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An international conference on all aspects of philosophy and economics to celebrate the 20th anniversary of the Erasmus Institute for Philosophy and Economics (EIPE)

This conference is part of the "*Erasmus Happiness Week*", jointly organized and hosted by the Erasmus Institute for Philosophy and Economics (EIPE) and the Erasmus Happiness Research Organization (eHero) at the Erasmus University Rotterdam.

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Wednesday, March 22nd

Happiness and Wellbeing: Conceptual Issues
(Wed 15:45-17:15, Room M1-04: Zaal Montreal**)**

"A New 'Evaluativist' Theory of Happiness"

Mauro Rossi
rossi.mauro@uqam.ca; *UQAM*

Abstract

The goal of this presentation is to put forward a new evaluativist theory of psychological happiness (henceforth, simply 'happiness'). Since my theory can be regarded as an improved version of Haybron (2008)'s emotional state theory of happiness, I will present it by contrasting it to the latter.

Traditionally, the happiness literature has been dominated by two main theories: hedonism and life satisfactionism. According to the former, happiness consists in a positive balance of hedonic states, such as pleasures and displeasures (e.g. Carson, 1978; Davis, 1981; Kahneman, 1999; Feldman, 2010; Morris, 2011). According to the latter, happiness consists in an attitude of satisfaction towards one's life considered as a whole (e.g. Telfer, 1980; Nozick, 1989; Sumner, 1996; Vitrano, 2013). Haybron's theory offers a third way. According to him, happiness consists in in a broadly positive balance of emotions, moods, and mood propensities. While Haybron takes these to be affective states, he does not regard them as necessarily pleasant states. Thus, his theory differs from both hedonism and life satisfactionism. Contrary to the former, Haybron does not take pleasures to be the sole happiness-constituting states. In fact, he thinks that some kinds of pleasures, i.e. sensory pleasures, are at most occasional sources of happiness, but not constituents of it. Contrary to the latter, Haybron denies that cognitive judgments of satisfaction are essential constituents of happiness. According to him, happiness is an entirely affective phenomenon.

I believe that Haybron's theory is on the right track, particularly in its emphasis of the role of emotions and moods. At the same time, his theory remains unsatisfactory in some crucial respects. Consider the following questions. What do emotions, moods, and mood propensities have in common? In other words, what is it that makes them, and only them, happiness-constituting states? Haybron's answer is that these are all central affective states and that only central affective states are happiness-constituting. This seems initially promising. However, Haybron goes on to define 'centrality' in purely causal-dispositional terms. According to him, for an affective state to be central is for it to dispose the individual to do certain things or to experience other affective states, in a productive, persistent, pervasive and profound way (Haybron, 2008: 130-131). This account is problematic for at least three reasons. First, it does not vindicate the claim that only emotions, moods, and mood propensities are happiness-constituting states. Indeed, there are other affective states that seem to possess the dispositionality that Haybron deems sufficient for counting as happiness-constituting states. For instance, some sentiments (e.g. the love for one's partner) may clearly be 'central' in Haybron's sense. Second, Haybron's account downplays the importance of 'affectivity' for happiness. In fact, insofar as happiness is conceived of as an entirely affective

phenomenon, one would expect the 'affectivity' of emotions, moods, and mood propensities to play a special role in the explanation of why these states, and only these states, are happiness-constituting. However, affectivity remains almost invisible within Haybron's causal-dispositional account. Third, by including both occurrent states (i.e. emotions and, arguably, moods) and dispositional states (i.e. mood propensities) in the same basket, Haybron somehow blurs the distinction between occurrent and dispositional happiness.

In this presentation, I want to put forward a more compelling version of the emotional state theory of happiness. My main focus will be on occurrent happiness. To begin with, I will argue that mood propensities (that is, dispositions to experience particular moods) should not be regarded as constituents of occurrent happiness. The first reason is that mood propensities do not have a phenomenology, whereas occurent happiness seems to necessarily possess a phenomenological dimension. The second reason is that it is possible to account for the explanatory and predictive role of occurrent happiness ascriptions in everyday life (which motivated Haybron's inclusion of mood propensities within the class of happiness-constituting states) simply by appealing to the dispositional properties of occurrent affective states.

Next, I will focus on emotions and moods, as paradigmatic happiness-constituting states. I will argue that both of them are kinds of affective responses to values. I will consider emotions, first. After noticing that all the most plausible contemporary accounts (including cognitive and attitudinal theories) characterise emotions as intimately related to values, I will offer some reasons to favour a perceptual theory of emotions, according to which the latter are perceptual experiences of evaluative properties (de Sousa, 1987; Goldie, 2000; Tappolet, 2000, 2016; Döring, 2003; Prinz, 2004). I will then focus on moods. Against the orthodox view, I will argue not just that moods have intentional content, but also that they have evaluative content. More specifically, I will defend the view that moods are perceptual experiences of valued possibilities (for similar, though distinct, accounts see Price, 2006; and Tappolet, forthcoming).

According to my theory, then, both emotions and moods are affective experiences of value. (More specifically, they are different species of the same genus of affective evaluative states.) This is what they have in common. This is also – I will argue – what makes them happiness-constituting states. A new 'evaluativist' view of happiness thus emerges. According to it, happiness consists in broadly positive affective experience of values; or, in other words, in a broadly positive affective evaluation of one's circumstances.

I will conclude by mentioning two implications of this view (for reasons of time, however, I will not defend these claims in my presentation). The first is that all affective states that are constitutively linked to values and that, as such, can be regarded as kinds of 'felt evaluations', should count as constituents of happiness. Candidate happiness-constituting states of this sort are, in contrast to Haybron's own view, sensory pleasures – at least if one conceives of them in an evaluativist way (see, e.g., Cutter & Tye, 2011; O'Sullivan and Schroer, 2012; Bain, 2013). The second is that the proposed account of occurrent happiness naturally suggests a corresponding account of dispositional happiness, according to which the latter consists in a positive balance of dispositional affective states, such as sentiments and mood propensities.

"A Comprehensive Sense of a Good Life Includes Valuing Life"

Jacob Bethem
jbethem@asu.edu; *Arizona State University*

Abstract

Ethicists often ask, "What is the good life?". This question is asked typically not just to know what a good life is but also to put it in action in your life and to make the world a better place. The purpose for asking about the life usually matters—different purposes require different constructs, and the sense of well-being in one inquiry may not satisfy a different reason for asking, such as a medical examination compared to abstract philosophical theorizing. We must take care not to equivocate these various meanings. When we are asking in the abstract philosophical sense, it is important not to get stuck in a merely individualistic sense of how that life is going, as the context of relations to other people and to the environment are also significant and can change the assessment of a life that is otherwise going well prudentially.

"Happiness" and "flourishing" are common ways of explaining well-being or human excellence, but they need more help for operationalizing the good life, or what we might also call "sustainable living". Well-being and sustainability are both wicked concepts, and uniting them is no easy task. My argument here is that emphasizing the value of other lives within both environmental and community-oriented notions of well-being can provide further support to sustainability operationalization while also improving our understanding of well-being. With a more comprehensive notion of excellence, sustainability, and well-being, we can lead better lives and improve the world.

This discussion explores some of the research in the fields of sustainability and well-being, as well as where the two concepts overlap. Sustainability is missing something—without a built-in formulation of ethics, we might have the impression not just that anything can be sustained, but that potentially anything should be sustained. This oversimplified understanding of sustainability misses the point of the history of the environmental movement that the term represents. Some of tried to unite the two. John O'Neill experiments with theories of well-being to help provide moral guidance to sustainability, but I will argue that his discussion is left unfinished. Catherine O'Brien also tries to merge well-being and sustainability to improve both, but her view of "sustainable happiness" does not escape similar biases against overconsumption. Oscar Kjell's "sustainable well-being" solidifies the interdependencies that others note but does not designate the values that are still missing. However, Bryan Little's work provides a more comprehensive approach to well-being that can guide sustainability with a little more help.

I conclude by suggesting further avenues of research that unite well-being and sustainability and propose that the value of life is the missing piece that can provide guidance for determining how to live.

“Deception, Self-Deception and Happiness”

Mark Chekola
mchekola@yahoo.com; *Minnesota State University Moorhead, World Database of Happiness*

Abstract

When persons judge their lives to be happy these judgments include beliefs about their lives and how they are going. Such beliefs could be about significant relationships, their own traits, qualities, activities and accomplishments, prospects for the future, etc. There are various ways such beliefs can be inaccurate. What impact does this have, if any, on the accuracy of the judgments of happiness and well-being? This paper focuses on deception and self-deception and how they might affect happiness and well-being. Some claim that first-person judgments of happiness are incorrigible: they cannot be wrong, even if based on beliefs that are incorrect. Some philosophers and social scientists claim that some forms of self-deception enhance, and perhaps are even necessary for happiness. Works of literature, such as Henrik Ibsen’s "Wild Duck" and Eugene O’Neill’s "The Iceman Cometh" suggest that “Life Lies” are essential. I consider some issues concerning how to understand the nature of self-deception and examine claims by psychologists Shelley Taylor and Timothy D. Wilson of the commonness and importance of various forms of self-deception. Criteria are developed for evaluating everyday forms of self-deception. I discuss how deception and self-deception present problems for happiness, and how first-person happiness judgments can sometimes be incorrect. Considering some arguments presented by James Griffin and William Sumner I show that mental state and some life satisfaction views of happiness may fail to capture the importance of actual states of affairs with regard to judgments of happiness.

Happiness and Wellbeing: Its Measurement
(Wed 15:45-17:15, Room M2-10: Zaal Rochester)

"The Role of IRT Models in the Validation of Well-Being Measures"

Elina Vessonen
esmv2@cam.ac.uk; *University of Cambridge*

Abstract

Psychometric measures of well-being have become increasingly popular among well-being researchers and welfare policy-makers. These instruments, which are usually questionnaires asking people to rate (aspects of) their welfare on a pre-specified scale, used to be primarily employed by psychologists (Angner 2011). Recently, though, these instruments have received attention among economists and policy-makers. For example, the UK Treasury has started to explore the possibility of using psychometric measures of subjective well-being in (or alongside) cost-benefit analysis, in order to help determine which public policies to fund (Fujiwara & Campbell 2011). Psychometric validation, i.e. tests that are used to establish the quality of the measurement properties of a psychometric instrument, are also used by welfare economists when they create and improve so called stated preference techniques for the purposes of economic analyses (Bateman & al 2002). In light of this rise in enthusiasm about psychometric measures of well-being, it has become increasingly important to scrutinize the process by which psychometric instruments are validated. The trouble is that there is an abundance of competing proposals specifying what validity amounts to, but there is no consensus on which approach is correct and why.

The list of things that have been proposed as requirements for measure validity is long: construct validity, content validity, structural validity, discriminant validity, convergent validity, external validity, factorial validity and so on and so forth (see e.g. Markus & Borsboom 2013). Techniques for testing these different types of validities are equally many and largely contested. The obvious problem is that if we do not have agreement on what validity of a psychometric instrument amounts to and how to test for it, we cannot easily differentiate between good and bad, useful and useless measures of well-being. This paper contributes to the clarification of this measurement mess by scrutinizing the function and usefulness of a particular method that has been proposed as a crucial step in psychometric validation: Item Response Theory models (IRT models). IRT models specify a relationship between observed test scores, characteristics of test items (i.e. individual questions) and the target construct. The usefulness of IRT models, just like other aspects of validation, is contested in psychometric literature: on the one hand, it has been argued that their usage is a crucial component of psychometric validation; on the other hand, in practice many measure validation exercises are carried out without IRT (e.g. Hobart et al 2007).

Motivated by this lack of agreement, this paper suggests an answer to the question: what, if anything, is the role of IRT models in the successful validation of a psychometric measurement instrument? I will treat successful validation as a broad notion such that answering the above question amounts to showing how the usage of IRT models contributes to establishing that a proposed psychometric instrument is appropriate for measuring the attribute that it is supposed to measure. To provide an answer, I first defend three interrelated conditions that a successful validation exercise needs to fulfill in order for it to establish the appropriateness of an instrument for the measurement task it is intended for. Developing the three conditions outlined by Cartwright, Bradburn and Fuller (2016), I argue that good measure validation requires:

i) characterization of the target construct,
ii) specification of representational conditions applicable to the target construct, and
iii) collecting evidence that the proposed instrument yields a meaningful numerical representation of the targeted construct.

I explicate these three conditions and their relations, with special focus on the explication of the technical notion of meaningfulness of a numerical representation. Once I have argued in favor of the significance of all three conditions, I use them to explain the role of IRT in the validation of psychometric instruments. I argue that tests that study the fit between data and an IRT model help us fulfill condition iii) by providing evidence for the meaningfulness of the numerical representation. In other words, they tell us how to interpret the resulting numerical representation in terms of relations between objects that are in the extension of the target concept. The argument has implications for the improvement of psychometric well-being measures.

"Combining Subjective and Objective Indicators of Well-Being and the Strange Case of Hybrid Philosophical Views"

Shiri Cohen Kaminitz
coshiri@gmail.com; *Hebrew University*

Abstract

Recent years have witnessed the development and implementation of national indicators supposed to represent the well-being, welfare or quality-of-life (the three concepts are used here interchangeably) of societies and nations. This development has gained the attention of governments and international institutions, such as the EU, OECD and UN, each of which chooses or develops their own indicators or indices of indicators to represent well-being.
Essentially, the project of developing the indicators begins with an acknowledgement of the problem of leaning too heavily on GDP per capita as an exclusive indicator of well-being. Among the many kinds of indicators and new measurement frameworks, one can identify three basic categories: accounting/monetary approaches, objective-conditions approaches and subjective approaches (often conducted by broad surveys of life satisfaction). Each of these three categories has its own particular intellectual roots, trajectory of scientific development and history of implementation, as well as an extensive literature concerning its analytical advantages and pitfalls. In the field of national indicators, a consensus has arisen concerning the need for the implementation of a plurality of (existing) indicators in order to best represent well-being.

This paper assumes that meaningful ascription of the different methods of combining different indicators of well-being is the next necessary challenge for the field, a challenge that could gain much from a deeper discussion of the philosophical underpinnings of the various methods of combination. The challenge of addressing systematically the combinations (or weights) of indicators is recognized in the most comprehensive and thoughtful studies. In particular, as Stiglitz, Sen and Fitoussi have pointed out: "Several aggregate measures of quality of life are possible, depending on the philosophical perspective taken and the question addressed," yet "while assessing quality-of-life requires a plurality of indicators, there are strong demands to develop a single scalar measure" (Stiglitz, Sen, Fitoussi, 2009, 55, 56). Fleurbaey and Blanchet, pushing these points further, and warning us of the dangers of being satisfied with a 'dashboard' or a multiplicity of

unconnected indicators derived from the various statistical systems, argue for the need for 'a synthesis phase' in order to move from detailed statistics to aggregate summaries, thereby making the complex data meaningful and usable. They also warn against settling for existing indices, pointing to their somewhat contingent character and at times even arbitrary methods of weighing and combining indicators (Fleurbaey and Blanchet, 2013, 237-242).

The present paper, written in the light of these reflections, is composed of two parts. The first part (1) presents different existing and speculative ways of connecting conceptions of well-being to the question of selecting and combining well-being indicators. The second first presents the hybrid conception within the philosophy of well-being (2.1), and then elaborates on and analyzes the special implications of accepting the hybrid perspective with regard to possible combinations of subjective and objective indicators (2.2). Concerning the first part: interestingly, it appears that each of the basic categories of indicator is supported by a distinctive philosophical justification, which stems from different conceptions of well-being in philosophy. The subjective-objective divide is set as a convenient starting point for bridging categories of indicators with philosophical theories of well-being because, while the three categories of indicators are divided on the objective vs. subjective question concerning what are we measuring (whether measuring subjective preferences/pleasures/attitudes, or societal circumstances), philosophical approaches present either subjective or objective factors as ultimately constituent of well-being.

Scholars have indeed connected the three different kinds of indicators to the three general accounts of well-being: mental-state accounts, desire-fulfillments accounts and objective-list accounts (building on the influential taxonomy provided by Derek Parfitt, 1984). The first two are subjective in essence, while the third is objective. Hence, life-satisfaction surveys are sometimes connected with mental-state accounts and sometimes with desire-fulfillments accounts; in the two cases, with subjective approaches of well-being. Objective indicators have been connected with objective-list theories of welfare and sometimes with other objective approaches like Sen's theory of functionings and capabilities.

It should be kept in mind, though, that the connections are not as straightforward as might at first appear because the basic questions behind the two divides are significantly different: the philosophical concern is to discover what ultimately constitutes an individual's well-being, while the indicator categories refer to the most appropriate methods of assessing and measuring social well-being. There are actually two gaps here: one, is the gap between the question of ultimate source and the question of preferable method of access (the question of measurement), the other is the gap between an individual's well-being and the welfare of the society as a whole. This dual gap will be dealt with in the first part of the paper.

The second part of the paper focuses on the question: what is the philosophical significance of the different methods of combining objective-indicators (or indices) with subjective-indicators? More concrete, it is asked: what is the difference between pluralist and hybrid views of well-being in this respect?
The argument made will be that indices constructed of a plurality of (existing) indicators (both objective and subjective) are connected, naturally, with objective or pluralistic views of well-being, but do not reflect hybrid view about well-being.
Thus, although hybrid views of well-being have been considered – especially in the past 30 years – as promising theories of well-being by prominent philosophers, nevertheless they have not yet been explicitly connected to the question of indicator-combination. The particular contribution therefore made by this paper consists in a better understanding and analysis of this particular lacuna, and suggests paths to implementing this view within the indicators' sphere.

"Can Welfare be measured using Preference-Satisfaction?"

Willem van der Deijl
wjavanderdeijl@gmail.com, *Erasmus University Rotterdam*

Abstract

Welfare is a central normative concept in economics, and is most commonly understood in economics as preference-satisfaction. For most of the twentieth century it has been the standard view in economics that this concept is not measurable in the form of an index, even though some inferences could be made about it in market contexts (e.g. ceteris paribus, welfare increases when income increases). In the past decade economists have started to rely more and more on indicators of subjective wellbeing (SWB), such as happiness and life-satisfaction measures, to estimate welfare and the impact of economic phenomena (such as growth and unemployment) on welfare. Such indicators of SWB have challenged the assumption that welfare indices are not feasible, however, they do so by deviating from the view that welfare should be measured in terms of preference-satisfaction.

In recent years, (partly) in response to these developments, a number of economists have developed preference-based index measures of wellbeing, novel in their kind. One influential development in this regard is Daniel Benjamin et al.'s "Beyond happiness and satisfaction: toward wellbeing indices based on stated preference." (The American economic review 104:9 (2014): 2698-2735) in which a general framework is proposed for the development of preference-based welfare indices in the policy context. At the same time, preference-based measures of health related quality of life have been used in health economics for decades, but these measures have recently been generalized to get at measures of quality of life in general. One notable example of such a general measure is the ICEpop CAPability measure for Adults (ICECAP-A; Al-Janabi, Hareth, Terry N. Flynn, and Joanna Coast. "Development of a Self-Report Measure of Capability Wellbeing for Adults: The ICECAP-A." (Quality of life research 21:1 (2012): 167–176). While using a capability framework, it uses preferences to weigh the significance of different capabilities for welfare..

To what extent do these measures succeed in developing preference-based welfare measures that are both faithful to normative and theoretical commitments of preference-satisfactionism as well as relevant for policy? In order to answer this question I specify that the main preference-satisfactionist commitment is that it is a non-paternalist view on welfare. Rather than a substantive view on welfare, it is a formal one, which avoids that any substantive claims about welfare do not resonate with the subjects about which they are made. I call this the Resonance Attraction.

Furthermore, I present a framework in which I suggest that a preference-based measure of welfare captures to what extent the world coheres to how we would like it to be, described in a finite number of dimensions. I distinguish the issue of ordinality of preferences from comparability and how this relates to policy questions. The main argument that I present is that while there is no conceptual problem in formulating a preference measure of welfare due to feasibility constraints that come with a policy-relevant instrument a preference-based measure of welfare cannot simultaneously be policy-relevant and do justice to its normative commitment: the Resonance Attraction.

In order to demonstrate the argument I present a detailed study of, first and foremost, Benjamin et al.'s framework, and its proposed suggestions to make pragmatic tradeoffs between theoretical rigor

and feasibility. I then compare this framework with the ICACAP-A measure, which chooses different options in similar methodological choices.

In this article I first discuss Benjamin et al.'s proposed theoretical framework and their suggestions for making this framework empirically measurable in a feasible manner. Benjamin et al.'s theoretical framework formulates an individual index of welfare, but one that only tracks within-individual ordinal changes that are not interpersonally comparable. I argue this implies that it is limitedly useful in terms of policy. In order to measure it, they propose to use stated-reference methods that make respondents report hypothetical choices. I discuss Benjamin et al.'s commitment to not restricting the space of possible aspects of life. They select 129 aspects of life to make up wellbeing from a variety of different contexts, such as objective lists from philosophy and other empirical work (e.g. based on SWB). Due to this commitment, it becomes unfeasible to construct individual preference maps indicating how all possible values of such aspects of life are valued compared to each other. Consequently, they use population preferences rather than individual preferences to keep their method feasible. While the commitments Benjamin et al. make are normatively highly attractive in the preference-satisfaction framework, they strongly limit the usefulness and validity of the measure as a policy instrument.

Benjamin et al.'s method contrasts strongly with the ICECAP-A, originating from health economics. Its developers base themselves on structured interviews and focus groups to identify only 5 functionings/capabilities that make up wellbeing, compared to the 129 dimensions Benjamin et al. employ. Furthermore, they calibrate their scale from 0 (having no capabilities at all) to 1 (having all capabilities fully), allowing for interpersonal comparisons, making it useful for policy. However, even with 5 aspects, there are still a large number of different wellbeing states that can be compared. They rely on group preferences as indicators of how valuable each of the capabilities is for wellbeing. Due to the limited number of capabilities considered, and due to the usage of group preferences to weigh the different capabilities for individual welfare, it becomes possible that this measure of welfare does not resonate with a person's view on what matters for his or her wellbeing.

In a final section I analyze the tradeoffs that a measure of welfare needs to make. I argue that a measure cannot be highly policy-relevant, be faithful to the theoretical commitments of a preference-satisfaction view, and at the same time be empirically feasible. The reason for this is that comparability and cardinality on the one hand, and individualism of a measure on the other, both are highly data-demanding. A feasible measure of welfare thus requires making a tradeoff between policy-relevance and being faithful to the preference-satisfaction view.

"Well-being of Older Workers and Retirees in Europe: Comparing Life Satisfaction and Capabilities"

Lieze Sohier, Luc Van Ootegem and Elsy Verhofstadt
lieze.sohier@ugent.be, luc.vanootegem@ugent.be, elsy.verhofstadt@ugent.be; *Ghent University*

Abstract

One of the most important solutions to solve the financial challenges of the pension system in Europe is the encouragement of longer working careers (European Commission, 2012). This will benefit the older workers financially. Little attention has been paid in this debate to the non-monetary effects of working at older age. The aim of the paper is to investigate the impact of working at senior age (aged 50 or more) on the individual's well-being.

The literature on this subject is mixed. Many empirical authors find a positive effect of retiring on overall well-being (Bossé, Aldwin, Levenson, & Ekerdt, 1987; Cho & Lee, 2014; Latif, 2011; Reitzes, Mutran, & Fernandez, 1996), others find no significant effect (Crowley, 1985; Warr, Butcher, Robertson, & Callinan, 2004) and another group of researchers find that workers report a higher well-being level than retirees (Kim & Moen, 2002; Richardson & Kilty, 1991). The variety of overall well-being measures, the heterogeneity of the retirement process (Wang, Henkens, & van Solinge, 2011) and the principal use of cross-sectional samples (Latif, 2011) can explain the differentiation in results.

In our paper we compare the effect of the employment status (employed or retired) on two different well-being measurements: life satisfaction and capabilities, controlling for individual heterogeneity and the change in the level of well-being during retirement in a panel context. We do this by using a fixed effects (FE) estimation method. This approach takes all time-constant unobserved personality traits into account. More than 50 percent of the variation in subjective well-being is explained by personality (Pagán, 2013). Additionally we include age, the support of the partner and the personal health and financial situation as important determinants of life satisfaction (Dolan, Peasgood, & White, 2008; Helliwell, Layard, & Sachs, 2012; O'Donnell, Deaton, Durand, Halpern, & Layard, 2014; Stiglitz, Sen, & Fitoussi, 2010). By including the years in retirement, we account for the change in the level of well-being during retirement (Atchley, 1976).

Our research question is two-folded: first, have senior workers a lower, equally or higher well-being than their retired peers? Second, is there a different answer to the former question when we use a satisfaction or a capabilities measure of well-being?

This study uses data from the Survey of Health, Ageing and Retirement in Europe (SHARE). SHARE contains detailed micro data of the senior population in Europe (Börsch-Supan & Alcser, 2005; Börsch-Supan et al., 2013). We use the second (2006-7), fourth (2011) and fifth (2013) observation period and include following countries: Austria, Belgium, Denmark, France, Germany, Netherlands, Spain, Sweden and Switzerland. We limited our sample to the individuals who are between 50 and 75 years.

We use two overall well-being variables. We consider the traditional life satisfaction indicator (as in Stiglitz et al. (2010) and as suggested by the OECD (2013)) and compare this with a capabilities measure. Capabilities are defined as the options or opportunities individuals have in life, which are essential to evaluate individual well-being (Alkire, 2005; Fleurbaey, 2006; Robeyns, 2006; Schokkaert, 2009; Sen, 1985, 1993). The capabilities framework is theoretically and ethically appealing, but implementation is a real challenge. Here we measure self-reported or perceived capabilities (Van Ootegem & Verhofstadt, 2015) by using nine of the twelve questions of the CASP-measure (see table 1). CASP is a multi-dimensional measure of quality of life and contains four subscales: Control, Autonomy, Self-realizations and Pleasure (Hyde, Wiggins, Higgs, & Blane, 2003). We excluded the latter category. The capabilities indicator is computed in two ways (see table 1). Our first capability measure (CAS_index) is a simple index of the Control, Autonomy and Self-realizations questions (nine items). As second capability measure we weighted the nine items by performing an explanatory factor analysis. This resulted in two factor scores (CAS_CF and CAS_UF). The questions that are explained in the first factor score (CAS_CF) capture constraints with age, family or money (constrained freedom), the second factor score (CAS_UF) contains positively formulated questions about opportunities in life (unconstrained freedom).

Using life satisfaction as well-being measure, we find that workers report no significantly different level of life satisfaction when retiring, given their personal characteristics and the context in which they made the retirement decision. But after two years in retirement, retirees do not see the advantages of their new situation any more or realize that being retired is less nice than they thought originally such that they report a lower significant life satisfaction level than at the beginning of retirement. Using capabilities as well-being measure, we obtain a different story about the impact of working at senior age on overall well-being. Senior workers report a higher level of capabilities when retiring. Additionally, after two years in retirement (or other thresholds), retirees do not experience a difference in their capabilities. Additional estimations show us that the magnitude of the effects is slightly affected by a change in the income and health situation when retiring and by group characteristics (gender and different job characteristics).

We conclude that the evaluation of the well-being of older workers and retirees in Europe crucially depends on the well-being measure used. Which well-being measure should be preferred, depends on policy.

"Organizatinal Aesthetics and The Promise of Happiness: How Aesthetic Experiences of Employees Contribute to Happiness"

Steven de Groot, Mathieu Weggeman and Joan Van Aken
sdegroot@kultifa.nl, *Zuyd Hogeschool*; M.C.D.P.Weggeman@tm.tue.nl, *Technische Universiteit Eindhoven*; vanaken@dsri.eu, *Design Science Research Group*

Abstract

People spend about eight thousand days in their life in organizations, which are dominated by'rational rules' of effectiveness and efficiency legitimated by the paradigm of 'scientific management' (e.g. Taylor, 1911). Since the 60s' the concept of stress, work overload or other concepts with clear affective components have been studied by psychologists like Freeman, Glass, Singer, Stoklos and Milgram. As part of this more humanistic paradigm on work and organizations, likewise stimulated by the growing attention of ´human´ issues in organizations like meaning, happiness, and spiritually, the field of organizational aesthetics was explored in the 90s´ by Sandelands and Buckner, Strati, Ramirez, Gagliardi, Guillet de Monthoux, Alvesson and Berg, and Linstead. Stimuli in work and organizations (Organizational Aesthetic Stimuli: OAS) trigger aesthetic experiences of employees. During this aesthetic process an employee (perceiver) will come to an aesthetic judgment and emotion (e.g. Leder et al., 2004). Aesthetic judgments are often expressed as a degree of beauty or ugliness. Emotions often are expressed in terms of degree of arousal or activation combined with a judgment expressed in a degree of pleasantness (e.g. Russell, Ward and Pratt, 1978; Watson, Clark and Tellegen, 1988). Hereby, two types of aesthetic experiences can be distinguished: positive aesthetic experiences (PAEs: positive aesthetic judgment plus a degree of arousal, causing positive emotions such as affection, pleasure, happiness, pride, optimism, and enthusiasm) and negative aesthetic experiences (NAEs: negative aesthetic judgment plus degree of arousal, causing negative emotions such as frustration, anger, disgust, unhappiness, and disappointment,).

This study - as part of the research of a dissertation on organizational aesthetics - focused on the question of what aesthetic experiences employees have during their daily work, what conditions are needed for having these experience (i.e., what the drivers are), and which profits they consider for them and for the organization. We have chosen for a limited group of professionals, namely surgeons and teachers of elementary schools.

For collecting aesthetic experiences (PAEs and NAEs) of employees self-reports were used. Self-reports are increasingly used to collect daily experiences (e.g. Sørensen, 2008; Sandelands and Buckner, 1988). By using the method of self-reporting (a BEL-book: Beauty Experience Log Book), ten respondents for two weeks have daily registered about 400 registered aesthetic experiences (PAES and NAES). For each registered aesthetic experience respondents were asked to register their aesthetic judgment as well as the emotional impact in a Stendhal scale (affect grid). In this two-dimensional Stendahl-grid Stendhal-scale (based on Russell, Weiss and Mendelsohn, 1989) respondents ranked their aesthetic experience on a scale of beautiful to ugliness (X-ax) and on a scale of high versus low emotional impact (Y-ax).

The qualitative data was manually coded (selective coding, based on the defined categories of OAS after the previous empirical studies) supplemented with often used categories of events such as goal progress, interaction with colleagues, and influence or control of Basch and Fisher (Basch and Fisher, 1998). This was preceded by a cross-case analysis. Given the large number of PAEs a

progressive approach was taken by considering codes that were mentioned by at least 30% of respondents.

For the teachers, the average scores of PAEs on degree of beauty and degree of emotional impact are 2.6 (SD 1.1) and 2,8 (SD 0,8) . The average scores of NAEs on degree of beauty and degree of emotional impact are -2.3 (SD 0.9) and -2,7 (SD 1.0). Interaction with colleagues (8,2), Interaction with students / parents (7.8), Goal Achievement (7.4) and Acts of students (7,3) are the highest rated events (aesthetic judgment x emotional Impact).

50% of the their PAEs concern the experience of beauty of a result (goal achievement, of which 18% are slightly off, something to achieve). Followed by acts of customers (39%, in particular students and to a lesser extent, parents of students), the interaction with them (31%, Interaction with customers), contacts with colleagues (19% interaction with colleagues), receiving recognition (15%) and aesthetic experience of goal progress, in particular those within the classes (12% goal progress). For the surgeons, 46% of the PAEs concern the experience of beauty of a result (goal achievement), mainly mentioned in relation to an operation or treatment of a patient. Also often registered PAEs are contacts with colleagues (38% interaction with colleagues), acts of customers (15%) and goal progress (12%). Receiving recognition, goal progress, interaction with colleagues and goal achievement are the highest rated events (aesthetic judgment x emotional impact). The average scores of their PAEs of surgeons on degree of beauty and degree of emotional impact are 2.7 (SD 1.0) and 2,7 (SD 0,9) . The average scores of NAEs on degree of beauty and degree of emotional impact are -2.7 (SD 1.0) and -2,8 (SD 1.1). For surgeons, receiving recognition (10,2), interaction with colleagues (8.1), and goal Achievement (7.0) are the highest rated events (aesthetic judgment x emotional impact).

In the used Stendhal-scale affect is based on dimensions of positive (high versus low) and negative (high versus low) affect whereby eight several are described: exulted, enthusiastic, excited, aroused, fearful, nervous, hostile, drowsy, sluggish, quiet, calm and happy (e,g. Basch and Fisher, 1998). Reflecting on the collected data on aesthetic experiences (PAEs and NAEs), in particular emotions like alert, excited, elated (caused by PAEs) and nervous, tense (caused by NAEs) are remarkable. Other studies on organizational aesthetics (e.g. De Groot, 2014) show that aesthetic experiences of employees strongly correlate with thier affective commitment, represented by the outcomes pride, work pleasure, and flow experiences. Many other studies demonstrate that affective commitment can be considered as a predictor of performance (e.g. Meyer et al., 2002; Meyer et al, 2004; Allen et al., 2003; Meyer Smith, 2000; Rhoades et al., 2001).

Concluding, this study shows that aesthetic experiences, in particular PAEs, affect positive emotions of employees like pleasure and happiness as well as they can be considered as the promise of performance. Happiness by beauty. Wouldn’t that be a wonderful challenge for organizations?

"Individual Preferences and Well-Being: Identifying the importance of Life Dimensions for the Well-Being of the Flemish Population"

Haya-Al-Ajlani, Luc Van Ootegem and Elsy Verhofstadt
Haya.AlAjlani@UGent.be, Luc.VanOotegem@UGent.be, Elsy.Verhofstadt@UGent.be; *Ghent University*

Abstract

The notion that economic growth and well-being are synonymous is repeatedly challenged. Indicators of economic growth, such as GDP, do not account for individual well-being, inequality, poverty, and other non-monetary aspects that affect well-being. Moreover, monetary measures of well-being such as income are regarded as poor indicators of well-being since a higher income does not automatically correspond to a higher level of well-being (Peich and Pestel, 2011). Therefore, when the aim is to measure well-being, a multi-dimensional consideration of all that matters for well-being –whether it is monetary or non-monetary- is necessary (Stiglitz et. al, 2009; OECD, 2011; Helliwell, Layard and Sachs, 2012; Fleurbaey and Blanchet, 2013).

This paper presents a multi-dimensional evaluation of individual well-being based on five life dimensions: health, income, education, family life, and social life. Individuals have different preferences with regards to what matters most for their well-being. To reach a valid evaluation of a person's well-being, these preferences must be respected (Ringen,1995; Ravallion, 2011).Consequently, this paper follows a non-paternalistic approach; this approach judges the importance of the aforementioned life dimensions to well-being based on the preference of the sampled individuals and what they believe matters for their well-being.

We aim to answer three research questions: (1) How important are the five life dimensions for the well-being of the Flemish society, (2) what are the individual characteristics that determine this importance, and (3) how do the existing outcomes (i.e. the self-reported existing well-being level) for the life dimensions affect the importance given to them. Data for this paper is obtained from the SCV survey (Social and Cultural Changes in Flanders) for the year of 2013. This survey is commissioned by the Flemish government and contains a representative sample of 1515 individuals that live in Flanders. In order to determine their existing outcome, these individuals are asked to rate their current level of health, income, education, family life & social life on a scale from 0 to 10.

To satisfy our objective of a non-paternalistic analysis, we apply two methods.
The first method is the direct rating method (DR). It is a conventional method to capture individual preferences and it is particularly prominent in marketing research. Each respondent is asked to rate - on a scale from 0 to 10- the importance of the five dimensions (health, education, income, family & social life) to their well-being. A well-known index that applies the DR method is the OECD 'Better Life Index'. This index allows respondents to reveal their opinions (preferences) about the importance of ten dimensions. However, unlike our analysis, this index is not representative. Despite its simplicity, the DR approach has not been significantly applied in research concerned with multi-dimensional well-being (notable exceptions are by Guio et al., 2009 and Bossert et al., 2013).The second method we implement is the point allocation method. The point allocation question (PA) requires that participants allocate 100 points over these five dimensions based on how crucial each of the five dimensions is to their well-being. DR and PA often lead to different results as their dynamics differ (Doyle et.al, 1997; Bottomley and Doyle, 2013).

The PA question presents participants with a trade-off. Since individuals need to divide X points over N dimensions, giving an additional x points to one aspect decreases the number of points available to assign to the remaining dimensions. This trade-off is expected to make individuals more alert to the importance they are placing on each dimension (Bottomley et al., 2013). Unlike PA, DR entails no trade-offs as individuals rate each dimension separately. This can lead to a lack of differentiation between dimensions where participants give a high rate to each and every dimension considered (Krosnick and Alwin, 1988 ; McCarty and Shrum, 2000). The latter scenario is likely to occur because people always prefer to have a higher level of health, income, education, family life and social life all together (Takeuchi, 2014). Nonetheless, the lack of trade-offs in DR makes the rating task less difficult for participants as they do not need to keep track of the points already used and of those remaining (Bottomley et al., 2000).

Despite the above-stated differences, literature remains inconclusive with regard to the more appropriate method to apply in order to capture individual preferences. Consequently, our measure of preferences is not restricted to either one of these two methods, we rather compare and contrasts preferences generated by DR to those generated by PA. Summary statistics reveal that on average, health is the most important dimension for well-being. The average PA for health is 34 points, while the average PA for the second most important dimension, family, is 22 points. Preliminary results of linear regression show that females, older individuals, and unemployed individuals assign a higher DR to health, income, and family life. However, with PA, older individuals and unemployed individuals only give health a high allocation of points, whereas females, pensioners, and full-time workers give income higher points. Results also show that participants who have a partner or kids perceive family life to be essential for their well-being as they assign a high DR, and PA to the family dimension. Overall, respondents who state that they currently have a high level on a certain dimension, will also attribute a high importance to that dimension.

In line with the literature, PA and DR lead to different results. Health, income, and family life are likely to matter the most for the Flemish individual. Respecting individual preferences regarding what is crucial for well-being can be a strong guiding tool for policy makers. If governments are able to acknowledge these preferences, they can invest in the life dimensions that enhance the well-being of their citizens the most.

Thursday, March 23rd

Fair Distribution
(Thurs 11:00-12:30, Room M1-08: Zaal Leuven)

"Outline of the CSOL-theory of Distributive Justice"

Ingrid Robeyns
I.A.M.Robeyns@uu.nl, *Utrecht University*

Abstract

Most theories of distributive justice consist of one metric and one distributive rule. While this may make sense for theory of justice as an ideal, it is questionable whether this is convincing for an institutional theory of distributive justice. These latter theories need to account for many more tensions, undesirable effects and feasibility constraints that are relevant for society as we know it.

In this paper, I outline an account of an institutional theory of distributive justice, which, by lack of a better name is called the CSOL-theory: Capabilitarian-Sufficientarian-Opportunities-Limitarian. The SCOL theory consists of the following elements: (1) functionings and capabilities are the appropriate the metric of justice; (2) secure functionings as the first (lowest) sufficiency threshold; (3) secure functionings as a second (lower) sufficiency threshold; (4) limitarianism in material resources at the upper end of the distribution preventing riches; and (5) an account of equality of opportunity in between the thresholds of (2) and (4). I will sketch the institutional implications of this theory, and ask which ideal theory could justify this institutional theory.

"Aggregation and Theories of Fairness"

Stefan Wintein and Conrad Heilmann
stefanwintein@gmail.com, heilmann@fwb.eur.nl; *Erasmus University Rotterdam*

Abstract

Theories of fairness have recently been challenged by A. C. Paseau and B. Saunders. They argue that a theory of fairness should be aggregative: given any two fair division problems, the result of treating them as separate problems should coincide with the result of treating them as a single aggregated problem (`Fairness and Aggregation', Utilitas 27 (4), 2015, 460 - 469). They prove that no non-trivial theory of fairness can be aggregative. They conclude that theories of fairness are therefore problematic, or at least incomplete. In this article, we observe that there are theories of fairness, particularly those that are based on cooperative game theory, that do not face the problem of non-aggregativity. We use this observation to argue that Paseau and Saunders' universal claim that no non-trivial theory of fairness can guarantee aggregativity has to go: only a restricted version of that claim can survive. We further suggest that non- aggregativity should not be seen as a problem for several reasons: for one, there are acceptable theories of fairness that do not face the

problem and for another, two key assumptions behind adopting aggregativity can be questioned. Finally, we point out that there are considerable methodological differences between theories of fairness that are based on the notion of claims and those that are based on cooperative game theory. These methodological differences over choices to the fairness theorist in terms of what kinds of conditions to full and what kinds of information to consider when dividing fairly.

"Incorporating Responsibility Into Justice: An Argument for Desert and Against Luck Egalitarianism"

Huub Brouwer and Thomas Mulligan
brouwer@fwb.eur.nl, *Erasmus University Rotterdam*; thomas.mulligan@georgetown.edu, *Georgetown University*

Abstract

If we take Aristotle at his word, the there was broad consensus in the distributive justice debate in the fourth century BC: "All people agree that what is just in distribution must be in accord with some sense of desert" (Nicomachean Ethics, 1131a). That is, it was accepted that justice is a matter of giving economic agents the goods—jobs, income, and the like—that they deserve.

This consensus has been lost. Indeed, none of today's dominant theories of distributive justice provides any foundational role for desert. For utilitarians, desert is only instrumentally valuable, when it advances general welfare. Rawlsian egalitarians would require that we enrich the stupid, wicked, and indolent—if that (maximally) redounded to the benefit of the least-advantaged class (Rawls 1971). And libertarians like Robert Nozick (1974) have no interest in establishing any pattern of distribution: If unrestrained markets lead to an idle rentier class which monopolizes the product produced by diligent workers—well, so much the worse for workers. In 1971, John Kleinig already wrote that "the notion of desert seems by and large to have been consigned to the philosophical scrap heap" (p. 71).

In our judgment, this is a mistake. The time is right for desert to reenter the debate about distributive justice. Our primary purpose in this essay is to rebut those who wish to claim that desert has already made a comeback, in the form of luck egalitarianism. For example, Nicholas Barry regards luck egalitarianism as "both an egalitarian and a desert-based theory" (2006: 102), and Richard Arneson (2004) and Larry Temkin (2011) advance accounts of desert and luck egalitarianism which have little conceptual space between them.

In fact, the two theories are conceptually distinct and immiscible. Getting clear about these differences and analyzing which of the two is more plausible is the purpose of this essay.

Outline:
In §1 we argue that desert, and desert alone, can withstand the barrage of criticisms that ideal theorizing has come under of late. Most compelling to us is the recent formal work by David Wiens (2015a, 2015b, and 2016), who has proven that unless we can demonstrate that our preferred theory

of justice is feasible—that there is a path to its implementation in the actual world—we are simply unjustified in believing that it serves as a guide to making our world as just as it can possibly be.

In §2 we discuss the ways in which luck egalitarianism and desertism have been defined, identifying their essential features. Next, in §3, we adduce a series of thought experiments that show that luck egalitarianism and desertism are distinct, in seven different ways:

(1) Information: In order to assess the justice of a community, the luck egalitarian needs to know the distribution of a currency of justice and what part of this distribution is due to option luck. The desertist, on the other hand, needs to establish the distribution of desert bases (such as productive contribution or effort) and how that maps into the total amount of a currency of justice.
(2) Communitarianism: for luck egalitarians, whether there is justice in the world turns solely on how individuals fare, in their happiness or in possession of certain goods (and, in particular, how they compare in this respect to others). While a desert-based theory may be a fully liberal one, its concept is broad enough to incorporate communitarian considerations (such as economic efficiency, or the quality of the culture), if the theorist desires—and, indeed, she often does.
(3) Welfare as the currency of justice: luck egalitarians widely endorse welfare as the currency of justice. Although there is no conceptual reason why the currency of desert-based justice cannot be welfare, this account of currency, so far as we know, has never been adopted under desert–and it never will. The reason is that many desertists can (and choose to) accommodate communitarian considerations in their theory – a capacity that, as we just pointed out, luck egalitarians lack.
(4) Respect: To decide what part of the distribution of a currency of justice is due to option luck, the luck egalitarian needs to gather so much information about people's lives, that she may fail to treat people respectfully (Carter 2011). Some forms of desertism (those that do not rely on an infinite number of desert bases) can escape this problem.
(5) Level-sensitivity: Luck egalitarianism is essentially comparative and hence cannot deem as unjust a society in which bad brute luck is neutralized but the population is artificially kept at near starvation levels of resources (Feldman 2016: 138-139). Desertism is more flexible than luck egalitarianism in this regard: It can accommodate both non-comparative and comparative considerations, and hence pick up on this injustice.
(6) Scope: Luck egalitarianism, with its focus on whole lives, cannot deem as unjust a society in which people's incomes are spread extremely unequally over their life cycle (McKerlie 2001). Some forms of desertism (again, those that do not rely on an infinite number of desert bases) can escape this problem.
(7) Non-ideal guidance: To the luck egalitarian, being denied a job owing to racism, when one knows or should know that this will be the result, remains an instance of bad option luck. Luck egalitarians can only condemn this racism indirectly by appeal to the structure of society. The desertist, on the other hand, can condemn such a racist act directly.

We argue in §4 that these seven differences show not only that luck egalitarianism and desertism are conceptually distinct, but also that desertism is more flexible and, at least in some of its variants, more plausible than luck egalitarianism. Everything luck egalitarianism can do, desertism can do better. We consider and counter some common objections to desertism in §5. We conclude in §6 that the time is right indeed for desert to reenter the debate about distributive justice.

"Causal Theories, Models and Evidence in Economics—Some Reflections from Biology"

Michael Joffe
m.joffe@imperial.ac.uk, *Imperial College London*

Abstract

One important focus of economic methodology has been on models, e.g. the extent to which they are able to provide explanations (Reiss 2012 and ensuing discussion in JEM). This approach fits well with the idea that philosophical analysis should begin with the practice of scientists, here specifically with economists.

This paper takes a different, complementary, approach. It starts from a cross-disciplinary, comparative perspective on theory development more broadly. Following Marshall's famous dictum, it focuses on biology which, like economics, studies an open-ended and highly complicated reality. The aim is to propose a concept of theory, and of theory development, distinct from models; and also to discuss the relationship between theory in this sense and modelling.

The proposed concept of theory is derived from considering important examples from biology. They include the germ theory of disease, and numerous specific theories about physiological systems such as the human blood circulatory system. Similar theories occur outside biology too, e.g. tectonic plate theory.
A theory of this type seeks to understand a phenomenon or group of phenomena by combining a core set of concepts relating to the causal mechanism with a broad range of evidence. The latter includes evidence on the proposed mechanism, as well as of the "difference making" type. The theory evolves over time, and is modified to accommodate new findings as they arise. It can also encompass heterogeneity, as in the distinct behaviour of different infectious diseases; and is "allowed" to be incomplete in the sense that it does not have to encompass the whole domain – not all diseases are infectious. In addition, multiple causation is often a feature, and the theory includes an account of how the different causal factors interrelate (e.g. how nutritional status affects susceptibility to infection).

Several attributes distinguish these theories from models. In addition to the features described in the previous paragraph, they have an ontic rather than an epistemic focus. And they are phrased in the language of causation – e.g. composition/structure, flows and capacities – not of mathematics. However, mathematical models can be developed that relate to specific components of the theory, for example epidemiological models of infectious diseases in populations (different for different diseases), and models of physiological processes such as the biophysics of the circulation. Thus, models are nested within the broader causal theory; this crucially means that scientists are aware of what is omitted in such models.

Such theories do exist in economics, as in recent work on the nature and origin of money (Ryan-

Collins et al 2012; Bank of England 2014). Another example is the systematic empirical work on the growth of the modern state (Lindert 2004).

However, they are not typical of economic practice. Before the crisis of 2007-08, macroeconomic models notoriously assumed a perfectly operating financial system, without frictions. Problems originating in the financial sector were therefore invisible both to modellers and users. Prevailing practice is to review available relevant models and choose the most suitable, seen as a matter of judgement. Yet without an overarching theory that describes the overall situation causally, the model and its accompanying story define the scope covered by the theory, so it is easy to ignore important omissions.

Combining evidence with causal theory, in a two-way iterative process – so successful in biology and other natural sciences – could be usefully applied in economics. Either the causal idea or the empirical evidence can come first. For example, some aspects of the economy are straightforward to explain if starting with the evidence, rather than with standard theory. An example is the Lucas puzzle (or "paradox") (Lucas 1990), which attempted to explain why capital does not all flow, as standard theory predicts, from capital-rich to capital-poor countries, because the latter would have higher returns in accordance with diminishing returns. Subsequently the puzzle became more extreme, with massive poor-to-rich country flows, especially from China to the United States. The causes of these flows are widely understood, e.g. among economic journalists (multiple quotes from The Economist magazine): high profitability and savings rates in China generated quantities of capital that exceeded its economy's absorptive capacity. The puzzle occurs when starting from standard theory – largely because the wrong causal direction is assumed: the quantity of capital is seen as the causal factor, rather than the outcome of Chinese economic success. The key is that high levels of profit and domestic savings generated a flow of capital that had a massive causal impact (present author, submitted).

Another problem can occur when models are not embedded in a wider causal theory. It involves the relationship of the content of the available models with substantive knowledge that is not incorporated in this "theory". For example, economists working on the Lucas puzzle must know how China came to be a huge capital exporter – everyone does. So, introducing the relevant observations would have no impact on them, because it is information they already have. Bad theory can therefore be protected by the co-existence of substantive knowledge with "theory" that does not incorporate it.

Typically, the term "theory" is equated with models in economics. It tends to be based on axioms and assumptions, rather than on evidence as in natural sciences like biology – which is odd, given the sophisticated and effective methods for causal inference and for testing predictions against evidence used in economics research.

Economics could benefit from developing the type of theory described above in the context of biology. This would include the observed characteristics of natural science theories enumerated above: they are based on a variety of types of evidence; evolve over time and accommodate new findings as they arise; they can encompass heterogeneity, incompleteness and multiple causation. And crucially, they have an ontic focus, and are phrased causal terms.

Equivalents to the causal language used in the natural sciences like biology can readily be found in economics, as with the causal force of capital flow in the Chinese case outlined above. Another is the capacities of economic agents, and the initiatives they take.

"Mechanistic Models in Economics and the Prospects for Interdisciplinary Integration"

Jaakko Kuorikoski and Caterina Marchionni
jaakko.kuorikoski@helsinki.fi, caterina.marchionni@helsinki.fi; *TINT, University of Helsinki*

Abstract

Recent philosophy of science attributes several virtues to mechanistic models. Not only are mechanistic models considered explanatory, but they are also claimed to provide the scaffolding for the integration of knowledge across disciplinary boundaries. In this paper we examine the role that field-specific norms about explanation play in selecting and re-shaping representations of mechanisms through theoretical models. Specifically, we analyze cases of both intra-level integration involving models from economics and sociology (various accounts of preference formation) and of inter-level integration involving models from economics and psychology (competing models of bounded rationality). We show that variations between field-specific norms concerning what counts as a mechanistic model pose obstacles to mechanistic integration. Moreover, the mechanistic ideal presupposes that the systems investigated are functionally and structurally decomposable in such a way that the part-whole relations provide a neat picture of how the theoretical concepts in the different fields are related. However, social reality is rarely so neat.

We suggest that the ideal of mechanistic integration should function as an ontic constraint on model building, the satisfaction of which is achieved via two routes: the creation of inter-field models, which build conceptual connections between the original fields, and interdisciplinary triangulation, which, if properly understood, works in spite of and even thanks to variations concerning norms about explanation. In the end, we briefly reflect on the implications of this constraint on the identity of economics as a discipline by contrasting the picture of mechanistic integration with the views of Dan Hausman and Don Ross.

"Toward a New Classification of the Current Forms of Process Tracing"

Virginia Ghiara
vg201@kent.ac.uk, *University of Kent*

Abstract

Process tracing (PT) has been widely discussed both in the social sciences and philosophy of the social sciences. However, despite its pervasive use and a growing body of methodological literature on PT and causal mechanisms, there has been little advance in the development of this methodology. This presentation aims to contribute to this progress by offering an investigation articulated throughout two parts.

The first part of the presentation provides a new classification of the categories of PT. Recently, Beach and Pedersen (2013) advanced the debate on PT by identifying three variants of this methodology: theory-testing PT, theory-building PT, and explaining-outcome PT. Theory-testing PT aspires to test the hypothesis according to which a specific causal mechanism is present in a target population. To achieve this purpose, according to Beach and Pedersen, a single case has to be selected to verify whether the hypothesized mechanism is operating as predicted. Theory-building PT, rather than starting with a hypothesized mechanism, begins with no hypothesis. The purpose is to formulate a generalizable hypothetical mechanism from the collection and analysis of empirical observations. Finally, explaining-outcome PT's ambition is to craft a minimally sufficient explanation of a particular outcome such as a specific historical event.

In the first part of the presentation, I extend the classification offered by Beach and Pedersen by distinguishing between three methodological approaches that can be used when PT is performed: case-study PT, large-scale PT, and experimental PT. Each of these variants can be used to test hypothetical mechanisms, build mechanistic hypotheses or explain specific outcomes, as shown in Table 1. Case-study PT is performed when scholars select a representative case of the target population and collect mechanistic observations. Large-scale PT, rather than being focused on a single representative case, involves a larger class of cases and is based on the analysis of data related to this cohort. Finally, experimental PT requires both the selection of a model population and the design of a specific experiment through which the mechanism can be traced.

I argue that this distinction entails significant implications for research design. For instance, the selection of a representative population with average features is essential in order to perform correctly case-study PT. In this case, social scientists have to study carefully which features are idiosyncratic and whether typical characteristics play any role in the mechanism under study. In fact, the choice of a non-representative case of the target population might invalidate the findings or lead to inconsistent results. Moreover, experimental PT requires not only the selection of a model population, but also the development of an adequate experiment. Indeed, research participation effects such as demand characteristics (Orne 1959) and the Hawthorne effect (Adair 1984) could nullify every discovery obtained through this method.

Besides offering a detailed distinction of the categories of PT, the classification provided by this presentation enables us to clarify how the emergence of new trends in the social sciences can enhance our ability to perform these forms of PT. In particular, the second part of the presentation is focused on the different ways in which big data could be used to trace causal mechanisms. Over the

last years, a data-deluge has led to the collection of numerous and varied data about large populations. Even though this phenomenon has often been related to the advance of quantitative methods in the social sciences, I argue that this new available material can also be employed in different ways to perform every form of PT.

On the one hand, both the massive amount of information about a particular phenomenon and large cohort's data seem particularly suitable to formulate mechanistic hypotheses through the method of theory-building PT or explaining-outcome PT. In fact, while in-depth observations can be useful to recognize the potential relevant parts of a mechanism and its functioning, data about large cohort can be mined to identify correlations that may indicate the operation of a causal mechanism. On the other hand, data about large cohorts might enhance social scientists' ability to test specific causal mechanisms by means of theory-testing PT. In addition, big data can be used to perform every methodological variant of PT: intuitively, large-scale PT can take advantage of big data, however this data can be used also to achieve in-depth study or to collect evidence by means of natural experiments.

Overall, the presentation provides an in-depth overview of the categories of PT. This classification might contribute to foster the dialogue between disciplines using diverse variants of PT, such as behavioural economics (whose findings are often based on experimental PT), history (in general employing case-study PT) and sociology (whose research, after the emergence of big data, is increasingly based on large-scale PT). Furthermore, this distinction could also lead social scientists to employ different forms of PT to strengthen the findings obtained through this method. This idea is similar to the concept of "triangulation" proposed in several sciences: given that every form of PT presents specific limitations, different approaches can be adopted so that, if the results are consistent, scholars can conclude that the finding is not the artefact of a particular form of PT. Finally, the concluding part of the presentation paves the way for further research on the current methodological developments of PT and on its possible future. In the social sciences, the method of PT has been studied for the last decades, however, given the current trends, it is worth questioning how PT should be updated in the era of computational social science.

Improving Rational Choice Theory
(11:00-12:30, Room M3-03: Zaal Aberdeen)

"Conditional vs. Unconditional Attitudes to Risk"

Ittay Nissan-Rozen
ittay.nissan@mail.huji.ac.il, *Hebrew University of Jerusalem*

Abstract

The orthodox treatment of attitudes to risk (understood in terms of behavioral dispositions to choose some lotteries over others) in economics is to explain them using a reference to features of the agents' preferences over sure-outcomes.

Psychological evidence indicates, however, that people's choices under conditions of uncertainty are influenced also by their attitudes to risk itself. The inability of the orthodox view to account for such evidence has led several scholars to suggest decision-theoretic frameworks in which it is possible to express agents' attitude to risk independently of their preferences over sure-outcomes.

In this paper I adopt one of the strategies used in this line of research, namely representing agents' attitudes to risk as agents' attitudes to "risk propositions" in Richard Jeffrey's decision-theoretic framework, in order to draw an interesting distinction between two different types of (non-reducible to preferences over sure outcomes) attitudes to risk. I then demonstrate the importance of this distinction by applying it to the Allais Paradox.

In order to represent attitudes to risk in Jeffery's framework, we can assume that the set of propositions over which the agent's desires and beliefs are defined includes propositions of the form "the probability of A is x", where A is another proposition in the set and x is a number in the unit interval. Let us call such propositions, risk-propositions.

Let A be a proposition and let A* be a risk-proposition that assigns a probability of x to A. Let c(.) be a rational agent's credence function and let d(.) be the agent's desirability function. It follows from Jeffrey's desirability axiom that:

1. $d(A^*) = d(AA^*)c(A|A^*) + d(-AA^*)c(-A|A^*)$

We can also assume in the context of our investigation that:

2. $c(A|A^*) = x$ and $c(-A|A^*) = 1 - x$

From 1 and 2 we get:

3. $d(A^*) = d(AA^*)x + d(-AA^*)(1-x)$

Equation 3 says that the agent's attitude to risk with respect to A and x is determined by the agent's desires for AA* and –AA*. For example, if x=0.5, d(AA*) = d(A) and d(-AA*) = d(-A), the agent is neutral to risk with respect to A and x: If d(A) = 100 and d(-A) = 0, the agent's desire for "the probability of A is 0.5" equals 50 (which is exactly half of how much the agent desires A).

More generally, let us define the agent's "expected desirability from A with respect to x", denoted, ED(A, x), in the following way:

ED(A, x) = d(A)x + d(-A)(1-x)
We can now define:

a. The agent is risk-averse with respect to {A, x} iff d(A*) < ED(A, x).
b. The agent is risk-seeking with respect to {A, x} iff d(A*) > ED(A, x).
c. The agent is risk-neutral with respect to {A, x} iff d(A*) = ED(A, x).

Now, in Jeffrey's framework the agent's desire for the proposition AA* can be expressed as a function of the agent's desire for A, of her conditional desire for A* given A and of her desire for the tautology (which in Jeffrey's framework represents the agent's desire for the status que):

4. d(AA*) = d(A*|A) + d(A) - d(T)

The conditional desire for A* given A, d(A*|A), should be understood as the agent's evaluation of A* under the assumption that A is the case. Thus, the expression d(A*|A) can be interpreted as representing the agent's conditional attitude to risk with respect to {A, x}: When d(A*|A) = d(T), the agent does not care at all about having a certain probability for A being the case, when she evaluates this prospect under the assumption that A is indeed the case. It makes sense, then, to take this condition to express conditional risk neutrality with respect to {A, x}. Similarly, we can define conditional risk aversion with respect to {A, x} using the condition d(A*|A) < d(T) and conditional risk seeking with respect to {A, x} using the condition d(A*|A) > d(T).

Notice that, unlike in the case of unconditional attitude to risk, as defined above, an agent's conditional attitude to risk with respect to a given proposition, A, and a probability value, x, can be different from the agent's conditional attitude to risk with respect to the negation of the proposition, -A, and the corresponding probability value, 1-x.

This last observation leads to an interesting distinction, absent – to the best of my knowledge - from the current literature. If an agent is conditionally risk averse with respect to both {A, x} and {-A, 1-x} she must be, it is easy to prove, unconditionally risk averse with respect to {A, x} (and thus, of course, with respect to {-A, 1-x}). However, it is also possible to prove that an agent can be unconditionally risk averse with respect to {A, x} while being conditionally risk seeking with respect to either {A, x} or {-A, 1-x}.

These two different sources for one's manifested attitude to risk may correspond to two different psychological states one might be in. For example, an agent might be risk-seeking with respect to the prospect of dying while fighting for a cause she takes to be a noble one, because she values the risk of dying both under the assumption that she will die and under the assumption that she will not die, but she can also be risk-seeking with respect to this prospect because while she disvalues the risk of dying under the assumption that she will die, she values – to a greater extent – the risk of dying under the assumption that she will not die. In the latter case, it would be wrong to describe the agent's risk-seeking behavior as either emerging from her "risk-seeking nature" or from her attitudes toward sure-outcomes. Rather her risk-seeking behavior is a result of her conditional desire for the risk of dying given that she will not die.

I demonstrate how this type of attitude to risk can be used in an elegant explanation for the typical choice pattern in the Allais Paradox.

"Evaluating Heuristics for Risky Choice"

Patricia Rich
ptlrich@gmail.com, *University of Bristol*

Abstract

Proponents of Ecological Rationality argue that we should evaluate the rationality of choice-generating processes instead of the choices themselves, and for many purposes there are good reasons for doing this. This paper explores how this can be done, arguing largely via example: I simulate simple choice processes to choose between lotteries, and compare their performance records. The first criterion of evaluation is conformity to the Expected Utility (EU) axioms; Ecological Rationality's hallmark Priority Heuristic stands out for violating both transitivity and independence relatively often. An important objection to this method of evaluation is that there is no demonstrated connection between axiom violations and objectively poor performance or subjective choice satisfaction in practice. Therefore a second criterion is applied: I compare the processes in terms of the expected values of their choices. The Priority Heuristic again performs poorly, but Minimax performs much worse. This highlights the tension between (axiomatic) coherence and correspondence (to external performance metrics). Finally, I address the objection to coherence head-on and connect coherence and correspondence: I show that violations of both transitivity and independence carry measurable costs in terms of expected value. This analysis supports a number of conclusions, including that process evaluation is viable, that EU evaluation is valid and indeed indispensable, that objective performance must nonetheless be considered, and finally that the Priority Heuristic is less ecologically rational than several much simpler choice processes.

The goal of this paper is to model and defend a method of evaluating choice rationality. When it comes to the fundamental task of evaluating choices, there are two compelling approaches available that vie for dominance. The first, standard in economics, is to apply the EU axioms to see whether a choice pattern is coherent. This is a compelling method because (1) it accommodates the subjective aspect of choice goodness (via utility), (2) the axiomatic test is applied to observed choices and is therefore relatively scientific, and (3) the axioms themselves are intuitive, sensible, and bolstered by many proofs and arguments. A newer approach is advocated by psychologists under the banner of Ecological Rationality; they forcefully criticize EU, proposing that processes -- especially simple heuristics -- should be assessed according to how well they perform in their context of application. In contrast to the coherence standard of EU, this is a correspondence approach because processes are meant to be judged on objective scales according to how "fast, frugal, and accurate" they are. Despite EU's virtues, this approach is also quite compelling, firstly because how and why people make the choices they do has undeniable intuitive relevance for the rationality of those choices, secondly because this information is invaluable to the project of improving people's choices, and thirdly because external performance metrics (such as real earnings and time spent) must be acknowledged to have some importance.

I provide a detailed argument elsewhere that both of these methods contribute something important, and I propose a particular way of combining them: in brief, we should evaluate processes by simulating them and applying the relevant axioms (here, the EU axioms) to the results. This paper develops a case study that bears out this methodological claim. This case study evaluates the rationality of the Priority Heuristic and several other well-known choice procedures: Minimax, Maximax, the Hurwicz Criterion, and a hybrid between Minimax and expected value maximization. These processes are compared according to their performance on a lottery choice task; I use 171

lotteries from the decision science literature, many of which were randomly generated, so that each process has 29,070 risky choices to make (each chooses between every pair of lotteries in the test set). The first step in evaluating the processes is to compare their rates of violations of the EU axioms; the PH stands out for its high violation rate. I exposit a main objection to this method of evaluation, coming from Ecological Rationality: why think that violating those axioms indicates poor performance by agents' own lights? I then show that comparing the processes using the plausibly more objective criterion of expected value delivers similar results, though Minimax's relatively low expected value makes salient the way in which choice coherence and objective gains can pull in different directions. The final step of the analysis addresses the question of whether the relationship between the Priority Heuristic's axiom violations and diminished objective earnings is systematic or accidental. I show that the relationship is systematic, and that in fact, choice coherence and objective gains coincide in a strong sense in this setting: axiom violations are associated with significant foregone profit. Specifically, violating transitivity or independence costs the chooser a bit more than 30% of the available gains (in expectation), and in the case of transitivity, there is reason to view this as an underestimate of the real costs.

I show how we can use a hybrid method to address the rationality of choices in straightforward situations of risk in a satisfying way. This hybrid method is (for many purposes) better than either EU or Ecological Rationality on its own because it incorporates the important intuitions behind each of them, enabling us to evaluate processes without abandoning either the clear normative criteria provided by axioms or the notion of subjective utility. At the same time, it is important to check up on the relationship between coherence and external performance metrics when this is possible. Apart from the methodological point, this case study has direct implications for the rationality of risky choices because a range of simple choice processes of independent interest are evaluated and compared. It is noteworthy that the simple standard procedures from decision theory -- although intended to apply to situations of uncertainty -- mainly outperform the predictively-successful and more complex Priority Heuristic in situations of risk.

“Homo Socialis: A naturalistic alternative for the Rational Choice Model”

J.W. Stoelhorst and Alan Page Fiske
j.w.stoelhorst@uva.nl, *University of Amsterdam*; afiske@anthro.ucla.edu, *UCLA*

Abstract

The rational choice model has a central place in economic theory, yet the ‘model of man’ that it implies has been widely criticized. But what is the alternative?

In this paper, we propose a naturalistic model of man based on Relational Models Theory (Fiske, 1991, 1992). We argue (1) that from a naturalistic perspective, economic welfare depends on solving problems of interdependence, (2) that in the course of human evolutionary history, this has led to cognitive adaptations aimed at the regulation of interdependent relationships, (3) that these adaptations have resulted in a social and moral cognition that is organized around four universal forms of social relations (Communal Sharing, Authority Ranking, Equality Matching, and Market Pricing), (4) that these four relational models are the building blocks of socio-economic institutions. We contrast our naturalistic model of man, which we will refer to as Homo socialis, with the Homo economicus model featuring in rational choice theory, and we derive some general implications for the design of welfare-enhancing institutions.

When considering the role of rational choice in economic theory, we should distinguish at least three aspects of its use: (1) its 'model of man' (Homo economicus), (2) the methodological role of this model of man (i.e., its central place within an optimization-cum-equilibrium approach), and (3) its place within the larger 'Invisible Hand' story about economic welfare in standard economic theory (that free markets guide the actions of rational self-interested agents towards collectively optimal outcomes). Our targets in this paper are (1) and (3).

The rational actor model 'works' because it is consistent. The standard economic theory of welfare as driven by 'perfect decentralization' (Demsetz, 1988) posits a world in which mutually beneficial interactions are exclusively arm's length and mediated by an impersonal price mechanism. In other words, Homo economicus can be rational and self-interested because she lives in a world that is free of social relationships.

The fundamental problem with this model is not just that it gets the model of man wrong, but that its story of economic welfare is incorrect. Historically, economic progress is not about market exchange, but about joint production. Human evolution is the result of solving the problems of interdependence that go hand in hand with joint production – problems like the social dilemmas featuring in, for instance, public good and trust games. In interdependent situations, rational self-interested actors defect – in other words, in these situations the standard model (of perfect decentralization maximizing collectively welfare) does not apply. The key point of human evolution is that it allowed us to overcome the collective action problems inherent in joint production – in other words, that we evolved beyond acting purely in our short-term self-interest.

The broad outlines of the ultimate explanation of our ability to overcome collective action problems seem relatively clear (e.g., Bowles & Gintis, 2011; Stoelhorst & Richerson, 2013). And there is a rapidly accumulating body of evidence about the proximate mechanisms involved in relating to others in interdependent situations, such as social preferences, trust, and reciprocity (e.g., Van Lange et al., 2014), including the neurological systems that are implicated (e.g., Rilling & Sanfey, 2011).

What the combination of the ultimate and proximate explanations of human cooperative behaviors seems to suggest is that we evolved two cognitive 'modes': a 'strategic' and 'moralistic' mode, where the function of the strategic mode is to instrumentally pursue our short-term self-interest, and the function of the moralistic mode is to regulate cooperative social relations. On this view, the rational actor model, through its Homo economicus model of man, captures the strategic part of human nature. But it ignores the moralistic part that is the main driver of human welfare.

We build on Relational Models Theory (Fiske, 1991, 1992) to propose a model of man that incorporates both our strategic and moralistic cognitive modes (Figure 1).

At the top are the four relational models: Communal Sharing (a relationship of unity, community, and collective identity), Authority Ranking (a relationship of hierarchical differences based on legitimate authority, accompanied by the exercise of command and complementary display of deference and respect), Equality Matching (a relationship among equals manifested in balanced reciprocity), and Market Pricing (a relationship where people compute cost/benefit ratios) (Fiske, 1991). The models are cognitive frames that people use, automatically and often unconsciously, "to plan and to generate their own action, to understand, remember, and anticipate others' action, to coordinate the joint production of collective action and institutions, and to evaluate their own and

others' actions" Fiske (2004: 3). These four models capture the moralistic part of our cognition, with morality understood as social relationship regulation (Rai & Fiske, 2011).

The middle part of the model, 'null relationship', refers to framing interactions in impersonal terms: others' conceptions, goals and standards are simply ignored (Fiske, 1992). Finally, at the bottom, 'asocial relationship' refers to using other people as a means to an ulterior, selfish, end (Fiske, 1992). Together, the middle and bottom part of the model capture the two possible manifestations of the strategic part of our cognition.

Based on this model, we argue (1) that the use of the rational choice model in standard economic theory results in a very problematic conflation of the null relationship (which is based on self-interested and amoral) and MP (which is inherently moral and requires a normative commitment to the spirit of a contract), (2) that economic 'contract' theories (e.g., agency theory and transaction cost economics), which extend standard economic theory to problems of institutional design, are based on a very problematic conflation of the asocial relationship (which is based on self-interest 'with guile' and immoral) and AR (which is inherently moral and requires legitimate authority), and (3) that real world solutions to the social dilemmas of collective action in interdependent situations, such as Ostrom's (2000) design rules, work because they help actors avoid null and asocial relationships by using a judicious combination of CS, AR, EM, and MP to frame joint production relationships.

The Role of Economists
(11:00-12:30, Room M3-04: Zaal Auckland)

"Revisiting the Performativity of Economics: Do Economists Make Neoliberalism?"

P.W. Zuidhof
P.W.Zuidhof@uva.nl, *University of Amsterdam*

Abstract

This paper revisits the debate over the performativity of economics, to ask whether economics makes neoliberalism. The notion of the performativity of economics as it was first broached by sociologists such as Michel Callon (1988) and further substantiated in the studies of MacKenzie (e.g. 2006) for instance, carried great promise for recasting the relationship between economics and its worlds. If economists indeed perform markets as the authors in MacKenzie, Muniesa and Liu (2007) wondered, this dramatically alters how one conceives of the place of economics in the world it studies. Despite its promises, the notion of economic performativity nonetheless continues to be met with various kinds of skepticism (e.g. Didier 2007, Santos and Rodriguez 2009; Miller 2011).

Reviewing the debate on the performativity of economics this paper finds that it has largely focused on epistemological issues. Picking up on insights from the rhetoric of economics and related discursive approaches, this paper argues that an exclusive focus on epistemological questions and restricted conceptions of performativity tends to gloss over the broader potential of economic performativity that are particularly relevant for understanding the political performances of economics. As part of an attempt to understand the role of economics in the propagation of neoliberal policy, and for deciphering the oft-heard locution "neoliberal economics", this paper explores the political dimensions of economic performativity. Using the example of how theories of emissions trading have been put into practice, it seeks to offer an outline of how economic theory has been instrumental in performing pronounced neoliberal, political rationalities. The paper proposes that in order to understand the political performances of economics, both MacKenzie's Barnesian and Callon's actor-network notion of performativity, need to be supplemented with an account of the discursive performances of economic theory.

"The Epistemological Break in Economics"

Erwin Dekker and Pavel Kuchar
e.dekker@eshcc.eur.nl, *Erasmus University Rotterdam*; pavel.kuchar@hotmail.com, *International Business School Suzhou*

Abstract

The gap between economic beliefs and lay beliefs is often believed to be large (Garnett 1999; Caplan 2001; Dixon et al. 2014; Wobker et al. 2014), frequently leading to researchers claiming that consumers and voters are ignorant (Caplan 2011). Similarly economists are often in agreement that economics has little influence on policy (Stigler 1982; McCloskey 1990; Klamer and Meehan

1999; Frey 2006). On the other hand there is a widespread belief that economic ideas are more influential than ever in our day and age, both the Washington consensus and neoliberalism are believed to be the direct outcomes of the influence of economic ideas (Mirowski and Plehwe 2009; Burgin 2012). Prima facie it seems hard to square these two contrasting views. That is not for lack of trying, economists have argued that their ideas are not more influential, because of rent-seeking problems (for an overview see Rodrik 2014). People know better, but since these ideas clash with their interests they adopt different belief. Another explanation might be that experts are in charge of policy-making so that we should examine the economic knowledge of experts.

This paper offers an alternative explanation of this paradox. It will argue that to understand the gap between the knowledge of economists and the public, and between economists and policy experts the notion of an 'epistemological break' is of great help. The concept of the epistemological break is developed in French philosophy where it first denotes a kind of Kuhnian scientific revolution, resulting in an epistemological break between the two paradigms. But in the work of Althusser it is developed in relation to Marxist social theory, in which it comes to stand for the need for a fundamental break between the perspective of the theorists and that of his subjects (Balibar 1978). A similar break can be said to have occurred when analytical philosophers critiqued the philosophy of ordinary language.

It is argued that most of twentieth-century economics has been characterized by a similar epistemological break between common-sense or lay understandings of the world and 'scientific' or expert understandings of the world. Some have associated this with modernism in economics, in which representation is problematized, and "appearances deceive" (Klamer 1987; Fullbrook 1997; Emmett 1999; Leonard 2006; Klamer 2007). As Lavoie argued: "Whether understanding the meanings operative in everyday life is taken to be below them (and best left to undergraduates) or above them (and best left to angels), economists seem to agree that their scientific discourse of economics should dissociate itself from the everyday discourse of the economy" (Lavoie 1990). This paper will explore what this epistemological break consists of, and to what extent it is a necessary condition for economic theorizing (Lemieux 2014).

From this analysis an alternative understanding of the gap between the knowledge of the economists and laymen is developed, which emphasizes the way in which economic concepts such as the market and values such as competition have been integrated in lay understandings. This suggests an alternative explanation of the empirical results on supposed economic ignorance. These are indeed true on the level of basic policy questions and basic knowledge of economic theory, but they miss the way in which economics has been influential in altering the lay perspective on the economy and society.

"Economics imperialism: Further Expansions"

Uskali Mäki
uskali.maki@helsinki.fi; *TINT, University of Helsinki*

Abstract

This is a further expansion on my earlier account of economics imperialism, and it discusses further expansions of economics to domains and disciplines as cases. The paper looks at the concept of scientific imperialism – and economics imperialism as its special case - by reflecting on its relationship to normative considerations. It proceeds through two sets of questions. First, considering that interdisciplinary transfer / travel / trespassing happens all the time throughout

science, what distinguishes imperialistic from non-imperialistic trespassing? My preference is to draw a vague line that is normatively neutral, so the concept of scientific imperialism itself should not imply denouncing the phenomenon. That the line is going to be vague is an implication of deciding to define the concept disjunctively, in terms of subsets (with family resemblances between them) of a set of properties such as expansion, intrusion, unification, inequality, asymmetry, domination, hegemony, control, superiority, exploitation. Second, do normative standards of epistemic performance depend on whether trespassing occurs or not; and whether it is imperialistic trespassing or not? This divides into two further issues: a. The epistemic pursuits and alleged epistemic achievements of imperialistic science often look similar to those of non-imperialistic science (eg expansion, unification, novelty); should they be assessed differently, even in terms of different standards? b. The alleged failures in the epistemic (and perhaps other) pursuits of imperialistic science often look similar to those of non-imperialistic science (eg explanatory failure, crowding out of other lines of research); should they be assessed differently, even in terms of different standards?

In answering such questions, I am generally attracted by the idea that imperialist science is to be judged by the same standards that we apply to all science, together with the generally advisable proviso that the standards and their application are often to be adjusted so as to be responsive to the peculiar characteristics of each specific type of case. The approach sticks to the earlier strategy of normatively assessing economics imperialism by proceeding case by case, invoking ontological, epistemological, axiological, and institutional constraints that any case must meet to be recommendable. The recent literature on biological markets will be scrutinized to examine the reach of the concept of economics imperialism and its normative dimensions.This is a further expansion on my earlier account of economics imperialism, and it discusses further expansions of economics to domains and disciplines as cases.

The paper looks at the concept of scientific imperialism – and economics imperialism as its special case - by reflecting on its relationship to normative considerations. It proceeds through two sets of questions. First, considering that interdisciplinary transfer / travel / trespassing happens all the time throughout science, what distinguishes imperialistic from non-imperialistic trespassing? My preference is to draw a vague line that is normatively neutral, so the concept of scientific imperialism itself should not imply denouncing the phenomenon. That the line is going to be vague is an implication of deciding to define the concept disjunctively, in terms of subsets (with family resemblances between them) of a set of properties such as expansion, intrusion, unification, inequality, asymmetry, domination, hegemony, control, superiority, exploitation. Second, do normative standards of epistemic performance depend on whether trespassing occurs or not; and whether it is imperialistic trespassing or not? This divides into two further issues: a. The epistemic pursuits and alleged epistemic achievements of imperialistic science often look similar to those of non-imperialistic science (eg expansion, unification, novelty); should they be assessed differently, even in terms of different standards? b. The alleged failures in the epistemic (and perhaps other) pursuits of imperialistic science often look similar to those of non-imperialistic science (eg explanatory failure, crowding out of other lines of research); should they be assessed differently, even in terms of different standards?

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recommendable. The recent literature on biological markets will be scrutinized to examine the reach of the concept of economics imperialism and its normative dimensions.

Supplementary Session
(Thurs 11:00-12:30, Room M3-05: Zaal Praag)

"Does the Rational Self-Interest Model Rest upon a case of Mistaken Identity?"

Robert Allinson
rallinson@soka.edu, *Soka University of America*

Abstract

Adam Smith, a professor of Moral Philosophy at the University of Edinburgh famously taught that the pursuit of self-interest will conduce to the good of the whole of society. Smith's philosophy is based on two fundamental assumptions. The first is that when the self acts to pursue her or his self-interest, the self identifies her or his self with the individual self. The second is that if her or his self-interest is to be identified with the interest of the individual self that such a pursuit will be in the best interest of society as a whole. From these two premises, the odd altruist, acting for the sake of the other may be viewed either as an exception due to an abnormal variation or as a necessary built-in corrective to enable the model to function without running the risk of breaking down by the production of unsustainable inequality. While altruists exist, they are either outside the system or enable the functioning of the system.

As a consequence of the adoption of Smith's philosophy, the world has undergone an incredible economic development. At the same time, it can be argued that the result of the adoption of Smithian economics has been the production of enormous economic inequality. The social experiments to reach economic equality by replacing the motivation based on the individual self with the motivation based on the other have been abandoned as failures. Attempts to go to the opposite extreme, to replace the identity of the self with the identity of the other, have not worked. Witness the examples of Russia and China. The reason, I would argue, that the social experiments to produce equality have not worked is that they have made the opposite mistake to that of Smith. They have defined the self in terms of the other.

The argument of this paper is that in order for a new economic philosophy to be successful, it must take into account motivation that produces economic action, but at the same time does not result in unacceptable inequality. In my paper, I would like to raise the question if the seeds for a new definition of economics are already contained in current attempts by economists to define economics. If it is the case that an examination of current economic definitions reveals that self-interest cannot be defined without taking into account other-interest, it may be that a solution can be proposed that is not based on a case of mistaken identity.

The new definition of economics to be proposed herein is built upon six definitions of economics that are found in standard economics textbooks of economics. This paper goes through each of the six definitions in detail to determine if the current definitions already implicitly contain the concept of the other. The argument of the paper is that a sustainable philosophical definition of economics can be generated that includes the technical needs of the old definitions in order to satisfy the criterion of economic soundness and includes a new, more explicit ethical dimension that can avoid the excesses of the Smithian model that have produced unacceptable levels of economic inequality.

"Is Everyone Self-Interested? Bernand Mandeville Versus David Hume"

Fernando Morett
f.morett@lse.ac.uk, *London School of Economics and Political Science*

Abstract

In this paper, I compare the moral psychology of universal self-interest from the Dutch physician Bernard Mandeville with the combined moral psychology of self-interest and sympathy from David Hume. Both Mandeville and Hume have been largely read as philosophers but not as scientists. I discuss their work exclusively as a case of science; in particular as a case of early modern science, presenting the controversy between the two as a case of theory choice under the normative methodology of the vera causa from the eighteenth century, using inductive support, experimentum crucis and simplicity as criteria. On all three criteria I conclude that Mandeville's theory of universal self-interest wins the controversy.

A choice made using epistemic criteria such as inductive support, experimetun crucis and simplicity holds important effects and it is highly relevant. It protects and empowers theories and scientists against political abuse and undue influence from state, religious and corporate institutions as well as from undue influence from rival scientists. Bernard Mandeville was called twice by the Grand Jury of Middlesex accused of 'running down Religion and Virtue as prejudicial to Society, and detrimental to the State' (Speck, 1978, p. 362; Bald, 2014, p. 105). His ideas were banned in courts and his books burned by executioners in the streets of France (Sedlacek, 2011, p. 185). He was challenged by Protestants, Catholics and atheists alike. His books remained in the Index Librorum Prohibitorum until its last publication in 1966. Besides Hume, Smith and Hutcheson; Bishop George Berkeley, Bishop Joseph Butler and Jean-Jacques Rousseau also joined as his opponents. Mandeville virtually lost the battle during those years, and for a long time after. Nonetheless, his theory remains relevant to the current debate in economics and political science, where self-interest and sympathy are debated as the right foundations of theories and policies (Arrow, 1977; Becker, 1976; Brennan and Buchanan, 1981; Downs, 1957; Harsanyi, 1977).

David Hume, Adam Smith and Francis Hutcheson regarded the psychology of self-interest advanced by Thomas Hobbes and Bernard Mandeville as the rival theory to be defeated; it was a theory making 'so much noise in the world', Smith reports (1790, p. 313). Hume positively praises Mandeville's theory and method in the Treatise (1739-1740, pp. 5, 407). This volume pays more attention to self-interest than An Enquiry Concerning the Principles of Morals, in which Hume discusses the disinterested passions of humanity and benevolence in more detail. Both Hume and Smith were highly critical of Mandeville's theory, which they considered to be 'malignant' and 'wholly pernicious' because it leaves no grounds for 'a pleasing sentiment of sympathy and humanity'. They actually allude to Mandeville with epithets such as 'sportive sceptic' and 'superficial reasoner', whose reasoning is 'ingenious sophistry' (Smith, 1790, pp. 308, 312; Hume, 1772a, pp. 35, 48, 54, 95)

Hume strongly criticises the self-interested individuals described by Mandeville who are 'monsters' 'unconcerned, either for the public good of a community or the private utility of others' (1772a, p. 48); they are replicas of Ebenezer Scrooge who even at Christmas shows no humanity, no concern for others (Dickens, 1843). In contrast, Hume describes a polite, sympathetic and utilitarian individual who, despite being self-interested, is capable of performing acts of disinterested benevolence and humanity; that is, a Scrooge who is morally reformed by secularised Christian values. Hume not only claims that true disinterested charity and beneficence exists grounded in the

natural sentiments of humanity, but he also claims that these sentiments can ‘overpower’ and ‘over-balance’ self-interest (1772a, p. 77; 1739-40, p. 313).

The debate between the psychology of universal self-interest, and the combined psychology of self-interest and sympathy is about true causes or true motives of moral behaviour. This debate is concerned with the ‘metaphysical’ part of human psychology. That is to say, with unobservable entities and mental processes whose study Mandeville (1732a, p. 3) described as analogous to the study of the inner anatomy of the human body performed in medicine. Using the same naturalistic analogy from Mandeville, Hume explains that the moralist is a painter who is concerned with the beauty of moral behaviour, portraying it with ‘the most graceful and engaging airs’, whereas the moral anatomist is concerned with ‘the most hideous and disagreeable’ parts analogous to the ‘the inward structure of the human body, the position of the muscles, the fabric of the bones, and the use and figure of every part or organ’(1772b, p. 8; 1739-40, p. 395). Hume did not consider himself to be a moralist but a moral anatomist; he did not write any substantive normative moral argument. This is why the moral philosopher Francis Hutcheson criticised the Treatise because of its lack of ‘Warmth in the Cause of Virtue’ (Greig, 1932, p. 32, Letter 13, 17th September 1739).

Compared to Hume, Mandeville was epistemically more cautions, more rigorous and was more conscious of the uncertainty involved in making inferences to unobservable entities, and processes in the mind. For tackling the problem of rival explanations, Mandeville’s own method has two important steps. The first one consists of a test and a deductive inference, that is, any claim on disinterested benevolence as the motivation for action must be tested against the rival hypothesis, namely self-interested motives. This test takes the form of a refutation. If the rival hypothesis becomes refuted, then via a disjunctive syllogism the alternative one significantly increases its chances of being true. The second step supplements the first one, first by adding a detailed, penetrating and sharp description of how self-interested motives may operate in the particular case under scrutiny, and second by adding numerous cases where self-interested motivations are confirmed via a simple induction, leading to a generalisation from other similar cases. In contrast, Hume relies on enumerative induction only, and the number of cases he presents is rather small. Moreover, the folk psychology he relies on had already been discredited by Thomas Hobbes in Britain, and by François de La Rochefoucauld in France.

“Towards Property-Owning Democracy by Private Property of Personal Data”

Francis Cheneval
francis.cheneval@philos.uzh.ch, *University of Zurich*

Abstract

In this paper we present philosophical justifications of property rights of personal data. We argue that property of personal data provides control over a moral realm of personhood and at that same time sustained ownership of a resource that produces a basic income. Within the proper regulatory framework personal data can become a productive means of capital gains for people who hitherto have little or no access to such a form of income. From the point of view of a Rawlsian theory of justice, personal property of personal data can bring substantive progress on the path towards the realization of a property-owning democracy (POD), which is a social system that essentially offers widespread and sustained access to productive assets. What is at stake regarding the subject matter of this paper, however, is much more than just a specific problem emerging in Rawlsian theory. These are times of rapid transformation of production in which workers get replaced by machines, of excessive economic inequality due to unequal access to productive assets, of large-scale transfer of personal information to digital companies leading to massive digital heteronomy. In this context we think that this proposal of a basic income via property rights over personal data is of considerable importance, independently of the stance one might take on Rawlsian theory of justice.

"Kant and the Self-Ownership Thesis: the reasons behind a difficult marriage"

Fabrizio Ciatti
fabrizio.ciatti@gmail.com, *Erasmus University Rotterdam*

Abstract

Libertarianism in the version articulated by Nozick qualifies as a theory of justice premised on people's rights. Its advocates appeal to the self-ownership thesis (SO) to argue that individuals have inviolable property rights over their body that impose thick constraints on what others may do to them without their consent. Kant's authority is often invoked to buttress SO and the libertarian political philosophy built on it. The chief purpose of this essay is to undermine a Kantian defense of the libertarian view of justice, whereby individuals are left free to do what they want with their bodies, powers and material holding as long as no property right is violated. I set out to accomplish this task by calling into question the compatibility between the Kantian ethics and the self-ownership principle which lies at the heart of Nozickean libertarian theories. If my arguments succeed at undermining this compatibility, then there will also be valid reasons to doubt a Kantian support of the libertarian view of justice in general.
The essay deals with questions of political philosophy and ethical aspects of economics.

Outline. First I will introduce the self-ownership thesis, the concept of a rights-based theory and the alleged tie between SO and Kantian categorical imperative. In section 1. I will try to demonstrate that the respect of Humanity (second formulation of the categorical imperative) does not and cannot license some acts that SO tolerates. In section 2. I will challenge Richard Taylor's attempt to derive the self-ownership rights from Kant's duty-based morality.
A closer examination of his attempt will also teach us a valuable lesson about the relation between rights and duties. Libertarianism with its stress on SO takes on a view whereby rights are primitive with respect to duties. Reverting this order after putting duties first would determine the loss of salient features of a theory premised on SO. If that is the case, the possibility to tie together libertarianism and Kantian ethics becomes even more remote.

Both sections converge, though in different ways, towards the conclusion that libertarianism and Kantian ethics can hardly stay together.

"Nudge versus Boost: A Distinction without a Difference"

Andrew Sims and Thomas Mueller
andrew.sims@uclouvain.be, thomas.muller@uclouvain.be; *Universitè Catholique de Louvain*

Abstract

There are two sorts of philosophical problems that dog behavioural public policy (BPP), which is an approach to state intervention which uses knowledge about the bounded rationality of human reasoners—largely from behavioural economics—in order to achieve policy aims. The first of these is normative and the second is epistemological. The normative controversies concern the compatibility of BPP with liberal ideals. For instance, it is not clear that BPP can be effective in the absence of manipulation, nor whether it undermines the autonomy of individuals with respect to their self-regarding choices. The epistemological controversies concern the effectiveness of BPP as well as the evidence base upon which it rests. For example, can we take experimental evidence alone as warrant for the effectiveness of BPP, or does it require evidence from the policy context as well? How coherent is the policy approach with the theory that it is its supposed basis?

Till Grüne-Yanoff and Ralph Hertwig (2016) have introduced a distinction between sorts of BPP that they suppose will help resolve at least some of these issues. This is a distinction between nudge policies (already long discussed after being introduced by Thaler and Sunstein 2008) and boost policies. Nudge policies are based upon the Heuristics and Biases programme in cognitive psychology. Nudge policies exploit individuals' biases through alteration of "choice architecture" in order to have them make better choices with respect to health, wealth and happiness, and that align better with choices that they would have made under ideal conditions. Use of nudge policies necessitates that the policymaker know the goals of the policy targets, know the way goals are distributed across targets, be less error-prone than those targets, and that the policymaker is benevolent.

Boost policies are based upon the Simple Heuristics programme in ecological psychology. This programme understands the bounded rationality of individuals in terms of the environments to which the mechanisms of reasoning are adapted. Boost policies therefore aim to change the choice architecture such that these adaptive quirks are taken into account (for example, representing information in a form that facilitates good reasoning). Use of boost policies necessitates that the individual can see how error affects her decision-making, be able to stop this influence, and also that the individual has a minimal motivation to learn new heuristics and has a minimal competence in using them.

Our contention in this paper is that this is a distinction without a difference, and therefore that it cannot do any work in distinguishing good policies from bad ones with respect to the standards embodied in the normative and epistemological questions that occupy this literature. There are three ways in which we might successfully distinguish between two kinds of BPP. First, we might distinguish between them on the basis of paradigmatic examples; second, we might distinguish between them on the basis of differences in the mechanisms by which they achieve their end; third, we might distinguish between them on the basis that they stem from distinct theoretical programs. We suggest that none of these strategies are successful.

The appeal to paradigmatic examples is not successful. There are three examples that are appealed to in Grüne-Yanoff and Hertwig (2016). These are: i) giving the individual information in the form of natural frequencies rather than probabilities, since people are better statistical reasoners when using this form of statistical information; ii) training people to be better statistical reasoners; and iii) teaching people heuristics in order to allow them to make better decisions under non-ideal conditions. Our claim is that each of these cases can be interpreted in terms of either a nudge policy or a non-BPP policy, because the mere behavioural details of the policy in every case underdetermine the way it is to be interpreted. Secondly, the fact that the two putative kinds result from distinct theoretical backgrounds—the Heuristics and Biases; and the Simple Heuristics programs—cannot help us in making a principled distinction between nudge and boost. As the authors of the approach themselves note, there is not a complete coherency between policy and theory in any case; many individual boost policies do not fully conform to the theoretical program of Simple Heuristics, and the same is true for nudge policies and the Heuristics and Biases program. Furthermore, it appears that only one of these programs can be correct, since they are competing models of bounded rationality. So if policies are individuated by their relation to a research program then they are either all nudges or all boosts. That seems unsatisfactory in the context of making sense of the questions that dog BPP.

Nor will a mechanistic approach work. That is because—and at least in the context of the simple mechanistic model that is introduced in a forthcoming paper by Grüne-Yanoff and Hertwig—one can demonstrate that some boost policies appear to fall under the mechanistic operation that they claim characterises nudges; for instance, the provision of information in a form that facilitates statistical reasoning appears to be an alteration of the choice architecture, even though they characterise boosts in terms of expanding the heuristic repertoire. Furthermore, the way that they distinguish between mechanistic models in the first place is premised on the distinction between research programs, and so may fall into the error we identified just prior—it homogenises BPP in a way that they wish not to do.

It therefore seems that relying on a distinction between kinds of BPP threatens to seduce us into thinking that we can make wholesale normative judgments on either kind without being careful about the individual cases. We conclude this paper by suggesting one possible way to answer the normative question about BPP on a different basis—case by case and with the aid of a framework based on the analysis of moral responsibility in normative ethics. However, this alternative approach is not yet fully developed.

“Saint-Simonian ability: Is there a link with Capability?

Adrien Lutz and Antoinette Baujard
adrien.lutz@univ-st-etienne.fr, antoinette.baujard@univ-st-etienne.fr; *University of Saint-Etienne*

Abstract

“To each according to his ability, to each ability according to his work” (“à chacun selon ses capacités, à chaque capacité selon ses œuvres”) constitutes the founding aphorism of the Saint-Simonian doctrine (1825-1832). This doctrine aims to improve the fate of the greatest part of society, and is hence closely related to social justice and to the valuation of criteria such as utility and capacity. The former may refer to Bentham’s utility; the latter may refer to Sen or Nussbaum’s capability. This paper focuses on the latter.

The fact that the French word capacité remains translated as capacity, ability or even capability is confusing. The influence of ability is deeply present in the Marxian doctrine and their famous motto "to each according to its needs" (Marx 1875 inspired by Blanc 1849). In the end of the 20th century, the Capability approach emerged as an alternative to both Rawlsian and utilitarian theories of justice, to consider issues of human development and quality of life. One may argue that these a priori differences would be quite easy to explain when recalling the importance of the historical context of emergence of these alternative approaches: the Saint-Simonians were born in the project of historical progress and to make reform, while the capability approach was meant to become a step argument in discussing the relevance of modern theories of justice. We argue that such differences deserve a stronger emphasis since in the field of social justice, the Saint-Simonian approach of ability and the Senian approach of capability both find their roots into an account of individual characteristics. Analogies can be quickly pointed, so much so that one might be tempted to move from analogy to belief in conceptual links. It is tempting to consider both theories as close, and to think from there.

Insofar as the persisting question is always blurring the picture of any presentation of the Saint-Simonians doctrine, this paper aims at making a definitive point as to whether this reconciliation of capability and ability is justified or not. We shall argue that the links between the Saint-Simonian ability with Sen's or even with Nussbaum's capability approach convey more source of confusion than potential for enlightenment. Conversely, the disentanglement of these two concepts provides new insights on both approaches of justice.

First we provide elements of definition. Ability – translating the concept of capacité henceforth – refers not only to individual property – skills and knowledge – but also to social positions; capability, a concept that appears much later, is a material that measures justice. Then while the Saint-Simonian concept of ability is enshrined in a thought on social justice, capability remains at the basis of several theories of social justice. Saint-Simonianism suggests the organisation of the whole society – all social institutions – ultimately derives from individual abilities while the capability is defined as a proper material to think about justice itself, in order to value what individuals may accomplish in reality given their freedom of choice.

Second we aim to draw the lines between production and consumption. Individual characteristics are indeed taken into account both in the Saint-Simonian capability approach and in the Senian capability approach. However being able is not a sufficient condition to reach to social goal, as the improvement of social lot or the enjoyment of the full range of social choices. In this respect, the ability approach is a reformist theory of production; in contrast the capability approach understands social justice by considering consumption.

Third we oppose secular merit to needs: the Saint-Simonian theory of social justice focuses on merit more than need as it aims to provide opportunities to improve social lot. In this respect, the Saint-Simonians do not deal with final goods as far as their major concern is production. Then the Saint-Simonian concept of merit lost its previous religious strand in favour of concerns about efficiency and effort. Conversely the capability approach does not refer to production as such an approach deals with broader concerns that escape this narrow prism. We may argue along similar lines that capabilities embody concept of needs as well as merit.

In summary our paper concludes that the differences between the concepts of capability and ability are at all stages. A capability is an evaluative space for justice, defining what counts for individuals, while ability is a property of individuals. The former may be defined essentially in the domain of consumption, while the latter is clearly thought as a contribution to production. Capabilities are

what is primarily important, while production is eventually what important in the Saint-Simonian view. Finally these differences reveal a contrast in the focus values: the ability approach insists on efficiency, while the capability approach focuses on agency.

"The Unrealistic Realist Philosophy: the Ontology of Econometrics Revisited"

Mariusz Maziarz
mariusz.mm@gmail.com, *Wroclaw University of Economics*

Abstract

Previously, philosophers of economics and philosophically oriented econometricians who considered the question if empirical economic models can be interpreted in a realist way usually discussed case studies. Hitherto voices dealing with the ontology of econometrics are based on analyzing chosen examples of econometric modeling. These choices, I believe, favored the realist interpretations by their explanatory and predictive success. Additionally, by picking up a case study (i.e. a single model focused on a subject; on the relation between economic growth and public debt, for instance), one cannot see the feature of econometrics making the realist interpretations lack descriptive adequacy. Namely, econometric models lack robustness what means that either small changes in the sample (i.e. adding or excluding several instances) or minor modification of model specification (e.g. employing another averaging scheme, fitting linear or nonlinear curve etc.) can influence results to a high degree. In fact, such minor modifications often lead to emerging contrary results, where two, similarly justified, models support contrary hypotheses.

My aim is to deliver a new argument against the realist approach to econometrics. There are two types of the reductio ad absurdum argument. If one argues in favor of a thesis, one starts from its contradiction and shows that the denial of the thesis leads to an inconsistency. On the other hand, if one attempts to refute a thesis, one shows that either absurd or false results follow from its acceptance. In the same vein, I attempt to read out in a realist way a few case studies of econometric researches that exemplify the emerging contrary result phenomenon originally observed by Robert Goldfarb (1995; 1997) and show that the only defensible form of realism is the modal one defended by David Lewis (1986b), which is a ridiculous and counterintuitive conclusion. Hence, the realist position on econometrics is false.

In general, a realist interpretation of econometrics entails accepting the point of view according to which, for instance, the following equation:

(1) $q=\alpha p+u$

q - quantity of a good X sold at a price p;
α - estimated coefficient;
p - price of a good;
u – error term.

is a stochastic law describing the relation between price p and quantity q (cf. Cartwright 1989). However, if the regression of the law of demand instantiates the 'emerging contrary result' phenomenon, then there are two similarly justified models that justify two contradictory hypotheses (H1: $\alpha\leq 0$; H2: $\alpha>0$) what raise the question about the relata of these models. In detail, I characterize interpreting econometrics in line with scientific realism (Cartwright 1989), critical realism (Lawson

2007), perspectival realism (Hoover 2010) and argue why these approaches lack descriptive adequacy when applied to a more realistic reconstruction of econometrics.

The line of argument is following. First, I discuss the hitherto realist approaches to econometrics. Second, I, in order to present a more realistic view of econometrics, I discuss several case studies: (1) Sala-I-Martin (1997) article the lack of robustness of empirical growth economics, (2) my own experience with modeling of relation between individualist/collectivist culture and economic development, 'emerging contrary result' phenomenon (ERR) described by Goldfarb (1995; 1997) and two pieces of cliometric analyzes which instantiate ERR: (3) the Reinhart-Rogoff controversy and (4) research focused on empirical verification of expansionary fiscal contraction hypothesis. Third, I argue that the previously offered versions of realism are inadequate to the practice of econometrics, which, at the deeper examination, is revealed as methodologically unsettled and full of contradictions, and offer my own approach, i.e. modal realism. Finally, I show that accepting this form of realism, which, as I argue, is the only descriptively adequate realist ontology, is an absurd philosophical position.

I do not indicate which of the alternative philosophical positions (e.g. instrumentalism, empirical constructivism etc.) is most adequate or should be chosen due to other reasons. Instead, I show how rejecting the realist interpretation of econometrics may diminish popularly held belief in strictness of its methodology and raise awareness of a greater dose of skepticism needed when recent econometric results are employed to policy-making.

"Instrumentalism and Realism in Contemporary Macroeconomics – The Assumptions issue, Again"

Peter Galbacs
Galbacs.Peter@uni-bge.hu, *Budapest Business School*

Abstract

For the last few decades, considerable attention has been paid to the methodology of mainstream economics. It is not accidental that economics is encircled by methodological debates. If its relevance is at stake, this can be either refuted or proved most efficiently at a methodological level. Arguments for and against mainstream economics underline the methodological homogeneity of mainstream economics, while serious though almost neglected arguments can be found for a view according to which the long history of mainstream economics could be described as a sequence of methodological breaks. I argue, firstly, for a sharp demarcation by new classical macroeconomics from the Friedmanian instrumentalism and, secondly, the realism of new classicals. I make efforts to identify the epistemological principles underlying Lucas' models and to highlight the signs of that demarcation as well. I concentrate on the techniques through which new classicals could put their models into an indirect relationship with reality. It is also highlighted that the common terminology, according to which the assumptions of abstract economic models are uniformly regarded as "unrealistic", actually refers to two different techniques. From these approaches, there is only one which is justified to be labelled as realist.

In this lecture I try to demonstrate: new classical macroeconomics was a realist movement and it was organised along such purposes that by achieving them the traditional opposition of realism and instrumentalism could be left behind. First, I emphasize the methodological break between

Friedmanian instrumentalism and new classical macroeconomics. Then I briefly refer to why and how economists construct theoretical assumptions in order to build realist models and theories. Subsequently I discuss this methodology in the case of new classical macroeconomics. After this, I shed light on the suggested distinction between the two subsets of unrealistic assumptions as a final confrontation between the realist and instrumentalist methodologies. Here I argue that the epistemological connection between socio-economic reality and unrealistic assumptions should always be scrutinized in order that realist and instrumentalist models could be separated from each other. Then, turning back to new classicals, I explain how new classicals committed to realist efforts could construct assumptions in order to have models that support understanding and connect them with socio-economic reality.

I highlight that economists were normally bound to choose between realism and instrumentalism, that is, between ontic relevance and good empirical performance. According to my reasoning, new classical macroeconomics, despite the applied forms of abstraction and idealization, should be regarded as a realist system. These researchers definitely broke away from the Friedmanian principles. If this attempt of mine were successful, the views on the scope of new classical macro would be clearer. As my direct purpose here is to scrutinize the way how realism often cited in the philosophy of economics manifests itself in a well-defined theory, the present analysis has the hopes of providing some new insights into the role of isolation in economic models.

“Some Epistemological issues in the Verification of Economic Models”

Orestis Papakyriakopoulos
orestis.papakyriakopoulos@tum.de, *Munich Center for Technology in Society, TU Munchen*

Abstract

It is argued that economic theory faces certain problems in the integration of knowledge regarding socio-economic phenomena, which are rooted in the verification process of economic models. Micro- and Macroeconomic theories consist of a set of idealized ceteris paribus models, which are verified through econometric modeling. The verification process, as part of the scientific method, is violated in economic science due to issues located: 1. in properties inherent in ceteris paribus clauses, and 2. in the construction process and interpretation margins of econometric models. Consequently, the acceptance of knowledge in economics might not always emerge from a consistent application of the scientific method.

The way economic science describes and predicts socio-economic phenomena, as well as incorporates new knowledge is sometimes questionable, due to violations existing in the application of the scientific method. Therefore, the aim of this paper is to unravel some epistemological problems in the scientific verification of economic models. I begin by presenting a sketch of the scientific method used in economic science. On this basis, I explain some properties of ceteris paribus micro- and macroeconomic models that obstruct scientific progress. Finally, I turn to the description of certain biases in the construction and interpretation of econometric modeling, which lead to a false verification of both the cp – theoretical and econometric models.

One common view of scientific methodology assumes that hypotheses are confirmed or falsified by the verification of their observable consequences (Fig. 1a). In economic science, each hypothesis

emerges from the existing theories, which partly consist of theoretical cp- micro- and macroeconomic models and focus themselves on the description and prediction of specific socio-economic phenomena, by assuming certain qualitative causal relations between economic variables.

The testing of the theoretical models is performed through the creation of econometric models,which provide statistical relations between economic quantities in the form of formal statements. Hence, for the acquirement of knowledge, two verification procedures are carried out that ought to ensure the validity of both the theoretical and the econometric models (Fig.1b). The first procedure tests the predictions of the theoretical models based on the formal statements of the econometric models. The second tests the validity of the predictions of the econometric models in respect to empirical data. Two epistemological problems arise in these two verification processes, as I explain next.

My first concern regards the theoretical framework on how socioeconomic phenomena are analyzed. Jean-Philippe Bouchaud (2008) states that economists tend to accept and use theoretical assumptions even if they contradict empirical observations. I argue that this behavior is a consequence of the ceteris paribus clause. Micro- and macroeconomic models are of qualitative nature, constituted by few economic variables that yield predictions if all else is held equal, i.e. ceteris paribus. I adopt Nickels' (2010) view that ceteris paribus models are treated as generalizations that tolerate exceptions. Hence, cases that are not compatible with the existing theories, when observations do not confirm the theoretic predictions, do not lead to the rejection of the initial theoretic hypotheses. This behavior signifies a violation of the scientific method, because the nature of the cp-models protects the tested statements against their potential falsification (Popper, 2005).
The second issue in the verification of economic models lies in a violation of the scientific method applied to econometric models. Econometric models process large datasets and present formal statements in the form $y= f(x1,\ldots,xn,e)$, where y is a variable that is described as a function of the variable set $X= (x1,\ldots,xn)$ together with the error term e. The variable set X is chosen by the researcher, who performs tests for the mathematical consistency of the model, the statistical significance of the variables and seeks the minimization of the error. As Black (1982) has stated, an omission or addition of an extra variable can radically change the statistical significance and the magnitude of influence of the rest of the variables, and consequently the chosen final model is only "the final tip of an iceberg of dozens if not hundreds of unpublished alternative formulations" (Schrodt, 2006). This denotes a relative creative freedom in econometric model construction, which allows economists to develop econometric statements that obey the micro- and macroeconomic theoretical models, and to avoid econometric statements that contradict them. A prominent example of this behavior is Koopmans' (1949) proposed solution of the identification problem , in which he suggests that econometric models have to be developed by inserting a predefined variable structure and rejecting other alternative formulations, in order for them to be able to describe the supply and demand curves exactly as dictated by the theoretical cp-models. In this case, the fact that the econometric model is able to frame the same formal statement as the theoretical cp-model is treated as adequate for the acceptance of both theoretical and econometric model. This behavior violates the scientific method though, because the acceptance of the econometric model is not a result of a coherent verification process that tests its predictive ability by observing empirical data.

In this paper I have criticized the way economic science evaluates its theoretical consistency, incorporates new knowledge and draws predictions about socio-economic phenomena. I proposed a version of the scientific method applied in economic science. I've then focused on two violations that arise in the verification process, namely the toleration of exceptions in the ceteris paribus models and the problematic verification of econometric models, which leads to a false verification of both theoretical and econometric models.

Well-Being (and Decision Theory)
(Thurs 13:30-15:00, Room M3-03: Zaal Aberdeen)

"Re-individuation of Alternatives and Relevant Descriptions"

Hadrien Mamou
hadrien.mamou@gmail.com, *London School of Economics and Political Science*

Abstract

Standard decision theory provides an axiomatization of expected utility theory. Simply put, it shows that if an agent's preferences respect a series of axioms, then she can be said to be acting as if maximizing an expected utility function. These axioms are then usually considered as normative axioms of rationality, some of them being fairly uncontroversial: transitivity of preferences is for instance widely accepted in the literature. Given this axiomatic framework, different normative approaches can be taken with respect to rationality. One view called, Moderate Humeanism, claims that as long as some consistency constraints are respected, any preferences should be acceptable: self-destructive preferences and cold-blooded pragmatism are just equally rational as long as they yield consistent preferences. Under this view, the axioms laid out by standard decision theory are considered prima facie as tentative reasonable requirements of rationality in that they capture intuitions of consistency as minimally as possible. Humeanism thus claims that all preferences, taken individually are rational; they only may not be so if they are not consistent between each other. On the other hand, anti-Humeanism claims that there exist irrational preferences, even when considered in isolation.

In this passage of Weighing Goods, Broome provides a new argument against Moderate Humeanism. He argues that, if no additional constraint of rationality is added, an uncontroversial axiom such as transitivity becomes a vacuous requirement: it does not constrain preferences in any way; therefore, Broome argues, Moderate Humeanism collapses into a fully permissive theory of rationality, that allows any set of preferences: Extreme Humeanism. Extreme Humeanism being widely unattractive, Broome has shown by appeal to a reductio strategy that anti-Humeanism is the only plausible position.

I this paper, I will focus on the specific argument about reindividuation strategies that Broome uses to show that Moderate Humeanism collapses into its extreme counterpart. Broome's argument can roughly be put as follows. Consider Maurice, who prefers staying home when given the choice between staying home and going to Rome; who prefers going to Rome when given the choice between Rome and mountaineering, and who prefers mountaineering when given the choice between staying home and mountaineering. Maurice has allegedly intransitive and therefore irrational preferences. However, one could plausibly argue that after all, Maurice's preferences are not actually irrational since Maurice does not represent this situation through three but four alternatives: he distinguishes "mountaineering when given the choice between home mountaineering" and "mountaineering when given the choice between Rome and mountaineering".

Maurice justifies this distinction by arguing that he would not want to look like a coward by refusing mountaineering and staying at home, while he would enjoy looking cultured by choosing Rome over mountaineering. This redescription of the alternatives is formally called reindividuation of the outcomes: the set of outcomes is redescribed in a way that better captures the agent's attitudes, and may make a difference for the decision making process.

While Broome suspends his judgement as to whether Maurice has rational preferences, he contends that unconditionally allowing for reindividuation gives rise to the "emptiness problem": transitivity becomes an empty constraint, as any preferences over a set of outcomes can be rationalized by a reindividuation of these outcomes. However, Broome argues that Moderate Humeanism could not accommodate restriction of reindividuation strategies, so that it cannot escape the emptiness problem. Consequently, Moderate Humeanism collapses into Extreme Humeanism.

In my paper, I introduce what I believe is an implicit claim attached to any decision problem in decision theory: when given the description of a decision problem, we are also given a key assumption: the description is sufficiently relevant: nothing more is needed (and available to the decision theorist/adviser) in order to make a decision. I give several arguments in favor of this claim. First, if the description of the decision problem was not judged relevant, there would be no point in acting based on that description; I believe that it would in fact undermine the whole normative enterprise of decision theory. How can you possibly prescribe anything if you cannot trust some description of the problem that you are trying to solve? This is why, in decision theory textbooks, once a description of the decision problem is given, the theory usually gives a clear-cut assessment of the rational attitudes and decisions so that every relevant aspects of the problem are given. This assumption of relevance is what gives normative substance to decision theory as we know it. If a theory aims at evaluating the consistency of a set of choices, it has to assume an underlying set of attitudes to the world, AND suppose that these attitudes capture all that is relevant to the decision from the decision maker's perspective. Second, if the representation is not supposed to be relevant, then the inconsistencies could very well come from the incorrectness of the representation itself: an agent could systematically blame the representation and save the agent's reasoning.

The major implications of this claim are the following: we can now object to Broome that decision theory and Moderate Humeanism (as we know it) do not allow reindividuation and thus reckless rationalization of agent's attitudes, but often have nothing to say about it: normative interpretations of representation theorems just tell you how to be consistent based on one given representation that is deemed relevant. In the remaining of this paper, I consider three objections against my view and attempt to defuse them. And I make a case for a separate field of investigation aimed at establishing the permissible relevant representations of a decision problem. In this new view, two phases of the process of decision making can be distinguished. The first one which is representational, and the second (standard) one about the assessment and resolution of the decision problem given a particular representation. The second process has nothing to say about the first and vice-versa, precisely because we want to be able to explain, understand and justify what is a bad decision given a correctly represented decision problem.

"Utility in Rational and Moral Choice"

Bastian Steuwer
B.Steuwer@lse.ac.uk, *London School of Economics and Political Science*

Abstract

One of the most famous arguments in moral philosophy is Rawls's separateness of persons objection. According to the argument, it is impermissible to apply principles for individual rational choice to situations of moral choice. The argument compares individual choices, studied by decision theory, with moral choices, studied by moral philosophy. A modern example of this argument is Otsuka's and Voorhoeve's criticism of prioritarianism (2012). Otsuka and Voorhoeve imagine a case where we have two children, one able-bodied and one disabled, who will benefit differently from a move to the city or to the suburbs. Here we should be guided by our concern for the disabled child even if this will not bring about the best outcomes for the siblings taken together. This judgment is reversed however in a case where we have to decide for one child who may turn out disabled or able-bodied. Here we should maximize expected utility unlike what prioritarianism would mandate.

Decision theory and moral theory have evolved separately from one another and developed the notion of utility separately. This opens up the possibility that when decision theorists and moral philosophers talk about utility, they talk about different things. This seems to pose a problem for the evaluation of the separateness of persons objection. If the utiles we compare for individual choice and for moral choice mean one thing for decision theory and quite another for moral theory, we cannot make such comparisons anymore. My aim in the paper is to search for a common notion of utility in the two disciplines and to evaluate in how far this influences the separateness of persons argument.

My discussion starts with the von Neumann/Morgenstern measure of utility, the orthodox position in decision theory. The von Neumann/Morgenstern measure of utility constructs a cardinal index of utility from ordinal preference relations among risky prospects. I first discuss revealed preference theory, the theory that our preference relation is revealed by our choice behavior. The theory resembles most closely an actual desire or actual preference satisfaction view of welfare according to which what makes one's life go best is constituted by the satisfaction of desires we happen to have. Problems with the actual preference satisfaction view, I argue, should lead us to a rational preference satisfaction view. This view requires, as I shall argue, several conditions on preferences that are not imposed in revealed preference theory. Instead of identifying preferences with choice behavior we need to consider preferences as comparative evaluations along the lines Daniel Hausman has suggested (2011).

While these revisions bring the notion of utility in decision theory closer to an acceptable theory of well-being, I will argue that they are ultimately insufficient. The von Neumann/Morgenstern measure of utility combines evaluations of outcomes and assessments of risks in a way that, morally, ought to be kept separate. The measure is only appropriate when the individuals in single-person cases and the individuals in the multi-person cases have identical attitudes towards risk. In a response to Otsuka's and Voorhoeve's argument mentioned earlier, Derek Parfit has disputed this (2012). Parfit claims that in the single-person case we ought to be guided by a special risk-aversion. Otsuka replies to this by pointing out that the von Neumann/Morgenstern measure already includes risk attitudes in the measure of utility (2015). I shall argue that this reply shows the weakness and

not the strength of Otsuka's and Voorhoeve's case against prioritarianism, for it relies on a notion of utility which may be apt for decision theory but not for moral argumentation.

My discussion of the problematic role risk plays in the von Neumann/Morgenstern measure leads me to a discussion of alternative decision theories which distinguish between a risk component and a utility component. One such non-standard decision theory is Lara Buchak's risk-weighted expected utility theory (2013). Another alternative is proposed by Richard Bradley and Orri Stefánsson and includes a separate function of preferences for risk (Forthcoming). Both proposals, I shall argue, have the resources to overcome my challenges and to provide a common notion of utility for decision theory and moral theory. However, this comes at the cost of rejecting expected utility theory.

The next possibility that I examine is to adopt an alternative view of well-being that is not preference-based. Instead of a preference-based account of well-being we could adopt an objective list theory of well-being according to which what makes one's life go best are various items on an objective list of goods. Orthodox decision theory could accommodate these conceptions of well-being by applying the von Neumann/Morgenstern framework not to the preference relation but to the betterness relation. I will reject this proposal for similar reasons as the standard von Neumann/Morgenstern measure of utility. It implausibly implies risk-neutrality about objective goods.

An alternative way to express objective list theories is by adopting the older framework of utility as proposed by Jevons and Marshall. Under this proposal, utility is a quantity of good that is introspectively accessible. I shall raise two problems for this model: dealing with trade-offs between different objective goods and translating quantities of a good, say pleasure, into quantities of well-being. One response taken by Griffin (1986) and Otsuka (2015) is to adopt a preference-based measure of objective goods. But even if we grant a solution to these problems, I will argue that the model of Jevons and Marshall fails to justify expected utility theory since it would require risk-neutrality among objective goods.

My conclusion is the following: Either our decision theoretic notion of utility is inadequate for moral argumentation or, in the case of risk-weighted expected utility or the Jevons/Marshall model, we cannot justify expected utility theory. This creates problems for the separateness of persons argument. Without expected utility theory our principles for rational decision-making become less alike utilitarian principles for moral decision-making. It becomes more contentious that there are shifts from intra-personal to inter-personal cases. This is not to say that separateness of persons arguments cannot work, but that we need to do more to establish them.

"Scientific Knowledge Creation and Well-Being"

Pawel Kawalec
pawel.kawalec@kul.pl, *John Paul II Catholic University of Lublin*

Abstract

The link between innovation and wellbeing has been empirically established (e.g. Dolan & Metcalfe 2012), but still little is known about the underlying causal mechanisms (Glaser & Laudel 2016). Within the framework of capabilities approach (Sen 1985; Angner 2013) the paper aims to contribute to the study of the role knowledge in the causal mechanisms. The paper focuses on scientific knowledge as constitutive of innovations (Greenhalgh & Rogers 2010). Following the dominant trend in more recent science studies (Kitcher 2001; Evans & Collins 2002; Mirowski 2011; Zamora-Bonilla 2016) scientific knowledge is accounted for in its 'mode of creation' (Gibbons et al. 1994). Despite voluminous literature on 'knowledge production' little has been said on its influence on welfare, while the focus is on generic characterization of knowledge contribution to economic growth (following the original contributions of P. Samuelson and V. Bush as well as economists affiliated at NBER in 1950's) and its impact on world-wide inequality stemming from international regulations of intellectual property rights (Stiglitz et al. 2014) with some scattered remarks on knowledge's instrumental influence on consumption enhancement (Bruni & Porta 2007).

Taking into account variations in e.g. data sources, inferential patterns, institutional settings, dissemination patterns, etc. I distinguish two basic forms of scientific knowledge generation: generative vs. production models. I discuss the structure and information flows in the two models in detail and also instantiate them with two historical cases: Tycho Brahe's Uraniborg and the origins of industrial research at General Electric in the first two decades of the 20th century.

I further argue that the two models manifest two opposite causal pathways towards wellbeing. The generative model is constitutive of the capacity for knowledge creation and its inherent link with wellbeing, while the production model undermines this capacity. I elaborate several arguments to support this claim. To substantiate it, I rely on (Mirowski 2011) analysis of CRO as paradigm case of the production model and its deficiencies. Based on (Brogaard 2014) I argue that the generative model is a prerequisite for human flourishing as it requires the institutional setting, upon which functionings based on scientific knowledge and on its tangible outcomes depend for the pertinent community. Next, I use a modified version of B. Latour's and S. Woolgar's research cycle (Hessels et al. 2011) to support the adequacy of the generative model with regard to institutionalized scientific practice. I also use the empirical evidence on factors of scientific productivity (Heinze et al. 2009) to motivate the projected causal link between the generative model and wellbeing.

I conclude by indicating how to explore the ontological grounds for the generative model by an extended application of T. Williamson's theory of knowledge (2002) as a prime and broad condition, which integrates internal and external aspects of scientific knowledge, its creation and assertion in institutionalized practices of scientific communities.

"Scientific Value of Value Judgments: Criticisms to Sen's Argumentation for Scientific Normative Economics"

Mehmet Taylan Cuyaz and Ali Berk İdil
m.taylan.cuyaz@gmail.com, *METU Philosophy*; aliberkidil@gmail.com, *METU Political Science and Administrative Science*

Abstract

Normativity has always been a topic in philosophy in regards of a discussion whether it could be integral part of scientific inquiry. Economics naturally took its share from these discussions and there exists an important dispute about normative economics -which has crucial differences compared to positive economics- integration to scientific inquiries. We aim to underline the points which makes normative economics a matter of disagreement. It is important to emphasize that it is not our aim to show that normative economy is completely excluded from scientific inquiry rather our aim is to show that some proofs which provide basis for it to be included completely, is subject to criticism. The argumentation which we will try to provide criticism against Amatrya Sen's Collective Choice and Social Welfare.

In his mentioned book, in chapter 5, Sen states that we are not able to construct policy recommendations without including any ethical claims. He adds that these ethical claims are still considered as value judgments even if they are accepted by everyone or most of the individuals. Further, he assumes that, (Sen, 1970, p. 57-58) "unanimous value judgments may provide the basis of a great deal of welfare economics, but this is so not because these are not value judgments, but because these value judgments are acceptable for all."

Sen's suggestion for producing policy recommendations are based on three elements: namely: factual premises, value judgments, and logic. In Sen's contemplation, with these constituents, welfare economists will be able to make certain inferences and accordingly, formulate some policies. Therefore, from this point, Sen claims that, (1970, p. 64) "…one can have a scientific discussion on the … value judgments … by examining underlying factual premise or the logical derivation" and "rejection of a value judgment may be purely scientific" thus, there will be a "possibility of fruitful scientific discussion on value judgments."

Considering the arguments given above, much can be said about Sen's arguments on the possibility of a scientific endeavor that will be put into normative sentences. One of the most essential criticisms could be derived from Ayerian ethics. Although Sen definitely seems to be aware of Emotivist and Ayerian model of ethics, he still does not seem much convinced about the nature of ethical values as described by those views. Sen attempts to avoid the potential criticism of Emotivist ethics by trying to show that ethical judgments could be derived from descriptive sentences; under circumstances in which an ethical value is adopted unanimously. However, Ayerian model of ethics does not ignore ethical propositions within the field of science only because they are unverifiable or only because of Hume's Law; he deems ethical judgments literally non-significant because of the vagueness that they inherit. Ethical concepts in this sense are

incapable of being placed in the center of a debate about its truth. Although Ayer himself accepts that within certain frameworks, some ethical judgments could follow each other in a logically coherent manner and any individual may find empirical data that may increase their inclination towards adopting a certain ethical agenda, emotion would still be the central tenet of ethical values and judgments and hence cannot be used in scientific language in a definite way.

Another confusing point is that Sen seems to be evaluating ethical judgments within isolation. Not only because ethical judgments cannot be analyzed by themselves because they are mostly vague; it is also for ethical judgments are greatly interrelated. For example, to say anything about "equal pay" first requires analyzing and comprehending what is "equal"; and for the word "equal" there should also be another word that should be under investigation. This leads us to either an infinite regress in which vague concepts follow each other to save themselves from being vague or it will lead to a highly interconnected network of concepts. These will cause all ethical judgments to be valid only within a certain sphere of political ideology, which could be labeled as "objective" to a certain degree but it is obvious that it cannot be a basis for the claims of Sen which calls normative economics in this sense "purely scientific".

Some may claim that some ethical judgements may be constructed in a way that prevents any literal vagueness, like Sen's example of unanimously accepted value judgments. However such an argument does not deal with all the problems that occurs within it. First, even if we reach moral judgments that are not vague, neither our positive theories nor our language of economics would not permit a pure objectivity. For instance, take the term "well-off" or "better-off", these two terms have different reference points for Marxist and Liberal economies, even if we try to achieve a pure objective term "being well", it will still be necessary for the economist to investigate the term within one sphere of politics or the other. There is only one example given here but with more complex arguments, the number of different sphere of politics could be much higher. In here, we will arrive at the incommensurability of economic terms between different ideologies. Even if we try to clarify our terms or make all economic statements with the help of comprehensive dictionaries, we cannot achieve an understanding of economics which is free from any biases, which are in a certain degree influenced or and in confirmation with a certain ideology.

This argumentation is also in line with Quine's "On the Nature of Moral Values", in which he assumes that (1979, pp. 477-478) "we can judge moral act only by the moral standards themselves" and thus "Science thanks to its link by observation, retains some title to a correspondence theory of truth, but a coherence theory is evidently the lot of ethics." Some other valuable arguments could also be derived from Quine's Reply to Morton White's article "Normative Ethics, Normative Epistemology and Quine's Holism". Although Amartya Sen's and Morton White's aims do differ in a sense, their formation of structures that they used to form "heterogeneous conjunctions" or "policy recommendations" are in similar fashion. Regarding this, we assume that, taking Quine's criticism into account about this issue will be fruitful.

To sum up, the aim of this work is to show that the proof for a purely scientific nature within normative economics is not as strong as it is defended by Sen. First criticism is given from the works of Ayer and in general emotivist ethics which defend that ethical judgments are neither verifiable nor logically literal or meaningful. Second set of criticism is mostly inspired from the works of different philosopher, namely Quine, Duhem, Hempel and Kuhn, on scientific testing which we have applied to the claims of Sen and consequently observed that terms are not only vague but they are also inclined to be understood from very different perspectives even when their definitions may seem exact. This is mostly due to political ideologies and its effect on economics. Lastly the absence of observation sentences for moral sentences is emphasized and reconsidered, which is defended prominently by Quine also considering within the framework of epistemology. In

general multiple criticism to Sen's arguments for a normative economics which is tried to be integrated with scientific understanding is given and explained.

"Non-Epistemic Values and Policy Relevance in Macroeconomics"

Henrik Roeland Visser
hrvisser@uni-bielefeld.de, *Bielefeld University*

Abstract

In the philosophy of science community it is nowadays widely accepted that science is not value-free, and that the classical ideal of objectivity is an illusion. The argument that philosophers of science most commonly invoke to explain the role of non-epistemic values in science is the argument from inductive risk. This argument claims that it is impossible to resolve scientific uncertainty, that is, to draw inductive inferences based on uncertain evidence, without reliance on non-epistemic values. This argument originates in traditional philosophy of science debates on theory acceptance and the problem of underdetermination. Instead of trying to overcome the obstacles to value-freedom, philosophers of science now recommend that scientists are transparent about the role of non-epistemic values in science and engage in public discourse on what 'the right kind of values' are. Are scientists themselves able and willing to do so?

In my research I analyze how policy economists deal with the role of non-epistemic values in their research and policy advice. The following observations are based on an analysis of macroeconomic research institutes whose primary client is the government. How do they evaluate and communicate the uncertainties that are inherent to their research methods? Although the variety of non-epistemic values that impacts their research, and vice versa, is usually well understood, they often prefer to avoid controversial value disagreements. Rather than to explicitly engage in value conflicts, they avoid such conflicts and appeal to the ideals of value-freedom and objectivity instead. When it is pointed out to them how a specific methodological step requires an inductive inferences that (implicitly) relies on non-epistemic values, economists typically suggest it may be wise to cease making such inferences. Given that philosophers have already shown these ideals of value-freedom and objectivity to be unfeasible, should we conclude that such an evasive strategy of economists is simply mistaken?

I argue that this conclusion is not justified. The reason why philosophers of science are unhappy with the evasive strategy, is that it rejects the obligation of scientists to judge whether uncertain evidence provides sufficient warrant to accept or reject an hypothesis. To explain why this rejection of the first premise of the argument from inductive risk is wrong, philosophers of science cannot appeal to the argument from inductive risk itself. What is required is an independent argument, which is usually found in a moral obligation for scientists to produce policy relevant knowledge. This interpretation emphasizes that the conclusion which is typically drawn from the argument from inductive risk, namely that the role of non-epistemic values in science is inevitable, is contingent on the commitment to a certain normative ideal of policy relevance. The argument from inductive risk does not prove the unconditional impossibility of keeping non-epistemic values out of science.

We can now interpret the evasive strategy of economists in a different light. In refusing to embrace

the role of non-epistemic values in science, they are not rejecting the validity of the inductive risk argument itself, but the premise that prescribes a commitment to policy relevance, on which the argument from inductive risk relies. To understand this, we need to have a closer look at what the commitment to policy relevance entails and why some economists reject it. For the institutes I have studied, the meaning of policy relevance is the ability to provide policy makers with information on the likely behavior of those economic variables which play a role in their policy decisions. Such decisions are often controversial, meaning that they affect a variety of economic interest groups with very different values; there is no consensus on a set of non-epistemic values on which economists could rely to make inductive inferences. To rely on a specific set of contested values would amount to picking a side; allowing others, who do not share the same values, to reject their policy advice as biased.

This risk explains the hesitation economists have towards openly embracing the role of non-epistemic values. Without a reputation as provider of unbiased policy advice, their research is worth little more than the partisan claims of the different competing interests groups, which would reduce the ability of economic research institutes to effectively provide policy-relevant advice. Our interpretation of the skepticism of economists towards non-epistemic values requires a further nuance. It is not policy relevance itself that economists reject. On the contrary: policy relevance is the reason why economists are hesitant! Considerations of policy relevance thus pull in opposite directions; as motivation to openly embrace the role of values in science and as a reason to be cautious in doing so. What is rejected is a conception of policy relevance that requires economists to always provide answers to the questions of policy makers, even in the face of high uncertainty and controversial value disagreements.

To summarize, this analysis shows that the argument from inductive risk does not unconditionally prove the inevitability of non-epistemic values in science. It is instead contingent on a commitment to a very narrow conception of policy relevance. Policy relevance in its common interpretation is a broader concept that both stimulates and urges caution in dealing with non-epistemic values in science. This analysis has two important consequences. Firstly, what role non-epistemic values do and should play in science is not a question that can be answered in general, divorced from a specific context. Instead we should ask the empirical and pragmatic question of how the ability to provide policy relevant advice is best safeguarded in a given society and policy context. Secondly, this means that the answer to this question will vary in different contexts. In some contexts scientists will need to openly engage with non-epistemic values, while in others the traditional ideals of value-freedom and objectivity remain both feasible and desirable. The economists who still appeal to these ideals are not mistaken. Instead, their appeal to these ideals is based on a keen awareness of the role that values play in science and policy!

"Justification of Behavioural Public Policies as Evidence-Based Policies: The Case of Incentivized Smoking Cessation Programs"

Osman Caglar Dede
dedecaglar@gmail.com, *Erasmus University Rotterdam*

Abstract

In this paper, I offer a fresh look at the evidential evaluation of behavioral public policies. I focus on Incentivized Smoking Cessation Programs (ISCPs) as instances of behavioral public policies. I reconsider the evidential appeals and limitations ISCPs in addressing widely-held desiderata of public health policies such as alleviation of health disparities across social classes and improvement of health outcomes in disadvantaged groups. In doing so, I contrast two critical perspectives from which evidential challenges to ISCPs can be addressed. The first perspective corresponds to how ISCPs are currently evaluated by their proponents through laboratory/field experiments and by evidence-rating institutions/systematic reviewers. The second derives from social epidemiology.

On the basis of this analysis, I respond to the current philosophical debates about which kinds of evidence behavioral public policies need, focusing on Till Grüne-Yanoff's recent account that demands mechanistic evidence. I defend a twofold claim: (1) the from social epidemiology is detrimental to the justification of ISCPs as evidence-based public health policies, yet it is understudied by the currently received evidential assessments of ISCPs; (2) the challenge from social epidemiology is not encompassed by Grüne-Yanoff's recent account.

ISCPs are evidence-based public policy programs –underpinned by behavioral economics and neighboring disciplines- that target at promoting smoking cessation through diverse financial reward schemes. Behavioral economic studies of addiction and non-volitional behavior inform ISCPs and similar incentivized public policy programs. Firstly, behavioral economic models provide a theoretical rationale for considering why ISCPs could be effective in smoking cessation. Secondly, they inform design of discrete configurations of ISCPs in order to make them efficacious across different policy contexts. Specifically, the proponents of ISCPs argue that behavioral economics is useful in discovering more effective use of incentives, across different contexts, through provision of more comprehensive information regarding how incentives fail or succeed in leading behavior change.

Two main considerations render ISCPs promising in the eyes of public health scholars and systematic reviewers. One is the consideration that ISCPs are efficacious interventions in smoking cessation, as yielded by increasing number of randomized controlled trials (Cahill and Perera et al 2015 for a recent systematic review). The other consideration is that ISCPs can potentially be efficacious in reducing smoking rates among particular policy target groups of smokers such as deprived citizens which are most vulnerable to smoking and highly resistant to behavior change interventions- hence can be used to address smoking-related health inequalities (Office 2010, Higgins et al 2012, Cabinet, Marteau and Mantzari 2015). Indeed, a basic motivation behind the behavioral approach to public policy is to rectify the limitations of the traditional policies in approaching the challenges of promoting behavior change among particular risk groups (e.g. Mullainathan and Shafir 2014). The proponents of ISCPs argue that when behavioral susceptibilities of these groups are not taken into account, use of financial incentives may even harm them.

However, the proponents argue, absent further fine-grained and comprehensive evidence than currently produced, ISCPs [and similar behavioral policies] cannot be justified as evidence-based public policies to tackle with health inequalities and the specific health conditions of disadvantaged groups. Specifically for these ambitious policy desiderata, the proponents and the systematic reviewers argue, the behavioral interventions should be based on further evidence predicting (i) which discrete configurations of behavioral interventions work long enough to lead positive behavior change in disadvantaged people in a population; and (ii) relative impact of these interventions across and within the members of different social groups. Furthermore, the proponents and the systematic reviewers hold that to gather such evidence is viable and enough to give license ISCP's capacity to address aforementioned problems in public health.

My first claim is that gathering the sort of evidence that is currently demanded by the systematic reviewers and the proponents of ISCPs [i.e. evidence of which configurations of ISCPs work in disadvantaged groups with what sort of susceptibilities] would not be enough to justify ISCPs' capacity to address the ambitious public health problems. This argument is informed by the perspective from social epidemiology. The first challenge to ISCPs is that a policy's capacity to promote long-term smoking cessation in socially and economically disadvantaged smokers in isolation from structural associates of that behavior does not license the claim that the policy achieves sustainable improvements in targeted individuals or groups' smoking-related health conditions. The reason is that unless the policy counteracts those structural determinants, the behavioral change is unlikely to be persistent or likely to lead unintended consequences (such as substitution of smoking with another negative health behavior). The second challenge follows from the first. The evidential underpinning of ISCPs does not give reasons to believe that widespread implementation of ISCPs would promote positive population-level health consequences. I conclude that evaluations of ISCPs should not only be informed by the narrow methodological account that the most proponents and systematic reviewers assume. The challenges from social epidemiology should be responded for evidential justification of ISCPs.

My second claim is that the challenges from social epidemiology are not encompassed by the Grüne Yanoff's recent challenge to behavioral public policies (2016). I firstly argue that although pursuit of mechanistic evidence might be helpful in generating novel ISCP programs, defining ISCPs with reference to unique causal mechanistic models is a tall order because ISCPs do not strictly differ in their presupposed mechanistic models [they only differ in their configurations]. Secondly, I argue that, even if such ideal, evidentially sophisticated ISCPs are developed and offered, it is still ambiguous how having access to [ideally construed] mechanistic evidence allow us to motivate or address the critique from social epidemiology. Furthermore, because most of the behavioral models consist of micro-level components, following the prescriptions of mechanistic approach implies a reductive strategy into the investigations regarding whether an ideal ISCP diminishes existing smoking-related health inequalities, or relative persistence of negative health behavior across socio-economic classes. That strategy, I argue, is not immune to the challenges from social epidemiology.

The paper proceeds as follows. In the first part of the paper, I introduce ISCPs, discuss in what sense they are underpinned by behavioral economics [and neighboring disciplines], and comment on ISCPs' evidential appeals and challenges, as judged by systematic reviewers and the proponents of ISCPs. In the second part, I raise two critiques from social epidemiology to ISCPs and argue that the methodological prescriptions offered by the systematic reviewers and the proponents do not address the challenge from social epidemiology. In the last part of paper, I introduce Grüne-Yanoff's account and ask whether gathering further mechanistic evidence, in the sense that Grüne-Yanoff demands, would justify ISCPs as evidence-based public health policies.

"Inequality and the Logic of Positionality"

Alan Thomas, Alfred Archer and Bart Engelen
ap.thomas@york.ac.uk, *University of York*; A.T.M.Archer@uvt.nl, *Tilburg University*; b.engelen@tilburguniversity.edu, *Tilburg University*

Abstract

In this paper we analyze how the logic of positional goods poses a problem for inequality. The argument with which are concerned takes the problem of inequality to be historically obsolete. [Brennan, 2007, 2014] It claims that there is a condition on permissible inequalities, namely, that the representative worst off person can fully participate in society without shame. This criterion was first articulated by Adam Smith and has been endorsed by Sen and Pettit. [Smith, 1776/1982; Sen, 2009; Pettit, 2012] However, it is argued by those sympathetic to capitalist forms of economic organization that this criterion can, in our historical circumstances, easily be met. In the affluent West this problem has been solved. [Deaton, 2013] Notably, consumption inequality is not as high as income and wealth inequality: today's poor enjoy a "hidden prosperity" because the very same processes that make our societies unequal also make material goods comparatively cheaper and more accessible. [Cutler & Katz, 1992; Edsall, 2013]

In this paper we diagnose the way in which an appeal to positional goods undermines this line of argument. Its orthodox interpretation appeals to the problem of collective self-defeat. Originated by Hirsch, this argument (developed by Frank and Turner) argues that the logic of positional goods means that everyone must engage in "defensively necessary" competition for goods on a "winner takes all/loser takes none" basis. [Hirsch, 1976; Frank, 2011; Turner 2012] This is so, even though the chance of being the winner is either very low, or nonexistent. If the ultimate measure of a good society is wellbeing, then positionality introduces frustration and self-defeat into the evaluation of the outcomes of market competition.

We sympathise with this standard argument – but only up to a point. Defenders of inequality can argue that wellbeing depends on range of values that can be traded off inter se. [Tomasi, 2012] Positional goods will be involved in just such a trade off: social status is a "nice to have", but tradable for more income and wealth –themselves forms of social power. Shame is tied to status; but social status is not mandatory. The worst off can enjoy other forms of social power – such as material power on the market. We think that this rejoinder is convincing, but it fails to address a deeper argument about positionality that is present in Hirsch but brought out more clearly by Tilly and Robin. [Tilly, 1998; Robin, 2013] The logic of positionality has a deeper social effect: it entrenches unequal class position such that the logic of positional goods subsumes the category of material goods. Trade offs between these two classes of goods are no longer possible: capitalist societies become positional "through and through" such that shame in one's relative poverty becomes inescapable and Smith's criterion unrealizable.

On this diagnosis, it is built in to the structure of affluent societies that they must be divided into classes: one class sets the terms of positionality, another class aspires to those positional goods, while a third class makes do with merely material necessities. There is a stratification of wealth and taste, but this is no mere contingency: the relative position of these classes is determined by the market. But once this stratification is in place, then there is no option of appearing in society without shame if you are relegated to the "third class" who have solely market purchasing power. Our argument is that the logic of positionality now recurs in a more serious form.
T
he positionality argument needs to be set in an historical context. [Hirsch, 1976, p. 161] For critics of modernity, such as Nietzsche, the modern marketplace is the cultural location of the democratization and hence banalization of taste. Hayek, by contrast, valorizes the market as a place of heroic legislation of value, but only for a privileged social class. [Robin, 2013]

Capitalism is inherently, not contingently, patrimonial in its social structure. Inherited advantage puts in place a class of people whose role is not merely to consume, but to legislate new terms of taste. Consumers do not transmit preferences via the market to a productive class: a patrimonial class functions like an aesthetic avant-garde in determining what mass taste will be. Patronage steers production towards the mass production of items previously the exclusive preserve of the rich. Culturally, the marketplace can be the location of heroic action. That is why appearing in public in a cheap pair of trainers is, indeed, a source of shame – an archaic notion. The economy is a battleground – a place of both glory and shame. This deeper logic of positional goods appeals to the cultural logic of capitalism: its class stratification determines what counts as shameful in the first place. We conclude that shame is inevitable in unequal societies so that Smith's criterion cannot be met even in the context of our affluent societies. The way in which the logic of positionality impacts on putative defenses of inequality has, in our view, been insufficiently appreciated.

"Ethically Based Capitalism: Ayn Rand's Account of Political Philosophy and Economics"

Ziemowit Gowin
ziemowit.gowin@gmail.com, *University of Warsaw*

Abstract

Ayn Rand proposed a philosophical system which covers almost every area of philosophy: metaphysics, epistemology, aesthetics, ethics, and philosophy of politics and society. The heart of her system (which I shall call objectivism from now on and use the predicate 'objectivist' accordingly) is her ethics - based on the objectivist metaphysics as well as on the objectivist epistemology. The objectivist ethics - known under the name of rational egoism - has its direct socio-political implications from philosophical point of view.

The goal of this paper is to show how moral approval of capitalism is based on a particular ethical theory, namely the objectivist ethics. During my presentation I will present: (1) some essentials of her view of the nature of human beings; (2) her ethics: both meta-ethics and normative ethics; (3)

the socio-political implications of the objectivist ethics.). As to (2), I will focus on: (a) the concept of value as derivative of the concept of life; (b) the notions of good and bad values; (c) happiness (happy life) as the fundamental value; (d) the concept of virtue with respect to objectivist virtues.

Later on, I will speak about relationships between human beings in a rational society (i.e. how people should treat and even perceive each other), and, particularly, about the so-called Trade Principle. Then I shall present Ayn Rand's view of individual rights - mainly, the right to live and its corollary: the right to property. This will allow me to move on to the last sections, namely her account of the role of government and the only - according to the objectivist ethics - moral political (and, eo ipso, economical) system which is laissez-faire capitalism.

"Linking Inequality and Happiness: the Impact of Fairness Perceptions in Europe"

Licia Bobzien
l.Bobzien@phd.hertie-school.org, *Hertie School of Governance*

Abstract

The attempt for happiness is a decisive motive shaping individual (life) choices and is often conceived as one of the most desirable states in life. Self-evaluated happiness is unequally distributed across individuals and across societies. Disclosing the puzzle of which determinants explain variation in happiness levels motivated research among a variety of disciplines. While the association of individual factors such as (relative) income, occupational status, health, or age with individual happiness seems to be robust and nearly undisputed, the impact of societal factors like economic growth, unemployment rates, or economic inequality on happiness is far more ambiguous. This paper focuses on the relationship between income inequality and happiness. Empirical findings vary depending on data sets, model specification, and operationalisation used for the respective analysis. Besides inconsistent empirical findings the underlying theoretical mechanisms remain unclear as well.

While past studies asked whether self-centred (monetary) motives or relative concerns/social comparison mediate the relationship, this paper offers an approach based upon fairness perceptions. It argues that individual happiness levels do not directly depend upon income inequality per se; they are rather interrelated with underlying fairness conceptions individuals have towards those inequalities and how individuals relate these conceptions to their perceived environment. The analysis distinguishes between procedural and distributional fairness to analyse the impact of fairness evaluations of inequalities on self-reported happiness. This paper contributes to the existing literature by, firstly, directly modelling the hypothesised mechanism instead of relying on a black box. Secondly, it develops and uses an innovative index for distributional fairness at country level. And, thirdly, it applies a longitudinal cross-country research design with panel data at cross-country level and trend data at individual level (specified in a multi-level model) instead of purely relying on either cross-sectional data or longitudinal within-country data as often done by past research. This allows to implicitly control for time-constant unobserved heterogeneity by using fixed effect specifications at country level and allows to consider the temporal dimension by analysing trend data in a multilevel model. As micro data, the European Values Study (1999-2001 and 2008-2010)

is used and extended by country level variables. The hypothesis is tested for 24 European countries. Happiness is operationalised by the general evaluation of the own life on a likert scale.

Procedural fairness is operationalised based upon the idea that procedural fairness incorporates two dimensions. A process is procedurally fair if (i) the rules per se are evaluated as fair (normative procedural fairness) and (ii) the execution of these rules is evaluated as fair (institutional procedural fairness). Distributional fairness is conceptualised as an individual judgment of a preferred level of a certain kind of (in)equality. Following, distributional fairness is defined as the difference between individual perceptions of a distribution and the individual preference of how a distribution ought to look like: If individuals perceive inequality in accordance to their values it is here defined as distributionally fair. This variable is calculated from the International Social Survey Programme - Social Inequality III (1999) and IV (2009) and used as country-level variable.

This paper provides a direct test of fairness perceptions as a mechanism linking inequality and happiness; the empirical evidence is generally supportive of the presence of the mechanism following the theoretical argument that procedural fairness perceptions as well as distributional fairness perceptions mediate the empirical relationship between factual inequality and happiness. Thus, a conceptual framework which directly models fairness perceptions appears to be a fruitful approach: Not only self-centered motives or relative comparisons seem to be relevant dimensions for happiness but also fairness perceptions. Perceiving procedural and/or distributional fairness is associated with higher individual happiness; thus, the consideration of fairness perceptions can provide more detailed information about the underlying mechanisms and may enable to further explore why the statistical association between inequality and happiness seems to be rather vulnerable.

Measurement and Triangulation
(Thurs 15:30-17:00, Room M1-09: Zaal Bergen)

"Visualising Ignorance"

Marcel Boumans
m.j.boumans@uu.nl, *Utrecht University*

Abstract

The future is not only unknown, it also includes dangers and threats to our current economic status quo. The only thing we know about the future are expectations based on extrapolations of the past, the rest is uncertainty in the Knightian sense, that is, non-measurable uncertainty. To protect us from these dangers, in economics there is a longer history of attempts to "tame" uncertainty, that is, attempts to bring these threatening uncertainties "under the control of natural or social law." By the phrase "under the control of social law," Ian Hacking (1990) meant to denote that it is covered by a statistical distribution. To use Knight's terminology, taming of non-measurable uncertainty is to present it as "risk," that is, as "measurable uncertainty."

Attempts to turn the uncertain future into a measurable phenomenon were published since the late 1990s by the Bank of England. The Bank of England Inflation Report presents the uncertainty of future inflation as a fan of differently shaded red areas, where each area represent 10% of the total area in which future inflation could appear. While the sources of uncertainty are various and heterogeneous, the charts present uncertainty as a homogenous and measurable phenomenon. To understand this process of homogenization, it will appear to be fruitful to compare uncertainty with dirt, also considered to be a source of danger, and to use Mary Douglas's account of our attempts to clean a 'culture' from pollution. In other words, we will analyse this process of homogenization as a cleaning process.

I see cleaning as an epistemic activity. To better understand this specific epistemic activity, this paper will discuss the associated "ontological principle" that enables this activity and makes it intelligible (Chang 2009), namely the "principle of the equal distribution of our ignorance" (Hanson 1969).

The central question is how the Bank of England arrives at a visualisation of ignorance, even at a quantification of it. This process is based on a model that maps choices about economic assumptions onto an inflation forecast. To generate the probability distribution, one could ideally evaluate all the possible alternative assumptions numerically using this model. In practice, however, a specific form for the distribution – (two piece) normal distribution – is assumed and only a limited number of alternative assumptions are evaluated. These alternatives are used by the Bank's Monetary Policy Committee to "calibrate" the three key parameters of this distribution: mode, variance and skewness.

The application of the normal distribution is, however, conditioned on certain assumptions about the distribution variable. The implicit assumptions for the application for the normal distribution are the ones that underlies the Central Limit Theorem, that is, the theorem that justifies the use of the normal distribution when there are no other explicit justifications offered, e.g. by showing an underlying mechanism that determines the shape of the distribution. The Central Limit Theorem says that under very general conditions the sum of n independent variables, distributed in whatever

form, tends to normality as n tends to infinity. The application of normal distribution, whether in one or two pieces, presumes that the different sources of uncertainty, such as the complexity and rapid change of economic behaviour and unpredictable shocks varying greatly in size, all have the same distribution, are independent and are very large in number. Because of the diversity of the nature of these sources, these assumptions are obviously too strong.

When the normal distribution is used as the model for uncertainty for cases of which (it is not known whether) its legitimating assumptions do not apply, these assumptions actually function as homogenizing assumptions. They assume away the heterogeneity of the various sources of uncertainty and assume that these sources instead are identical, independent and in large number. The specific nature of the homogenisation process depends on the nature of the material that needs to be homogenised. To explore the underlying principles that enabled the Bank of England to picture and even quantify uncertainties, Hasok Chang's (2009) account of ontological principles will show to be helpful. According to his account, to each well-defined, basic epistemic activity, there is an associated ontological principle that enables it and makes it intelligible. An ontological principle dictates the kind of basic properties of an entity that is investigated.

The ontological principle that can be distinguished in the case of developing the fan chart, without claiming that it is most fundamental or basic, is the principle of insufficient reason or the principle of the equal distribution of our ignorance, as called by Norwood Hanson (1969). The basic idea of this principle is that a number of possibilities are equally probable when we know no reason why one should occur rather than any of the others.

It appears, however, that the ontological properties are those of the epistemic activity and not of the phenomenon being investigated. As a result the "taming," "purification" or "homogenisation" does not take place in the world but only in our representations of it.

"Variable Definition and Causal Inference: The Role of Independent Component Analysis"

Marco Capasso, Lorenzo Casini and Alessia Moneta
marco.capasso@gmail.com, *Scuola Superiore Sant'Anna*; lorenzodotcasini@gmail.com, *University of Geneva*; amoneta@sssup.it, *Scuola Superiore Sant'Anna*

Abstract

Causal discovery methods based on graphical models are widespread in the social and biomedical sciences and are proved to be very powerful under a set of conditions. Much literature has discussed violations of assumptions such as the causal Markov condition and the faithfulness condition (cf. Cartwright, 2002; Dawid, 2010). Less importance, however, has been given to the problem of variable definition.

Woodward (2016); Eberhardt (2016); Korb et al. (2011) have recently pointed out the problem that arises when the variables object of investigation have no pre-defined, easily identifiable causal roles, because they are "aggregates," that is, sums or averages of variables with heterogeneous causal roles, or simply because, for a variety of reasons, they

do not completely "transmit" causal information. Moreover, Spirtes and Scheines (2004) have pointed out the case of "ambiguous manipulation", in which an observed variable such as total cholesterol is "ill-defined", in the sense that it is the sum of two non-observed variables, viz. HDL (high density lipoproteins) plus LDL (low density lipoproteins), with different causal roles: LDL cholesterol causes heart disease, while HDL cholesterol prevents heart disease. Thus, an intervention on total cholesterol is ambiguous and has unpredictable effects, since it is not specified whether it acts on the causal path linking LDL cholesterol to heart disease or rather on the path linking HDL cholesterol to heart disease (or on both paths).

Notwithstanding the attention to ill-defined variables, a rigorous and comprehensive study of the cases where the problem of variable definition arises and makes troubles for causal inference has not been yet produced. This study seems particularly desirable in special sciences like economics, in which the applications of methods for causal inference must often cope with complex or "non-standard" settings. In this paper we attempt to precisely fill this gap. Moreover, we propose to address the problem of variable definition by means of a modeling approach grounded on "independent component analysis" (ICA), viz. a statistical technique that under the assumption of non-Gaussianity and linearity transforms a set of data in a mixture of independent shocks. We show that in several cases our proposed approach can solve the problem of variable definition. Furthermore, we show that in other cases, where it cannot reliably infer causal structures in the presence of ill-defined variables, our approach is at least able to flag their presence. Our argument shall proceed as follows.

In the first part of the paper, we show that simple structures over three-variable sets including one ill-defined variable violate the causal Markov and the faithfulness condition. Moreover, we show which (possibly "unfaithful" and "non-Markovian") (un)conditional dependencies and independencies are implied by different structures including such a variable. The aim is not just to highlight violations of the aforementioned assumptions, but to find some regularities in the relationship between structures with aggregates and conditional independence properties. For example, in a structure X <-- Z --> Y in which Z is the sum of two variables, each of which causes independently X and Y respectively, X may be unconditionally independent of Y, but, conditional on Z, become dependent on Y . Thus, in this case popular causal search algorithms would detect a collider whereas the original structure is instead a graphical fork. Next, we show what (un)conditional dependencies are implied when the original structure is a graphical chain where the ill-defined variable Z is in the middle, and the analogous pitfalls that emerge in the application of causal search algorithms to this case.

In the second part of the paper, we present the ICA approach and demonstrate how its application to the cases of ambiguous manipulations not only helps detect the problem of variable definition but also uncovers the true causal structure under assumptions as weak as those made by typical causal search algorithms (e.g. causal sufficiency, non-strictly deterministic causes, non-Gaussianity, linearity). Many studies (cf. Shimizu et al., 2006; Moneta et al., 2013; Capasso and Moneta, 2016) have already shown the potential of ICA for causal inference. However, ICA's potential for detecting which variables transmit the causal information, or—in the language of Spirtes et al. (2000)—which graphical causal paths are "active", has not yet been studied. To this end, we distinguish between "common" shocks—each of which affects two or more variables on the same active path—and "idiosyncratic" shocks—each of which affects a single variable. We argue that a structure with an ill-defined variable features patterns of common and idiosyncratic shocks that are

different from those featured by a structure over variables with unambiguous causal roles. To illustrate this point, we start from the simplest cases of two-variable sets and then move on to the cases of three-variable sets analyzed in the previous section. For example, in the case of a structure comprising just an ill-defined variable like "total cholesterol" and an effect of only one part of it (e.g. a specific disease caused by HDL only), the structure of the shocks is constituted by a common shock (corresponding to the active path HDL, viz. disease) and two idiosyncratic shocks (one for each variable). If one were to observe HDL instead of total cholesterol, one would detect just one idiosyncratic shocks since the shock to HDL is completely transmitted to the disease and is thus a common shock.

In sum, we propose a search procedure that allows one to detect cases of ill-defined variables via the identification of latent sources of variation by means of ICA. In some cases, building on the regularities found in the first part of the paper with regard to the relationships between structures with aggregate variables and (un)conditional independencies, our proposed search procedure also exploits conditional independence tests. In consequence, the search procedure naturally lends itself to correcting typical mistakes of causal search algorithms in the presence of ill-defined variables.

"Diversity of Models as a means to truer explanations in economics"

N. Emrah Aydinonat
aydinonat@gmail.com, *University of Helsinki*

Abstract

Recently Dani Rodrik, a prominent economist, proposed an account of economic models that puts a strong emphasis on the diversity of models in economics. However, most philosophical accounts of models and criticisms concerning mainstream economics often ignore the diversity of economic models and concentrate on the (un)realisticness of a single model. The present paper elaborates on Rodrik's account and explains how the diversity of economic models helps us in improving our understanding of economic phenomena. The paper argues that (i) the explanatory power of a theoretical economic model should be evaluated in the context of a relevant cluster of models and competing explanations of the phenomenon under investigation, (ii) diversity of models in economics increases our chances to provide better explanations of particular economic facts, and (iii) the recognition of the diversity of models in economics also helps us understand how the results obtained in the abstract realm of models travel to the realm of policy making. More generally, the paper explicates the explanatory import of highly abstract theoretical models in economics by way of underlying the relation between highly abstract theoretical economic models and explanations of particular economic facts, or events.

"The Explanatory Problem for Classical Rational Choice Theory"

Kate Vredenburgh
kvredenburgh@fas.harvard.edu, *Harvard University*

Abstract

A common complaint about classical rational choice theory — dating at least back to Sen (1970) — is that it is does not explain choice. In this paper, I focus on two versions of the problem: Firstly, that rational choice theory doesn't represent the causal mechanisms that produce choice behaviour; secondly, that revealed preference approaches suffer from a problematic circularity. I argue that the causal and circularity problems have something surprising in common: Each can be resisted by adopting a different background theory of explanation, unificationism. The successful response to these two problems thus reveals a deep disagreement over the nature of explanation in theories of choice, and has important ramifications for explanation in the social sciences more generally, as it makes a strong case for explanatory pluralism.

The two problems I pose are both problems for descriptive versions of rational choice theory, i.e., theories that aim to explain why people choose as they do. The way that classical versions of rational choice theory go about this explanatory project is to first ascribe a preference relation over the available options to an agent. The preference relation explains an agents choice if it rationalizes that choice function, i.e., if, in every context, the agent chooses the most preferred option, as given by the preference function. In slogan form: preferences explain choices.

Classical rational choice theory is an elegant, tractable framework that, if it can make good on its explanatory promise, explains a wide variety of choice behaviour. As I said previously, however, many critics have argued that classical rational choice theory does not explain choice behaviour. A wide variety of problems that have been posed under the label of "explanatory problems to classical rational choice theory." As mentioned above, I focus on two explanatory problems: The causal problem and the circularity problem.

The guiding idea behind the causal problem is that classical rational choice theory doesn't represent the explanatorily relevant causal structure well. I first deal with a popular, but unsuccessful version of the causal problem, the incompleteness problem. I then discuss two serious problems, based on two ways to make the guiding idea more precise. The first version of the causal problem argues that classical rational choice theory represents some of the relevant causal structure, but not enough. The explanations are highly abstract, i..e, compatible with many different underlying psychological causes. This seeming virtue, however, is actually a vice: The explanations given by rational choice theory are not informative enough about these underlying psychological causes. Thus, they are bad causal explanations. The second version of the causal problem argues that classical rational choice theory does not represent the actual causal structure at all, and so is not explanatory. One common response to this second version of the problem is that it is, explanatorily, good enough that agents act "as if" they had the preference function ascribed to them by rational choice theory. This response, however, only works in cases where we don't know the actual causes of some

phenomenon, and can only generate a set of possible explanations. In the case of rational choice theory, we do know the actual causes, so this line doesn't work.

The causal problem is a general problem, applying to different classical rational choice frameworks. The circularity problem by contrast, targets revealed preference approaches, which take choice as basic and define preferences in terms of choices. I distinguish between two interpretations of the relation between choice and preference in revealed preference theory (RPT): Identity RPT, which takes preferences to be identical to choices, and grounded RPT, which takes preferences to be grounded in choices. I argue that both interpretations involve a problematic circularity, once the following two principles about explanation are assumed:

No Self-Explanation: No fact (partially) explains itself.

Transitivity: If some fact F explains some fact G, and G explains a third fact H, then F explains H.

Both problems assume a background account of explanation as giving information about constraints, or why the explanandum had to come about. This assumption is plausible: Science is in the business of discovering laws and robust patterns that constrain the phenomena (including causal mechanisms). However, there is another type of explanation in science, unificationism. If the classical rational choice theorist adopts a unificationist account of explanation, she has the resources to resist the causal and circularity problems.

On the unificationist view of explanation, explanation is a matter of discovering mere patterns in reality, and then classifying states of affairs as instances of those patterns. Consider an explanation of why a particular sample of sodium burns yellow. A perfectly good explanation says that all samples of sodium burn yellow. Notice that this explanation does not give any information about underlying mechanisms. Instead, explanation is a matter of re-arranging information to fit under a pattern.

This unificationist view pairs nicely with revealed preference approaches. The explanations in revealed preference approaches are sometimes disparaged as "mere" summaries of choice behaviour. However, for a unificationist, there is nothing mere about it. Revealed preference approaches efficiently package information about patterns in choice behaviour, and this simplicity allows theorist to fit particular instances of choice behaviour into these patterns. Thus, revealed preference approaches are a strong motivating case for explanatory pluralism in science.

They don't, of course, just complement each other. Unificationism allows the revealed preference theorist to escape the circularity problem, by either denying No Self Explanation or denying Transitivity (or both). Unification is friendly to self-explanation: Patterns explain instances, but what are patterns, after all, except a set of instances? Transitivity also looks suspect for the unificationist: A pattern explains its instances, but why think that anything that causes the pattern explains the instances as well?

The recourse to unificationism is also available to the rational choice theorist more broadly. Unificationist theories of explanation also help resist the causal problem for rational choice theory. The resistance is straightforward: If theories do not have to give causal explanations, then they cannot be faulted for not doing so (as long as they give unifying explanations).

“On the Explanatory Import of Game Theory”

Philippe van Basshuysen and Phillipp Wichardt
P.C.Van-Basshuysen@lse.ac.uk, *LSE;* philipp.wichardt@uni-rostock.de, *University of Rostock*

Abstract

We give a general model of game theoretic explanations with the help of causal graphs. The model makes essential use of the fact that game forms screen off the explanandum -- a given social phenomenon -- from other factors such as physical circumstances, desires and beliefs of social agents. As a consequence, game theoretic models make social structures amenable to systematic policy interventions. The fields of market and mechanism design make essential use of this fact, and we take their success to confirm our model of game theoretic explanations.

“A formal framework for deliberated preferences”

Olivier Cailloux and Yves Meinard
Olivier.Cailloux@dauphine.fr, yves.meinard@dauphine.fr; *Université Paris-Dauphine*

Abstract

Introducing their so-called “reason-based theory of choice”, Dietrich and List noticed that, although the relationship between reasons and actions has been extensively investigated in the philosophical literature, rational choice theory and, more broadly, decision theory almost entirely ignore the concept of reason and strive to account for action and rationality exclusively in terms of preferences and beliefs. Despite Dietrich and List efforts, the gap remains large between philosophical and choice theoretic approaches. This gap has tended to insulate the literature on rational choice and decision theory from important debates that have been developed in the past thirty to forty years in the philosophical literature.

Among the most influential approaches having shaped these debates, Scanlon highlighted the links between reasons, justification and familiar moral ideas such as fairness and responsibility; Habermas’ “theory of communicative action” articulated the importance of justification and argumentation as distinctive features of rational action; and Rawls launched what was to become the debates on the “acceptability” (Estlund) of reasons and arguments for public justification purposes.

All these debates could greatly benefit from being couched in the formal terms carved out by the rational choice and decision theory literature. Reciprocally, the above-mentioned elaborate, reflexive debates on the role that reasons and arguments play in decision-making can help to escape some of the perennial difficulties plaguing decision theory. In this work, we hope to contribute to such an interdisciplinary dialog, by articulating a framework liable to link the formal literature on decision theory and the philosophical literature on argumentation and justification.

This framework is inspired by Goodman's and Rawls’ notion of a reflective equilibrium, but also

owes much to Roy's seminal ideas, in particular his ground-breaking view that the individual who benefits from decision-aid most of the time does not herself know her own preferences, and that one of the tasks of the analyst is to build a shared understanding of what those preferences consist in. On the technical side, our approach to modeling relations between arguments is largely inspired by formal argumentation theory.

To elaborate our framework, we start from a decision situation, where an individual requests decision support. In this context, we formally define, as a commendable basis for decision-aid, this individual's deliberated preference. The latter represents the individual's perspective towards a topic after she has considered all arguments possibly relevant to the situation. We explain how to formalize this notion thanks to binary relations over the set of arguments considered relevant to the topic and the set of propositions included in the topic. These binary relations allow capturing individuals' stances about whether a given argument supports a given proposition in the topic, and whether a given argument attacks another argument, in the sense that it turns it into an invalid argument.

We then explore the difficulties associated with the ambition to confront models of deliberated preference to empirical reality. These difficulties mainly stem from the fact that, whereas an individual can be assumed to know, or at least to be liable to express or reveal spontaneously her preferences in the usual sense, we cannot assume that she knows, or is liable to express spontaneously, her own deliberated preferences. An elicitation procedure that would simply ask the individual to state the propositions that he deems supported would accordingly not provide any guarantee that the stated stances are deliberated. Empirical validation should therefore involve confronting the support and attack relations hypothesized by models and the ones endorsed by empirical individuals. Once this is admitted, additional difficulties arise. Indeed, a straightforward empirical approach that would query the individual until it finds a pair of arguments for which a model and the individual disagree would be uselessly stringent by eliminating models differing on arguments that are never used to decide on the topic of interest. Empirical validation therefore appears to be real challenge.

To tackle this challenge, we identify conditions that, if fulfilled, (i) justify one in believing that the search for a valid model is meaningful, in the sense that there truly is such a thing as determinate deliberated preference of the aided individual to be found at the end of the day, and (ii) permit to unequivocally frame a procedure to determine whether or not a given model captures these deliberated preferences. Our conditions concern the way the aided individual reasons and the decision situation. We also introduce a formal notion of validity of a model, that rests on empirical confrontation of the model claims to the individual's stance in the decision situation. We prove theorems guaranteeing that a valid model exists if our conditions are satisfied, and that valid models correctly capture the individual's deliberated preferences.

We then explore the philosophical significance of our proposed framework. We argue that our concept of deliberated preference owes its normative credentials both to its normative foundations (the idea of rationality based on arguments) and to its reference to empirical reality (the stance that real, empirical individuals hold towards arguments and counter-arguments, on due reflection). Although our approach does not exactly match Rawls' or Goodman's version of the "reflective equilibrium", we argue that, if applied to the topic of justice, our framework provides avenues, not only to formalize Rawls' reasoning, but also to entrench its empirical dimension. We then highlight that our framework opens promising avenues for future research involving both philosophical and decision theoretic approaches, as well as empirical implementations in the context of deliberative forums and other deliberative institutions.

"The Incentive to Share in the Intermediate Results Game"

Remco Heesen
rdh51@cam.ac.uk, *University of Cambridge*

Abstract

The social value of scientific work is highest when it is widely shared. Work that is shared can be built upon by other scientists, and utilized in the wider society. Work that is not shared can only be built upon or utilized by the original discoverer, and would have to be duplicated by others before they can use it, leading to inefficient double work. To put the point more strongly, work that is not widely shared is not really scientific work. Insofar as science is essentially a social enterprise, representing the cumulative stock of human knowledge, work that other scientists do not know about and cannot build upon is not science. The sharing of scientific work is thus a necessary condition not merely for the success of science, but in an important sense for its very existence. Luckily, scientists have a good reason to share their work. Scientists want to receive credit for their work, as credit is crucial for career advancement in science. Credit is awarded according to the priority rule, which says that the first scientist to publicize a discovery takes all the credit for it (Merton 1957,Strevens2003). So scientists have a credit incentive to share their work (and share it quickly). There is some reason to think that there is no credit incentive to share in the case of intermediate results: discoveries that are perceived by scientists. Scientists, it is thought, are incentivized to keep their intermediate results secret in order not to help their competitors beat them to the larger discovery. This claim has been upheld by a number of authors, e.g., Strevens (2017, pp. 2–3), Resnik (2006, p. 135), Dasgupta and David (1994, p. 500), Arzberger et al. (2004, p. 146), Borgman (2012, p. 1072), and Soranno et al. (2015, p. 70). In contrast, two recent papers have argued that sharing is incentivized despite the potential for future discoveries, as long as partial or intermediate discoveries are rewarded with credit roughly proportional to their difficulty (Boyer 2014, Banerjee et al. 2014).

This paper discusses and extends these arguments. Boyer shows that "[t]here exist simple and plausible research situations for which the incentive to publish intermediate steps is sufficient" (Boyer 2014, p. 29). His game-theoretic model of scientists working on a "chain" of research problems (with each link in the chain dependent on the correct solution of the previous links) reveals an incentive to share under, by his own admission, highly idealized conditions. Hence it cannot by itself support general claims about the incentives faced by scientists regarding intermediate results sharing. Banerjee et al. (2014) address this worry by providing a model that relaxes Boyer's assumptions that there are only two scientists, that the scientists are equally productive, that different intermediate results are equally hard to achieve, that intermediate results can only be achieved in one order (the "chain"), and that scientists share either all or no intermediate results. Banerjee et al. (2014) show that sharing is a Nash equilibrium in their model if sufficient credit is given for intermediate results, relative to later results that depend on them. This means that, if all other scientists follow a policy of sharing their intermediate results, a single scientist who keeps her intermediate results secret will not improve her expected credit by doing so. In this paper I show that, in most cases, the equilibrium identified by Banerjee et al. is unique. In particular, rational credit-maximizing scientists share their intermediate results whenever these are

awarded at least as much creditassubsequentresults, relativeto their difficulty. I then draw out three philosophical lessons. The first lesson regards the explanation of the communist norm, the social norm which enjoins scientists to widely share their results (Merton 1942). If one thinks that scientists generally do not have an incentive to share their intermediate results, then the origins and persistence of the communist norm require a special explanation (Strevens 2017). I argue to the contrary that no special explanation is required: all that is needed for the communist norm to arise and persist is scientists acting in their self-interest. The second lesson regards the benefits of collaboration. Boyer-Kassem and Imbert (2015) have recently argued that scientists have an incentive to collaborate even if collaboration has no synergetic effects, using the model of Boyer (2014). I extend their argument based on the more general model of this paper. The third lesson poses a dilemma. Previous work has argued that the priority rule gives scientists a credit incentive to choose the research program that it is socially optimal for them to choose (Strevens2003). This argument assumes that the credit given for scientific research is proportional to its social value. In this paper I argue that credit should be proportional to diffculty in order to incentivize sharing and collaboration. But I also argue that there are cases in which social value and diffculty come apart, so that sharing and the optimal distribution over research programs cannot simultaneously be incentivized. By emphasizing credit incentives moderated by the priority rule, this paper falls in the tradition of Kitcher (1990, 1993), Dasgupta and David (1994), and Strevens (2003). Like those papers, I pick one aspect of the social structure of science and show how the priority rule has the power to shape that aspect to science's benefit.

"Governance of Perspectives in a Relational Partnership Society"

Pieter Ruys
ruys@uvt.nl, *Tilburg University*

Abstract

What is the distinction between a social enterprise and a private firm when both intend to behave commercially as well as socially? The usual characterization that distinguishes a for-profit-enterprise from a non-profit enterprise, or a state-owned enterprise, is based on institutional and legal attributes that don't clarify the reason of their existence. The standard economic modeling embeds the social and public enterprise in an environment that is almost exclusively based on monetary and utility generating incentives. This paper deviates from this neoclassical approach and relies on the relational approach that is built on relational capacities (including happiness and well-being) of agents. A relational capacity is modeled as a ternary relation, in which a pair of agents interact to generate an outcome in which both have contributed. A common opinion may result, for example, from the interaction between two discussants. Or a common action results from the interaction between partners in a transaction.

In this paper a modeling vehicle within the relational approach is introduced that allows for distinguishing generic rules of motion from the transient properties of a target system. The vehicle is a mathematical structure of ternary relations, called a projective geometry, which is symmetric and closed, defining the generic part of the roles and rules of the target system. The actual, transient properties are mapped into this generic structure, resulting in an asymmetric and open structure, called a relational partnership. Its duality property gives a direct correspondence between the relational capacities of partners and their behavioral domain, spanned by Economic, Human, or Imperial perspectives on the one hand, and available means and resources on the other hand.

That common modeling vehicle allows for comparing widely different organizations that are transformations of a partnership. It also characterizes these organizations by the degree in which they focus on businesslike economic values and resources, on cooperative human values and resources, or on emotional imperial values and resources.

The observation that any organization is a composition or an amalgamation of these identities makes it hard to govern such a hybrid organization. One result of the relational approach in this paper is the theorem that the hybrid character of the partners' perspectives and identities, can be resolved by making partners accountable in appropriate behavioral domains. But the existence of the modeling vehicle allows also for expanding the partnerships. Firstly, by associating a partner with parallel clones that are endowed with the same generic vehicle, and with perspectives parallel to the partner's actual perspectives, and such that the aggregation of clones results in outcomes that correspond with the macro behavior of the original partner. Inversely, this property allows for decentralization of partnership-decisions under much more general conditions than the decentralization property in a market economy under a uniform price system. This stabilizes a society. Next, the expansion of a partnership by associating a partner with micro-partners. These are constructed by giving parallel clones more freedom, so they can evolve to micro-partners in the partnership, from which diversity emerges. Such a partnership may perform as partner in a higher level partnership. These partnerships together form the hierarchical partnership society.

Since it has been shown that each organization or transaction in such a partnership society consists of two distinguishable, but interacting bipolar domains: (i) the legal transaction domain and (ii) the mindset domain, their interaction can be analyzed. In the transaction domain, the outcomes at different levels are determined by the authority and resources of partners on that level, from high level to lower levels. Conditional on the availability of suitable embodying institutions, this mental structure is transformed in, and co-evolves with the transaction mechanism in force. In the mindset domain, the common values and expectations are formed from partners' perspectives and experiences, from a low level to a higher level. Both processes together create a 'welfare growth spiral' in the evolving partnership society, while identifying an enterprise as econ, human or imperial by the domain on which it is focused and in which it is accountable.

Since perspectives and organizations are not restricted to the Econ domain with its objective of achieving maximal profits or utility, the objective of balancing of perspectives and resources within each partnership generates a well-being growth spiral for each partnership, including society as a whole, surpassing the outcomes of the standard approach.

“Has Populism Reached Economics?”

Irene van Staveren
staveren@iss.nl, *ISS*

Abstract

This paper attempts to understand why we generally feel that some normative concepts in economics are unproblematic whereas others feel uncomfortable or misleading. I present three criteria to distinguish between the two and argue that these will help not only to characterize what a problematic normative concept is in economics but also why the use of such concepts do not necessarily contribute to good economics, in the sense of the Dutch code for good academic practice. Following this discussion, the paper will present a case study on the concept of "ethnic fractionalization", popular in research on the social determinants of economic growth in developing countries.

Friday, March 24th

Well Being (Nihilism and Relativism)
(Fri 11:00-12:30, Room M1-08: Zaal Leuven)

"Nobody Needs a Theory of Human Well-Being"

Tyler Desroches
tyler.desroches@asu.edu; *School of Sustainability, Arizona State University*

Abstract

What is the relation between philosophical theories of human well-being and public policies directed at improving human well-being? More specifically, must we answer the philosophical question – what is a good human life? – before designing a policy intervention that promotes human well-being? After all, without knowing the correct theory of well-being, how could one conceivably claim that a well-being public policy actually succeeds (or fails)? One problem for this "priority view" is that philosophers remain deeply divided on the question of human well-being. There is no consensus on the true theory of human well-being and, arguably, none is forthcoming. Every major theory of well-being, such as hedonism, Aristotelian flourishing, and preference or desire satisfactionism, is subject to counter-examples. If the priority view is correct, then it would seem that we must either rest on our laurels, waiting for philosophers to decide on the correct theory of well-being or enact no well-being public policies altogether.

Unsurprisingly, a growing number of scholars reject the priority view. Dan Hausman, for one, argues that economists in particular require no commitment to any philosophical theory of human well-being when prescribing optimal well-being public policy. On Hausman's account, the satisfaction of a person's informed and self-interested subjective preferences should be treated as evidence for the claim that, when such preferences are satisfied, the person is made better-off. No matter which theory of human well-being is judged to be true, there is an evidential relation between the satisfaction of a person's "spruced-up" preferences and their own well-being. Gil Hersch provides an alternative view. He develops an intermediate account of well-being, which is not a theory of human well-being, but a construct that accommodates the essential features of every major philosophical theory of human well-being, all the while serving as the basis for adjudicating between well-being public policies.

Finally, Dan Haybron and Valerie Tiberius drive a wedge between philosophical theories of human well-being and well-being policy when they argue that well-being policy should be designed to promote "personal welfare values", or the values that people believe, correctly or incorrectly, have bearing on their own well-being. Specific theories of human well-being should never be endorsed by a liberal democratic government for the purpose of devising a well-being policy because this would risk violating the principle of liberty. While each of the preceding scholars is correct to suppose that we ought to reject the priority view, this paper will show there are problems with every proposal. It will be argued that if and when well-being policy is ever justified, the group of people affected by such a policy should decide for themselves, together, which policy intervention would

make their lives go best. On this account, philosophical theories of well-being are – at most – inputs into a collective decision-making process that is fundamentally political.

"Well-Being and Context-Dependence"

Mats Ingelstrom
mats.ingelstrom@philosophy.su.se, *Stockholm University*

Abstract

An implicit idea underlying much research and normative theorising about well-being is that 'well-being' means the same thing regardless of the context, or the purpose, in which it is being assessed. If this assumption is false, it is not clear to what extent insights from one field of empirical study carries over to other fields.

Anna Alexandrova has lately argued that, for the purpose of important practical projects, we should abandon the search for one unified theory of well-being that applies in all contexts. Specifically, she has argued that the very notion of well-being, as it is being used across sciences, policy and personal deliberation, depends on the context in which well-being ascriptions are being made. She claims that when scientists and others ascribe well-being, both the standard/threshold and the constituents of this ascription depends on the context: "Notice that both the threshold that separates well-being from ill-being, and what is thought to constitute well-being, varies. … I shall call these threshold and constitutive dependence, respectively" (2013, 313)

The claim that the notion of well-being depends on context is interesting and warrants further investigation. Furthermore, Alexandrova's claim rests on plausible descriptions of how scientists and others ordinarily make well-being ascriptions.

In this paper I construct and critically discuss a detailed argument (1–6 below) for the context-dependence of well-being. The argument can be spelled out in two versions: one concerns whether ascriptions of well-being are correct, the other whether they are justified.

My argument is, for a person p, in a context C:

(1) A correct (justified) ascription of p's well-being may vary with C.
(2) C can vary without there being any change in the facts about p.
(3) For identical facts about p: A correct (justified) ascriptions of p's well-being may vary with C.
(4) The only facts constituting a correct (justified) ascription of p's well-being are facts about p.
(5) If the facts constituting a correct (justified) ascription of p's well-being are identical in two contexts, then the correct (justified) ascription of p's well-being is the same in these contexts.
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(6) For identical facts about p: Which facts are constituently relevant for a correct (justified) ascriptions of p's well-being may vary with C.

I discuss this argument and I argue that in its correctness-version, the argument is unsound, and in its justified-version it does not have the implication that well-being is context-dependent.

“A New Argument Against Moral Relativism”

Greg Bognar
greg.bognar@philosophy.su.se, *Stockholm University*

Abstract

In recent years, an extensive body of research has grown out of economic experiments carried out in different countries and cultural settings. It has been found that in situations involving bargaining, cooperation, and the provision of public goods, people do not behave as models of economic agents would predict. In particular, people appear to follow certain norms of distribution, they are willing to punish unfair behaviour even at some cost to themselves, and they do not always pursue their own economic self-interest even when it seems irrational not to do so. Moreover, it appears that these results hold regardless of the economic, cultural, and social environment.

In this talk, I ask what implications these findings have to debates in moral philosophy -- specifically, whether we can draw any conclusions on their basis for the debate between moral relativism and moral universalism. I begin by clarifying what moral relativism is, as there are different views that have been identified with it. In particular, I argue that moral relativism is a naturalist ethical view: it holds that moral properties fit into the scientific view of the world, and that relativism gives the best account of them. After this, I spend some time on methodological issues, discussing how an argument against this view can be built. I argue that (i) all parties to the debate should agree that there must be a naturalistic way in which the truth of claims about moral relativism can be evaluated, and (ii) any argument for or against moral relativism must be inductive. This makes, I argue, economics and other social sciences relevant to the evaluation of moral relativism.

Building on the methodological points that I develop and defend, I go on to argue that the results of some economic experiments do provide us with the building blocks for an argument against moral relativism. They show that there are some norms that apply universally in a relevant sense, that can be recognized as moral norms, and that are at least pro tanto justified. Thus, I argue that empirical results in experimental economics can help settle some substantive issues in ethical theory. In the last part, I spend some time providing an interpretation of the experimental results, explain how they fit into the argument against moral relativism, and conclude by defending my overall argument from some possible objections.

Causal Inference in Econometrics
(Fri 11:00-12:30, Room M1-09: Zaal Bergen)

"Robustness arguments in meta-regression"

Aki Lehtinen
aki.lehtinen@helsinki.fi, *University of Helsinki*

Abstract

Edward Leamer's (1978,1983) discussions on specification searches and the lack of robustness of econometric findings have been influential in the practice of econometrics: Top economics journals require robustness tests (sensitivity analysis) as a matter of course for any econometric paper. Yet, there are some arguments that diminish the force of 'inferential' robustness that are not applicable for e.g., derivational robustness. In particular, if the functional form of an econometric model is correct, then it is not necessarily the case that the model should be robust with respect to various irrelevant regressors in the specification. In other words, it is well known that both omitting relevant variables and including irrelevant ones lead to biased estimates. Hoover (2006) puts this point by positing that the true model need not be robust. The reason why derivational and inferential robustness are different lies in the kinds of inferences that can be made: in derivational robustness the irrelevancies are shown not to affect the inferences, but in the inferential case irrelevant factors may be correlated with the independent variable and thus introduce distortions.

If an econometric model is incorrect, then the parameter estimates derived with it will be biased. This is presumably the reason why Woodward (2006) and Aldrich (2006) think that the true functional form must be included in the set of specifications that are tested in a sensitivity analysis. However, are we ever in a position to know what the true functional form is? If not, then we are never in a position to say that a demonstration of robustness, or lack of robustness, enhances or weakens an econometric inference. This raises a general question: what are the circumstances under which a proof of robustness enhances an econometric inference?
In this paper, I will look into meta-regression in order to study this question. When Stanley and Jarrell (1989) introduced meta-regression into economics, they took it to be providing a method of analyzing the problem of specification searches. Meta-analysts may study the sensitivity of results to alternative model specifications by running a meta-regression. A meta-regression may show sensitivity or lack of sensitivity with respect to a large number of factors in study design: choice of data (individual or aggregate, different countries, different time-periods), functional form, method of regression (OLS, linear, generalized, GMM, AIDS, Rotterdam etc.), type of regression (time series, panel, cross section) author, data source, etc.

This paper discusses a case study on alcohol own-price elasticities. The case provides further characteristics with respect to which an econometric study can be sensitive: type of beverage (beer, wine, spirits, and how they are defined; where to put the alcopops?), age and sex of consumers, functional form of elasticities (Marshallian or Hicksian), heavy drinkers or general population, are alcohol taxes used as proxy for prices or not, monopoly provided or not, retail or on-spot consumption, taking into account trade across borders or moonshine or not. It is clear that since the results may depend on such a huge variety of factors, the derived estimates will usually be different in the different primary studies.

The study of alcohol elasticities was selected because it seemed to provide a theoretically easy case. I thus tried to find a case in which the effect size to be studied would be theoretically as simple as possible so that the estimates could at least in principle be rather homogeneous, and more importantly, there could be a true functional form to be found. The reason for selecting the case is thus that it allows me to study Woodward and Aldrich's claim that the true functional form must be included in the set that is tried.

Meta-analysts distinguish between fixed (FES) and random effects (RES) models. The former presuppose that the estimates from the various primary studies concern a given single effect. The idea is thus that all the studies concern the same population. Random effect models, in contrast, presume that the studies included in the meta-regression are sampled from different populations so that the set of effects is presumed to be heterogeneous. In this sense, then, the RES models violate Woodward and Aldrich's assumption: there cannot be a true primary study functional form for a RES because there are several that are considered equally correct. Both kind of models are tested for robustness, but the interpretation of the results of sensitivity analyses is different. In a RES model the lack of robustness (of the meta-model) provides us with further information on how the effect depends on various factors. In a FES model the lack of robustness means that the meta-model is fragile, and some authors (e.g., Nelson and Kennedy 2009) recommend 're-thinking the analysis' in such a case. Thus, we know in principle the circumstances under which robustness provides the results with epistemic support.

However, the issue is more complex than this because there is a second-order problem of selecting the right kind of meta-model. While FES models are commonly taken to be reasonable when the purpose of the meta-analysis is providing an accurate estimate of the size of some effect in some population, they should not be used for out-of-sample predictions. In contrast, RES models are more useful for e.g., policy because they are taken to incorporate information on how the effect size depends on various circumstances. But the purpose of the investigation is not the only relevant factor for deciding which model to use. There are standard statistical tests to determine the degree of heterogeneity of a set of estimates (Q-test or the related I2 test). However, many meta-analysts do not think that one should proceed by simply running the test and then choosing the appropriate model. This is because heterogeneity in estimates may derive from many different sources, and the meta-analysts cannot be sure whether it is because the primary studies are comparing 'apples and oranges' or for some other reason mentioned above, or because of publication bias. Some meta-analysts claim that this is an issue that they cannot solve qua statisticians, but rather depends on the case at hand. The practical solution seems to be that some meta-analysts run both FES and RES models, thereby testing their meta-models for robustness. The very existence of such a practice testifies the difficulty of finding out the true meta-analytic functional form, and robustness tests within primary studies testify the same difficulty.

“In Defence of Statistical Minimalism”

Julian Reiss
julian.reiss@durham.ac.uk, *Durham University*

Abstract

It’s not a secret that statistical inferences about large data sets are made against a backdrop of assumptions about the data generating process. Nor is it a secret that we seldom have good reason to believe that the assumptions are met in most observational contexts, especially, but not only, in the biomedical and social sciences. Empirical researchers examining these contexts tend not to pay much attention to the dependence of the validity of inferences on the truth of the background assumptions, however. Worse, or so I shall argue, none of the techniques the literature offers to ameliorate the problem work. The two-fold goal of this paper is to draw attention to this problem and offer an alternative, statistical minimalism, that seeks to make valid (causal) inferences while relying on a minimum of statistical background assumptions.

“New People, Same Fights: Wold and the Cowles Commission”

Jennifer Jhun
jennifersjhun@gmail.com

Abstract

There is a rift in the history of econometrics, centered around the issue of causation, that, begins (largely) unified with Jan Tinbergen but yet branches in at least two distinguishable directions in the latter half of the 20th century. In this paper I return to a debate between Herman Wold and the Cowles Commission, the latter of which - one could say - emerged as victorious when it comes to the history of econometric thought. But this paper is an urge to appreciate Wold more so than we do in general philosophy of science, and given the contemporary debate surrounding issues of explanation and idealization in modeling, the topic seems apt. As Morgan (1991) puts it,

At first sight, it looks as if Wold's disagreement with Haavelmo and the Cowles group was only a matter of different competing model forms within the same probability-based econometrics research program. But this would be a simplistic judgement for, when we delve below the surface level of model forms and their estimation, we find the protagonists are arguing about more fundamental issues: static versus dynamic models, equilibrium versus disequilibrium models, causes and how they are to be introduced and interpreted in econometric models, aggregate versus individual (or group level) variables and description versus explanation.

Wold advocated for an alternative to the simultaneous equations method (SEM) preferred and advocated by the Cowles Commission. Like Wold, figureheads of Cowles such as Haavelmo (1943) and Koopmans (1950, 1953) envisioned SEM as bringing together theory and measurement in order to capture causal information.[1] But Wold instead suggested recursive models involving causal chains a la process analysis, rather than non-recursive simultaneous equations. He felt that this

system would suit economic analysis better since it dealt with disequilibrium behavior without equilibrium assumption, capturing the economy as a dynamic system. Furthermore, the recursive system could distinguish causal direction in a way that SEM could not.[2] On the surface, this seems to be quite attractive. After all, as a policymaker, would it not be ever useful to be able to trace a causal process within the model itself? Unfortunately, in the end, despite there seeming to be very little of substance that would uncontroversially damn Wold's methods as inferior to those of Cowles': "Wold's recursive model form, shorn of its process analysis interpretation, was relegated to a minor special case of the general simultaneous equations system"(Morgan).[3]

I argue that the distinction is actually not as severe as history (nor Wold, nor his opponents) make it seem. First, despite the fact that structural modeling is associated more explicitly with intervention and invariance, I clarify that Wold's methodology does not exclude invariance, either. In fact, it is only if we clarify these aspects of Wold's views that it turns out that his thought is continuous with the way econometrics has progressed since (with its developments in structural methods). If this is correct, and if most econometric work from the 1950's onward has assumed a somewhat interventionist or manipulationist interpretation of causation (ultimately aimed towards thinking about policy), the supposed bifurcation becomes even more baffling from the standpoint of philosophy of science. Second, I argue that this debate furthermore highlights the (sometimes unwarranted) movements that economists make between the following distinctions: endogenous/exogenous and independent/dependent variables, as well as muddling the notion of simultaneity - obscuring the role that causation actually plays in economic models. Lastly, I argue that Basmann's (1965) result that recursive and simultaneous systems models are observationally equivalent (a paper which was taken to snuff out the debate) misses the point of Wold's motivation precisely by neglecting the role of causation he intended and that we clarify here.

"The Evolutionary Explanation of What? A Closer Look at Adaptationist Explanations of Risk Preferences"

Bengt Autzen
b.autzen@bristol.ac.uk, *University of Bristol*

Abstract

In common parlance risk refers to the possibility of harm, injury or loss. Among decision theorists and economists, however, risk is associated with a different concept. Rather than identifying risk with the possibility of harm, risk refers to uncertainty or, more precisely, the dispersion of outcomes in a probability distribution. As such, risk is typically measured by statistical quantities such as the variance of a probability distribution. While it is rather intuitive that human beings are averse to risk given the risk-as-possibility-of-harm reading, it is an interesting question of whether – and if so, why - human beings are averse to risk in the risk-as-dispersion sense.

A variety of explanations of human attitudes towards risk, understood from now on as risk as dispersion, have been offered. Besides explanations originating in psychology and the social sciences, evolutionary explanations have recently been put forward in the philosophical literature. Okasha (2007) offers an adaptationist explanation of risk aversion. Formally, an agent is said to be risk averse if and only if she prefers x USD for certain to a lottery with expected monetary value x USD. An agent is said to be risk seeking (or risk loving) if and only if she prefers a lottery with expected monetary value x USD to x USD for certain. Okasha's argument invokes results from theoretical biology established by Gillespie in the 1970's. Gillespie (1973, 1974) demonstrates that natural selection is sensitive to both the mean and the variance of the offspring distribution when organisms evolve in stochastic environments. Building on Gillespie's work, Okasha postulates a 'currency shift' from numbers of offspring as the quantity of interest in evolutionary models to money as the quantity of interest in an economic setting and concludes that there is an evolutionary rationale for being sensitive to the variance of a lottery.

Okasha's account has been criticised on the grounds that it misconstrues its explanandum. Rather than explaining that human beings are risk averse, Schulz (2008) argues that explaining human attitudes towards risk requires the explanation of both risk aversion and risk seeking. Schulz refers to the phenomenon that people both purchase insurance and play the lottery. Buying insurance products amounts to risk averse behaviour since agents prefer a certain small loss due to the payment of the insurance premium over an uncertain but potentially extremely large loss. In contrast, playing the lottery amounts to risk seeking behaviour since agents prefer an uncertain large gain over a certain small loss due to the purchase of the lottery ticket. A requirement for an evolutionary account of human risk preferences is to explain both these behaviours.

Schulz's objection raises an important question: what is the correct explanandum for an evolutionary explanation of human risk preferences? This paper will follow up this question. In particular, the paper explores an intriguing parallel between the debate about evolutionary explanations of human mating preferences and evolutionary accounts of human attitudes towards risk. Adaptationist explanations of human mating preferences have been criticised on the grounds

that the mating preferences postulated and subsequently explained by evolutionary psychologists fail to be a universal feature of human nature (Buller, 2005). By the same token, I will argue that human attitudes towards risk fail to constitute a universal preference and as such do not constitute an adequate explanandum for an adaptationist explanation. More specifically, I will consider the fourfold pattern of risk preferences postulated by prospect theory (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992). Prospect theory is widely held to be the most promising descriptive account of decision making under risk. The theory stipulates that for events with moderate to high probability agents are risk averse in the gains domain and risk seeking in the loss domain. For gains and losses with low probability, however, the pattern is reversed. This feature of prospect theory is referred to as the 'reflection effect' and the resulting risk preferences are said to follow a 'fourfold pattern'. I will argue that the experimental evidence does not support the idea that the fourfold pattern of risk preferences constitutes a universal preference.

"Behavioural Experiments and the Evolutionary Explanation of Prosocial Behaviour"

Vaios Koliofotis
vkoliofot@hotmail.com, *Erasmus University Rotterdam*

Abstract

Following the introduction of experimental methods in economics, an ongoing research question is whether the findings of behavioral experiments allow an evolutionary explanation (e.g. Henrich et al., 2005; West et al, 2007; West et al 2011; Bowles and Gintis, 2011; Vromen, 2012; Rand and Nowak, 2013). My aim is to examine in what way experimental research can provide evidence that prosocial behaviors are adaptations, that is, they have evolved by a selection processes such as natural selection. I contribute to the existing literature by proposing a specific methodological approach in examining the evolution of prosocial behavior that explicitly links behavioral responses in laboratory experiments and the field with evolutionary explanations. This method of analysis includes four steps in which candidate behavioral adaptations are inferred prior to and independent of theoretical approaches about their evolution in the ancestral environment.

S1) Identify the behaviours of interest that occur under present conditions.
S2) Model the ancestral environment in which selection processes might have taken place and compare it with the current environment in which prosocial behavior is identified.
S3) Construct an evolutionary model that provides an explanation for the evolution of the strategy (i.e. trait) that relates to behavior of interest.
S4) Test the model's predictions in the laboratory or the field.

The method of evolutionary analysis that I propose proceeds as follows. In the first step, researchers analyze behavior in contemporary human populations and how it is influenced by experimental rules imposed in the laboratory environment or the field (Plott 1979; Smith 1982; Guala, 2005). The participant's behavior in the experiment consists of the choices that they make in accordance with their characteristics (i.e. endowments, initial information, human capital and preferences). An economic experiment elicits behaviour that can be interpreted in the light of rules that bridge the

gap between economic theory and the actual experiment. The set of experimental rules provide environmental stimuli, specify admissible choices available to the subjects and determine how these choices translate into final observable outcomes.

Since behavioral adaptations that humans have in the present were selected in the ancestral environment, in the second step, researchers identify specific selection pressures in order to model the ancestral past. Human evolutionary history can be considered a mosaic where different components of adaptations occurred under environments with different characteristics. Irons (1998) proposed the concept of the "Adaptively Relevant Environment" (ARE) that consists only of key characteristics of different environments that humans interacted with and resulted to a fitness benefit for the candidate adaptation in question. For the evolution of prosocial behavior the ARE is not so much the physical environment (i.e. climatic changes) but the social environment that consists of other humans that were themselves undergoing evolution.

In the third step there is an examination of whether strategies related to experimental behavior are the result of natural selection in the past. Based on the selection pressures that operated during human evolutionary history, the question is why a trait (i.e. strategy) has evolved compared to other traits in a population. An answer is provided based on the winnowing effect of natural selection and direct/indirect fitness costs and benefits. This investigation is conducted by constructing theoretical models of adaptive (i.e. fitness maximizing) behaviour, frequency dependent selection, social learning and/or group selection processes.

These evolutionary models provide a novel source of considerations regarding the behavior of subjects in the laboratory. New hypothesis based on prosocial behavior can be formulated by considering selection processes in the ancestral environment. In the fourth step, economists can conduct standard behavioral experiments to determine whether subjects in present have the inferred behavior. For example, many approaches in the literature analyze ultimate (i.e. evolutionary) explanations by adopting group selection frameworks (Boyd et al., 2003; Gintis, 2000). The implication is that prosocial behaviors resulted in outcomes detrimental to the individual but beneficial for the group that one belongs. Thus, in this final step, what has to be considered when forming the experimental rules is the between- and within- group interactions and the complex relationship between individual and group welfare. More specifically, experimental researcher must condition behavior on the group membership of experimental subjects since their responses might be group-sensitive.

This method of analysis in which candidate behavioral adaptations are inferred prior to and independent of theoretical approaches about their evolution in the ancestral environment has two implications:

First, researchers can consider behaviors as products of natural selection, irrespective of whether they provide current fitness benefits. The so-called "economic-demographic paradox" (e.g. Vining, 1986; Low 1993; Kaplan, 1996) breaks the link between a fitness proxy such as money and reproductive fitness, at least in experimental subjects from western societies (e.g. Clavien & Chapuisat, 2013). While in most preindustrial societies material resources enhanced reproductive success, modern industrialized populations lack the strong positive correlations between material resources and reproductive fitness. However, based on the methodological framework that I propose, researchers are not providing adaptive (i.e. fitness maximizing) explanations of prosocial behavior in every environment and therefore, correlations between the outcome of behaviour in an experiment and current reproductive success is not necessary for an evolutionary explanation. Adaptations relate to evolutionary time and prosocial behaviors are the product of a history of reproductive success.

Second, researchers can refrain from providing psychological explanations related to motivational factors or cognitive processes. For the methodology that I propose the most appropriate level to study human adaptations is behavior and associated strategies and not desires, motives or human cognitive machinery. The main benefit in delineating motivations driving a specific behavior from the choice-behavior itself and its monetary or fitness consequences is avoiding an underdermination problem: even a precise and complete description of a subject's behaviour in a behavioral experiment does not allow researchers to draw unambiguous inferences about their motives. This is because there is a many-to-one relationship between motives and behavioral choices, whereby many equivalent motives can result to a given outcome.

"Relief in Games"

Paolo Galeazzi and Zoi Terzopoulou
pagale87@gmail.com, zoiterzopoulou@yahoo.com; *ILLC*

Abstract
Since the well-known work of Savage (1954), evidence that demonstrates regular violations of different norms of rational choice which have been proposed has been provided continuously by the empirical scientists (e.g., Tversky and Kahneman, 1981). A significantly challenging step for utility theories to overcome seems to be that human preferences depend not only on the choice task itself, but also on the context where the task is presented (Slovic and Tversky, 1974). Therefore, alternative approaches have been generated nowadays concerning rational decision making (e.g., Dietrich and List, 2016), which have progressed away from the simplified idea of maximizing expected value. In this paper we keep in mind a wide framework of context dependent choice, and we explore different representations of decision situations and games. Specifically, we introduce relief in games and we argue that relief maximization is a decision rule able to explain and predict certain systematic violations of the expected utility theory. We investigate phenomena such as the asymmetric dominance (AD) effect, where agents seem to take into consideration inferior options. Moreover, we prove that in a population consisting of relief maximizers and of maximinimizers, and with reference to a class of games where AD is present, maximizing relief is the only evolutionary stable choice mechanism. We conclude that relief maximization contributes to capturing a more realistic view on rational choice.

Given a game, one main question concerns what strategies of the agents are prescribed by choice rules that can be called rational. For instance, the maxmin rule suggests that a rational player chooses a strategy whose worst case scenario payoff is the maximum of all the other strategies' worst case scenario payoffs. It has already been noted that agents' diverse choices in different contexts give rise to alternative decision principles too, as for instance the regret minimization (Halpern and Pass, 2012; Loomes and Sugden, 1982). It is indeed the case that an outcome may generate the feeling of regret, when as things have turned out an alternative choice would lead to a more desirable outcome, but also the feeling of relief, when the idea that things could have been worse offers to the decision maker an additional satisfaction. For example, consider a decision situation in a card game, where you can choose to bet \$0, \$50 or \$100 on your win. Suppose that you decide to bet \$50, but in the end you lose the game. Then, you probably experience some relief, induced by the fact that you did not bet \$100 (and also some regret, by the fact that you did not bet \$0). Apparently, the possibility of a worse (or a better) outcome can influence your interpretation of the actual outcome.The relief-value of a strategy in a specific scenario is defined as the difference

between the payoff of that strategy and the payoff of the worst strategy in that scenario. Intuitively, the more you avoid worse outcomes, the more relief is increasing. The minimum of all relief-values in the different scenarios of a decision situation or a game is called the relief of a strategy. According to the maxmin-relief rule, a rational player chooses a strategy with the greatest relief.

Relief maximization is a decision rule directly related to worst replies and consequently to dominated strategies, in contrast with other known decision rules, such as maxmin. Having this in mind, we quote the following relevant statement from Cooper et al. (1990): "To the extent that the selection of a strategy (equilibrium) is influenced by payoffs associated with a dominated strategy, this provides evidence against any selection criteria which ignore these strategies." Thus, our motivation to analyze relief maximization as an interesting decision criterion is apparent.

Slovic (1975) argues that decision makers tend to prefer an option that they find better on some important attribute, because such a decision can be easily justified to oneself and to others. Later theories on reason-based decision making further developed the idea that the decision process involves the search for a dominance structure in which one alternative can be interpreted as dominant over the others (Montgomery, 1994). The construction of a dominance structure for the decision maker via an alternative representation of the decision problem can provide a reasonable explanation of the final choice on the grounds of justification.

The phenomenon of reason-based choice is explicitly reported on the AD effect. Strategic AD can be observed in a game in which a player has one strategy that strictly dominates just one of two or more other strategies. Thus, if an asymmetrically dominated strategy increases the player's preference for its dominating strategy relative to the other strategies in a game, strategic AD is said to occur. Colman et al. (2007) study this phenomenon empirically, using 3x3 games which extend 2x2 games by adding asymmetrically dominated strategies. In their work, the strategies that were dominant in the 3x3 games were chosen significantly more frequently than in the 2x2 games, where they were not dominant. In our paper we show that relief maximization is in accordance with this behaviour. Notably, the following proposition holds: In 3x3 games where a strategy of a player is dominated by exactly one of the two other strategies and there is no other dominance relation, relief maximization prescribes the selection of the dominant strategy.

After establishing an argument in favor of the descriptive value of relief maximization with regard to the AD effect, we further wonder if a relief-based analysis of decision problems can also be evolutionarily beneficial. In case of a positive answer, the result would support the survival and use of relief-based reasoning for at least some decision situations. We use a multi-game approach that is an extension of standard models in evolutionary game theory (see Galeazzi and Franke, 2016). We work with a population of players among maximinimizers and relief-maximizers, where each player represents a decision rule. At each time, a 3x3 symmetric game with one asymmetrically dominated strategy is randomly selected and two players are randomly chosen from the population and paired to play it. Each player plays an action as prescribed by her decision rule, and the monetary payoffs (or evolutionary fitness) that they achieve are recorded. Next, another game is selected, two players are matched, and so on. We prove that a population of relief types is the only monomorphic evolutionarily stable configuration in this decision scenario, where AD is involved.

To conclude, this paper enriches the research direction that tries to ask and answer questions about the exact role of context in decision making. In particular, relief maximization is shown to be a descriptively accurate and normatively appealing decision criterion with respect to a robust empirical phenomenon: the asymmetric dominance effect. Our results can be considered to form only a beginning, towards a more complex formal model of context dependent, dynamic choice rules.

"How Scientific Realism Changed History"

Marlusz Maziarz and Bartosz Scheuer
mariusz.mm@gmail.com, bartosz.scheuer@ue.wroc.pl; *Wroclaw University of Economics*

Abstract

The Reinhart-Rogoff controversy exploded in April 2013, when Herndon, Ash, and Pollin (2014) published a draft of their Cambridge Journal of Economics article aimed at describing the failed attempt of replicating Growth in a Time of Debt (Reinhart and Rogoff 2010). The controversy got the immediate attention of professional economists, popular-press commentators, and economic policy-makers for several reasons. First, the two articles deal with the question if public debt harms economic development, what is one of the most piercing macroeconomic problems today. Second, Growth in a Time of Debt was very influential. Several most prominent politicians cited the 90% debt-to-GDP threshold hypothesis as a reason for austerity at the Treasury, what made Krugman (2013) call it "surely the most influential economic analysis of recent years". Third, even though only a limited number of economic analyses is replicable, discovering a miscoded Excel formula which failed in calculating average by excluding five alphabetically first instances is extraordinary.

Among close to two thousand articles focused on the 90%-threshold hypothesis, two explanations can be reconstructed. On one hand, several commentators misinterpreted Herndon, Ash, and Pollin (2014) article by focusing on the spreadsheet error only (Clemens 2015; Muslu 2015; Grimson 2014; Stevenson and Wolfers 2013). In contrary to their viewpoint, the spreadsheet error biased the results in a minor way (up to 0,3 pp.) On the other hand, others did not commit the previously mentioned error but present the point of view according to which all the three drawbacks indicated in replication are mistakes indeed. In my recent JEM article, I reconstruct in detail and refute these two explanations (anonymized in press). They lack descriptive adequacy because a careful analysis (1) of how each of the three drawbacks biases the results show that choosing weighted or unweighted averaging scheme makes the results differ (up to 1,7 pp.). Additionally (2), excluding the spreadsheet error, the other two drawbacks indicated by Herndon, Ash, and Pollin (2014) should be analyzed in terms of methodological decisions, choosing alternative, similarly justified methods instead of mistakes (Hamilton 2013).

During the presentation, we aim at showing that the fallacious explanations correspond to the philosophical presuppositions of mainstream philosophy of economics. The below-described cases are exemplary and will be further developed. (1) Scientific realism is, in short, often defined as a positive attitude towards best scientific theories (Chakravartty 2016, for instance). And, since most philosophers of science were trained in the case of the historical developments of physics, most successful theories are believed to be the most recent ones. This belief that theories converge on to the true description is also grounded by the viewpoint on epistemology most scientific realists hold, i.e. fallibilism. According to this position, better theories and models replace the falsified ones (cf. Putnam 1982, Mäki 2011). The belief in the progress of science implies that more recent methods leading to novel results are also better what suggested that the methods employed by Herndon, Ash,

and Pollin (2014) are more justified that those leading to confirming the 90%-threshold hypothesis. (2) One of the central tenets of logical positivism, the belief in a unified scientific method, corresponds to the situation that the commentators did not grasp the fact that both methods employed by Reinhart and Rogoff (2010) and Herndon, Ash, and Pollin (2014) are similarly justified.

On the contrary, the descriptively adequate explanation offered recently (anonymized in press) is in line with a heterodox approach to the philosophy of economics, scientific constructivism, namely. According to this approach, the difference in the method could be seen as a result of belonging to different thought collectives (defined as by Fleck (1979, p. 83)) and being parts of different thought styles produced by those collectives. Contrary to the mainstream philosophy of economics (scientific realism), constructivism considers any cognitive activity as a social process in which so-called 'objective reality' appears as a final result of a process of creation of knowledge (not as an effect of a discovery as it is in realism). Elements of that 'reality' appear as independent of cognitive activities but solely because particular thought style contains so dense network of elements of knowledge (active and passive [Fleck 1979; p. 83]) that it produces an illusion of objectivity. In other words, for members of thought collective scientific facts appear as elements of 'independent reality', meanwhile, those facts are "thought-stylized conceptual relations" [Fleck 1979, p. 83]. What should be pointed out the whole process is social and linguistic in its nature contrary to realist vision of cognitive activity as a subject-object relation. In this perspective, the RR controversy could be interpreted not in terms of 'progress', 'true' and 'false' results or 'better' and 'worse' method but, rather, in terms of incommensurate thought styles. Consequently, each method should be considered as adequate (and justified) but only for members of particular thought collective as well as the difference in results should be seen as a product of differences in 'thought-stylized conceptual relations'. Put otherwise: if one shows that RR and HAP contain different conceptual relations of active and passive elements of knowledge, then there is no contradiction between their results.

"Why is General Philosophy of Science As Yet Irrelevant for Economics and What Can be Done against This?"

Paul Hoyningen-Huene and Nicolas Wuthrich
hoyningen@ww.uni-hannover.de, *University of Zurich/University of Hannover*;
N.Wuethrich@lse.ac.uk, *LSE*

Abstract

In the literature on the philosophy of economics, it is tried very often to establish contacts to the general philosophy of science. Typically, the question is whether the well-known general philosophies of science like those of Popper, Kuhn, Lakatos, or Feyerabend capture essential features of economics (see for example Backhouse (2012), Blaug (1975), Blaug (1980), Caldwell (1991), Hausman (1992), and Pheby (1991 [1988])). Equally typically, the results have as yet been very disappointing. None of these standard philosophies of science seem to be appropriate for economics. Neither is there something like Popper's falsification pattern (Popper (1959 [1934])), nor Kuhn's paradigms (Kuhn (1970 [1962])), nor Lakatos' research programs (Lakatos (1970); this philosophy comes closest to economics but still severely differs), nor Feyerabend's methodological anarchism (Feyerabend (1975)). Of course, these philosophies were developed with the natural sciences in view, especially physics. However, in those central aspects that the general philosophies of science capture, economics does not resemble physics, in spite of attempts by economists to the contrary (see for instance Friedman (1953), p. 4). How can this fact be explained?

We propose the following solution to this puzzle. Economics clearly does not feature paradigms in Kuhn's sense, i.e., paradigmatic solutions to central research questions that are unanimously accepted by the relevant scientific community as correct and therefore as guidelines for further research (see Hoyningen-Huene (1993), chapter 4). However, econom-ics features something that resembles paradigms in Kuhn's sense which we call "orienta-tional paradigms". Like in Kuhn, orientational paradigms may be found on a more general and a more specialized level, namely, as broad generalizations or as exemplars in the form of models. In contrast to Kuhn's paradigms, orientational paradigms are not necessarily be-lieved to be correct (or at least: believed to be committing guidelines for research) in the relevant scientific community. Rather, orientational paradigms are necessary reference points that have to be used in the presentation of research, be it in agreement or in disagree-ment with them. We illustrate the use of orientational paradigms in economics by a number of examples, including the rational actor model (see, e.g., Kreps (2012)), the rational expec-tation hypothesis (Muth (1961)), Akerlof's model of asymmetric information (Akerlof (1970)), the Nash Equilibrium as a solution concept in Game theory (Nash (1950)), the CAPM in asset pricing (Sharpe (1964)), the Modigliani Miller theorem in corporate finance (Modigliani and Miller (1958)), and simple DSGE models in macroeconomics (Kydland and Prescott (1982)). In all of these cases, ensuing work in the pertinent areas typically refers to these orientational paradigms, either approvingly or disapprovingly. This explains their ex-tremely high citation rates (or they have entered the textbook level such that they are not accompanied by explicit citations any longer).

It is orientational paradigms that sets economics apart from all the other social sci-ences that lack the reference points of orientational paradigms. In spite of the partly severe disagreement about their validity, orientational paradigms provide a special kind of unity to economics in founding much more coherent communication and exchange between different schools and areas. Typically, lack of orientational paradigms in the other social sciences leads to impoverished communication

and exchange between different schools and conse-quently, to less productive interaction between them.

If general philosophy of science is to make serious contact with economics, its orien-tation must not be entirely based on the natural sciences, as it did at least in the last one hundred years; a broader framework is needed. Recently, such a broader framework has been developed, namely, the systematicity theory of science (Hoyningen-Huene (2013)). System-aticity theory's subject are all academic research fields, that is besides the natural sciences also mathematics, the engineering sciences, the social sciences, and the humanities. The central thesis of systematicity theory is that science (in this broad sense) is more systematic than other forms of knowledge, especially everyday knowledge. The higher degree of sys-tematicity presents itself in nine different dimensions, among them epistemic connectedness and critical discourse. Systematicity theory describes an increase in systematicity in any of the dimension as advantageous for the respective discipline. Therefore, the role of orienta-tional paradigms in economics can be understood as advantageous for economics in increas-ing the degree of systematicity particularly regarding the dimensions "epistemic connected-ness" and "critical discourse". Because orientational paradigms provide shared reference points for supporters as well as for opponents of the respective paradigms, they increase in a systematic way the epistemic connectedness (or coherence) of economics as a discipline. By the same token, critical discourse in the community of economists is facilitated and en-hanced by the existence of orientational paradigms because economists are forced to express their views with reference to the pertinent orientational paradigms.

If true, our analysis also allows responding in a new way to the widespread criticism of the economic discipline after the recent financial crisis. The deep divisions in economics and the unrealistic assumptions have to be put into perspective: although prima facie rele-vant lines of criticisms, they miss the important function that organizational paradigms play in economics. In fact, our thesis opens up a new self-perception of the economic discipline with important implications for how the discipline interacts with external criticism.

"What are philosophical theories of models good for?"

Melissa Vergara Fernandez
vergarafernandez@fwb.eur.nl, *EIPE-Erasmus University Rotterdam*

Abstract

Interest by philosophers in the use of models in science has increased significantly in the last couple of decades, generating a vast literature, which addresses different issues related to model use in science. Three broad categories of these issues can be identified. First, the ontology of models, which addresses mainly questions such as what models are or how they are related to theories. Second, the semantics of modelling, which mainly tries to identify how models are related to the things they are models of: their targets. Finally, the epistemology of models, which attempts to address questions related to the things we can learn from models. Naturally, this division is purely analytical: how one takes models to be (ontologically) determines, at least to an extent, their epistemology and how they relate to their targets—and viceversa.

Some models haven't fared particularly well in the last few years. The failure to anticipate the

financial crisis and to handle the economic recession that followed has been amply documented as a failure of economic models. The failure to predict the rise of Donald Trump to the United States presidency continues to have both laypeople and experts aghast. An obvious move is thus to turn to the literature on models in order to attempt to understand this. How can the accumulated understanding over the last decades of the philosophy of models be used to make sense of these failures? How can we use the extant philosophical theories of models to determine what exactly went wrong? In this paper I shall argue that despite the vastness of the literature, there isn't an analytical framework which can be used to answer these questions and thereby offer guidelines that may help overcome model failures. I offer two reasons for this, one with respect to the semantics of models, and the other with respect to the epistemology of models. With respect to former, I shall discuss the conditions that a theory of scientific representation has to have according to Frigg & Nguyen (2016). Frigg & Nguyen (2016) provide these conditions by building on the different accounts of scientific representation provided so far. I shall suggest that these conditions are silent with respect to measures of epistemic success. At most, they attempt to track successful representation, but that is not sufficient to judge why or how a model has failed—as is our case now. With respect to the epistemology of models, I shall suggest that the literature has focussed too much on trying to show the epistemic import of models, specifically how scientists can actually learn from models by using cases of success—e.g. Grüne-Yanoff (2009), Reiss (2012), Morgan (2012), Weisberg (2012),Ylikoski & Aydinonat (2014)—at the expense of attempts to establish conditions for learning, or, at the very least, attempts to analyse whether and why certain models have failed.

I shall finally suggest that the analytical framework that allows us to answer questions about why some models fail hasn't been developed because this hasn't been the primary aim of the philosophy of models. Instead, this literature seems to have been attempting to make sense of models vis à vis previous characterisations of science that ignored the role of models or dismissed them as unimportant. So the aim seems to be to make justice to models by placing them in a philosophically-sound reconstruction of scientific practice. While this by itself is not obviously problematic, it does raise an important question about the purpose of our philosophical accounts of models, namely, whether the philosophical accounts of models are meant to be insightful for our philosophical accounts of science, or for our scientific models. More generally, it raises a question about the aim of philosophy of science: is it for philosophy or is it for science?

Fair Policies: Concrete Cases
(Fri 13:30-15:00, Room M1-08: Zaal Leuven)

"Justice and the Limits of Money"

Louis Larue
louis.larue@uclouvain.be, *Université Catholique de Louvain*

Abstract

An impressive variety of new forms of money has appeared in recent decades – such as LETS, Time Banks, Carbon currencies or Local currencies (Blanc, 2011; Lietaer, Arnsperger, Goerner, & Brunnhuber, 2012). These currencies have two common features: they are all designed in order to serve a limited number of purposes, and their possible uses are limited in accordance with these purposes (Blanc, 2011). They may thus be called special-purpose currencies. The goal of this paper is to investigate whether these limitations are just and legitimate. It is thus primarily concerned with ethics and finds its place within liberal egalitarian theories of justice. One has indeed to see whether these new currencies if applied to the whole society, would make it more just and efficient. Money is an object of justice: it is one of the means which opens the possibility for people to lead the life they wish, and one of the government's instruments for social and economic policies. The principles which guide its creation, circulation and enjoyment should thus be a concern for ethics. The scope of this paper is however limited to one normative question: Should a currency's purchasing power be circumscribed to a specific community or area? Should what money can buy be limited according to who is the buyer or the producer?

This paper studies three types of limitations. The first is geographical: a currency may only buy goods produced in a certain area. The second is communitarian: a currency may be used only to sell and buy commodities to other users of that specific currency. The third is related to some environmental or social features of the good exchanged: the purchasing power of a currency may be limited to goods produced in fair and/or environmental friendly conditions. The key point is that special-purpose currencies are intentionally designed not to be applicable in all situations. This limited scope differs sharply from the habit, so usual in economics (Tobin, 2008), of considering money as an all-purpose means of payment acceptable for virtually all goods and services available on a market.

The distinction between special-purpose and all-purpose currencies raises several normative issues. A first concerns the extent of commodification, the extent of what money can buy. A second issue is a matter of who may buy whose goods. A third issue is related to monetary policy: should it be managed at the local or the global level? What is its proper scope? This paper focuses mainly on the second issue. It is not primarily concerned with monetary policy as such and refers to it only slightly. Neither does it consider thoroughly commodification, which has already driven much debate and would need a separate study (Anderson, 1990; Sandel, 2012). Clearly, the first and the second issues are related but separate questions. Everything may be for sale, babies included, but still, as an inhabitant of one particular town, one may only buy babies born in that area. On the contrary, the scope of what one can buy may be limited, but money may not be constrained by geographic or communitarian criteria.

Should a currency's purchasing power be circumscribed to a specific community or area? I

demonstrate that such limitations are actually inefficient and unjust. They are inefficient because they limit drastically the possibilities of monetary policy while the goals they aimed at may be reached more efficiently by other means, such as redistributive policies. And these limitations would happen at the expense of justice. They focus on the wrong means – money - which is one of the primary vehicles of distributive justice. "Income and Wealth, Rawls (1982, p. 166) writes, are all-purpose means for achieving directly or indirectly a wide range of ends." They are one of the primary goods which must be redistributed according to the principles of justice. Setting geographical or communitarian limits to money may thus be harmful for two reasons. First, it would sharply reduce people opportunities. This would be fine if this was the only and most efficient way to reach greater goals, beneficial to all. But there are many other ways to reach the same goals without impeding money from fulfilling what justice requires. Environmental law and fiscal policy may well take care of environmental issues. Rethinking money is not the only option. Second, such restrictions would greatly hinder egalitarian distributive policies, since different special-purpose currencies would not be able to apply in different domains or areas.

The main conclusion of this study is that even if the goals of those special-purpose currencies (whatever there are) may be relevant and often in line with justice, the chosen means is not the right one. Money is the wrong target: we may perhaps find limits to what money can buy, but not to who may use it neither to whose goods may be bought with it.

"Integrated Assessment Models: How to Use Them Appropriately"

Eugen Pissarskoi
eugen.pissarskoi@fu-berlin.de, *Institute for Ecological Economic Research*

Abstract

Integrated Assessment Models (IAMs) provide a promising tool for the justification of a climate policy goal. By the means of an IAM economists intend to calculate the impact of different climate policies on social welfare and to identify a climate policy that maximizes (intergenerational) social welfare. Thus, IAMs' results are intended to support the descriptive premise in a welfarist argument justifying a certain climate policy goal. It has the following structure:

Normative Premise: Policy makers ought to implement the climate policy that maximizes the (expected value of) intergenerational social welfare.
Descriptive Premise: Climate Policy A maximizes the (expected value of) intergenerational social welfare.
Conclusion: Policy makers ought to implement climate policy A.

The normative premise of the welfarist argument has been controversially debated for a long time (it is supported by e.g. Goodin (1995); Adler and Posner (2006); Posner and Weisbach (2010); criticized by e.g. Sen (1979); Anderson (1993); Sagoff (2004)). In the last years justifications of the descriptive premise have also come under attack. Economists (Pindyck, 2013, 2015; Rosen and Guenther, 2015) as well as philosophers (Betz, 2008; Frisch, 2013) have criticized the IAMs claiming that they neither calculate the impact of climate policies on social welfare nor their expected value. They object that the IAMs' neglect uncertainties and presuppose contested non-

epistemic values and criticize that the results of IAMs pretend a non available precision of our knowledge about the future impacts of climate policy strategies. Pindyck (2013, 3) even warns that the use of IAMs' results in public debates could mislead the public. Therefore the current IAMs are not appropriate for a justification of the descriptive premise in the welfarist argument. This criticism has triggered off a debate about the role of IAMs with regard to the justification of a climate policy. Some authors, I shall call them "IAM-optimists", claim that the shortcomings of the current IAMs can be eliminated by a new generation of IAMs (e.g. based on Agent-Based-Models, c.f. Farmer et al., 2015; Revesz et al., 2014). Others, let call them the IAM-pessimists, doubt that IAMs can be modified in a way that they will reliably calculate the welfare impacts of climate policies (e.g. Pindyck, 2013, 2015). As an alternative, Pindyck suggests to justify the descriptive premise in the welfarist argument by more simple cost-benefit analysis (CBA).

In my paper – although I agree with the criticism directed at the current IAMs – I shall argue that both alternative positions – that of optimists as well as pessimists – are flawed. Firstly, I shall argue that neither the suggestions of the IAM-optimists nor the simple CBA suggested by Pindyck allow to adequately justify the descriptive premise of the utilitarian argument. In order to justify the descriptive premise we need foreknowledge about welfare impacts of different climate policy options or their expected value (i.e. the probabilities for the occurrence of the available options). I shall claim that neither optimists' nor pessimists' suggestions allow us to forecast the impacts of climate policies on social welfare deterministically or probabilistically. Against the optimists I shall object that a new generation of IAMs – although it might overcome some of the weaknesses of the current IAMs – will not allow justifying the descriptive premise. For we lack precise or probabilistic foreknowledge about socio-economic parameters (future GDP development, development of GHG-saving technologies etc.) and these uncertainties will spread upon the results of the modified IAMs. Against the pessimists I shall object that the simple CBA as suggested by Pindyck also requires knowledge about objective probabilities for certain parameters which we do not possess.

I believe that both optimists and pessimists mistakenly presuppose that climate policy can be justified only if we know which climate policy maximizes social welfare or its expected value. I shall argue that there are reasonable justifications for a climate policy even if we bite the bullet of Knightian uncertainty and accept that we know merely possible impacts of climate change on the social welfare. In this case, climate policy has to be justified by a possibilistic argument, i. e. an argument that contains a principle for decisions under Knightian uncertainty. I shall propose a structure for those arguments:

Descriptive premise: Description of the possible consequences of the available acts.
Evaluative premise: Evaluation of the relevant consequences on the basis of a certain value theory.
Decision principle: If a decision maker is under Knightian uncertainty and the value of possible consequences is represented by the matrix V then the decision maker ought to choose the act that is recommended by the choice rule C.
Conclusion: recommendation of a certain act.

This argumentative structure allows that different principles for decisions under uncertainty are applied, e.g. the Core Precautionary Principle (PP) or the Catastrophic PP (Gardiner, 2006; Hartzell-Nichols, 2012), and also risky decision principles (I shall exemplarily suggest one).

Finally, I shall argue that IAMs can contribute relevant knowledge to the elaboration of such possibilistic arguments. Their task would be to calculate possible socio-economic consequences for different climate policies and to verify ranges of possible consequences for climate policy options. In order to do this the structure of the IAMs has to be substantially changed, for their task would

consist in verification of socio-economic possibilities and not in identification of welfare maximizing climate policy options. Thus, I shall defend a middle ground position between IAM-optimists and -pessimists with regard to the role of IAMs: I shall agree with pessimists that IAMs cannot be modified in a way that they will justify the descriptive premise in a standard welfarist argument. But, contrary to the pessimists I shall agree with the optimists that IAMs can contribute relevant knowledge for climate policy decision making; and I shall agree with them that the IAMs should be substantially modified in order to fulfill this task. However, I suggest not to try to develop an IAM that would provide a justification for the descriptive welfarist premise, as the optimists propose, but to develop models integrating climatic and socio-economic systems with the goal to describe the range of possible impacts as complete as possible.

"The War on Drugs as a Cause of Global Democratic Inequality"

Darren Corpe
darren.corpe@gmail.com, *University of Calgary*

Abstract

The Global Commission on Drug Policy has recently issued a statement and analysis calling for an end to the war on drugs. Colombian President and Nobel Peace Prize winner Juan Manuel Santos has also urged governments around the world to rethink the war on drugs. In that light, I have analyzed the harm done to people, society, and democratic equality by the current war on drugs. This harm occurs from direct and indirect sources that single out and persecute vulnerable groups in society, undermining their democratic equality—by which is meant secured basic capabilities that ensure a person can function physically, intellectually, and socially at a basic level. These groups include minority groups like victims of childhood trauma who turn to street drugs and the often desperately poor who turn to producing, transporting, and selling them. The war on drugs has, thus, turned out to be an affront against the democratic equality of vulnerable groups in society in a number of ways.

Directly, the war on drugs undermines the democratic equality of many people because widespread harm is done to drug producers, transporters, and sellers who are persecuted, shot at, chased down, incarcerated, and bullied. This is all done for the sake of supposedly protecting society by reducing the number of drug users. However, the war on drugs can be shown to increase the number of drug users as well as the harm done by drug use rather than to avert these harms.

Democratic equality is further undermined, indirectly, by the messages governments send when they endorse the war on drugs. The war on drugs leads people to believe that the lives of citizens and denizens are more important than the lives of people living abroad. In attempting to protect individuals at home through reducing the number of drug users and the amount of harmful drugs available, the wellbeing of countless millions abroad in drug producing and trafficking states is compromised. Endorsing this policy indicates to the public that the democratic equality of those people affected abroad is less important than the democratic equality of local people who are supposedly protected at their expense. In this way, the war on drugs affirms a civil theory of justice, i.e., that justice only exists under a shared civil authority (e.g., Thomas Nagel), at the expense of

more cosmopolitan theories of justice, which hold, for example, that justice means upholding pre-political, universal human rights (e.g., Martha Nussbaum).

There are essentially six loci of potential harm in the operation of any drug market: growing (1), processing (2), transportation (3), markets (4), possession (5), and consumption (6). The production, transportation, and sale of licit drugs, i.e. the domain of the first four loci, does not appear to be (by, I think, a reasonable hypothesis) any more dangerous than the production, transportation, and sale of other licit goods (transportation of drugs may even be less dangerous than, say, the transportation of liquid nitrogen or gasoline). The production, transportation, and sale of illicit drugs, on the other hand, is the cause of substantial inherent harm to persons—like gang violence, cartel violence, murders, robbery, police standoffs, and so forth. Decriminalizing (currently) illicit drugs would, therefore, entirely wipe out all of the abnormal harm found within the first four loci of potential harm for markets in (currently) illicit drugs. Very few, if any, government policies have such a positive hypothetical outcome.

Nevertheless, following a civil justice perspective, a government's analysis of harm (potentially) would not give much weight to the impact of decriminalization on these loci, since most of the people in these loci are not citizens (for example, they may live and operate in Columbia or Mexico and so will not factor into the protective concerns of the US government). Humanitarian sympathies alone can go out to these people, but not concerns about justice. This analysis would differ crucially if a government were to follow a cosmopolitan conception of justice. In that case, the total reduction of harm in the first four loci would play a substantial role in their analysis of how (or whether) to intervene and form a decriminalized and regulated market. Furthermore, by ignoring this enormous potential to reduce the harm done by government policies, a government will be sending a harmful message to individuals and societies about the acceptability of global democratic inequality.

In a hypothetical decriminalized and regulated system, the only potential abnormal harm would be found within the final two loci of potential harm, the possession and use of drugs like cocaine and heroin. The possession and use of cocaine and heroin would arguably be less harmful if markets in those drugs were decriminalized in some way. The statistics and other data backing this claim will be presented in the paper. By disregarding the prospect of a decriminalized system and continuing to treat users of (currently) illicit drugs in ways that endanger their lives, wellbeing, and ability to function as democratic equals, governments are sending a range of harm provoking messages that there is no need to secure democratic equality for drug users. This is similar to the bad message sent by governments when they affirm that there is no need to secure global democratic equality for individuals around the globe whose lives have been destroyed by the first 4 loci of potential harm due to the current war on drugs.
My analysis of the domestic harm done by the war on drugs utilizes statistics from peer reviewed journals, government statistics, newspaper articles from Canada and the US, and the data presented in a recent book by Johann Hari called Chasing the Scream: The First and Last Days of the War on Drugs.

How do Economic Models Explain?
(Fri 13:30-15:00, Room M1-09: Zaal Bergen)

"Interventions in Economy: When Causes make differences"

Ahmet Dincer Cevik
dincorcevik@gmail.com, *Mugla Sitki Kocman University*

Abstract

The nature and the status of model theoretical explanations in economics is a hot debate. It seems that in economics models are developed for the purpose of explaining particular problems and that no explanatory model can be universally applied at all times and at all places. It seems that such models do not have the status of, say, Hardy-Weinberg Law or Fisher's fundamental theorem in population genetics. I suggest that this can be due to the fact that whereas idealized conditions in the antecedent of Hardy-Weinberg Law and Fisher's Fundamental Theorem allow them to be applied more generally, idealized conditions in economic models are more sensitive to contingencies. Of course, there is a difference between such models in economics and biology on the one hand and the physical laws on the other. However, if I am right, there seems to be a further difference between mathematical models in biology and mathematical models in economics. This part of my research is still quite in its infancy.

A widespread intuition is that good explanations require citing laws of nature or lawlike generalizations. But some authors (Rosenberg, 1994) hold the view that even if there are generalizations in the special sciences, these generalizations are insufficiently lawlike to be able to feature in good scientific explanations. Thus, it seems that the special sciences do not provide successful scientific explanations and, consequently, should not be thought of as being "genuine" sciences. There is a clear need to deepen the existing philosophical analyses of these issues in order to achieve more clarity about the explanatory potential of the special sciences and – on that basis – about their scientific status before jumping to any premature conclusions. I will approach this problem complex by focusing on how generalizations are being used in explanatory practices in economics.

Interventionists often use examples from the social sciences, especially from economics, to show the adequacy of their theories. On the basis of the following argument I will try to deal with the issue of whether the interventionist model of causal explanation can explain the causal nature of explanations in economics:

1. Interventionism requires active counterfactuals the antecedents of which are made true by a special sort of exogenous process (intervention).
2. Restricted generalizations such as Philips Curve and Okun's Law work some time period as a causal explanation of a phenomena in question but fail outside of this time period.
3. If what we say in 3 is true, then it is not clear how such generalizations can support counterfactuals. For example, even if we hold all the other facts fixed, we cannot say that if inflation were higher, then unemployment would have been lower than it is. The reason is that after 1970s Philips Curve has been disconfirmed. However, this disconfirmation does not tell us that past explanations based on it were bad ones to start with.

4. 4. If such restricted generalizations do not support counterfactuals in the right way, then they cannot be invariant. Or this will raise questions about how invariant a generalization must be to provide a causal explanation.

"To what extent Economic Explanations are distinctively Mathematical?"

Lukasz Hardt
lhardt@wne.uw.edu.pl, *University of Warsaw*

Abstract

Some philosophers argue that many explanations in science are distinctively mathematical (e.g., Lange 2013, 'What Makes a Scientific Explanation Distinctively Mathematical?', BJPS). They do their work quite similarly like symmetry principles do in explaining: by limiting the set of events that could emerge. Or, in Lange's terms, they explain by appealing to mathematical necessity. Also, they can do their job while not using laws of nature as well as they can explain without citing causes of explanandum. Thus it is worth checking whether distinctively mathematical explanations are present in special sciences, including economics. The goal of my paper is to check to what extent economic explanations are distinctively mathematical. In doing so I am to focus also on the problem of distinguishing distinctively mathematical explanations from non-causal explanations referring to some mathematical facts. Since economics is to a large extent a modelling science, I will check how distinctively mathematical explanations do their work in economic models. The rationale for focusing on models, abstract entities isolating some aspects of their targets, in investigating the role of distinctively mathematical explanations comes from the fact that what makes these explanations non-causal is that they "ignores (and requires that one ignores) various physical details about the system of interest and appeals to a particular abstract structure of the physical system" (Batterman 2010, 3, 'On the Explanatory Role of Mathematics in Empirical Science', BJPS). So, one may find similarities in modelling economic phenomena and explaining them using distinctively mathematical explanations. Such explanations provide us with understanding of economic phenomena if appealing to laws and causal structure of the world is impossible.

“How possibly could how-possibly explanations explain?”

Philippe Verrault-Julien
verreaultjulien@fwb.eur.nl, *EIPE-Erasmus University Rotterdam*

Abstract

One puzzle, or even paradox (Reiss 2012), concerning highly idealised economic models is whether and under what conditions they can explain the world. Models misrepresent reality, and yet appear to be explanatory despite the fact that our best theories of scientific explanation require faithful representation (e.g. Woodward 2003; Strevens 2008). For some commentators, one way out of this conundrum is to view these models as providing 'how-possibly explanations' (HPEs) (e.g. Grüne-Yanoff 2013a, 2013b; Ylikoski and Aydinonat 2014). They contrast HPEs to 'how-actually explanations' (HAEs), the latter being empirically confirmed whereas the former are not, or at least to a lesser degree. However, there is a disagreement on how viewing economic models as HPEs actually solves the paradox. Indeed, whereas someone like Grüne-Yanoff holds that HPEs simply serve a different explanatory purpose and do not faithfully represent the world, Ylikoski and Aydinonat claim that HPEs afford, like HAEs, understanding of the world by representing faithfully certain relations of dependence. The former account solves the paradox by denying that economic models explain in the usual sense while the latter does it by showing how HPEs achieve faithful representation. Regarding economic models as HPEs therefore does not straightforwardly solve the paradox. Rather, it raises a host of questions about the nature of HPEs. For instance, are HPEs a species of explanation? Do HPEs explain? Are HPEs explanatory, and if so how? If they do not explain, what sort of epistemic contribution do they make? Unfortunately, accounts of HPEs already on offer do not clearly answer these questions.

My paper purports to provide an account of HPEs that clarifies the nature of HPEs in the context of solving the puzzle of model-based explanation. How possibly could how-possibly explanations explain? The short answer is that they can’t, at least not following extant accounts. Indeed, an explanation-seeking why-question is one that demands knowledge of actuality. According to prevailing philosophical theories, to explain requires to give a faithful account of both the explanandum—e.g., a phenomenon of interest—and of the explanans—e.g., the causes of the phenomenon. More precisely, to explain requires to provide knowledge of the actual explanandum and explanans. HPEs do not provide such knowledge. However, viewing HPEs as being basically false HAEs obscures the fact that HPEs in fact *do* provide knowledge, just of a different sort. As how HPEs and HAEs are called suggest, I submit the modal notions of 'actuality' and 'possibility' provide the relevant dividing lines between HPEs and HAEs. The crucial feature that distinguishes HAEs from HPEs is not the empirical truth of the former and the falsehood of the latter, but instead the sort of knowledge they provide. Whereas HAEs provide knowledge of *actual* explanantia and explananda, HPEs provide knowledge of *possible* explanantia or explananda. For instance, widely discussed (see e.g. Sugden 2000, 2009; Aydinonat 2007; Grüne-Yanoff 2013b) models such as Banerjee's (1992) model of herb behaviour or Schelling's (1971, 1978) model of residential segregation do not provide knowledge about the actual causes of their targeted phenomenon. Rather, they provide knowledge of a possible mechanism. Models justify the beliefs we have concerning certain possibility claims. This suggests that HPEs are indeed a species of explanation in that they justify beliefs we have about possibility claims. Insofar as HPEs are 'explanatory', they are only explanatory with respect to possibility, not actuality.

This characterisation sheds light on the contrasting views about HPEs in two different ways. Firstly, it implies that it is in a sense right to deny that HPEs faithfully represent (e.g. Grüne-Yanoff

2013b). Indeed, HPEs do not faithfully represent the actual explananda and explanantia, only possibilities. It is thus correct and valuable to deny they represent in that sense. However, it is also right to consider that they faithfully represent (e.g. Ylikoski and Aydinonat 2014), as long as the domain is restricted to possibilities. This suggests that unqualified claims of (un)faithful representation can be misleading and that we better be clear about what exactly is represented. Secondly, it allows to clarify the exact epistemic contribution of models. Some models provide knowledge of actuality, others of (im)possibility about a given explanandum or explanans. This implies that models that do not offer HAEs are not necessarily epistemically vacuous, as is sometimes argued (e.g. Hausman 1992; Alexandrova 2008). Such models can be HPEs and thus contribute to our knowledge, albeit differently. It also implies that depending on the knowledge provided by a given HPE, it may, for instance, contribute to learning (e.g. Grüne-Yanoff 2009, 2013b) or understanding (Ylikoski and Aydinonat 2014).

My paper makes two main contributions. Firstly, it contributes to the literature on economic models by providing a framework with which we can appraise both our philosophical views about economic models and the contribution of models themselves. One important puzzle about economic models is whether or not they 'explain' or are 'explanatory'. A precise account of HPEs is one step in the direction of solving this puzzle. Secondly, it contributes more generally to the literature on HPEs by proposing a novel account that does not rely on the degree of empirical confirmation (see e.g. Hempel 1965; Brandon 1990; Bokulich 2014) as the criterion of demarcation between HPEs and HAEs. My proposal is instead to use modality, i.e. actuality and possibility, to distinguish these two types of explanations.

"Fuzzy Cardinality: A Case Study on how Neuroscience can inform Economics, and how Economics can inform Neuroscience"

Thomas Mueller
thomasvismara@hotmail.com, *UCL*

Abstract

Neuroeconomics and more specifically neuroutility measurements have adressed in recent years a set of methodological and philosophical criticisms, about the meaning, the (technical) feasibility and the possible economic impact of neuroeconomics (Camerer, Loewenstein, and Prelec 2005; Harrison 2008; Fumagalli 2013; Fumagalli 2015; Fumagalli 2016; Kuorikoski and Marchionni 2016).

In particular, Fumagalli (2013) raises a set of criticisms concerning "neuroutility", a "signal that can be accurately measured in the activation patterns of specific neural areas (p. 326)". We may summarize those criticisms as follows:
- How is it possible to link neuroutility with economic choice?
- Does neural utility allow interpersonal comparisons?
- How may neuroutility impact on the standard foundations of economic theory?
- how exactly these empirical investigations are supposed to foster the replacement of a mathematical construct such as decision utility?

My purpose will be to answer to these criticisms by a specific case study:

In recent years, several authors coming from the neuroeconomic field (Zizzo 2002; Stuphorn 2006; Padoa-Schioppa and Assad 2006; Webb et al. 2016), have claimed that the brain may encode utility cardinally, and that this neuroutility could be measured, and applied to economical reasoning.

I will analyse the content of their most recent results, and provide a possible methodological interpretation of those results in terms of choice axioms. I will focus on the internal logic and meaning of those findings, and on a possible interpretation of neuroutility measurement in terms of decision theory, and thus economic theory.
My assertion is the following: despite what (Webb et al. 2016) claim, neuroutility measurements are not restating cardinal utility, and therefore neuroeconomics is not helping in solving a theoretical economic problem by providing a decisive empirical test in a Popperian sense. Nonetheless, I think that part of Fumagalli's criticisms may find an answer: what neuroutility, and more specifically Webb and al. allow is a definition of a new measurement scale, and therefore of a new concept, which is neither cardinal nor ordinal utility. Such a new concept requires a logical interpretation of the empirical findings and the definition of completely new concepts.
Let us sketch briefly the most relevant findings, and their methodological meaning: brain scanners allow to measure brain activity in the presence of a set of given stand-alone goods, and associate each good to a numerical value.

When a subject has later to choose between pairs of goods, the numerical ranking is predictive on the choice: the larger the difference in numerical value he associated with the pairs of goods, the higher the chance to prefer one good to the other.

While this suggests that the numerical value, and not only the ranking order are relevant to the choice, it also implies systematic violation of standard rationality axioms. Whatever the numerical values, there is a chance of choosing the less "neurouseful" good. As we will show, the very fact of rationality failure is (counterintuitive) crucial to the claim that cardinal and not only ordinal ranking matters.

Based on the empirical findings, we will advance a proposal for a new set of "fuzzy rationality". Fuzzy rationality will be compatible with the main claim of cardinal measurement (that values matter) but also with the main ordinal appeal (that "twice as useful" is a meaningless statement).

The final result of my case study will not be to link neuroscience and economics (in a reductionist sense), but would be the replacement of some economic concepts with new ones. It will thus provide an example of how neuroutility may impact on the foundations of economic theory, and mainly on his mathematical foundation. While saving the virtues and the desiderata of both cardinal and ordinal utility measurement, fuzzy utility measurement is a new concept that is both based on neuroscientific findings and on theoretical economics. While this case study will remain on a quite simple, and arguable basis, it provides new light on the possible mutual enrichment of neuroscience and economics.

"The Relevance of 'Free Will' for Economic Theory: Uncertainty, Intentionality and Economic Behaviour"

Joachim Guntzel
guentzel@dhbw-ravensburg.de, *Baden-Württemberg Cooperative State University, Ravensburg*

Abstract

"Free will" is a theme which is seldom – if at all – tackled in economic theory, since it seems to be regarded by economists as a purely philosophical topic. Within the standard model of neoclassical mainstream economics, "homo oeconomicus" simply appears to be a maximizing unit, lacking any kind of free will (and, after all, not even needing such a free will). On the other side, the central role that uncertainty plays in macroeconomics, especially of a Keynesian type, is widely recognized today. Keynes hereby understood uncertainty as a fundamental characteristic of an unforeseeable reality which cannot be managed by any method of probability calculus. This approach to economic behavior gives way to think about the role that the philosophy of mind – mainly the concept of intentionality – can play for the explanation of economic behavior. (see Güntzel, 2016)

Against this background, the proposed paper poses – and tries to answer – the following two questions:

1) How can Keynes´ concept of probability and uncertainty be connected with a more general philosophical foundation within the philosophy of mind (esp. the concept of intentionality)?

2) How can “free will” be given more space with regard to economic behavior, and how can it be seen as an integral part of a picture of man beyond “homo oeconomicus”?
Following these research questions, the paper divides into three parts: Part one presents an approach to integrate the philosophical concept of intentionality (in the specification of John Searle) into Keynes´ conception of probability and uncertainty. Here, the specific characteristics of Keynes´ analysis of probability, especially the impossibility of any kind of probability calculus in many every-day situations, have to be taken into account. The paper therefor enhances and clarifies an approach which has been presented in Güntzel (2013)

The second part concentrates on the problem of “free will” within economic theory (for example, in the context of economic ethics) and the fundamental problems that arise when free will is considered as a factor to be taken serious in economics. In part three, finally, the conceptual approach from part one is enhanced and it is asked if this approach can be utilized to serve as a basis for the integration of “free will” into theories of economic behavior.

The integration of man being an entity with a free will, however, contains a number of difficult problems, one of them concerning the nature of “economic laws” facing the existance of such a free will. These problems have to be taken into account seriously, in order to raise the chance of acceptance from the side of economic theory.

The goal of this paper, thus, is to make a contribution to a better philosophical foundation of economic theory, especially with respect to the more realistic portrayal of man as a “mental being” within economic reasoning.

“Cognitivism in Behavioural Decision Research: A case for re-evaluating the dual-systems approach”

James Grayot
james.grayot@gmail.com, *EIPE-Erasmus University Rotterdam*

Abstract

Behavioral decision research in economics and psychology is problematically out of touch with debates in philosophy. Here “behavioral decision research” is short-hand for theories of bounded rationality that have developed out of the Heuristics and Biases program of Tversky and Kahneman (1974; cf. Kahneman & Tversky 1979; Kahneman, Slovic & Tversky 1982). From this program one cognitive framework has gained considerable popularity for explaining decision anomalies: the dual-systems approach posits that human behavior results from the interplay of conscious and unconscious processes involving associative memory, implicit knowledge, mood, latent emotions, and a host of other cognitive and affective mechanisms. Decisions can thus be understood based on whether they are generated by fast and automatic responses (“System 1” processes) or slow and deliberative judgments (“System 2” processes).

This paper argues that proponents of the dual-systems approach have missed out on important debates in philosophy regarding the nature of cognition. In particular, it is uncertain what counts as a cognitive process for many dual-system theories, which is to say, there is no set of principles or criteria to distinguish the cognitive from the non-cognitive. To illustrate why this lack of criteria is problematic I will draw upon an existing debate from the philosophy of cognitive science; this debate identifies two points of contention for cognitivist theories of mind and action.

Cognitivism is the philosophical thesis that what counts as a cognitive process is contained within the brain or central nervous system (intracranial cognition), and processes that occur beyond the brain-CNS barrier (transcranial cognition) are mostly irrelevant for understanding how individuals perceive, judge, and navigate the social world. Although philosophers have long debated whether the brain is the seat of consciousness – prompting 'brain in a vat' type thought experiments – only recently have they recognized how deeply entrenched the cognitivist thesis is in the special sciences. Philosophers' grievances against cognitivism can be organized into two sets of criticisms:

The first set of criticisms pertain to how the cognitivist interprets mental representation and how the content of representation, i.e. thoughts and propositional attitudes, acquire their semantic value. The only way to account for intelligence and intentionality, critics argue, is to hypothesize that cognition consists in organisms acting upon representations that are physically realized in the form of a symbolic code in the brain (Varela, Thompson, & Rosch 1993). This picture of representation as the outcome of a computation is riddled with theoretical obstacles. For instance, if cognition reduces to a series of computations, then we should expect there to be rules to govern how we experience the content of thought, i.e. a representational syntax. But, if there exists such a syntax, it would have to operate behind the veil of experience otherwise it could not serve its function as a rule. This issue was first raised by Dennett (1978, 1991) who suggested that if sub-personal processes run the show, then there are at least some mental processes that can't be known by introspection. As such, if representations issue from computations encoded symbolically in the brain, then not only do cognitivists have to account for how semantic content supervenes on a syntactic substrate (the classic "mind-body" problem), but they are further pressed to distinguish between types of non-physical mental representations, i.e. between conscious (experienced) thought and sub-personal (computational) thought. Jackendoff coined this the "mind-mind" problem to convey that it is equally as perplexing as the mind-body problem (1987, 1989).

The second criticism of cognitivism is that it fails to account for reasoning processes that extend beyond the individually embodied brain. Many insights from philosophical psychology and cognitive anthropology suggest that cognition is distributed across social and institutional structures. Authors refer to this as cognitive scaffolding and they argue that human reasoning, even basic social cognition, necessitates the use of external artifacts. These artifacts range from mnemonic tools and implements (cf. Clark & Chalmers 1998, Clark 1998, Haugeland 1998) to ecological niches and social networks (cf. Hutchins 1995; Clark 2003, 2006; Sterelny 2003; Ross 2006, 2007; Zawidzki 2013). This means that if we want to understand how perceptions are converted into meaningful action, researchers need study more than the functions of the brain and intracranial nervous system; they need to take an ecological approach to human intelligence, which means investigating how external information structures enable humans to acquire and attach semantic values to their actions (Clark 2007, 2008; Wilson 2004, 2005).

How do debates about cognitivism in philosophy impact behavioral decision research? First off, if the dual-system approach was developed as a cognitive framework for individual decision-making, it may neglect important insights regarding the development of critical reasoning faculties that are socially embedded and cognitively reinforced via intersubjective agreement, e.g., language learning and concept formation. Second, even if dual-system theorists are uninterested in the socio-cognitive

foundations of critical reasoning faculties, their understanding of mental representation is problematically opaque. In many cases it's not clear what the decision theorist means by "representation" and this is partly because they use the term to refer to all sorts of mental activities, from basic sense perceptions and memories to complex number manipulations and counter-factual beliefs.

So why do representations matter? Much in the same way that there are no principles for determining what counts as a cognitive process, if there are also no well-defined criteria for defining processes of mental representation, then it's entirely unclear what causes a System to become engaged when decisions are being made. Although the consensus view is that the evolutionarily old System 1 is always active and the newer system 2 intervenes when it is triggered, it varies from theory to theory what prompts the engagement of System 2. Thus, by failing to differentiate the cognitive from the non-cognitive, not only must the dual-system theorist account for sub-personal "computational" representations that are not a part of conscious experience, but they must also account for a litany of semi-conscious processes that fall in between automatic responses and deliberative judgments. By consequence, the majority of dual-system theories rely on post-hoc explanations to justify anomalous choice behavior.

"Welfarism and Welfare Economics"

Jean-Sebastien Gharbi and Yves Meinard
jean-sebastien.gharbi@univ-reims.fr, *University of Reims Champagne-Ardenne*;
yves.meinard@dauphine.fr, *University Paris-Dauphine*

Abstract

"Welfare economics" is so common an expression in economics that it is very rarely considered as a topic for discussion. One talks about "welfare economics" but not about the meaning of this phrase. Welfare economics is a branch of microeconomics dedicated to study collective (i.e. social) wellbeing (understood in terms of welfare). From this preliminary (and, of course, possibly disputable) definition, one can easily infer that all the classical microeconomics, from issues related to market efficiency to those of social justice and from public economics to social choice theory, fall into welfare economics. Referring to the classical distinction coined by John Neville Keynes between "positive" and "normative economics" (Keynes, 1890), it is commonplace to consider that "welfare economics" is the thematic subfield of economics studies dedicated to normative issues (Boadway & Bruce, 1984; Feldman & Serrano, 2005).

The normative dimension of welfare economics has been largely accepted, to the point that that this expression ended-up being used in the literature as a perfect synonym for "normative economics" (Graaff, 1957) and that it is not uncommon, until nowadays, to find the phrase "welfare economics" used to mean "normative economics". This habit was so strong that Arrow's impossibility theorem (Arrow, 1951), which led to the announcement of the death of welfare economics, was generally understood as the announcement of the death of normative economics. Similarly the rebirth of welfare economics in the early 1970s has also contributed to the renewal of normative economics, although this renewal happened at a time when alternative approaches to welfare economics (Rawls, 1971; Sen, 1980) were proposed. This last point highlights that identifying welfare economics with normative economics was not self-evident.

The emergence of alternatives to welfarist accounts in normative economics points out that welfare economics had during several decades made a lot of choices and assumptions that can be questioned. Since then, "normative economics" has been the field of confrontation between welfarist and non-welfarist approaches. Although there undoubtedly are non-welfarist approaches in contemporary normative economics, it is worth emphasizing that welfarist approach is still today, by far, the main approach used in economics to deal with normative issues.

This contribution aims at explaining the consequences of the clarification, launched and largely developed by Sen (1977, 1979), that there is a specific doctrinal content (namely welfarism) hidden behind the approaches to normative economic issues that claim to be purely methodological and technical. Indeed, this clarification deeply influenced the representation of welfare economics itself. Our focus is not the history of economic thought; we rather aim at providing a conceptual

explanation of the contemporary (and implicit) debate about the uses of the expressions "welfarism" and "welfare economics". This enquiry requires tackling whether normative economic issues can be addressed through purely positive approaches, as several authors claim (for instance, Fleurbaey, 1996). Consequently, it implies an assessment of the meaning and scope acknowledged to the conclusions of welfare economics.

The paper is organized as follows. In sections 2 to 4, we focus on the various definitions of welfarism and its variants and on delineating the relation and differences between them: section 2 presents Sen's famous and authoritative definition of welfarism (Sen, 1977, 1979); section 3 explains and discusses the already classical, but nevertheless quite recent, distinction between real and formal welfarism (Fleurbaey, 2003; Mongin & d'Aspremont, 1998; Rechenauer, 2003); section 4 is dedicated to another, and a bit less usual in literature, distinction between weak and strong welfarism (Fleurbaey, 1996).

From all these elements, section 5 identify how the recent evolution of welfarism has influenced the representation of what welfare economics is. We notably pay a specific attention to the debate about the extension of welfare economics: is welfare economics a subfield of economics studies or is it a candidate to encompass all economic studies? Section 6 is dedicated to the debate about the normative dimension of welfare economics and its theoretical implications. In other words in section 6 we straightforwardly ask if welfare economics relies on positivist grounds (Friedman, 1953). Section 7 is about the ethical underpinnings lying within formal welfarism (i.e. welfarism described as involving no ethical assumption). Section 8 briefly concludes.

"A Tale of Two Paradoxes in the Economics of Happiness and Income"

Andrè van Hoorn and Esther-Mirjam Sent
a.vanhoorn@fm.ru.nl, e.m.sent@fm.ru.nl; *Radboud University*

Abstract

The Easterlin paradox has come to be associated with the puzzling findings concerning national time-series evidence on happiness and per-capita income. These involve the observation that in several countries real income has risen but that, at the same time, subjective well-being (happiness) of the population has not increased or has even fallen slightly (Easterlin 1974). This observation is considered a paradox, for it is not in line with conventional economic thinking (Frey and Stutzer 2002; Van Hoorn and Sent 2016). Subsequently, much effort has been put into developing alternative theoretical explanations involving, in particular, hedonic adaptation (Helson 1964) and social status concerns (Festinger 1954). However, when introduced by Richard Easterlin himself, the paradox had a different interpretation. In this original interpretation the paradox referred to contradictory empirical findings on the relationship between income and happiness at the individual level and at the societal level, involving a fallacy of composition (Easterlin 1973, 1974). In other words, the interpretation of the Easterlin paradox has shifted from contradictory empirical relationships to an applied theory perspective.

We relate this shift to the more general turn towards applied theory that has occurred in economics as a response to aggregation problems encountered in the 1960s and 1970s. In particular, these

decades witnessed two main aggregation questions. The first question was whether there exist functional relationships among macroquantities obtained by aggregating relevant microquantities. This question featured prominently in the Cambridge controversies (Harcourt 1971). The second question was whether the functions obtained by aggregating microfunctions are the same as the macrofunctions derived independently. Here, the Sonnenschein-Debreu-Mantel result revealed that the standard micro model has almost no implications for macrobehavior (Rizvi 1994, 2006). From the 1980s onwards, partly in response to these two questions, economics took a turn towards applied theory. Questions concerning composition and aggregation were abandoned in favor of theoretical models dealing with specific (policy) issues (public, labor, urban, environmental) and corresponding empirical validation techniques.

The applied theory turn in economics was related to the availability of new statistical techniques, new data, and computerization as supply side factors. In addition, it may be connected to new patrons, new policy regimes, and new business demands on the demand side. As the take on the Easterlin paradox shifted alongside. what remained was a happiness war (Easterly 2011), characterized by heated exchanges among proponents of different applied theory perspectives.

With the applied theory focus on the present interpretation of the Easterlin paradox, core theoretical questions involving income as a key concept in economics as well as the mechanism through which income should have an effect on happiness remain unaddressed. Well before the emergence of aggregation problems in the 1960s and 1970s, some of the most influential economists have devoted much effort to elaborating the concept of income and its relation to welfare (e.g., Fisher 1930 [1906]; Friedman 1957; Pigou 1920 [1932]). In the raging income-happiness literature, however, such discussion of fundamental conceptual issues is absent. Researchers have succeeded in developing highly sophisticated ways of incorporating adaptation-level and social comparison theories (Festinger 1954; Helson 1964) in income-happiness research, using ever-increasing numbers of observations and control variables. In contrast, we conclude that the only promising future turn for happiness research involves a return to the original interpretation of the Easterlin paradox.

“Wicksell and Pareto on Distribution, Efficiency, and Well Being”

Marianne Johnson
johnsonm@uwosh.edu, *University of Wisconsin Oshkosk*

Abstract

No individual in the history of public economics has been subject to more contentious discussion than Knut Wicksell – and perhaps no concept subjected to more diverse interpretation than his unanimity rule. The story begins in 1896 with the publication of Wicksell’s public finance treatise, Finanztheoretische Untersuchungen, and starts to take shape in 1906 [1971] with the publication of Vilfredo Pareto’s Manual of Political Economy. Contemporaries who had much in common, Wicksell and Pareto were unable to come to a meeting of the minds on a number of fundamental theoretical issues relating to distributive justice and the nature and meaning of efficiency. What makes the Wicksell-Pareto discussion particularly interesting is the degree to which their ‘feud’ has continued in modern welfare economics as well as in public economics. In the latter case, the feud

is represented in the split between the public choice tradition of James Buchanan and Richard Musgrave's triparate approach of allocation, distribution, and stabilization.

The crux of the matter hinges on the relationship between Wicksell's unanimity rule and Pareto optimality. Buchanan declared equivalence between the two ideas; Musgrave vehemently disagreed. The purported correspondence of has been subject to much debate (Buchanan and Musgrave 1999; Head 1974; Johnson 2010; Medema 2005; Musgrave 1959; Silvestre 2003; Syll 1993; Wagner 1988; Uhr 1962). Some, like Pieter Hennipman, fail to understand why Wicksell did not "recognize that his doctrine and Pareto's were essentially alike" (1982, 62) though confessing that Wicksell and Pareto "would presumably have been astonished had they foreseen the future linking of their names" (1982, 47). Others, such as Carl Uhr (1962) and John G. Head (1974) see the relationship between Wicksellian unanimity and Pareto efficiency as hinging on the relative importance assigned to the various antecedent assumptions and requirements imposed by each. Head concludes that Wicksell showed "an intense awareness of the allocative and distributional problems of the Pareto criterion" (1974, 33), and it was the distributional requirement that otherwise distinguishes between Wicksell and Lindahl's "essentially Paretian theories of the budget"(1974,14).

Economists have thus viewed the requirement for a just distribution of income laid down by Wicksell variously. While, Hansjurgens dismisses this statement as "one sentence (!) in the whole book" (2000, 102), others feel the prerequisite was of crucial importance in understanding Wicksell (Musgrave 1959, Uhr 1962). Buchanan has largely ignored the requirement, choosing to focus on the efficiency aspects of Wicksell's claims. Hennipman concluded that Wicksell's "preoccupation with the problem of justice does not obliterate the analytical congruence" with Pareto efficiency (1982, 60). Yet, failure to consider distributional issues means "the economist qua economist cannot legitimately or usefully pronounce upon questions of income distribution" (Head 1974, 13). Musgrave pointedly claimed that the "Wicksellians who feature the allocation part of his system while overlooking his premise of just distribution thus misinterpret his message" (Musgrave 1999, 43)

This paper revisits Wicksell's one-sided debate with Pareto and considers the trajectory of ideas that led to an eventual convergence in public choice. The main goals of this paper are as follows. (1) To clarify Wicksell's reading of Pareto's welfare economics, including Wicksell's position on the first welfare theorem and the competitive market model. (2) To frame Wicksell's criticisms within the context of his economic world view, his political activism, and turn of the century economics. And (3), to consider the problem of allocation versus distribution within the models of Wicksell and Pareto.