perspective* (see Mayo, 1996) offers a highly promising framework in the context of which we may begin to get clearer on some of these crucial philosophical/methodological problems and make headway in their resolution. This framework enables one to foreground as well as work out solutions to the problems and issues raised by attempts to successfully deal with the links in (A)-(C), by formulating a refined version of the Fisher-Neyman-Pearson statistical inference framework. This puts us in a position to understand some of the problems raised by contemporary methodological discussions in economics/econometrics and sets the stage for making progress in an area where chronic problems have stumped any significant advancement for over three centuries.

### **G. Sucarrat (U. Carlos III Madrid), Econometric Reduction Theory and Philosophy: The First Stage in Hendry’s Reduction Theory Revisited**

The reduction theory of David F. Hendry provides a comprehensive probabilistic framework for the analysis and classification of the reductions (simplifications) associated with empirical econometric models. However, it is unable to provide an analysis on the same underlying probability space of the first reduction—and hence the subsequent reductions—given a commonplace theory of social reality, namely that the course of history is indeterministic, that history does not repeat itself and that the future depends on the past. Moreover, it cannot provide reduction analysis on the relation between continuous and discrete time models. Using concepts from philosophy this paper proposes

a solution to these shortcomings which in addition permits new reductions, interpretations and definitions.

The paper is available as <http://www.eco.uc3m.es/sucarrat/research/hendry.pdf>

**Saturday, 13 | 12.30-14.00 | 5B. RATIONALITY AND CHOICE**

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**M. del Corral & M. Jiménez Buedo (UNED), Choice and Commitment**

Our paper is motivated by a recent debate collected in *Economics and Philosophy* (21: 1, 2005) in which a number of contributors discuss Sen's position on the incapability of Rational Choice Theory to integrate commitment-driven actions. This debate stems from Sen's influential 1977 "Rational Fools", whereby commitment-based actions cannot be accommodated by RCT since these actions "break the link between individual welfare and the choice of action". This discussion presents a dialogue between Sen and Pettit, Hausman and other contributors and is representative of the current philosophical debate on the relationship between RCT and commitment in that the dispute is focused around the issue of the way commitment affects goals, preferences and actions, and the relationship between commitment and altruism. These debates are centered on the question of the relation between commitment-driven action and the formation of preferences, and they provide answers to Sen's critique in terms of the capability of RCT to accommodate the agent's internal deliberation capacities and the possibility of counter-preferential choices.

However, as we argue in this paper, this and related debates on the relationship between RCT and commitment are defective in many respects. First, they do not provide a clear definition of commitment, and the implicit definitions of the concept employed are normally vague. Second, and more important to our argument, they fail to distinguish the level at which Sen's critique can pose a challenge to RCT. Neither Sen nor the defenders of RCT against his critique have taken a clear position on the status of RCT in social science explanations. Thus they fail to clarify whether they take it as an analytical tool, as useful narrative or description of either ideal or real situation, or as an encompassing theory of action. We argue that commitment cannot stand as a definitive challenge for RCT at any of these levels. As a matter of fact, the concept of commitment is currently used unproblematically by many social science works employing RCT as their theoretical framework.

We contend that it is possible to define commitment in terms that are entirely compatible with the RCT framework and that this is clearly a necessary step for those aiming at protecting RCT from Sen's critique. This entails, though, that the definitions of commitment employed, cannot fail to distinguish between the content of the commitment and the commitment itself, and therefore cannot be related to the notions of preference, beliefs, or goals. We propose an alternative definition that distinguishes between a commitment and its content; in our account, commitments are best conceived, in terms of RCT, as instances of actions where a choice is not made. In our paper we further develop this definition showing how it is compatible with RCT seen as a tool. We therefore conclude that current philosophical debates on the supposedly problematic relationship between commitment and RCT are not well equipped to deal with this issue in ways that are relevant to the current social science practice.

**J. C. García-Bermejo (U. Autónoma Madrid), A Refinement and Extension of the Standard Rational Choice Rule.**

It is well known that when preferences are not acyclic, then the standard maximization choice rule does not solve all the choice problems that can arise in a space of alternatives. But it is perhaps not so well known that even when this rule yields a solution, it may not be satisfactory. The paper focuses on this question, and it defines a choice criterion for finite choice problems such that maximizing with respect to this criterion refines the standard maximization rule.

Begoña Subiza and Josep Peris (2005a: 505) show that maximal elements may be dominated even if preferences are complete and quasi-transitive. In a similar vein, the paper shows by means of several simple examples that, if preferences are not transitive or if they are not complete, then the solutions yielded by the maximization rule may not be satisfactory.

The possibility that preferences are not complete is familiar to any agent. And the possibility that preferences are not transitive has been extensively discussed in the literature. Fishburn (1991) summarizes the discussion.

Moreover, it is known that when preferences are not acyclic, the maximization rule does not give a solution to every choice problem. And, therefore, it is not a choice function. In addition, if preferences are not both complete and transitive, maximization may fail to satisfy some of the standard consistency properties of choice functions.

In the paper a choice criterion is built upon the given individual or social preferences so that maximizing with respect to this criterion (a) solves any choice problem at least as well as standard maximization does, and (b) it may offer a more satisfactory solution when the standard solution may be improved. In addition, maximizing that choice criterion gives always a non-empty solution to every choice problem, even when standard maximization does not give any solution. It also satisfies all the standard consistency properties on choice functions, independently of the properties that the preference relation may satisfy. It should also be remarked that when preferences are complete and transitive, maximizing in respect to the new choice criterion reduces to standard maximization.

However, maximizing with respect to the choice criterion mentioned above fails to fulfill a significant property that the standard choice rule does satisfy – Arrow's independence of irrelevant alternatives. The reason is that to solve a choice problem, the criterion uses all the available information about the comparisons between every two alternatives, instead of using only information about the comparisons between the options that can be chosen in the choice problem that is being solved.

The choice criterion and the choice rule that prescribes to maximize with respect to this criterion do not aim at tracing rational choice so clearly as standard optimization does. On the contrary, as soon as the standard optimization rule cannot be applied, doubts about the correct sort of behavior may appear. The question is then whether a given proposal is reasonable enough, or whether there is another one that can be considered to be a more reasonable option.

**C. T. DesRoches & Th. Wells (EUR), Pluralism about rationality in economics: theories as tools**

Current debates about rationality in economics often revolve around the question of truth while disregarding the contexts and purposes of development and use. Contributions like that of Viktor Vanberg ('Rational Choice vs. Program-based Behavior', *Rationality and Society*, 2002), are often linked to expectations for a unifying and veristic paradigm of rationality, which would win the prize of universal respect as the true foundation of behavioural science. In this paper we argue that rationality covers a variety of properties which are more or less prominent in different contexts, from efficiency in neo-classical economics to deliberation in political science, and that it is unlikely that any single overarching paradigm will succeed in covering every property that behavioural science might need. Instead, concepts of rationality should be assessed according to how well they perform relative to a particular problem-context. This performance is best understood by considering the original problem-context of their construction.

We argue for an instrumentalist approach of considering concepts of rationality as 'tools' for answering particular questions. Social scientists frame problems in ways influenced by the particular concerns, concepts, and methodology of their fields. The tools they develop will thus strongly depend on their original context and goals. Concepts of rationality are developed for different purposes and are dissimilar even between neoclassical economics and behavioural economics. We use the example of Rational Choice Theory (RCT) to illustrate how theories of rationality are developed within certain constraints. Criticisms that the assumptions of RCT are false still fail to persuade most economists to give it up because RCT is not intended to be a thesis about human psychology. RCT strictly imposes coherence restrictions over preferences which make it quite suitable for formally modelling the causal structure of strongly constrained situations. However, RCT is not effective for understanding how particular individuals reason and choose because it was not designed to do this (Satz, & Ferejohn 'Rational Choice and Social Theory', *Journal of Philosophy*, 1994).

Considering theories of rationality as tools developed for particular purposes in particular contexts focuses the attention on how and why they work, rather than their potential truth-value. This has a number of interesting consequences. First, it suggests that effective criticism will focus on the constraints of a rationality concept's construction for current or extended applications. We evaluate various alternative concepts of rationality to demonstrate that they are designed to answer quite different questions. Second, concepts of rationality are seen to be limited in terms of the kind of questions that they can ask. As Gary Becker and other 'expansionist' neo-classical economists have demonstrated, it is possible to deploy RCT in non-traditional domains, and this amounts to finding ingenious ways to ask neo-classical questions outside the original problem situation. Expanding the scope of a tool therefore requires convincing others that its original problem context is relevant there too, i.e. that these questions are the right ones.

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**Saturday, 13 | 12.30-14.00 | 5C. SCIENTIFIC PLURALISM AND EPISTEMIC DEMOCRACY**


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Symposium organized by  
R. De Langhe (FWO) & J. van Bouwel (Ghent U)

In recent years, scientific pluralism has become an intensively discussed issue in philosophy of science and social epistemology (e.g., Kellert, Longino and Waters, 2006). Economics provides an interesting domain of application for insights concerning scientific pluralism; so-called heterodox or real-world economists have been very active in challenging orthodox or mainstream economics and manifesting themselves as defenders of pluralism. One could raise the question whether these defenders are really pluralists themselves (cf., Van Bouwel, 2004), and – the question that follows promptly – what does it mean to be a real pluralist in economics; what does it require from individual researchers, economic theories or the discipline in globo (cf., De Langhe, forthcoming)? Answers to these questions are not straightforward: dissent does not equal to pluralism, pluralism does not necessarily further our epistemic goals, and dissent might obstruct our epistemic goals. Nonetheless, dissent, pluralism and the furthering of our epistemic goals appear to be intricately related. Our session aims to investigate how these topics play out in current discussions in economics and will make an attempt to clarify their interrelation.

More specifically, the session aims (1) to discuss possible understandings of pluralism and frameworks to interpret dissent among economists in order to make sense of the diversity and dissent we are faced with and (2) to suggest fruitful strategies or ways of dealing with dissent. In what way can dissent be endorsed by scientific pluralism? Can a thorough analysis of pluralism and dissent help us to formulate a form of epistemic democracy, briefly described as giving different epistemic values and interests in economics an equal voice? In what way can this further our epistemic goals?

Jeroen Van Bouwel (*Ways of dealing with scientific pluralism: Inquiring Helen Longino's epistemic democracy*) wants to evaluate the contributions of some social epistemologists concerning scientific pluralism and the (ideal) interactions between conflicting views. Therefore, he will analyze the heterodox economists' discussion on the limitations of neoclassical economics in light of Helen Longino's CCE (critical contextual empiricism)-norms. What does it teach us about the heterodoxy's interpretation of pluralism and their strategies in dealing with dissent? What does it teach us about Longino's CCE-norms; do they provide us with a blueprint for epistemic democracy in economics?

Ioana Negru (*From fragmentation to ontologically reflexive pluralism*) states that pluralism is a problem that is confronted by both insiders and outsiders when they enquire into the nature of economics: the fact that there are many diverse organising principles about one and the same phenomenon. Pluralism both relativizes differences and magnifies the decision-problem. As a consequence, the merits or otherwise of pluralism are quickly brought into focus. This paper aims to examine whether the pluralist treatment of different issues in economics is a good or bad development. More specifically the paper seeks to explore how economics can arrive at a position where pluralism fosters a form of conceptual and methodological openness that is critically tolerant i.e. one where alternative ideas are not dismissed out of hand but at the same time have to stand up to an accepted framework of critical analysis in order to gain acceptance within the canon of economic analysis. Opposition to the pluralist project in part stems from a misunderstanding of the

nature of pluralism: pluralism does not advance the acceptance of all theoretical and methodological frameworks concerning certain issues/phenomena in economics (i.e. relativism). Pluralism in the context of a Kuhnian incommensurability of paradigms is thus to be rejected. A pluralist approach can be beneficial as long as different strands of thought are connected. The key to making pluralism work is the actual exchange of ideas and not a plurality without conversations. This is akin to Mill's espousal of free speech as a mechanism of subjecting ideas to criticism. Habermas has argued that diverse theoretical and empirical approaches are needed to support informed social criticism. Thus it is important to remember that social sciences can still produce truth in their own mutually exclusive discourses. We contend that it is not possible to advance a concept of pluralism that is based on tolerance and respect of other perspectives but which is devoid of criticism.

Rogier De Langhe (*The tradeoff between specialization and diversity*) begins by noting that dissensus and pluralism both presuppose diversity. The relation between dissensus and pluralism on the one hand and the furthering of our epistemic goals on the other is then analysed from the perspective of the trade-off that exists between specialization and diversity. Both specialization and diversity have been attributed with the virtue of furthering our epistemic goals. However, because of the trade-off, neither of them should be maximized because then the other would disappear: total specialization leads to an intellectual monopoly, while total diversity entails fragmentation and incommensurability. Appreciations of dissensus and pluralism will thus require fine-tuning between specialization on the one hand and diversity on the other. How this might play out in the field can be illustrated with an example from the technology industry. Interestingly, software companies are confronted with their own version of the trade-off between specialization and diversity. On the one hand, more than in other industries, software companies tend to monopoly. On the other hand, their products need constant innovation, and innovation requires diversity. As software companies began realizing that their monopoly not only made them powerful but also vulnerable, they started weakening their own monopoly in their own search to fine-tune specialization and diversity, e.g. by releasing source codes. The release of source code shows interesting analogies with the idea of epistemic democracy, as I will advocate.

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**S. Hartmann, G. Pigozzi & J. Sprenger (Tilburg University), Consensus, Compromise and Judgment Aggregation**

Judgment aggregation studies the aggregation of yes-no judgments of the members of a jury on logically interconnected propositions into a consistent collective judgment set. For example, if the propositions in question satisfy the integrity constraint  $A \wedge B \Leftrightarrow C$ , then only the four collective judgment sets  $(1, 1, 1)$ ,  $(1, 0, 0)$ ,  $(0, 1, 0)$  and  $(0, 0, 0)$  are consistent and hence admissible. (Here and in the remainder, “1” represents a ‘Yes’ judgment, and 0 a ‘No’ judgment.) As the discursive dilemma shows, proposition-wise majority voting will not in general lead to a consistent collective judgment set. To arrive at a consistent collective judgment set, three procedures have been discussed in the literature: the premise-based procedure (PBP), the conclusion-based procedure (CBP) and the distance-based procedure (DBP).

According to these procedures, the jury can accept a judgment set that only a few (or even none) of the members of the jury voted for. This raises the question whether such a decision is really acceptable. Clearly, a decision based on PBP, CBP or DBP amounts to a compromise, and not everybody will be happy with the decision. The jury members agree to go along with the will of the others. The preferred solution, however, is to arrive at a consensus, whereby every jury member is in agreement with the final decision. This goal of this paper is to develop a model for the emergence of consensus in a judgment aggregation setting and to ask how this new aggregation method compares with suitably generalized versions of PBP, CBP and DBP.

Jury decisions allow a wide range of judgments and expertises to enter into the decision-making process. But how do we go about combining the varying judgments typically present in a jury charged with a decision? The literature on judgment aggregation typically assume *anonymity* according to which all jury members have equal weight in determining the collective judgment set. This assumption is too strong in situations where a committee has to make the *right decision*. In these situations the full available information of the jury members should be taken into account, and this information comprises the reliabilities of the various jurors. Note that different jurors might disagree on the reliabilities they assign to each other. For a jury of  $n$  members, let  $r_{ij}$  ( $i, j = 1, \dots, n$ ) be the reliability that juror  $j$  assigns to juror  $i$ , with  $r_{ii}$  being the reliability that juror  $i$  assigns to herself. We ask: How can this information be included in PBP, CBP and DBP? And how can it be used in a consensus model?

Generalizing PBP, CBP and DBP is straightforward. The jury (or a moderator who does it on behalf of the jury) assigns an *effective reliability* to each juror, which equals the arithmetical mean of the reliabilities all jurors assign to her:  $r_i^{\text{eff}} = (r_{i1} + \dots + r_{in})/n$ . This effective reliability is then the weight that the judgment set, which this juror submitted, gets in the aggregation procedure. We call the such-modified procedures  $\text{PBP}^{\text{rel}}$ ,  $\text{CBP}^{\text{rel}}$  and  $\text{DBP}^{\text{rel}}$ .

To model the emergence of consensus, we generalize the Lehrer-Wagner model. Lehrer and Wagner (1981) designed a model for the emergence of a consensus when a jury has to make a decision on the value of a variable – a real number in  $(0, 1)$ . A consensus is obtained by an iterated updating process, in which each juror  $i$  takes the judgment of the other jurors  $j$  (with  $j = 1, \dots, n$ ) into account and weights them with

the respective reliability  $r_{ij}$ . Lehrer and Wagner show that, under rather general conditions, iterating this updating procedure leads to convergence.

Representing judgments again by 1 (for ‘Yes’) and 0 (for ‘No’), this procedure can be generalized to the case of judgment aggregation. Let each juror  $j$  submit a (consistent) judgment set  $s_j^{(0)}$  and assign reliabilities  $r_{ij}$  to the other jurors. In each step of the iteration process, each juror calculates a weighted average of the judgment sets submitted by all jurors. Juror  $j$  will obtain  $t_1^{(0)} = (t_{11}^{(0)}, t_{12}^{(0)}, t_{13}^{(0)})$  with  $t_{11}^{(0)}, t_{12}^{(0)}, t_{13}^{(0)} \in (0, 1)$ . The new judgment set of voter  $j$  is  $s_j^{(1)} = [t_j^{(0)}]$  in which  $[t_j^{(0)}]$  is the admissible judgment set with minimal distance to  $t_j^{(0)}$ . This procedure is iterated (with the same reliabilities) until convergence is achieved.

This paper presents a comparative numerical study of the performance of  $PBP^{rel}$ ,  $CBP^{rel}$ ,  $DBP^{rel}$  and the consensus procedure. We are especially interested in the question which of the four procedures is best in tracking the truth. Do truth and happiness go together, or does a jury have to decide whether it wants its members to be happy decision-makers, or whether it wants to track the truth? To address this question, we generalize work by Bovens and Rabinowicz (2006), List (2005), Hartmann, Pigozzi and Sprenger (forthcoming) and Hartmann and Sprenger (forthcoming) and apply it to judgment aggregation.

### **S. Lerner (UBA & UNLA), Norms and Economic Theory: a Defeasible Formal Approach.**

Recent proposals have suggested that it would be necessary to extend the notion of limited rationality in order to include not only cognitive tools but also social norms and institutions, as well as emotions. Social norms can be seen as “fast and frugal behavioral mechanisms that dispense with individual cost-benefit computations and decision making” (Gigerenzer and Selten, Rethinking Rationality).

The purpose of this paper is to outline formal schemes of rational action explanation, including cases that involve norm-oriented action.

This study focuses on the logical aspects of a general explanatory theory. According to this aim, we propose a deontic default system, built as an extension and modification of Poole’s system. We call the new system PDDL (for Poole’s Default Deontic Logic). This system fits into the scheme of defeasible deontic logics; it allows us to reach conclusions in situations of incomplete information and makes it possible to revise previously obtained conclusions when additional information is added. Defeasible systems formalize a more permissive and, at the same time, weaker consequence relation as compared to that of classical logic. They are dynamic and unstable systems that model types of rational inference within contexts of imprecise information.

We show that PDDL allows us to explain rational action through the inclusion of certain assumptions and definitions of rationality. When we consider those definitions as defaults, we can then incorporate different, and even contradictory, rationality criteria into a theory. These criteria will (possibly) coexist in different extensions. We give examples regarding how to apply PDDL to the formalization of rationality under uncertainty. We show in what way PDDL allows us to model norm-oriented action through the inclusion into a theory of the deontic counterpart of the axiom of necessitation; in this case, the extensions containing this axiom will become T extensions.

We will also mention some metalogical results of the system.

On the other hand, we analyse how the incorporation of deontic operators allows us to reflect, at the syntactic level, the prescriptive function of rationality models; that is to say, in what way the deontic operators give us the possibility to explicit in our syntax a purely pragmatic normative dimension.

Lastly, we show that defeasible logic is actually an adequate tool to solve certain paradoxes that appear when rationality criteria are not met and new criteria should be applied in the resultant situation.

## **I. Moscati (Bocconi U.), How to Make Sense of Knowledge of Information Partitions**

Models of economic theory are peopled by agents who take actions on the basis of their knowledge and beliefs about the world, and about each other's knowledge and beliefs. The prevailing formal model for knowledge in contemporary mainstream economics is the so called state-space model introduced by Robert Aumann in 1976. Aumann's basic idea is that an agent knows an event if, in every state of the world the agent considers possible, that event occurs. This idea is formalized in a set-theoretic setting where the knowledge of an agent becomes an operator  $K$  mapping sets into other sets.

The state-space model makes it possible not only to represent what each agent knows about the world, but also what each agent knows about what other agents know about the world. This kind of knowledge – knowledge of what others know – is called interactive knowledge. In strategic environments interactive knowledge has important consequences on the actions agents take. Consider Ann and Bob, who both observe a certain event  $E$ . For instance,  $E$  may be Ann's effort in a principal-agent game. If Ann is uncertain whether principal Bob knows  $E$  she may choose a low effort ("Maybe Bob won't find out"). In contrast, if Ann knows that Bob knows  $E$ , she will probably choose a high effort ("I'd better work hard, or he'll fire me").

Given the strategic importance of interactive knowledge, it is fundamental to understand clearly whether interactive knowledge of an event requires any additional assumption. In particular, it seems that Ann needs to know how the information is imparted to Bob in order to know that he knows  $E$ , otherwise Ann would have no clue about what Bob knows. Under certain conditions the way information is imparted to the agents can be modeled also through their so-called information partitions  $c$ . In this case, the above issue can be also put as follows: to state that Ann knows that Bob knows  $E$ , is any additional assumption about Ann's knowledge of Bob's information partition required? This is the first question addressed in the paper.

In effect there are multiple levels of interactive knowledge. Level 1 is the one discussed above. Level 2 is about what each agent knows about what other agents know about her/his knowledge of the world. The staircase of levels of interactive knowledge escalates in the predictable way. A specific kind of interactive knowledge is common knowledge. An event is said to be common knowledge among a group of agents if all know it, all know that all know it, and so on ad infinitum. When higher levels of interactive knowledge or common knowledge are involved, the issue about knowledge of information partitions comes out again, at higher levels. In particular, when common knowledge is at issue the question becomes: to state that a certain event  $E$  is common knowledge among a group of

agents, is any additional assumption about common knowledge of their information partitions required? This is the second question addressed in the paper.

A basic problem with these two questions is that in the state-space model the knowledge operator  $K$  applies to sets, whereas information partitions are not sets. Therefore, the very expression “knowledge of information partitions” has no formal counterpart in the model. As a consequence, the contribution of the paper is of an interpretative rather than a mathematical nature: it is about “making sense” of knowledge of information partitions rather than proving theorems about it.

The basic step to reach this goal is a methodological distinction between the intuitive and philosophical understanding of knowledge on the one hand, and knowledge as modeled in the state-space formalism through the operator  $K$  on the other. In effect,  $K$  possesses a number of properties that are at odds with both commonsense and the philosophical analysis of knowledge. The paper focuses on two of them – Substitutivity and Monotonicity – and shows that they are sufficient to achieve simple knowledge, interactive knowledge and common knowledge of information partitions, and this without any additional assumption. This conclusion is completely in accord with Aumann’s, but the arguments he used to make his case appear debatable and are criticized in the Appendix of the work.

The paper is addressed to all scholars dealing with formal models of knowledge and interested in the notion of common knowledge. In particular, economists may be glad to be reassured that no additional assumption is surreptitiously introduced into their models when an event is said to be interactively or commonly known among a group of agents. However, they may be surprised that this depends on reasons other than those put forward by the standard view moulded by Aumann’s arguments. Furthermore, they may be concerned that assumptions about knowledge of information partitions are dispensable thanks to properties of the operator  $K$  that neither commonsense nor philosophy judges plausible. As regards philosophers, they may consider the state-space model of knowledge and its internal riddles as “an economist thing”. However, philosophers in the analytic tradition are familiar with formal models of knowledge, and common knowledge has become a major topic of research for them. Therefore, the internal puzzles of the state-space model (especially those involving common knowledge) and the solution of them suggested here may be of interest for philosophers too.

**Saturday, 13 | 15.00- 16.30 | 6B. HISTORY OF IDEAS**

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**M. Boumans (University of Amsterdam), Looking under the hood: Leontief versus statistical econometricians**

In economics one will find different kinds of empirical approaches to lay bare the structure of an economic general equilibrium (Walrasian) system. Generally, structure is not considered as a surface phenomenon, which can be observed directly. To uncover it, one has to “look under the hood”. This paper discusses two opposing approaches that nevertheless used the same metaphor to indicate the ambitions of their respective programs. One is the Cowles Commission econometrics approach, in this paper represented by Ragnar Frisch, Trygve Haavelmo and Tjalling Koopmans; the other is Wassily Leontief’s input-output approach.

Leontief used this metaphor to criticize the “new econometric school” that resulted from Haavelmo’s proposed probability approach. Leontief considered the econometric methods as providing only “indirect inference”. His proposed approach of input-output analysis was based on “direct observation”. Leontief remained throughout his life an important critic of the econometric approach and distanced himself on several occasions from this approach. It is in a fierce debate with Frisch in the late 1920s and early 1930s on the proper method of deriving statistical supply and demand curves, the so-called ‘Pitfalls debate’, that Leontief determined his lifelong position with respect to this new upcoming empirical approach called econometrics.

This paper discusses the outcome of this methodological debate, which resulted in an opposition between Leontief’s input-output analysis, and what Leontief called “statistical econometrics”. In distinction with the Cowles approach, Leontief aimed at inferring from these observations “principles of intermediate generality”, and not the too abstract “intelligible principles”, which lacked empirical meaning. This latter terminology is of Philipp Frank, who provided Leontief the epistemological foundation of his approach. Data about structures at the intermediate level could be best provided by “experts” of the phenomena under investigation. These experts are the professional practitioners in the relevant fields. To inquire them about the insights and information they have about the relevant phenomena was, according to Leontief, a more reliable strategy to get informed about structures. A mere statistical approach would never attain that kind of relevant information.

**R. Faucci (U. Pisa), Benedetto Croce’s contribution to “economics”**

Italian philosopher Benedetto Croce (1866-1952) showed a longlife interest in the economists’s work, and discussed at length the foundations of economic science with professional economists. Schumpeter underlines this feature by noting that “Croce himself [was] something of an economist and [was] associated, more than is the case with any other philosopher, with some aspects of the professional work of Italian economists” (History..., p. 778).

Circa 1895 Croce approached economics through his reflections on Marxism. At that time he was interrogating himself on the fittest theoretical approach to historical phenomena. The relevance of the “economic moment” had been often neglected by mainstream

historians and philosophers. On the contrary, Marx's "historical materialism" – which Croce indeed considered a sort of comparative sociology – not only could be viewed as a much more realistic approach to historical facts, but also one not in contrast with the more rigorous economic analysis provided by Marginalism.

Shortly after, in 1899, however, Croce came to the conclusion that the flaws of Marx's economics (his inability to transform surplus values into prices, and to clarify the meaning of the so-called law of the fall of the profit rate) hindered him from understanding the pace of history as well. Indeed, a wrong economic analysis could not offer the basis for an acceptable economic sociology. The integration of Marxism and marginalism was therefore impossible.

By 1900, after abandoning Marx for ever, Croce turned to "utility", in order to ascertain if its meaning in economics could help philosophic inquiry as well. Strictly following his friend Pantaleoni, Croce identified marginalist economics with the science of utility and self-interest, and assumed that economics had to highlight the hedonist behaviour of humankind. For this reason, he was unhappy with Pareto's broader view of economics as the science which only studied the "logical" [i.e. rational in the sense of aims-means coordination] actions. In a lively discussion with Pareto in "Giornale degli economisti", Croce maintained that any study of human action in the economists's sense had to include the consideration of the springs of human will. Contrarily to Pareto, Croce stressed that the category of "utility" (as the willingness to reach some useful results) should continue to be the cornerstone of all economic reasoning. Pareto tried to convince him that his own ordinalist approach to human choice disposed of the old-fashioned concept of utility, but Croce remained unsatisfied. In his *Philosophy of the Practical* (1909) he traced the division between two meanings of "economics": a) his own philosophical definition of "economica" as a moment of his "filosofia dello Spirito" (philosophy of the Spirit), consisting in "the will of the individual", i. e. inclusive of all non-moral conducts, especially political, as well as *stricto sensu* economic, behaviour; and, b) economics in the sense of the economists's job - a sheer collection of mathematically-like theorems of very limited theoretical significance but of (possibly) fair practical use for bankers, policymakers, and so on.

Finally, between 1928 and 1941 Croce discussed with another economist, Luigi Einaudi, on the relationships between economic and philosophical liberalism. This time, Croce scolded economists to have founded economics on two heterogeneous principles: a) the "principle of value" (here Croce apparently overlooked the outcome of his discussion with Pareto) and - this was the novelty - b) the "principle of *laissez-faire*". While Croce insisted that the former principle had really the dignity of quasi-philosophical tenet (although economists were not equipped in rigorously defining it), Croce considered the latter principle as a merely contingent policy device, which could not be given a general and undisputable significance. Einaudi objected that the "principle of value" had been rejected by all modern economists, and that *laissez-faire*, although it was not a principle in Croce's sense, was still necessary for a free society and even possessed a "religious" content.

The persisting confusion among economists between philosophical (universal) and empirical (historically determined) concepts allowed Croce to conclude that economic science had nothing to say about the meaning of Liberty. The philosopher's conclusion was that economists were unable to "philosophize" and had to born themselves to "calculate".

It is worth noting that in all this discussion neither Croce, nor Pareto, nor Einaudi manifested the doubt that "economic science" (in all the different meanings of the term) could be undermined by the interference of ideology, *Weltanschauung*, or value judgments. There was an implicit agreement that economics was "naturally" value free, and dissent

arose only as far as the scope and validity of its “principles” (main assumptions) were concerned. Nobody appear to have grasped (or even known) Max Weber’s teaching.

### **V. Neves (University of Coimbra), On the foundations of a qualitative tradition in economics: Marshall, Keynes and Coase**

Since Jevons (1871), at least, economics has been regarded as the domain of measurement and quantification – “the domain of the ‘universal equivalent’ (currency)” to use the words of R. Guesnerie –, a science which, “because it deals with quantities” (Jevons), could only be mathematical. It was then quite natural the consistent attempt to build economics on the standards of rigour typical of the ‘harder’ physical sciences. Formal mathematical modelling and econometrics became, particularly after the II WW, the central features of the economics profession.

Yet the overwhelming emphasis on the adoption of mathematics as the almost exclusively legitimate language and mode of reasoning of economics has always stirred up debate and dissent. This is the case of most non-mainstream schools of thought. But important reserves about the use of mathematics in economics may also be found in such prominent representatives of the economics profession as Marshall, J.M. Keynes or, more recently, Ronald Coase. Keynes, for instance, on various occasions, emphasised that the applicability of mathematical reasoning in economics was a matter of logic, that “qualitative logical analysis (i) *precedes* quantitative or mathematical analysis, and (ii) *determines the scope* of its application” (O’Donnell 1990: 35).

In a recent paper (2004), Donald Gillies, a Professor of Philosophy of Science and Mathematics, put himself the question “Can Mathematics Be Used Successfully in Economics?” and his answer was negative. In his view economics only superficially deals with quantities; numbers are in economics mere ‘operational numbers’, not estimates of real quantities. Attempts to model the discipline on physics cannot be successful. He then went on to recommend that economics emulated a rather more similar science – medicine – and adopted what he called a ‘qualitative causal methodology’.

No doubt, a persistent tension troubles economics almost from its early stages. This has to do with the tension between quality and quantity and how to deal with these two dimensions of its object of study. In this paper an attempt is made to look into the philosophical foundations of what seems to be an enduring “qualitative tradition” in economics. Attention will be given to the writings of Marshall, Keynes and Coase.

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**Saturday, 13 | 15.00- 16.30 | 6C. EXTERNAL VALIDITY**


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**M. Jiménez Buedo (UNED) & L. M. Miller (Max Planck Institute of Economics),  
The Relationship between External and Internal Validity**

In the last two decades the debates around the worth of the experimental method in economics, and in general, in the social sciences, have been many, heated, and salient both for practitioners and methodologists. This is mainly a consequence of the consolidation of the experimental method as a valid tool for economic research which, as a side effect, has reopened the discussion about the benefits and drawbacks of laboratory experiments in the social sciences. Despite the proliferation of debates, different authors have hardly agreed on one of the main concerns of experimental methodology: the problem of external validity, and in particular, its relation with internal validity. In relation to the validity problem, experimental economists, as well as the majority of experimental social psychologists and sociologists do follow the seminal works of Donald Campbell and his collaborators (Campbell and Stanley, 1963; Cook and Campbell, 1979).

There is a consensus among experimentalists considering internal and external validity of experiments as prerequisites for their worth. If an experiment is not internally valid, then, we cannot say that the treatment given in the experiment is the cause of the effect we observe. If an experiment is not externally valid, then its results cannot be said to hold outside of the experimental setting, and thus, even if internally valid, we cannot say anything relevant of the world. Quoting the classical definitions of Cook and Campbell (1979: 37), internal validity ‘refers to the approximate validity with which we infer that a relationship between two variables is causal or that the absence of a relationship implies the absence of cause’, and external validity ‘refers to the approximate validity with which we can infer that the presumed causal relationship can be generalized to and across alternate measures of the cause and effect and across different types of persons, settings, and times’. Starting from these definitions, it is commonly assumed that there is a tension between both sources of validity. Cook and Campbell (1979: 82), in their discussion about the relation between internal and external validity put it in the following way: ‘Some ways of increasing one kind of validity will probably decrease another kind’. We can find references to this tension in both social psychology (Brehm et al., 1999; Smith and Mackie, 1999) and economics texts (Guala, 2005). At the same time, this received-view coexists with positions that claim that the problem of internal validity are chronologically and epistemically antecedent to problems of external validity or even that the question of external validity is irrelevant to many types of experiments, in particular those that are theory-oriented (Guala, 2003; Thye, 2000, Kanazawa, S. 1999).

The aim of this article is to rethink the distinction between internal and external validity as is currently defined both in the philosophical literature on experiments and in the discussions that experimenters do of their work. We deal in particular with the notion as it applies to social science experiments. Our ultimate aim is that of questioning the widespread idea that there is a trade-off between internal and external validity. The argument is based in a classification of the different definitions of external validity available in the literature. We then discern the relation (both empirical and logical) of each of these with the definition of internal validity. We suggest that there are no grounds to posit a general relationship between internal and external validity and that this relationship ultimately depends logically, on the definitions of both type of validity favored by the methodologist, and empirically, on the goals of the experimenter.

### **C. Marchionni (Erasmus U. Rotterdam), The virtue of generality in economic models**

Economists often justify the neglect in their models of a host of details about the target system by appealing to the virtue of generality. They claim that the omission of details allows the construction of general models, that is, models that are applicable to a large number of (possible or actual systems) and that general models are ‘better’ than more specific models (cf. Matthewson and Weisberg 2007). Despite the widespread appeal to generality among economists (as well as among philosophers), an account of generality that recognizes it as a virtue and at the same time suggests constraints on when generality is a virtue, is still missing. This paper aims to fill this gap.

To begin consider the following examples. The first are game theoretic models of cooperation (à la Skirms and Binmore). A version of the argument from generality has been exploited to justify the neglect of various features of the evolutionary process leading to cooperation (mainly proximate mechanisms and features of the selection mechanism). First, those details are irrelevant to the outcome: cooperation is inevitable under the conditions set out in the models. Second, neglecting details makes the models general enough to be applicable to species whose behavior is in reality produced by quite different proximate mechanisms. The second example is the theoretical models of geographical economics. In response to the criticism that these models ignore what’s distinctive about agglomerations at different spatial scales (i.e. cities, national regions, international regions, etc.) geographical economists have argued that neglecting details about agglomeration at various spatial scales is ‘the price to be paid’ for providing a unified approach to agglomeration.

These cases demonstrate that generality is held to be a virtue and it is indeed invoked to justify the omissions of various causal details. The examples also point to connection between generality and unifying power. Although generality and unifying power are distinct (generality is an attribute of models whereas unifying power is an attribute of explanations), in the paper I propose that the virtue of general models resides in their potential to afford unifying explanations. Building further on this idea, I develop an account of generality that not only explains why it is a virtue, but also why it is not increased by neglecting just any detail and why increasing generality is not always desirable.

### **L. Mireles-Flores (Erasmus U. Rotterdam), Policy Relevance and External Validity in Economics**

In economics, there is general agreement that policy relevance is a desirable characteristic of scientific knowledge and, accordingly, that it is a virtue worth aiming for when developing theoretical economic accounts. Nevertheless, neither economists nor methodologists have offered an elaboration of what exactly policy relevance amounts to as a virtue of economic science. Existing discussions on policy relevance are generally ambiguous about the exact bearer and nature of the posited virtue.

Some authors identify the notion of policy relevance with the possibility of doing policy analysis from or within formal (mathematical or graphical) economic models, e.g. tax policy, monetary policy, or government expenditure analysis. In such cases the aim is to

explore and learn within the formal structure of theoretical models the different effects of changes (or shocks) in specific structural variables.

Other discussants approach the policy issue in economics from a more philosophical perspective. In these cases the basic idea is that: in order to attain policy relevance, economic science should aim for reliable causal claims. Reliable causal claims are those that state genuine causal relations, in contrast to uncertain or spurious causal relations. The discussion then tends to focus on how to formulate, evaluate, test, and support causal claims. In other words, finding an effective and reliable method of causal inference is taken to be a fundamental prerequisite for policy relevance in economics.

The idea that genuine causal claims are useful for policy purposes rests on the assumption that such claims are valid and stable in some general sense. This characteristic of causal claims—stated in a more proper and technical way—is usually labelled as external validity. The more external validity a causal claim possesses, the more confidence one might have on its effectiveness as a guide for policy. Accordingly, policy relevance is generally taken to be dependent upon external validity, to the extent that for some authors the degree of external validity seems to be even taken as a surrogate for the relevance.

In this paper, first I attempt to clarify the notion of policy relevance in economics and the locus of such a theoretical virtue. These two issues are intermingled: it seems that the notion of relevance can acquire a different specific meaning depending on whether the bearer of the property is a theory, a model, or a causal claim. There can be a close relationship between these different meanings of policy relevance, but they should be distinguished.

Secondly, I analyse the role of external validity in evaluating policy relevance. Does it have the same role in appraising the relevance of theories, models, or claims? More importantly, given the broadly accepted role of external validity as a desirable property of causal claims, how tight is the relation between external validity and policy relevance? To answer this, I analyse whether external validity is necessary, sufficient, or otherwise useful for a causal claim to be reliable for policy purposes. I suggest that external validity is not sufficient, nor necessary, but still recommendable in as much as it improves the reliability of the claim. Moreover, it appears that it is the policy relevance of a theoretical entity which to a certain extent might count as good evidence for its external validity. A proper account of the methodology of policy relevance and policy formulation seems to be still missing.