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BOOK OF ABSTRACTS

*The Relationship Between Normative Economics and Positive Economics
in Friedman, Mises, Marx and Popper*

The point of departure of this proposal is the definition of normative and positive economics as put forward by John Neville Keynes in his book *The Scope and Method of Political Economy* and popularized by Milton Friedman in his essay *The Methodology of Positive Economics*. The first part of this proposal aims at analysing three different ways of thinking about the relationship between positive and normative economics in the works of Karl Marx, Ludwig von Mises and Milton Friedman. It will be shown that while the positive economic theories of these three economists are very different from each other, all of them conceive the relationship between positive and normative economics in a very similar manner.

For Friedman, normative economics depends on positive economics because “any policy conclusion rests on a prediction about the consequence of doing one thing rather than another, a prediction that must be based – implicitly or explicitly – on positive economics”. This leads him to say that when it comes to disagreements regarding new economic policies that we want to implement, it is only the progress of positive economics that can let us settle them. This is why he thinks that positive economics should constitute the fundamental part of the economics discipline.

For Mises, it is the positive scientific analysis of the a-priori structure of human action that can help us to come up with different propositions –albeit mostly negative ones–, regarding what can be done and what ought to be done in the economic sphere. Mises thinks that without taking the teachings of the formal analysis of human action into consideration, new normative proposals for changing the economic situation would either be futile or lead to catastrophic outcomes.

For Marx, the biggest problem of what he considers as utopian and normative ways of thinking about socialism or communism resides in their lack of understanding of the structural features of what Marx calls the capitalist economic formation. Marx thinks that appropriate economic proposals regarding what ought to be done in opposition to capitalism can only emerge dialectically from within a scientific analysis of the existing economic reality.

It can therefore be argued that Marx, Mises and Friedman all concur with the idea that the positive representation of the existing economic reality should constitute the heart of economics, and they all subordinate normative economics and creative proposals for the future to the analysis of the existing economic situation. The animosity of both Marx and Mises towards what they call positivism has often prevented researchers from grasping the similarity of their understanding of the relationship between positive and normative economics to that of Friedman. It is true that they both differ from the latter in that they think that applying the scientific methods used in the natural sciences to economics is problematic. However, they do agree with him that economics, as a science, should mainly focus on an objective representation of the economic reality. In other words, they think,

similar to Friedman, that any attempt at proposing new norms regarding the way economic relationships between individuals ought to be should be subordinated and subjected to the prior formulation of an adequate representation of how economic relationships are.

The second part of this proposal aims at contesting the arguments put forward by all three economists in favour of the centrality of positive economics. It will therefore defend a normative approach to economic issues. To do so, it will rely on the epistemological works of Karl Popper. Accomplishing this task requires that we overcome a major difficulty: elucidating the differences between Popper and Friedman is not easy because in formulating his epistemology, Friedman relied on Popper's doctrine of the unity of scientific method. In order to deal with this obstacle, it will be shown that Popper's espousal of the doctrine of the unity of scientific method, most notably in *Poverty of Historicism* and partly in *The Open Society and its Enemies*, is increasingly abandoned in his later works. Even in *The Open Society and its Enemies*, we find the general contours of a distinctly normative proposition with regard to the epistemology of economics, but its presence in the book is usually occulted by the concomitant presence of Popper's initial epistemological position. It will be shown that Popper maintained these two different, even contradictory, positions because he could not, at the time, make up his mind about the exact nature of the relationship between theoretical and historical social sciences. It was only in two later texts, *The Logic of Social Sciences and Models, Instruments, and Truth*, that he abandoned the distinction between theoretical and historical social sciences, in order to argue in favor of the fundamental role played by history in all social sciences. By taking this development into account, we can come up with a more elaborated account of his second position in *The Open Society and its Enemies*.

I will therefore try to highlight Popper's way of conceiving a normative epistemological model for doing economics, before proposing, through a critical engagement with his work, a modified version of his normative epistemological model based on which if it is true that studying the past can help us to rule out some of the norms that we propose for the future, it cannot help us to conceive these new norms in the first place. This is why economics should focus primarily on the invention of new norms, while using the critical analysis of the past to reject some of these norms and retain others. The main advantage of the reasoning that I propose in defending the importance of using a normative epistemological model in economics is that it does not rely on any ontological or aprioristic arguments, but on purely historical and probabilistic, therefore open-ended, arguments.

Lukas Beck - University of Cambridge

The Econ within or the Econ above?

This paper addresses the debate between Hausman (2016) and Infante, Lecouteaux and Sugden (ILS) on whether behavioral welfare economics (BWE) is committed to an inner rational agent (IRA). I argue that the debate suffers from insufficiently distinguishing between different notions of rationality: procedural-rationality and structural-rationality. Introducing this distinction can shift the debate towards more fruitful questions.

ILS (2016a) argue that BWE is implicitly committed to the assumption of an IRA trapped in a psychological shell which distorts her choice-behaviour. The distortion is assumed to lead to choice-behaviour that is inconsistent with the agent's "true" or purified preferences, i.e. the preferences of the IRA. Consequently, normative economics ought to, first, reconstruct the preferences of the IRA and, second, induce the agent to act in accordance with them.

ILS offer three distinct criticisms of this approach (Hausman 2016, ILS 2016b). First, the methodological criticism that there is no psychological process that would constitute an IRA. Second, the epistemological criticism that it is difficult to reconstruct the preferences of the IRA. Third, the normative criticism that policies should facilitate choices instead of satisfying IRA's preferences. For ILS, the most important criticism is the methodological one. After all, if we deny the existence of an IRA, it is pointless to ask how we can know such an agent's preferences and whether we should aim at satisfying them.

Yet, Hausman (2016) denies that BWE is committed to an IRA. He argues that we can simply ask what the agent would prefer, if she were fully rational. Asking this question, according to Hausman, does not commit us to assuming a psychological process that would constitute an IRA. Instead, we simply rely on evidence about the agent and the world. For example, we could have evidence that the agent is concerned with her health and that fruit is better for one's health than cake. Therefore, we can conclude that the agent would prefer fruit instead of cake if she were fully rational. ILS (2016b), in turn, counter that Hausman's response shows that he is committed to the existence of an IRA.

I aim at furthering this debate by distinguishing between procedural-rationality and structural-rationality. I argue that this distinction can enable us to see how BWE can avoid a commitment to IRAs and thereby points towards more fruitful questions for resolving the debate than the ones raised by ILS's criticisms.

Procedural-rationality ascribes the property of rationality to agents who can execute certain psychological processes (Kacelnik 2007). According to this type of rationality, we cannot simply read off rationality by observing an agent's behaviour or getting information about her attitudes. Instead, we can call the agent rational only if she arrives at her behaviour or attitudes in the correct way, i.e. by reasoning or deliberating correctly. Consequently, in order to be considered procedurally rational one must exhibit a psychological process of the right kind. It is exactly such a process that ILS use to characterize IRAs. For them, IRAs are closely linked to the "capacity to form context-independent subjective judgements on the basis of error-free reasoning" (ILS 2016b). I hold that ILS's usage of the term rationality is generally aligned with procedural-rationality and that this leads to their view that BWE is committed to non-existent psychological processes.

Structural-rationality, on the other hand, is exclusively concerned with the relations between an agent's various attitudes (Broome 1999, 2004, 2013). An agent has the property of structural-rationality iff all her attitudes stand in appropriate relations to each other (Broome 2013). For example, the agent is rational if she has no contradictory beliefs or no intransitive preferences. In principle, an agent can have the property of structural-rationality without being able to execute any particular psychological process like reasoning or deliberating correctly. The agent could simply satisfy the requirements of structural-rationality by default or she could have had help from a friend who pointed out the mismatches between her attitudes and suggested how to resolve them.

I argue that Hausman's and BWE's claims about preference purifications can be reconstrued within the tenets of structural-rationality to avoid ILS's methodological criticism. This is the case because, in contrast to procedural-rationality, structural-rationality is not committed to any psychological process in virtue of which a rational agent is assumed to form her

preferences. Proponents of BWE can spot inappropriate relations between an agent's preferences (i.e. having intransitive preferences resulting from context dependent decision-making) that would prevent one from using these preferences as the basis for maximizing the agent's utility. They could then – based on evidence about the agent and the world – construct a set of rational beliefs and preferences out of the agent's unpurified attitudes. This set could then be used as a basis for utility-maximization. Consequently, structural-rationality allows one to ask what an agent would choose if she were fully rational without committing one to an IRA. Hence, BWE can avoid the commitment to a non-existent psychological process.

Yet, instead of resolving the debate in favour of BWE, distinguishing different types of rationality points towards new normative and epistemological questions: I.) Why should we be interested in the structurally rational preference-set instead of the real attitudes of the agent? II.) How should we arrive at a structurally rational preference-set from unpurified preferences? In particular, BWE must address whether there is a justified procedure sufficiently strong to lead us to a single set of structurally rational preferences and beliefs instead of multiple ones. With respect to I.), BWE can argue that there is (at least in certain circumstances) an evidential-link between utility-maximizing based on structurally rational sets of attitudes and the agents' wellbeing (see Hausman and McPherson 2009). Regarding II.), I argue that it is likely that any sufficiently strong procedure will involve substantial normative assumptions that go beyond requirements of structural-rationality.

First, I introduce the debate between Hausman and ILS. Second, I outline the distinction between procedural-rationality and structural-rationality, Third, I highlight the implications the distinction has for the debate and the questions arising from it. Finally, I suggest how proponents of BWE should address these questions.

Matteo Bianchin - Università di Milano-Bicocca

Explaining Ideology: Mechanisms and Metaphysics

Ideology has been recently reclaimed as a crucial tool to account – under the relevant “pejorative” understanding – for the fact that people accept and enact unjust or however oppressive social practices. On recent readings, however, ideology possesses a number of puzzling features – it looks both true and false, it is taken to play both a legitimating and a constitutive (or even causal) role with respect to such practices, it involves both cognitive and non-cognitive components, it possesses both a descriptive and a normative content (Haslanger 2012, 2017, Stanley 2015, Jaeggi 2009). This adds to traditional concerns about the interlocked explanatory and normative demands placed on a theory of ideology, the unrealistically pervasive irrationality it allegedly credits to agents, the vantage point from which ideology is detected (Heath 2001, Jaeggi 2009, Haslanger 2017, Celikates 2017). Ideology is commonly defined along functional, epistemic, and genetic dimensions (Geuss 1981, Shelby 2003, Celikates 2017). Unless it is specified how they connect and the relevant mechanisms are located to account for how ideologies originate, reproduce, and possibly collapse, talks of ideology may look at best redundant with respect to alternative accounts of the same facts in ordinary terms of strategic interaction (Heath 2001, Sankaran 2019).

I frame the account of ideology along the line of Epstein's distinction between anchoring and grounding relations, and locate ideologies at the level of anchoring mechanisms (Epstein 2015, 2016). I further endorse an etiological reading of functional explanations (Kincaid 1996) and draw on recent work on the epistemology of delusion (Bortolotti 2010, Gunn, Bortolotti 2018), Hacking's looping effects, and structural causal explanations (Haslanger 2016) to specify the mechanisms linking the functional, epistemic, and genetic features of ideology. I will argue that ideology performs the function of undergirding social practices by providing putative reasons to accept the framework principles that set the grounding conditions for the relevant social facts; it results when ordinary cognitive mechanisms responsible for common irrationalities are triggered under structural causal conditions shaped by the practices it supports; it turns embedded into these practices by looping effects that make it enacted by habit and embodied by cultural artefacts to the effect of both making its contents true to the relevant social facts and reinforcing the structural conditions under which the relevant cognitive mechanisms trigger. The epistemology of delusions here connects the function of ideology with its epistemic flaws and structural aetiology, while looping effects back up a functional explanation by providing the relevant feed-back mechanism: ideology persists because of its effects on the social facts it is designed to support.

Locating ideologies at the level of anchoring relations allows distinguishing between the (putative) legitimating function of ideology, the facts that constitute the relevant social practice, and the causal relations responsible for its unjust or oppressive character, thus tackling objections that trade on recent conflated readings (Sankaran 2019). Here the cognitive contents of ideology are both explanatorily and ontologically prior to their "materialization" by looping effect into constituent parts of social practices (Haslanger 2012, Celikates 2017), while looping effects are given a specific explanatory role within a functional account.

Recent epistemology of delusion allows accounting for the epistemic flaws and the structural aetiology of ideology without crediting agents with unrealistically defective cognitive capacities. On this view, delusions are doxastic states that are continuous with ordinary beliefs, result from common distorting mechanisms that occasionally affect every day reasoning, and may play adaptive defensive functions (Bortolotti 2010, Bortolotti, Gunn 2018). Clinical delusions occur when the triggering of such mechanisms is backed by psychopathological structural causes that account for their possible defensive role. I conjecture that ideologies have a similar aetiology: ideological delusions result when the same mechanisms get triggered under social structural causal conditions that account for the social function they perform. The relevant structural causes traditionally relate to power relations and social positions connected with actual social practices, while triggering causes are likely to be associated with a demand for justification, i.e. for reasons to accept the relevant framework principles, arising from the need to handle conflicts without resorting to overt violence.

Focusing on the metaphysics and mechanisms of ideology finally allows addressing the interlocking of normative and explanatory demands placed on the theory of ideology. Ideologies can perform a social function by virtue of their purported normative content because practices are imbued with normative motives (Bicchieri 2017), yet their flawed origins make them unsuited to anchor stable cooperation schemes. This may account for

how they collapse, prompting legitimation crises (Habermas 1988). A functional account of ideology thus relates to normative issues because it hints to counterfactual conditions under which cooperation would stabilize as the conditions of a practice whose framework principles are accepted without ideological coercion. Explaining ideology seems in this sense to entail a normative theory that comes close to a theory of justice designed to articulate such principles as part of a theory of cooperation.

Antonin Broi - Sorbonne Université

An Exploratory Look into the Foundations of Global Prioritization

Global Priorities research has recently been introduced as a new academic field which tries to answer the following question: "What should we do with a given amount of limited resources if our aim is to do the most good?" (Greaves et al., 2019). It finds its inspiration in effective altruism, a movement aiming at "using evidence and reason to figure out how to benefit others as much as possible, and taking action on that basis" (Centre for Effective Altruism's website), and defended by prominent philosophers such as Peter Singer (2015). To pursue this investigation, global priorities research draws heavily on economics and philosophy. Though many of the questions addressed by global priorities research seem to directly fall under well-defined areas of research (for example, issues of coordination among altruistic agents or risk aversion in altruistic choices clearly fall under decision theory), the main objective of global priorities research is carried out through global prioritization, which appears to be a new research program within economics. It deals with the evaluation and ordering of different altruistic opportunities according to how much good they bring about, where altruistic opportunities are commonly understood as various cause areas or problems, such as industrial animal farming or health in developing countries (MacAskill, 2015).

In this talk, I will examine the prospects of global prioritization as a successful research program in economics, by focusing on the challenges it faces. Interestingly, most of these challenges were already faced by cost-benefit analysis, another field of research in economics that shares the ambition of evaluating how valuable the outcome brought about by a given action is. Cost-benefit analysis seeks to answer "whether one or a number of projects or programmes should be undertaken and, if investable funds are limited, which one, two or more among these specific projects that would otherwise qualify for admission should be selected" (Mishan and Quah, 2007, p. 3). This is achieved by proposing an overall evaluation of whether the benefits of the project exceed its costs, where benefits and costs are supposed to include all the value and disvalue brought about by the project.

Three challenges for global prioritization stand out as particularly worthy of attention, and will be addressed successively:

- 1) How to account for the open-ended diversity and complexity of the consequences of an action? Cost-benefit analysis, in practice, artificially restricts the scope of consequences under consideration, by relying on a series of simplifying assumptions (Hansson, 2007). For

example, time discounting enables to ignore long-term consequences. By relaxing these assumptions, global prioritization might expose itself to intractable problems.

2) Some considerations already used for global prioritization within the effective altruism movement include considerations about cause areas. For example, the more neglected a cause area is (e.g. in terms of the amount of money spent annually on the cause area), the more positive impact we can expect an action within this cause area to have (Wiblin, 2017). This kind of consideration seems different from considerations stemming from an examination of the "internal" mechanisms through which a given action brings about value or disvalue. Cost-benefit analysis and cost-effectiveness analysis arguably deal only with the latter. This gives rise to the following question: How should we combine external considerations (such as neglectedness) and internal considerations to yield an overall estimation of the positive impact of an action?

3) How should global prioritization deal with normative matters? If it is to constitute an empirical investigation, it is necessary to determine how "doing the most good" should be interpreted in empirical terms suited for economic research. As moral philosophers are actively involved in global priorities research, the articulation between normative and empirical work seems to evolve in ways that markedly differ from that observed in other areas of economics such as welfare economics. It is an open question whether the proposed articulation can aspire to the kind of objectivity required by scientific research.

I will conclude this exploratory look into global prioritization by pointing at promising directions for further research.

Christopher Clarke – University of Cambridge

Does political science study two different types of causation?

In this paper, I characterize what political scientists mean by process tracing. I explain why many process-tracers think that the kind of causation that process tracing can uncover differs from causation qua difference-making, the kind of causation that statistics-based political science uncovers. I then argue that the best way of understanding the causation studied by process tracers is as partial sufficiency. I give a rigorous characterization of partial sufficiency, a notion somewhat similar to Mackie's idea of an INUS cause. I then explain why knowledge of causation qua partial sufficiency is valuable. And I defend causation qua partial sufficiency from several common objections. I conclude by exploring one problematic consequence of this reading of process tracing: the study of the causes of war (for example) is not the mirror-image of the study of the causes of peace (for example), and so the former can tell us little about the latter.

Ricardo Crespo - Universidad Austral and CONICET

For Carl Menger economic theory has the role of demonstrating (Darstellung) and understanding (Verständnis) (1889: 6) economic phenomena. The German verb ‘to understand’ and the noun ‘understanding’ (Verstehen and Verständnis), especially when Menger was writing, had a specific meaning related to the special way of explaining in the human sciences, which has to capture the intentional aspect of human actions: a ‘comprehension’, or ‘appreciation’ of their ends or motives, an interpretive task. He also states ([1883] 1985: 43) that ‘we understand it (a phenomenon) when we have recognized the reason for its existence and for its characteristic quality (the reason for its being and for its being as it is)’, that is, its end. Henry Sidgwick also states that ‘we require for the comprehension of economic facts some interpretation of the motives of human agents’ (1887: 30-31).

This consideration of the ends or motives of economic actions has been put aside by 20th Century standard positive economics for which the ends of economic actions are to be taken as given. Contrary to it, new economic currents as happiness economics, the capability approach, or civil economy, are concerned with ends. At the same time, the standard economic notion of preferences as ‘total comparative evaluations’ (Hausman 2012: 3) impedes a discrimination of ends. It has been criticized for this reason by, for example, Lehtinen (2013), Steele (2014), and Engelen (2017).

Simply put, ends are actually not given. As Frank Knight (1956: 128-129) points out, ‘given ends’ are not ends in themselves; ends are redefined in the course of the action itself. In other words, not only are means adapted to their ends but also – and even more frequently – ends are adapted to their means. Ludwig Lachmann also notes that ‘some of the knowledge relevant to the action will only be acquired in agendo’ (1971: 40). For James Buchanan choice is a process by which preferences are continuously renewing. He affirms that ‘choice, by its nature, cannot be predetermined and remain choice’ (1987: 35); ‘men can choose courses of action that emerge only in the choice process itself’ (1987: 78). The task of the economist is to discover what people want (1987: 16). For him, it is clear that individuals do not have given ends, and he thinks that, consequently, this fact blurs the dividing line between positive and normative economics (1987: 15-16).

However, the consideration of ends that will be analysed in this presentation is not that of normative economics. Neville Keynes considers normative economics as a part of political economy, ‘a normative or regulative science as a body of systematized knowledge relating to criteria of what ought to be, and concerned therefore with the ideal as distinguished from the actual’ ([1890] 1955: 34-35, all cursive by Keynes). That is, normative economics proposes ends as normative ideals. This presentation deals with the possibility of discovering the actual ends of economic actions as part of the role of economic theory, not as normative ideals of normative economics.

The paper will assess the scope and limits of possible interpretive methods for the consideration of ends in economic theory, and their relations with the methods of positive economics. Harold Kincaid (1996: chapter 6) supports an interpretive science that is good science. He proposes this list of evidential virtues of good science: falsifiability, empirical accuracy, scope, coherence, fruitfulness, objectivity (1996: 50-51). In fact, I think that this is the tendency of the new currents recently appearing in economics: behavioural economics, happiness economics, capabilities approaches, evolutionary economics, and institutional

economics. All these currents deal with ends and make efforts to empirically verify their findings sometimes 'importing' theoretical constructs and tools from sciences as psychology, sociology, or neurosciences.

Roberto Fumagalli - King's College London

On the Individuation of Choice Options

Standard decision theory builds on specific axiomatic requirements on agents' preferences, together with the representation theorems derivable from these requirements (e.g. von Neumann and Morgenstern, 1944, Savage, 1954). Such representation theorems demonstrate that if an agent's preferences satisfy specific axiomatic requirements, then this agent's choices can be represented as if the agent maximizes expected utility (e.g. Fumagalli, 2016, Okasha, 2016). Over the last few decades, several purported violations of decision theory's axiomatic requirements have been documented (e.g. Machina, 2008, Starmer, 2000, for reviews). Many of these purported violations can be accommodated by modifying how agents' choice options are individuated and formally represented (e.g. Broome, 1991, ch.5, Dietrich and List, 2016). In recent years, prominent authors have criticized these modifications for trivializing decision theory (e.g. Hausman, 2000, Steele, 2010), undermining the theory's falsifiability (e.g. Bhattacharyya et al., 2011, Hampton, 1994), imposing cognitively overdemanding requirements on agents (e.g. Bales et al., 2014, Gilboa et al., 2009) and hampering the internal coherence of decision theory's mathematical formalism (e.g. Alexander, 2012, Sugden, 1991).

In this paper, I draw on the best available empirical and theoretical works in contemporary decision theory to address these prominent criticisms. In doing so, I articulate and assess several different criteria for individuating and formally representing agents' choice options. The contents are organized as follows. In Section 2, I outline decision theory's axiomatic requirements and examine these requirements' reported violations. In Section 3, I explicate how such violations can be accommodated by modifying how agents' choice options are individuated and formally represented (re-individuation strategy). In Sections 4-7, I identify and address four major challenges put forward against this re-individuation strategy, namely: the trivialization challenge (e.g. Hausman, 2000, Steele, 2010); the falsifiability challenge (e.g. Bhattacharyya et al., 2011, Hampton, 1994); the challenge from cognitive overdemandingness (e.g. Bales et al., 2014, Gilboa et al., 2009); and the challenge from theoretical incoherence (e.g. Alexander, 2012, Sugden, 1991).

Over the last few decades, decision theorists have made substantial advances in developing versions of decision theory for choice contexts where one lacks precise probabilities (e.g. Bradley, 2017, Joyce, 1999) and well-defined utility functions (e.g. Buchak, 2013, Gilboa, 2009). However, comparatively little progress has been made in the provision of plausible and informative criteria for individuating and formally representing agents' choice options (e.g. Broome, 1993). My evaluation aims to fill this major lacuna in the decision theoretic literature and thereby contribute to the development of a descriptively and normatively adequate decision theory.

Altruistic trust in healthcare

Trust is the fabric of good healthcare. Without personal trust in their doctor, a patient would not reveal important health-related information, nor follow the doctor's medical recommendations. A lack of institutional trust in healthcare would make a sick person delay visiting a clinic (Taber 2015), allowing their condition to deteriorate and increasing the costs of their eventual future treatment. It is thus not surprising that trust in healthcare – both at personal and institutional levels – is associated with better health outcomes (Birkäuer et al. 2017) and reduction of healthcare costs (Dyer et al. 2003). Trust in healthcare also has a wider social meaning. Healthcare as a public institution is anchored deeply in the structures of the state (especially in the European welfare state model). As such, it can positively influence generalized trust in a society (which is a key fundament of its social capital, ergo its civil and economic prosperity), and contribute to building the legitimacy of contemporary democracy [Gilson 2003]). The ethical importance, pragmatic indispensability and curative potential of trust gives the question of how to promote and protect trust in healthcare a vital significance. This concern is additionally strengthened by the reported contemporary crisis of trust (Cf. Edelman 2017, OECD 2017), decreasing support for democracy (Cf. Diamond, Plattner 2015), changing patterns in the European welfare state (Cf. Ter Meulen 2017), and the constantly – and inevitably, due to the ageing of society and high prices of new medical technologies (Callahan 2009) – increasing costs of healthcare. Trust is thus a resource that can contribute to maintaining the endangered European model of healthcare based on justice, solidarity and universal access – as such it should be taken seriously in the policy of the state under the rule of law.

Objectives

The aim of my paper is to present the altruistic theory of trust in healthcare. This general aim can be divided into 4 specific objectives. 1) Firstly, I will confront two models of trust in healthcare – altruistic and calculative – and their theoretical foundations and practical consequences. 2) Secondly, I will present in greater detail the altruist concept of trust (which in my opinion is better than the value-rational logic of trust in personal relations), and ask about its psychological mechanisms, social embeddedness and decent limits (Margalit 1996). 3) Thirdly, I will ask philosophical-legal questions about the role of law and legal institutions in building, promoting and restoring such trust in healthcare, as well as the limits of legal framing (Tversky/Kahneman [1981]; Bohnet [2007]; Warren/Calvert [2014]). 4) Finally, I will ask about the wider social and political meaning of trust in healthcare.

Hypotheses

The first hypothesis claims that the nature of trust is best described by its altruistic (Mansbridge 1999) theory (or 'value-rational' theories, as Smith puts it (2005, p. 302) in an attempt to give the differentia specifica of trust distinguishing it from confidence). Trust cannot be closed into a narrow frame of self-interest and purely calculative, strategic

assessment of someone's trustworthiness (G. Möllering [2006] expresses this thesis by emphasizing the irreducible 'as if' element, while V. McGeer and P. Pettit [2018] illustrates it with many cases of the credit of trust being given room to grow into). Moreover, this theory assumes that not only cognitive, but also affective and axiological factors take part in the process of trusting. Results of empirical research on determinants of trust in healthcare confirm the accuracy of the altruist model. Patients trust doctors not only for their competence, but also for their ethos and such axiological reasons as honesty, confidentiality, dependability, fiduciary responsibility, fidelity, care, and communication (LoCurto, Berg 2016; Hillen 2013). That last quality should be honest, inclusive, compassionate, clear and comprehensive, coordinated (DeLemos et al. 2010) and respectful (Hallowell 2008). The second hypothesis claims that the necessary condition for the morality of trust is good management of mistrust (particularly the introduction of mechanisms of institutionalized distrust [Cf. Sztompka 2000]). Even if the first – altruistic – thesis can be summed up with the reformulated legal maxim, in dubio pro fides, trust is not always positive. One example of this is, undoubtedly, the encounters between doctor and patient, which is a situation of inequality, resulting as such from the asymmetry of the information available to the professional and the layman. Therefore, the aim of this part of the paper will be to analyse the psychological mechanisms, rational warrants and ethical limits of decent, with a particular focus on the specificity of healthcare relationships.

The third hypothesis claims that law can play, non-obvious, but important role in promoting decent trust in healthcare. In this part I will discuss different, more or less effective institutions of trust-promotion (part of which consist in the above-mentioned institutionalizing mistrust and protecting the weaker party) from a comparative and critical perspective. This institutional study must be, however, preceded by the more general philosophical-legal analysis of the role of legal framing in this domain (and its limits). Such 'critics of juridical reason' is necessary, due to both the specific nature of trust and concern for the quality of legal culture.

The fourth hypothesis claims that healthcare has a neuralgic place among state institutions, because citizens place in the hands of its officers their most vital assets – their life and health. Therefore, increased trust in this public domain can also bring about a growth of trust in other areas of political and social life. In this part of the study I will argue that this requires just resource allocation (Uslaner/Rothstein 2005) and introduction of possibly participative mechanisms. As Essaiason et al. (2012, p. 785) finely put it "personal involvement is the main factor producing legitimacy beliefs".

James Grayot - Erasmus Institute for Philosophy and Economics (EIPE)

Improving decision models: computational models of cognition versus enlightened Bayesianism

It is becoming common for behavioural economists and neuro-economists to model decisions as the outcome of dual processes. In [Anonymous (under review)], I raise challenges for "dualistic" decision models, i.e. models which rely on dual process theory as a

psychological framework. I argue that dual process theory is descriptively vague and possibly inaccurate regarding the internal dynamics of decision-making, and hence, that it may distort crucial aspects of the decision-making process. In response, I propose to investigate two alternative psychological frameworks as candidates for improved decision models.

Alternative 1: Decision field theory

Decision field theory (cf. Busemeyer & Johnson, 2004, 2008) is a computational model of decision making which uses a connectionist, neural network framework to represent preference formation. Rather than represent decisions as a deterministic set of cognitive processes, decision field theory represents choice options via a network of actions with interconnected property nodes; the value of a given action is affected by the attention weight which links an action to a given property. Attention weights are influenced by background beliefs and information but are inherently stochastic. A preference state is achieved when the accumulation of attention weights reaches a threshold and induces an action.

The primary benefit of decision field theory is that it offers a legitimately computational basis for human learning and inference by way of mathematical modelling and computer simulation (and, of course, behavioural experiments). This may be contrasted with both behavioural economic and neuro-economic models of decision-making which can be vague about what computation consists in for human decision makers. When applied to the study of decision making, such models provide a means of tracking utility optimization procedures in a way that can yield preference formation. This would constitute a more realistic basis for the information processing metaphors that economists tend to invoke.

The limitation of such a model is that it's not evident how individuals' mental states mediate the distribution of attentional weights to actions when decision field theory is interpreted as an artificial neural network—in this way, it is comparable to functionalist accounts of dual process theory which black-boxes processes like override and conflict monitoring functions which prevent automatic and impulsive behaviour from occurring. Yet, when applied directly to the study of the brain, the computational basis of decision field theory is better able to accommodate the “noise” associated with stochastic attentional shifting and this has great potential to explain both the causes of reasoning errors, and hence capture decision anomalies that concern behavioural economists, while also providing a realistic depiction of underlying decision processes. Individuals' choices are not formed through linear reasoning procedures, as dual-process-based economic models presuppose; real decision-making is messy and fragmented, and this is ignored by current dual process models (this also applies to some neuro-economic applications of dual process models).

Alternative 2: Enlightened Bayesianism

While Bayesian models traditionally offer little insight into the psychological basis of decision making, certain “enlightened” Bayesian models of cognition have the potential to unite rational analysis of the Bayesian program with cutting edge knowledge of cognitive mechanisms which do underwrite decision procedures. In Jones & Love (2011), several candidate models are proposed, each of which identifies a different area of cognition and/or perception that is integral to the decision process. While it remains to be seen how well

these models predict novel decision phenomena (many candidate models are being currently tested), there is reason to believe that a Bayesian model of cognition applied to local or specific cognitive and perceptual tasks could explain how decision anomalies occur without adverting to “bargaining games” or “trade-offs” between dual systems whose underlying functional characteristics aren’t well-defined. Enlightened Bayesian models of cognition seek to ground optimization procedures in the very mechanisms that cognitive science recognizes to be complicit in reasoning errors. If it can be shown that certain mechanisms, or clusters of mechanisms, abide by Bayes’ rule and “compute” optimization procedures, this potentially avoids many of the conceptual and ontological confusions generated by dual-process-based economic models.

Further, unlike computational models of cognition, which are most descriptive and hence most illuminating when applied directly to the brain, Bayesian models of cognition claim to apply to multiple-levels of analysis (to use Marr’s distinction). Although there are different models on the market, and it will take time to determine which are amenable to the purposes of economic modelling, some Bayesian models of perception claim to adequately bridge computational, algorithmic, and implementation levels in a way that does not conflate their functional characteristics. If true, this could provide a remarkable basis for grounding rational analysis that economists seek. But, this, like the former alternative, is speculative and requires testing in economic conditions before it can be declared viable or not viable.

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*Scientific change, cognitive resources and the importance of institutions -
a closer look on macroeconomic models*

While there is some work on the influence of cognitive schemata on scientific research, little has been said about the role of institutions in this context. Ronald Giere, for example, points out the importance of cognitive resources - or cognitive schemata - for scientific change in his book ‘Explaining Science’. Resources or schemata can, depending on the specific case, advance or inhibit scientific change. However, Giere focusses especially on the individual professional trajectory of scientists and gives only little attention to the question how the institutional context may contribute to the development of cognitive resources. In the present paper I will address the institutional aspect with regard to research in macroeconomics and to mathematical models, which, as I argue, have become a form of cognitive schemata. To discuss this topic, I will especially refer to another account of scientific research, the work of Mara Beller, to show how the implementation of scientific dialogue can help to gain cognitive resources but also to reveal and to question them. Beller argues that modern science has established institutions, like for example scientific journals or conferences, that allow handling disagreement. However, the case of macroeconomic research shows that institutions may also have the opposite effect. When they impose scientific standards which are not questionable anymore themselves, these institutions significantly narrow the margin for dialogue. The aim of this paper, which addresses

questions of social epistemology as well as of philosophy of economics, is to discuss the problematic laid out above and to briefly suggest possible ways forward.

The use of macroeconomic models in mainstream economics during the last decades can be considered as an example for cognitive resources in the sense of schemata, according to the definition of DiMaggio. He describes schemata both as representations of knowledge and information processing mechanisms as well as mechanisms that simplify cognition: "Highly schematic cognition is the realm of institutionalized culture, of typification, of the habitus, of the cognitive shortcuts that promote efficiency at the expense of synoptic accuracy (DiMaggio, p. 269)". Mathematical modelling has become a dominating methodological tool in New Classical and New Keynesian research since the mid-20th century. Beyond its methodological function, however, it is also a form of reasoning style (see Hacking 1992, Morgan 2012) in the sense that theoretical claims repose on methodological assumptions. As Mary Morgan stated: "For example, Quetelet's average man of the mid-nineteenth century is a statistically defined concept and so unthinkable without the adoption of statistical reasoning" (Morgan, p. 17). Methodological concepts, once adopted, have a tendency to become the evaluative standard for what is 'reasonable' or 'acceptable' in the discipline. Formalism in economics has more or less determined not only the assumptions but also what counts as a problem in mainstream economic research and what theory can be used (see Chick, also Backhouse and Krugman 1998). Thus speaking, some empirical economic phenomena not only are not discussed, but due to certain simplifications, like the individual representative agent or linearity assumptions, they are basically not even conceivable by current economic models (see Kirman 1992, 2010, also Buitter 2009). Thus models seem to have become the main schemata by which mainstream economists think and the lens through which economic phenomena are considered. These tendencies are implemented and reinforced by institutional devices like university curriculums and by the publication standards of the highest ranked journals of the discipline.

In this context, it is interesting to consider Mara Beller's dialogical account of scientific research. As she demonstrates in her work on the physicists who developed the quantum theory, scientific reasoning is fundamentally addressive in character. This means that scientists develop their propositions in response to the 'other'. Disagreement has an important role in this case, as shows Beller's example of Heisenberg who developed important parts of his matrix theory as a result of his dispute with his rival Schrödinger and his wave theory. The dialogical account of science is thus a counterexample to the Kuhnian notion of paradigm and the idea of commitment to a specific research program. Here, disagreement is not considered as an anomaly but as constitutive for 'normal' scientific research. With regard to my topic, I consider her account more as a normative requirement than as an empirical description of research in mainstream macroeconomics since I have rather identified the absence of disagreement and of dialogue – at least with regard to methodological fundamentals. What seems more important here is the question, how can institutions contribute to establish dialogue and somehow 'free' disagreement? Beller describes the institutionalization of scientific communication (journals, conferences etc.) as an implementation of dialogue and a way of handling disagreement. However, institutions also impose standards which, like in the case of macroeconomics, may leave only limited space for dialogue and disagreement.

To conclude and to suggest a possible way forward, I suggest that might be important to not only integrate problems from questions that arise within the discipline but to open the debate to questions from outside the academic sphere, like that of economic policy to better reveal and discuss persistent schemata question persistent schemata. For example, with

regard to the recent financial crisis, banking supervisors and some central bankers –which means in a wide sense economists concerned with an economic or financial policy - were much more alerted to financial turmoil already in the pre-crisis years than academic economists. For now, the methodology of the discipline largely is only evaluated against its own standards. Considering aims and problems from the political sphere could contribute to assess relevance and success of economic methodology from an outside point of view.

Paul Hoyningen-Huene - Leibniz Universität Hannover

A constructive critique of Sugden's view of economic models

In a series of papers from 2000 on, Robert Sugden has analysed the epistemic role of theoretical models in economics. His view is that these models describe a counterfactual model world that is separated from the real world by a gap. According to Sugden, this gap “can be filled only by inductive inference”. The putative inductive inference that Sugden constructs leads “from the world of a model to the real world”, based on “some significant similarity between these two worlds”. Sugden assumes that this significant similarity consists in their sharing the same kind of causality. However, in this regard, model cities are utterly different from real cities, contrary to Sugden: models may at best represent the causality of reality, the causality of a computer model, for instance, is given by moving electrons in the computer.

Here is an alternative way to explicate the explanatory use of models. Let us assume that we want to explain segregation in American cities by a model. In the first part, we will stay completely in the model world. We will need six steps. First, an outline of the model world must be given. The outline of Schelling's model world is a grid of squares, and some white and black chips. Second, in our case the state to be explained is a state with model-segregation. The goal is that the model will produce this state as its final state. Third, we must choose the central element of the model, its dynamics. We pick the dynamics of the Schelling model; it may be called model-mild-racial-preference-dynamics. Fourth, we must choose an initial state for the model. The “initial states” of American cities are all different from one another, and furthermore ill-defined. Thus, the representative function of the initial state of the model is dubious. In addition, for any specific initial state we wouldn't know if the final state produced by the model is the result of mainly the specificity of the chosen initial state. However, we want the final state of the model to be mostly the result of the (general) dynamics of the model. In order to circumvent these problems, we will have to run the model for all (practically: many) appropriate initial states, i.e. states with model-non-segregation: a random distribution of chips and spaces. We then hope that the final states will be independent of the initial states with respect to segregation. Fifth, we examine the final states produced by the model. All final states of the Schelling model display model-segregation, independently of the specific initial condition with model-non-segregation. Sixth, we have to interpret the result. Does the model explain model-segregation in the model world? Apparently, the explanation would work as follows:

- (1) Let in the model world a final state with model-segregation be given.
- (2) With any model-non-segregation initial state, model-mild-racial-preferences-dynamics lead to model-segregation as final states.

Therefore: (3) The given final state with model-segregation is explained by model-mild-racial-preferences-dynamics.

However, the step from (1) & (2) to (3) is not deductive, but abductive, which implies that it is not compelling. This can be seen very easily by constructing alternative explanations of (1). For instance, models with model-strong-racial-preferences, or model-apartheid-laws, or model-race-related-income-differences also lead to model-segregation as final states. Thus, any particular model producing a final state with model-segregation only delivers a potential explanation of model-segregation.

This result is quite general: models never produce actual explanations in the respective model world but only potential explanations. This is because alternative models may lead to the same final state, having the same status regarding their explanatory power. In order to transform a particular potential model world explanation into the actual model world explanation, one has to exclude all alternative explanations.

Now we come to the second part, the transfer of model results to the real world. In order to transfer results from the model world to the real world, there must be some “correspondence” of the elements of the model world with the real world. For the sake of argument, I shall make assumption (A) that states that this correspondence is completely unproblematic, i.e., results from the model world can be immediately transferred to the real world, without any diminishment of their explanatory force. The aim of (A) is to find out what the explanatory force of a model for the real world is at best. With assumption (A), does the Schelling model provide an explanation for real world segregation phenomena, e.g., race segregation? Clearly, it does not because the model tells us only that mild racial preferences are one possible mechanism producing racial segregation. Thus, according to the model one has to exclude all alternative possible explanations in order to get the actual explanation. In the real world, the identification of the actual explanation by excluding the other potential explanations may be tricky. Imagine we have a case of racial segregation in which we have empirically shown the existence of weak racial preferences. Now imagine that the city we are considering is also under apartheid law. Under these conditions, it is more plausible to explain the racial segregation by apartheid law rather than by actually existing mild racial preferences. Thus, in the given case the similarity of the mild-racial-preference model to the real world does not by itself promote it from “potentially explanatory” to “actually explanatory”. Thus, it is not a property of a model itself (for instance, its empirical adequacy, or its “credibility”) that transforms it from “potentially explanatory” to “actually explanatory”, but only the exclusion of all competing models. Thus, contrary to Sugden, the most problematic aspect of explanations of real-world phenomena by abstract models is not the transfer from the model world to the real world. It is rather that already in the model world, models only deliver potential explanations. Thus, also in the real world, models deliver at best potential explanations. To transform a potential explanation into the actual explanation, one has to eliminate all alternative potential explanations.

Michal Hubálek - University of Hradec Králové

At the end of the 1970s, Richard Lewontin's and Stephen Jay Gould's pluralistic viewpoints sparked the anti-adaptationist movement within evolutionary sciences. Gould and Lewontin championed the notion that evolutionists, instead of crafting plausible evolutionary explanations, often provide us with mere just-so stories (Lewontin 1977; Gould 1978). On the other hand, it was Gould himself (1989) as well as other historically-minded scholars who have argued that due to the nature of the subject matter, i.e., historical events, evolutionary scientists must inevitably employ narratives as explanatory devices (see Beatty & Carrera 2011; Currie & Sterelny 2017; cf. Hull 1975). While my intention is not to directly challenge the adaptationist programme as such, I attempt to demonstrate that plausible narrative explanations can be, in principle, successfully distinguished from just-so stories on rational grounds. The monography by Hugo Mercier and Dan Sperber "The Enigma of Reason" (2017), particularly the adaptive explanation they submit, serves me as a case study for evaluating evolutionary and/or adaptive explanations of the origin of individual phenotypes. The main goal of this paper is to defend more pluralistic and methodologically robust historical analyses of human evolution. Consequently, to oppose the view that narratives qua explanations imply epistemological nihilism and/or "anything goes" principle in the endeavour to grasp human (evolutionary) history.

Panagiotis Karadimas - National and Kapodistrian University of Athens

Buchanan's Thought Experiment and its Explanatory Virtues

The purpose of the presentation is, through the examination of Buchanan's thought experiment in constitutional economics, to show how thought experiments can provide explanations in social sciences i.e., how they can be used to answer several why-questions. Buchanan in his treatise "The Limits of Liberty-Between Anarchy and Leviathan" (Buchanan, 2000), calls us to imagine a two-person world where no laws and no property rights are established and people spend their time in defensive and predation efforts in order to secure the biggest possible proportion of goods at scarcity. He uses the term "Natural Distribution" to represent this fictitious state of affairs. Through diagrammatic representations he illustrates how people will realize that this situation takes a toll on their well-being and, in order to maximize their utility (Herfeld 2012), they proceed, acting as rational entities, to the establishment of property rights, that is to say, to a "Constitutional Contract". Constitutional contract thus emerges as the result of consent among rational individuals since it is people that "choose their own constraints" (Ibid. p.15) and not an external force. There are four conditions Buchanan puts forward here and which constitute the set of defined rights that individuals have given their consent to: Firstly, the terms of constitutional contract should provide us with a statement of behavioural limits of every person. Individuals who enter the society lay down their arms and expect the others to abide by the same rules. Secondly, constitutional contract establishes positive rights of proprietorship or domain over resource endowments capable of producing final goods. Thirdly, the constitutional contract should also make reference to the terms and conditions of enforcement. Such rules will clarify the role of the protective state as an enforcing agent. Fourthly, the terms need to define the rules under which the collectivity operates regarding the provision and finance of public goods. This is the legislative aspect of the community,

which specifies the limits of the productive state (Ibid, p.93). The protective state insures against deviants the rights which were unanimously agreed, whereas the productive state makes public goods obtainable to each individual who has condescendingly joined the constitutional contract. Buchanan guards against the outspread and the overpowers of the productive state since there is every likelihood that it will intervene in private economy insofar as there are no constitutional limits. Once a constitutional contract is established, then “post-constitutional contract” becomes possible. Post-constitutional contract consists in the scrutiny of the actions taken by individuals, such as trades or exchanges of goods, within the already defined rules of constitutional contract.

As this brief description of the thought experiment implies, Buchanan makes use of several representational and inferential strategies which serve as propositions that can answer certain why-questions.

Why-questions perennially play a prominent role in the discussion regarding explanation (Bromberger, 1966; van Fraassen, 1980; Mantzavinos, 2013; Mantzavinos, 2016). Let us say that the question “Why is the case that Pk?” emerges. Such a question can be identified as having an ordered triple: $Q = \langle Pk, X, R \rangle$ where Pk is the topic of the question, $X = (Pi, Pii, \dots, Pk)$ is the contrast class, and R is the relevance relation (van Fraassen *ibid.*, p.143). A proposition A is called relevant to Q if A bares relation R to couple $\langle Pk, X \rangle$.

Thought experiments serve as explanatory relations and given that under different contexts different why-questions emerge and thus different answers are required, the nature of the question determines which proposition of the thought experiment will be applied.

As we saw, Buchanan’s analysis hinges upon three conceptual schemata, as he himself characterizes them (Ibid. p.46). The concepts of “natural distribution”, of “constitutional contract” and of “post-constitutional contract” are conceptual representations of the respective fictitious states of affairs. Apart from these, at the constitutional level, Buchanan posits the representations of the productive and the protective state which delineate the role of the state as an enforcing agent. These representations (A) can function as explanatory relations by explaining Pk in contrast to the rest of X. As I will show, apart from these conceptual representations, Buchanan uses diagrammatical representations which can also serve as answers to why-questions.

There also inferential strategies, such as the postulate of utility maximization principle which makes possible the move from natural distribution to the constitutional contract and then to post-constitutional contract, that may answer a why-question as well.

For example, let us say that the question “Why does the government intervene excessively in private economy?” emerges. In this question, the topic of the question, namely that it is a matter of fact that government makes inroads to private economy is (Pk), and Buchanan’s representation of productive state (A) shows that the state can turn out to be oppressive insofar as it is left unchecked. Thus the absence of constitutional limits explains excessive governmental intervention in contrast to a set of alternatives (X). In this case the contrast class may include statements such as:

- The Government is restricted by constitutional prescriptions (Pi).
- The Government’s jurisdiction is limited fiscal wise (Pii).
- The Government intervenes excessively in private economy (Pk).

Thus in the question “Why is the case that Pk instead of Pi or Pii?” Pk is explained by A in contrast to the rest of X. The lesson to be drawn from Buchanan’s thought experiment is that several social issues expressed as why-questions can be tackled by thought experiments. The representational and inferential strategies of the thought experiments serve as propositions that answer particular why-questions.

Agent-Based Modelling and Methodological Individualism

Models called agent-based models (ABMs), which include some evolutionary game theory and multi-agent models, are important now in the social sciences because they do not require analytic solutions or strong assumptions about individual behaviour that are typical of general equilibrium type theories. They also seem to have the further virtue of realizing the methodological individualist program. This paper concerns the extent to which:

- (1) ABMs do explain entirely in terms of individuals and
- (2) they should explain entirely in terms of individuals.

The issues raised are not new (see Marchionni and Ylikoski 2013 for a good start) and are complex. Our contribution is to focus on a key subset of issues and provide new and/or more extensive argumentation.

Part I of the paper asks whether ABMs explain just in terms of individuals. Two common intuitive arguments are surveyed first. One points out that the material environment—physical, technological, ecological, etc.—is likely to be part of ABM explanations, so they do not stick just to the traits of individuals. Why however should individualists be denied such explanatory resources? Isn't the relevant contrast to "individual explanation" some kind of "social explanation"? A second argument is that ABMs will appeal to not just individual characteristics but also relations between individuals. Again, why should the individualist be denied such explanatory tools? Atomism is the view that everything can be explained by individual, non-relational properties but individualists need not be atomists. strategy of another. While neither argument works, they do point to important questions about the explanatory resources of ABMs.

Three other more compelling arguments are considered in part I. The first is that there is considerable use of ABMs to explain social phenomena by treating collective, social entities as the agents modelled. This line of reasoning is suggested in Marchionni and Ylikoski (2013) and much earlier in REF REMOVED, but we extend the argument considerably. We detail the extent of these kinds of social explanations, make clear that the appeal to collective entities can be essential to models, and show that various attempts to treat them as individualist fail. We then take up the special set of arguments against "intentional" collective agents in part II as we discuss normative individualist claims.

A second kind of more compelling argument against (1) is that ABMs sometimes explained via properties that are really properties of social or collective entities, not individuals. Marchionni and Ylikoski (2013) and Hoover (2001) make this point. While we are sympathetic, we think the argument can be refined and clarified. Why cannot such appeals can be reformulated as explanations, perhaps quite complex, in terms of individuals and their relations? Averages, for example, hold of populations not individuals, but they seem

hardly to support the anti-individualist program. We try to say when, where, and why group properties are and are not troublesome for the individualist.

A final attempt to show that ABMs are not individualist notes that they sometimes explain in terms of social roles and social roles presuppose facts about social entities. These claims go back early in the individualism debate, explicitly being made by Mandelbaum (1955). They have been uncritically employed without clear formulation. Yet the general idea is widespread among social scientists that some relations between individuals are social relations in a sense that preclude individualism.

We work through different versions of this understudied argument and present what we take are a series of currently open questions. A first task is to get clear on how ABM accounts of individuals are supposed to implicitly invoke social entities. The “presupposition argument” can be divided into roughly semantic or conceptual versions and causal versions. The semantical versions rest on contentious theories of meaning we argue and even if they succeeded, they would provide an anemic sense of anti-individualism. The alternative causal version of the presupposition argument is much more compelling. We argue that causal presupposition comes in degrees as it were (per earlier work—see ref removed) and that there are interesting open empirical questions raised by current ABMs concerning how much background causal structure they take for granted.

Part II of the paper then takes up the normative claim that ABMs should not be individualist. We note first that if the arguments of part I succeed, then on naturalist grounds we have *prima facie* reasons to think some non-individualist ABMs can succeed.

One set of argument for the claim that ABMs should not be at all social in nature (where that means “should not explain at all in terms of social entities such as firms, states, classes, etc.”) concern, roughly put, what can make things happen in the society. This argument states that in the social world, individuals are the only moving agents. Social entities move/act via the actions of individuals and thus social scientists should only provide ABMs of acting individuals agents. We are sceptical that this reasoning works and explain why.

A second worry about non-individualist ABMs is that they sometimes invoke social wholes that seem to be full-bodied “agents,” which follow strategies, act on preferences, etc. The worry is that collective entities cannot have the intentional states that agents need and thus ABMs should be individualist here and avoid the holist explanations. We work through this argument in some detail. There is a perfectly defensible approach to the legitimate attribution of intentional states as explanatory for individuals that can be applied to collective agents as well, if the right empirical conditions are met. That view is associated with Dennett, but really gets clearer expression in revealed preference theory in economics. Intentional states as real patterns with the right properties realized in the behaviour of collective agents can be empirically defensible and explanatory. Some ABMs arguably identify such patterns.

A third set of arguments—which are really more like intuitive considerations—is that ABM are only sufficiently deep explanations when they are in individualist terms. Numerous issues are obviously lurking here, and we only do a first pass at raising doubts about these kind of considerations. One reasonable measure of explanatory depth is the number or kinds

of explanatory questions that can be answered. Then there is reason to think that some ABM do not have an explanatory advantage, for some ABMs either are entirely or partially (“mixed” models that have individual and collective agents) in terms of social entities and explanations in social terms more generally may find patterns that are not available without looking at collective entities. Other notions of depth and their possible implications are considered.

Simon Lohse - Leibniz University Hannover

Social Emergence and Unpredictability

There is a long-standing debate concerning the meaning and the viability of reductionism in the social sciences. It is well known that this debate has many sub-aspects. It revolves around ontological matters (e.g. the reality of group agents), epistemological questions (e.g. “Can we reduce sociological macro-theories to micro-theories?”), methodological aspects (e.g. the demand to explain social facts with sole reference to individual actions) and related issues. In recent years, this debate has often been framed in terms of reduction and emergence: Are there (strongly) emergent properties of social systems, and if so, what are the epistemological and/or explanatory implications for methodological individualism? So far the respective discussions have focused predominantly on the possibility and the existence of emergent causal powers or downward causation in the social world (e.g. Sawyer, 2005; Elder-Vass, 2010). While some have argued for the existence of strongly emergent causal powers of social systems, others have claimed the opposite or attempted to reconcile reductionist and emergentist approaches in the social sciences. In this talk, however, I will attempt to shed some light on a lesser-discussed aspect of social emergentism/reductionism. I will discuss arguments for social emergence that are based on the idea that the properties of certain social systems could be in-principle-unpredictable and therefore emergent. After introducing the core idea of in-principle-unpredictability as a mark of strong emergence (and its epistemic rationale), I will examine and criticise three arguments that have been put forward to defend the idea that social systems are indeed unpredictable and therefore strongly-emergent.

First I will discuss Niklas Luhmann’s (1995) notion of intransparent historical systems as the basis of unpredictability in the social sciences. His main argument is based on the claim that there is no simple input-output-logic of complex social systems such as organisations, but that their behaviour depends on an evolved internal structure. According to Luhmann, this internal structure is not only extremely complex but ultimately intransparent to us (as it is for any social scientist) which is not only a pragmatic but a fundamental challenge for predicting the behaviour of organisations and other social systems. I will attempt to show that this argument fails as sophisticated reductionist individualists have the explanatory resources to address Luhmann’s challenge. These resources consist, first and foremost, in the use of middle-range theories of human behaviour and qualitative social research methods.

Next I will analyse the possibility of transferring C.D. Broad’s concept (1980[1925]) of

epistemic disconnected, and hence emergent, micro-macro-laws to the social sciences. The key idea is that the properties of a certain social whole could be undeducible even given “the most complete knowledge of the behaviour of its components, taken separately or in other combinations, and of their proportions and arrangements in this whole” (p. 59). This would be analogous to the epistemic situation in chemistry in 1925. I will examine the underlying logic of this argument and argue that it is very hard to make a plausible case for the epistemic disconnectedness of micro-macro-laws in the social sciences, given the current state of social scientific knowledge about the social micro-level.

Third I will scrutinize Achim Stephan’s idea (2011) of grounding the in-principle-unpredictability of social systems in the observation that even tiny changes on the micro-level of events may lead to drastic, non-linear changes in the development of a given social system. His proposal is based on the assumption that (some) social systems are deterministic-chaotic systems. Arguably, deterministic-chaotic systems are unpredictable (for finite beings like us) because they react extremely sensitive to the smallest differences in their initial conditions. I will attempt to show that Stephan’s proposal is hitherto a mere possibility without much empirical warrant. The main reasons for this is that he fails to establish “sensitive dependence to initial conditions” of social systems in the technical, mathematical sense of the term.

In the last part of my talk I will attempt to add a pragmatic dimension to these results and to sketch some consequences for the viability of reductionist approaches in the social sciences. I will conclude my talk with a brief recommendation for social emergentists who aim to make a case for the unpredictability of social systems, namely that they should put more effort in a serious empirical corroboration of their case (as opposed to getting tied up over the conceptual case for the in-principle-possibility of social emergence due to unpredictability).

Seán Muller - University of Johannesburg

*From ‘data mining’ to ‘machine learning’: the role of randomised trials
and the credibility revolution*

For decades it has been standard in econometrics to use ‘data mining’ as a pejorative phrase. Data mining, in the econometrician’s view, refers to the practice of searching datasets for relationships between variables and only reporting those that are found to be statistically significant. This has two main problems. First, it amounts to covert multiple hypothesis testing that renders the results of reported statistical tests incorrect. Second, it undermines the role of theory – with theoretical explanations being concocted ex post. The latter is fundamentally at odds with the Popperian-inspired model of empirical testing that many economists claim to subscribe to.

The above view of data mining is in marked contrast to that of practitioners in other literatures, such as computer science and engineering, where the concept is deemed entirely legitimate. The reason for the difference appears to have three main components: practitioners in these other literatures are not wedded to the simplistic Popperian model

favoured by economists; the manner in which data mining was implemented in these other fields was transparent and systematic; causal relationships are either less important, or less in question, in disciplines that use data mining.

In recent times, the gap between economics and other disciplines has begun to close.

Influential econometricians have begun to research, develop and use methods that in other disciplines would be called 'data mining' but in economics are now referred to as 'machine learning'. One obvious explanation for the sudden interest in machine learning is the recent availability of extremely large datasets ('big data') in the economic and social sciences.

However, I suggest that there are more important, methodological reasons for this development and, in particular, the role played by researchers who were also among those that championed RCTs.

The use of experimental and quasi-experimental methods in economics was intended to address the lack of credibility of previously-dominant methods for causal identification. In some respects, this was a rupture from the preceding literature, but in others it was a natural progression. On the face of it, randomised control trials (RCTs) represent a diametrically opposite approach to data mining by virtue of generating new data to test for a single, pre-specified, relationship. One could therefore argue that RCT-based approaches are consistent with the prior, negative view of attempting to identify causal relationships by searching for unspecified relationships in existing, non-experimental data.

Given this, the endorsement of machine learning methods by economists who also championed experimental methods seems puzzling; machine learning is in its essence a form of data mining. How can this be explained?

I argue that machine learning with large enough datasets presents advocates of RCTs, and quasi-experimental methods, with the prospect of escaping criticisms of the reliability and broader applicability of findings from such studies.

It remains widely unappreciated that experimental methods, like their predecessors, are also subject to forms of specification searching that undermine the credibility of published results. The most common form of this practice is estimating causal effects for subsets of the original population of interest. I provide a brief explanation of how that can compromise causal identification, statistical inference, or both.

Much as there were methods to address forms of specification searching in the earlier literature, so too have econometric methods been proposed to eliminate, or mitigate, the bias that arises from estimation of causal effects across subgroups.

More important for our purposes is the directly related literature on estimating 'heterogeneous treatment effects'. It is this literature, I argue, that can be used to explain why researchers who might previously have been labelled 'randomistas' are now proponents of machine learning methods: besides their role in analysing 'big data', these methods present a possible escape from some of the criticisms levelled at RCTs. It is the inclination of researchers to downplay criticisms of experimental methods that has led to the apparent oddity of a series of developments addressing matters that were never acknowledged by practitioners as problems. In short: while machine learning has been framed as a development that is largely independent of the debates about RCTs, causal identification and evidence-based policy, this is misleading.

The problem that machine learning has the (theoretical) potential to resolve can be crisply stated: results from randomised trials may lack external validity because the treatment effect estimated varies with the value of other variables. These variables are unknown ex ante and their distributions may vary across populations. One of the claims made by advocates of RCTs – albeit the subject of great deal of criticism and some concessions – has

been that they need not rely on ex ante theoretical knowledge. But with an unknown number of interacting ('confounding') variables the practitioner who wishes to use findings from an RCT to inform policy for a different, or broader, population cannot proceed. A primary motive for the adoption of machine learning methods, therefore, is a discreet attempt to resolve the challenges treatment heterogeneity poses for the external validity of experimental and quasi-experimental work. Machine learning methods enable experimentalists to search a-theoretically for heterogeneous treatment effects, which can be used to shore-up claims of external validity.

Juho Pääkkönen & Petri Ylökoski - University of Helsinki

Humanistic interpretation and machine learning

The objectivity of interpretive text analysis – humanistic interpretation – has been a hot potato in the social sciences since their beginning. The necessity of humanistic interpretation has been generally recognized, but many have retained their suspicions about the sources of bias that could influence the interpretive process. Thus the various attempts to formalize the interpretive process can be seen as attempts to make the interpretive choices more transparent and to control some possible biases. However, these attempts have met with opposition. Coding texts has been argued to be limited in terms of replicability and in its ability to account for nuances in textual meaning. At worst, coding procedures have been argued to impose interpretation on text data, distorting their underlying meaning structures and barring evidence important for forming a well-grounded interpretation (Biernacki 2012).

The development of recent machine learning based tools for text analysis have initiated the most recent debate about the objectivity of humanistic interpretation. While techniques based on supervised machine learning seem to share the same problems as their coding-based ancestors, unsupervised machine learning techniques seem to promise something new. For example, Lee and Martin (2015) argue that what they call cultural cartography – a structural analysis of meaning – will help to make interpretation more scientific. Instead of imposing interpretation on texts, unsupervised text analysis condenses information in text data into a simplified formal representation, which enables collective scrutiny of the represented textual meanings. Notice that nobody claims that computer-aided distant reading will replace humanistic close reading of documents. Rather, the claim is that incorporation of unsupervised machine learning techniques makes the interpretation not only much more scalable, but also helps to avoid many possible biases of interpretation that derive from the interpreters' preconceptions.

However, albeit formal modelling can improve the replicability, transparency and systematicity of text analytics (Nelson 2017), the methodological role of unsupervised machine learning in facilitating interpretation remains unclear. Most unsupervised methods require that researchers pre-specify modelling parameters, which influence how the texts are represented. While computational methods exist for estimating the optimal choice of parameters (Greene 2014), the results of computationally optimal solutions are often not

useful in interpretive research (Chang et al. 2009). At the same time, there is a lack of established practices for evaluating modelling results, with prevalent accounts drawing on vague notions such as interpretability (Jockers 2013) and background understanding of the modelled corpus (Mohr & Bogdanov 2013). Concurrently, calls have been made in the literature for accounts detailing how machine-assisted analysis of large text collections can improve interpretation of smaller text samples (Ignatow 2015), and the extent to which interpretative analysis can be automated (DiMaggio 2015).

In this work, we clarify the methodological role of unsupervised text analytics by a close examination of the uses of unsupervised machine learning in social scientific articles. More precisely, we will investigate prevalent ways of using topic modelling – a popular unsupervised approach among social scientists – in interpretive text analysis and analyse how it is actually used to support interpretation. We will distinguish between two uses of topic modelling and examine how they relate to interpretation as part of the analytical approach. First, topic modelling is used for organizing text materials in order to guide qualitative analysis. In this case, the aim is to make visible latent semantic structures across a text corpus, which can then be further subjected to a qualitative close reading. Unsupervised analysis does this through reducing the dimensionality of texts into a simplified structure, thus facilitating the interpretation of complex corpora. Importantly, in these cases, the evidence relevant for interpreting the discovered structures is produced through a close reading of texts guided by the model, and not by the formal representation produced through unsupervised analysis. This contrasts with the second way of using topic models, where the role of modelling is to measure the prevalence of a theoretical construct in text data. In these cases, the results of unsupervised analysis are used as evidence for the existence of latent meaning structures in a corpus, while close reading mainly serves to validate the modelling results. Thus, in this latter way of using topic models, unsupervised analysis works to shift the methodological role of interpretation to that of validating measured theoretical constructs, which are operationalized in terms of topic modelling results.

We argue that distinguishing between these different uses is important for understanding how unsupervised analysis can both support and steer interpretation of text data. In particular, in the case of using unsupervised modelling for organizing text materials, there is a need to develop methods for comparing different possible representations in terms of how they influence subsequent interpretation. On the other hand, in the case of measuring theoretical constructs, a crucial issue is that of understanding how formal representations can be validated through interpretative reading, and how do different methods of validation turn modelling results into evidence of latent meaning patterns in text corpora.

Alejandro Rosas - Universidad Nacional de Colombia, Philosophy Department

Trapping vs. trusting: common knowledge in apes and human infants

1. Introduction:
 - (1) Apes exhibit joint attention (JA) but, strangely,

(2) Apes hardly show episodes of shared intentionality.

The conjunction of (1) and (2) evokes perplexity, which I sketchily try here to resolve in the conclusion. But most researchers judge 1 false. My main aim here is to show that (1) is true. If I succeed, the resolution presented will enjoy some intuitive plausibility.

2. Joint attention: The metacognitive condition view.

According to a common intuitive comprehension of JA, subjects engaged in it must give signs of knowing that they are engaged in it, i.e., signs of a metacognitive state. In other words, two subjects in JA must not only attend both to the same content; they must both conceive of themselves and the other(s) as minds who have that content in the focus of their attention (Carpenter & Call 2013).

It is sometimes said that JA requires that subjects share a content just for the pleasure of sharing. I think we can agree that pointing to some object or situation with an emotional exclamation: "How pretty!" suggests an intention to share a mental state: pleasure or interest in the object. This requires that both subjects know (i.e., meta-know) they are attending together to the object and enjoying it. So I submit that "aesthetic sharing" is a privileged example of JA because it readily suggests the metacognitive condition. But "aesthetic sharing" is not a necessary condition for JA. JA can also happen, and probably oftentimes does, instrumentally for the sake of some collective action.

3. Can there be unwilling joint attention?

Schiffer ([1972]2002) gives a more general example: facing a candle on the table with open eyes normally leads to knowing that a candle is on the table and to knowing that facing a candle with open eyes leads to knowing that the candle is on the table. Thus, two subjects facing a candle and each other both know that the candle is on the table, and that they both know that they know, etc., that the candle is on the table. JA is naturally taken here to include common knowledge. Human infants can do it. It is not required, not even for adults, to have concepts of knowledge, or mind, or common knowledge, for subjects to be in a state of common knowledge. The ability to be in this state is presupposed in any meaningful social interaction, for it is what agents use as input to determine goals and coordinate collective action.

Schiffer's example of JA does not require an aesthetic interest in the object. It does not even require subjects to want the particular case of JA they find themselves in. JA can happen by accident, even by an undesired accident: for example, when one subject did not want the fact F and her knowing the fact F to be known by the other, and yet finds herself unexpectedly facing the other and the fact F. These are cases of "unwilling" JA. I argue that this is precisely the case in some prominent (unrecognized) cases of joint attention in apes.

4. Apes trapped in JA or using it to trap others

A subordinate ape feels "trapped" in an episode of joint attention with a dominant: trapped, because this episode will cost her no less than a desired piece of food. This happens when a subordinate and a dominant compete for food hidden by

experimenters in semi-natural enclosures (Menzel 1971, Hirata and Matsuzawa 2001). The subordinate witnesses the cache, but sometimes unwillingly gives it away by staring at and/or moving too keenly towards it. The dominant picks up the news through those cues. The dominant now knows, not just the food's location, but the subordinate's knowledge of it. Could it be that the subordinate has no inkling that she gave away the contents of her mind by features of her gaze? An experiment by Katie Hall et al. (2017) in the food-competition paradigm ruled this out. Subordinates seek joint attention with the dominant to a hidden low-value food, as a decoy, to improve their chances of retrieving a high-value food hidden elsewhere. This suggests that the subordinate knows how gaze-cues give away knowledge and intention. She behaves indistinctly in those contexts, from one who had the appropriate concepts. Apes do engage, sometimes unwillingly, sometimes deceptively, in episodes of JA.

5. Trusting instead of trapping

Human infants can also track others' beliefs, true or false, from very early on (Kovacs 2007) and they give signs of JA by their ninth month. But how is JA different in them when compared to apes? Unwilling JA is not common in infants. Rather, we see the opposite: from the first days after birth, the baby engages in face to face interactions with caregivers. Imitation of tongue protrusion (Nagy & Molnar 2004; Trevarthen 2011) and proto-conversations (Bateson 1975) instantiate face to face communication. Communication implies laying the contents of your mind in the open to serve as common knowledge. In this sense, babies seem eager to enter a sphere of common knowledge with others and want to establish a sphere of trust from early on.

6. Conclusion: Inborn openness.

The difference between apes and human infants lies not in their cognitive capacities: both have belief-like states about belief-like states (Buttelmann et al. 2009; 2017). It lies rather in their different attitudes towards opening their minds to others. Apes' sociality seems dominated by hierarchical and strategic attitudes: not the best environment to open your mind to others. Rules of equality are absent. Probably, humans enjoy an inborn attitude of viewing others as equals, of entering a social space of equals, a factor that disposes them to seek episodes of joint attention and the opportunity for communication. The lack of this attitude of equality determines that apes, while being capable of joint attention and common knowledge, give very few signs of a capacity for shared intentionality.

Andrea Salanti - University of Bergamo

All that glitters is not gold: the case of "mainstream pluralism"

The debate on scientific pluralism originated about four decades ago among philosophers of science, mainly as a critical reaction to earlier auspices in their field in favour of the unity of

science. Since then such a debate spread among various disciplines, with economics, starting from the 1980s, being no exception. Discussions about pluralism involved at least three different issues, that is, in chronological order: economic methodology, heterodox schools, and – more recently – mainstream pluralism (or pluralism within the mainstream).

One of my arguments here is that in all these cases advocacy (or, as more appropriate in the last case, practice) of pluralism arose from quite often unrecognized difficulties encountered in dealing with some fundamental problems.

Advocacy of methodological pluralism arose from recognizing the absence of a single conclusive final methodological or epistemological principle suited to economics, as was openly admitted, for instance, by two of its prominent advocates such as Bruce Caldwell and Warren Samuels. The latter, for instance, could not have been more explicit: “The case for methodological pluralism ultimately rests on the necessity of choice in the absence of a single conclusive final methodological or epistemological principle. We have to choose between alternative methodologies each of which has its own internal limitations and there is no single unequivocal, conclusive meta-principle on which to make that choice”. (Samuels 1997, 67). Indeed, if we look over the more recent introductory textbooks, companions or handbooks on economic methodology, apart from many interesting analyses of particular methodological issues, it is difficult to deny that the same conclusions can be agreed today as well as twenty years ago.

Moreover, the persistence of heterodox approaches within the discipline may be regarded, among other things, as one of the consequences of the same situation. However, advocacy of pluralism by many heterodox economists appears to be somewhat instrumental, in the sense that they are not prepared to embrace a theoretical pluralist perspective but only a loose methodological pluralist attitude in order to support and preserve their own favourite strand of heterodoxy.

More recently, the emergence within the mainstream of an assortment of new research programmes (usually identified as classical game theory, evolutionary game theory, behavioural game theory, evolutionary economics, behavioural economics, experimental economics, neuro-economics, etc.) having in common the adoption of research methodologies extraneous to the tradition of neoclassical economics, has been interpreted as the sign of an emergent “mainstream pluralism” (cp, for instance, Cedrini and Fontana 2017, Colander et al. 2004, Davis 2006).

However, in comparison with traditional approaches the recent literature in these “new” subfields (as well as in the “old” ones as presently practiced, for that matter) exhibits at least two remarkable novelties i.e.: 1) an unequivocal (more often implicit than otherwise) reliance on the appropriateness of partial equilibrium analysis, and 2) a comparatively intensified engagement in applied economics.

Scope of this paper is precisely to argue that any appraisal of the so called “mainstream pluralism” ought not to disregard the consequences of these two emerging traits.

Concerning partial equilibrium analysis, it must be firstly noted that discussions of its legitimacy traditionally aimed at singling out the logically admissible accounts of empirical

situations to which it could be applied and those to which it could not. In this respect the problem is to understand of which use such very strict conditions (of one kind or another) can be. Indeed, it is even too easy to observe that they are plainly impossible to be satisfied for any real existing market, and so doomed to identify nothing but “empty boxes”. My tentative suggestion in this respect is to try to rediscover the (Marshallian) virtue of approximation.

Moreover, it is undeniable that “the twenty-first century is the age of the applied economist” (Backhouse and Cherrier 2016, 1), but my readings on these subjects makes me less optimistic about what we may expect from this turn in economic research. I could provide a lot of examples (as the few ones reported in the paper) of applied research that I find quite irrelevant in the sense that they do not add anything to what we should know even before performing such pieces of applied works. By this I mean that, if someone asked me what should be expected as a result, I would had given the right answer simply relying on (economic) common sense. On this matter a good antidote could be an at least partial recovery of a priori reasoning in economics.

Matti Sarkia - University of Helsinki

*Narrative economics as a bridge between epidemiology, the humanities
and the social sciences*

In his 2017 presidential address to the American Economic Association, the recipient of the 2013 Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel, Robert Shiller, called for narrative economics, or “the study of the spread and dynamics of popular narratives, the stories, particularly those of human interest and emotion, and how these change through time, to understand economic fluctuations”. Shiller argued that several prominent economic and political crises of the 20th and early 21st centuries, such as the 1920-21 Depression, the Great Depression of the 1930s and the Great Recession of 2007-2009, may be explained in part by “the prevalence and vividness of certain stories, not the purely economic feedback or multipliers that economists love to model”.

To capture the importance of economic narratives in times of financial turmoil, Shiller proposed a radical expansion of the economic toolbox by bringing economics closer to the humanities and other social sciences. However, he did not advocate giving up the study of economic fundamentals or the real business cycle altogether, nor collapsing economics to literary studies and rhetoric. Rather, Shiller suggested the use of quantitative epidemiological models of disease dissemination and spread, in combination with approaches from the digital humanities, to study the propagation of “infectious” narratives in a population of economic agents.

My paper addresses the scope and resources of the research program of narrative economics that Shiller envisages. First, I will argue that there is a substantive connection between narrative economics and other fields of economic research, because narrative economics deals with the same domains of phenomena as other fields of economics.

Second, I will argue that narrative economics differs from other fields of economics due to its innovative methodological approach, which brings resources from epidemiology and the digital humanities to bear on the subject matter of economic science. Third, I will argue that narrative economics can enrich and deepen contemporary economic research, but that it is unlikely to bring about a revolutionary movement that leads to systematic critique of established economic ideas.

To begin with the substantive connection, narrative economics is concerned with the propagation of narrative structures only insofar as they have an impact on the consumption and investment decisions of individual economic agents. Fundamentally, narrative economics therefore deals with the choices of agents made under conditions of relative scarcity, which forces them to choose between mutually incompatible action alternatives (Robbins 1932). This being said, the epidemiological models that narrative economics uses are applicable to the propagation of economic narratives only at the level of the population, rather than at the level of the individual agent. Thus narrative economics zooms away from the psychological mechanisms that make some narratives particularly contagious (prone to be remembered) or easy to recover from (forget), while recognizing the influence that these features may have on the magnitude and dynamics of the narrative epidemics in question.

The epidemiological models that Shiller brings to bear on the subject matter of narrative economics are based on the compartmental SIR model of disease epidemics, which was originally developed by Kermack & McKendrick (1927). The model divides a population into the three compartments of susceptible (S), infective (I), and recovered (R) individuals. To capture the dynamic spread of an epidemic in a population, we also need the contagion rate c and the recovery rate r . A simple version of the SIR model can be represented by the following three formulas:

$$dS / dt = -cSI$$

$$dI / dt = cSI - rI$$

$$dR / dt = rI$$

The formulae say that the change in the number of susceptibles is an inverse function of the contagion rate times the product of the number of susceptibles and the number of infectives. The change in the number of infectives is in turn a function of the product of the number of susceptibles and the number of infectives minus the number of recovered individuals. And the change in the number of recovered individuals is a function of the recovery rate times the number of infectives. What matters for the dynamics of an epidemic is the ratio c/r , i.e. the rate at which the disease spreads to new susceptibles relative to the rate at which infectives recover and are no longer liable to spread the disease. Alternative variations of the SIR model can be formulated by allowing for mutation and reinfection, or by toggling the values of the parameters in the model.

Shiller's central methodological insight is that instead of the spread of a disease, we may interpret the SIR model to refer the transmission of ideas when popular economic narratives "go viral". However, not all of the rich semantic content and sequential temporal structure of prototypical narratives can be accommodated by the quantitative variables in the SIR model. Not even the methods of the digital humanities, such as text analytics and semantic search, seem to be fully adequate to this task. Thus I will argue that in order to capture the

content and structure of economic narratives, we also need qualitative methods from the humanities and social sciences, including historical and contemporary case studies, process tracing, and policy analysis.

Third and finally, I will argue that the research program of narrative economics is related to (but goes beyond) earlier ideas about the reflexivity of the economic agent and ways in which feedback effects that are based on self-reinforcing expectations may cascade through an economy, giving rise to financial booms and busts (Frey 2017; Soros 2013). Given the ways in which it regimented these ideas, narrative economics seems to be in a position to enrich economic science by uncovering previously unidentified mechanisms for important economic phenomena such as asset price formation, the fluctuations of the business cycle, and the development of economic and political institutions, as well as the downstream effects that these may have on political society at large. However, this is not because narrative economics questions some fundamental assumptions about the nature or subject matter of economic science. Rather, it is because it taps into new types and sources of data that economists have not hitherto explored, and uses innovative methodological tools that are tailored to these data.

Ute Schmiel - University of Duisburg-Essen

Economic analysis of law beyond the mainstream approach – a methodological view

The key question of the economic analysis of law is how to design legal rules, e.g. how to design tax law, competition law, property law, or contract law from an economic perspective. Giving an answer to whether certain legal rules are adequate requires a methodology for the economic analysis of law. According to the mainstream approach of economic analysis of law, legal rules should be deduced from the general value of neoclassical efficiency (Schäfer & Ott, 2004; Posner, 2007). However, we can find the objections to the mainstream approach that efficiency is a problematic value (Baker, 1975; Cooter, 1989; Mathis, 2009), and that the underlying theory contradicts reality (Sunstein, 1997; Mathis, 2009). Despite this criticism, there is, as yet, no alternative concept of the economic analysis of law. Because of that, the present paper aims to close this research gap and to develop a methodology of economic law analysis beyond the mainstream approach. In contrast to the mainstream approach which refers to 'as-if-instrumentalism' (Friedman, 1974), this paper takes a critical-rationalist perspective. Critical rationalism is established in social science (Radnitzky & Andersson, 1978; Cheyne & Worrall, 2006) but not in economic analysis of law. In particular, the paper uses a critical rationalist methodology based on Albert (1985, 1999) because Albert applies critical rationalism to social science. Furthermore, while others focus on epistemological questions of critical rationalism (Miller, 1994; Psillos, 2006; Sankey, 2006), Albert uses critical rationalism also in politics and economic analysis of law is part of politics.

Compared with the mainstream approach, the approach presented here differs in three main points. The first difference deals with the structure of the economic analysis. The mainstream approach accepts the general value efficiency and, because of this,

automatically accepts any legal rules that support efficiency and, in fact, their secondary effects (Posner, 1979). In contrast, a critical rationalist perspective denies undisputable values. Because of this, the mainstream top-down deduction and an automatic acceptance of legal rules do not work. Instead, economic analysis of law has to ask if a value (e.g. efficiency) is compatible with other values (e.g. with justice, freedom of choice). Since values are often not definite, we firstly have to interpret them. Furthermore, according to Albert's feasibility postulate that 'ought implies can', values are only adequate if they are realizable. If we can support by argument that the thus interpreted value complies with other values and is realizable, we have to analyse whether legal rules fit in with this interpretation of the value and we have to ask if the legal rules and their secondary effects are compatible with relevant social values (e.g. justice). Furthermore, we should compare the legal rules with other rules with regard to these points and we have to ask if they fulfil these requirements better than other legal rules (Albert, 1985, 1999).

Both, the interpretation of values and the critical discussion of values and rules have an empirical dimension and an ethical dimension. The second difference between the approach presented here and the mainstream approach refers to the empirical dimension. As we saw above, the empirical dimension means to ask if values are realizable and if legal rules cause secondary effects. To examine this, we need theories that are adequate in a critical-rationalist sense. The mainstream approach of economic analysis of law refers to neoclassical market behaviour and market theory. Yet, the theories of perfectly rational behaviour and perfect competitive markets do not fulfil the critical rationalist requirement that assumptions and hypotheses should be empirically confirmed (Albert, 1985; Gadenne, 2006). The first breach of critical rationalist rules is that the assumptions of neoclassical theory are neither fulfilled nor realizable. The second breach of critical rationalist rules is the missing possibility to explain many of the events we observe in reality, e.g. innovations and learning of individuals, bankruptcy, the use of money, or the existence of institutions such as firms (Shubik, 2007). In contrast to the mainstream approach, the approach presented here refers both, to the resource dependence approach (Pfeffer/Salancik, 2003) and Fligstein's market approach (Fligstein, 1996, 2001), which fulfil critical rationalist requirements.

Thirdly, the concept presented here differs from the mainstream approach regarding the ethical dimension. As we saw above, economic analysis of law has an ethical dimension because values need to be substantiated and values, legal rules and their secondary effects need to be compatible with other values. Because of the ethical dimension, economic analysis of law needs ethical concepts. The mainstream economic analysis of law dogmatizes efficiency and justifies this with the argument that efficiency is also in the individual's interest (Schäfer & Ott, 2004). Besides the fact that we do not have reasons to dogmatize any value from a critical rationalist perspective, we object that efficiency is not realizable and, in addition, we can-not find harmony between individuals' and common interests beyond perfect markets. According to the view presented here, freedom of choice and equality before the law are relevant values of a market order. In contrast to a neoclassical perspective, they are not instrumental but intrinsic values (Sen, 1988; Vanberg, 2014). It is obvious that we have to analyse what freedom of choice and equality before the law mean in detail. Important questions are whether we should interpret freedom only as negative freedom or also as positive freedom (Sen, 1988; Berlin, 2002a; Berlin, 2002b) and how freedom of choice is limited because of other social values. Therefore, we have to ask if aggressive tax planning, child labour, or other actions that are legal according to the relevant (foreign) laws also are legitimate. Thus, we need ethical concepts and we have to apply them

to economic issues.

In sum, economic analysis of law needs methodological rules, empirically confirmed theories and ethical concepts. As we will see, the approach presented here links economic analysis of law in particular and economics in general to other social sciences and to ethics.

Enrico Terrone - Universitat de Barcelona

Against Institutional Monism

I term 'institutional monism' the thesis that the distinction between institutions and practices is only a matter of degree. This view is endorsed by outstanding accounts in contemporary social ontology, for instance Searle's (2010), Epstein's (2015) and Guala's (2015). They all favour institutions as paradigmatic social entities, and treat practices as rough institutions, which lack formalization. For example, the university is an institution governed by a system of formalized rules, while friendship is a practice because the rules that govern it lack formalization. My aim is to criticize institutional monism and put forward a dualist theory, which takes institutions and practices as two distinct (albeit related) ontological kinds. For this purpose, I rely on the distinction between the attitude and the content of a mental state.

The content concerns what is represented by an occurrence of a certain type of mental state, whereas the attitude concerns how that type of mental state represents. If, for instance, I remember that the party was boring, I am representing that party as boring; therefore, 'being boring' is part of the content of my mental state. Yet, in remembering, I am also representing that party as past, but this is not part of the content of my mental state; I am not representing that party as being boring and as being past. In my experience, 'being boring' and 'being past' are not at the same level. The former is a feature that this memory state attributes to its object but might have not attributed to it, whereas the latter is an architectural constraint of the memory state as such, something built into its very nature. One cannot help but remember things as being past. Even though we use the preposition 'as' in both cases with the purpose of describing what is going on in the mind, there is a crucial difference between 'representing the party as boring' (content) and 'representing-as-past the party' (attitude). Following Kriegel (2005), I take the latter notation (representing-as-F x) to designate an attitudinal feature and the former (representing x as G) to designate a content feature.

I contend that there is a specific "normative attitude" that represents-as-due its content. Arguably, the English language lacks a term to designate it. We use sometime 'expectation' in this sense, but that term also has other meanings. This may perhaps explain the difficulty we have in recognizing the normative attitude, but it must not prevent us from identifying it. For instance, one represents-as-due queuing at the supermarket, and in this way one constrains one's behaviour (I represent-as-due that I queue) and evaluates the behaviours of others (I represent-as-due that other people queue).

Representing-as-due a certain content is what differentiates the normative attitude from mental states such as beliefs, which may have a normative content. In fact, representing-as-due queuing is not the same as representing-as-true that queuing is due. The former is a normative attitude whereas the latter is a belief that has a normative content. Being due is

inherent in the normative attitude whereas it is not inherent in beliefs. The latter enable the thinker to decide whether or not to treat something as due whereas the former automatically attributes the feature of being due to its content. Therefore, a normative belief must conceptually articulate the feature of being due thereby ascribing it to what one is thinking, whereas entertaining the normative attitude is sufficient to make its content due.

That being the case, I conceive of a norm as a prescription that derives from the convergence of normative attitudes on the same content. For example, the norm of queuing arises from the convergence of normative attitudes that represent-as-due queuing. A norm does not need to be supported by mental states, because it is already constituted by normative attitudes. The norm exists simply because various people represent-as-due a certain behaviour. It is the process itself from which the norm arises that enforces it. Then, I conceive of a rule as a prescription that derives from the convergence of non-normative attitudes (e.g. beliefs, desires, intentions) on the same normative content. Consider for instance the traffic rule that mandates one to stop at the red light. Unlike a norm, which supports itself in virtue of the normative attitudes that constitute it, a rule must be supported by cognitive mental states that are not essentially connected to it. For instance, the members of the society must recognize that stopping at the red light is due. Finally, I conceive of a practice as a system of norms, and of an institution as a system of rules. While a practice is a system in which one participates by representing-as-due certain behaviours, an institution is rather a system to which one adheres by recognizing that certain behaviours are due. This is what makes practices and institutions different from an ontological point of view.

As a consequence of this, practices and institutions require different ontological accounts. Here is where institutional monism falls short of the complexity of the social world. For instance, Searle (2010, 96) states that in order to construct a social fact “you must have a conceptual apparatus rich enough to represent deontology”, but this can fit only with institutions, not with practices, in which the normative attitude allow to enforce norms without the need of “a conceptual apparatus rich enough”. Likewise, Epstein (2015, 80) states that social facts are “anchored” in mental attitudes that establish the “frame principles” by which those facts are “grounded”. Yet, one again, this can fit only with institutions, not with practices, in which the normative attitude directly grounds social facts instead of limiting itself to anchoring them by establishing frame principles.

Mark Theunissen – Independent Scholar

Data-Centrism for the Social Sciences

In her recent book *Datacentric Biology* Sabine Leonelli brings into focus the concepts of ‘data-centrism, ‘data journeys’ and ‘data curation’ as central for the current knowledge production in the life sciences (Leonelli, 2018). The central claim is that the ubiquity of big data and tools for its integration and analyses across research fields challenges our understanding of data as relatively passive resource providing evidence for a given scientific hypothesis. Rather, “data-centrism” is the emergence of practices of data storing, integrating, sharing, and analyzing as central to the knowledge production of the life

sciences itself. This is especially important in practices of disseminating previously discarded data and the re-using of such data in different research contexts. Data-centrism results in new research programs, academic institutions as well as specific institutional responses from political and market stakeholders. Leonelli's emphasis on understanding data in terms of "data journeys", as moving between those that produce data, use it, interface with it and those that re-purpose it, gives a central place to the role of data curation, the management of data journeys as effective, i.e., productive of scientific knowledge. Hence, practices that previously were viewed as secondary belonging to the realm of technological service and support for proper scientific research, it has now become its own research expertise with a central role in the development of interdisciplinary research in the life sciences.

This paper re-contextualizes Leonelli's account of data-centrism for the life sciences to the different social sciences. This way we can hope to establish see which of the traditional social sciences are or will be least or most amenable to the idea of data-centrism and to see how far the notion can be made applicable across disciplines in the social sciences. In doing so I start with work that is already done, for instance Allison Wyle's work on the use of data in modern archaeology that brings us closer to challenges we might meet in the social sciences, the concrete challenges of using new data science in sociology encountered by early practitioners in the field (e.g., Kieran Healey, Justin Farrell et al), but also development in the digital humanities from within and without the humanities.

More generally, the paper aims to lay out what data-centrism, data-journeys and data-curation might look like in different social sciences ranging from anthropology to psychology to economy. Although this is only a general exploration of similarities and differences between different disciplines in their understanding of data and practices surrounding data management, it aims to establish at least the following.

- 1 Provide an overview of significant differences between the place of data in the social sciences and life sciences. Differences in what counts as data and theory based on data, but particularly in the way or degree to which data is valued as ranging from scientific to political.

- 2 Discuss the necessity of data curation as understood by Leonelli as a specialty within different social scientific disciplines. Does the role of a data curator already exist in some social scientific disciplines and should it take on a more central role in knowledge productions in different disciplines?

- 3 Finally, more speculatively, I argue that data curation understood as central in de-contextualizing data from one site of production and research in order to allow it to travel and to be reinterpreted by new users in other research contexts carves out specific fields of expertise for the different social sciences that do not yet explicitly exist but have the potential to address some of the central problems haunting many of the social sciences in both practice and theory.

Rethinking Social Agents

Introduction

The primacy of structure or agency as the main factors in shaping human behaviour is at the centre of a standing debate in the social sciences. Briefly, agency refers to individuals and their capacities to act upon their own goals, while structure refers to the social setting that influences the choices of individuals. On the assumption that agency is closely related to thinking, I challenge the dichotomy between 'Structure' and 'Agency' by arguing that both conscious (conceptual thinking) and unconscious (intuitive thinking) determinants of sociologically non-trivial actions are contingent upon social conditioning. Thus, I argue that agency is itself structured and hence the Structure Vs. Agency debate should at best be recast in terms of simple sociologically-trivial actions – the only kind of actions that we can actually perform autonomously and independently from structure.

Socialization & agency

Successful social interaction requires internalizing existing social norms and socially acceptable behavioural patterns – a process known as socialization. Socialization significantly influences reasoning and significantly compromises agency, as the following syllogism suggests.

1. Any agency-related claim in the social sciences is a claim about wilful action.
 2. Any wilful action is the result of a free choice.
 3. Any choice is the result of reasoning.
 4. Reasoning is thinking.
 5. Thinking is structured.
 6. If thinking is structured (5), then agency is itself structured.
- C1. The Structure vs. Agency debate concerns only simple sociologically trivial actions.
C2. Understanding the nature of social behaviour presupposes a prior understanding of the nature of agency.

Clarifications

Admittedly, there are instances of thinking that are not the result of rational processes (P3 & P4). For instance, it is often argued that the importance of reflection in decision-making is overestimated (cf. Dijksterhuis & van Olden 2006). Moreover, our inferences about things in our environment that are unknown to us are systematically error and biased prone, as they are based on 'quick and dirty' heuristics that provide computationally cheap solutions to complex theoretical and practical problems (cf. Kahneman, Slovic, and Tversky, 1982). In this sense, conscious reasoning might not be our only guide to decision-making. Nevertheless, structural features do influence decision-making processes so long as intuitions and heuristics are unconscious stored information influencing thinking that ultimately derive from our perceptual encounters with our sociocultural and physical environment.

On structured thinkers

Most often, concepts are construed as the building blocks of thoughts and as such play an important role for agency. Concepts are learned through experiences with instances of a

given kind, and specifically once the overlapping similarities across representations of these instances are selected, while the specific details and differences between them are ignored. The process on the basis of which this happens is called 'Abstraction'. Based on a similar process, we learn concepts that do not pick out tangible entities, like DEMOCRACY, by appealing to their linguistic labels and by tracking down the ways in which they are used in a given linguistic community.

In more detail, upon encounter with the first instance of a given kind, e.g. a dog, a representation is formed and stored in memory. When confronted with a subsequent instance of the same kind a further representation is formed, while a scanning process is initiated and a match is sought for in the subject's memory. Once a match is found, the stored representation becomes activated and drives selective attention in a top-down manner to the same parts of the currently perceived instance. For example, once a match is found when looking at a subsequent instance of a dog, the stored representation of a dog, will drive selective attention to the dog's overall shape as well as other parts of the current instance, like its head, legs and tail. This leads to formation of representations of the same parts of the perceived instances. Importantly, these top-down influences also drive storage of subsequently represented information to the same locus in memory alongside the original one. After a number of encounters, a bundle of representations becomes stored in a given locus in memory.

Abstraction picks out representations of the most commonly occurring features within a specific locus, in virtue of selecting information that is connected with each other with stronger connections. This claim is based on the ubiquitously accepted Hebbian rule of learning (Hebb 1949) according to which the connections between neurons that become frequently co-activated become stronger. On the intuitive claim that members of a given category are similar to each other, the neurons that ground perception of same-category-members will inevitably be associated with stronger connections due to these 'intra-category' similarities. The output of the abstraction process is a(n abstracted) representation that signifies the superordinate category, e.g. the set of all dogs.

Structured reasoners

Against the background of how concepts are acquired, I turn to how concepts operate in thinking processes. Concepts are associationistic in their causal patterns. As a result, all thinking, including what we experience as spontaneous thinking, is actually associative thinking. In this sense there is no thought without prior thought. This view of associative thinking builds upon three notions of associations.

- First, there are associations between a given concept and the appropriate word. e.g. the concept TREE with the word 'tree'.
- Second, there are associations between different concepts, e.g. GAME and BALL.
- Third, there are associations between different words, e.g. 'salt' & 'pepper'.

In virtue of these associations, concepts form network-like structures in the mind. Once a given concept becomes activated, those concepts that are strongly associated to it become sub-activated. The sub-activated concepts drive selective attention to specific aspects of the representational 'net' and in this way influence thinking patterns. Given the perceptual and sociocultural origins of these representations, thinking builds upon internalized structural features.

Thus, thinking – the very medium of rational thinking – and in turn agency are both

structured. Hence, we could possibly acquire a better understanding of social behaviour by focusing on the ways in which structure is internalized and how it influences agency.

Leonidas Tsilipakos - University of Bristol

Re-description and the posing of sociological problems

This paper contends that philosophical and sociological fascination with explanation is often misplaced, being logically secondary to the neglected topics of correct description and the well-formedness of research problems. In fact, given the chronic dissatisfaction, even by practitioners themselves, with the results of sociological inquiry, attention must be reasonably directed at the actual posing of sociological problems. I examine this by, firstly, identifying the close connection between the question of articulating research problems and (re-)describing actions and events, arguing that there are important limits to re-description. I then proceed to show that Wittgenstein's position on what philosophical problems are like can throw light on sociological problems too. I end by scrutinizing recent attempts by sociologists to turn 'the emotions' into an object of sociological research, focusing on the rationale provided for doing so and the arising definitional, conceptual and methodological issues.

Paul Roth's recent efforts (2017a, 2017b, 2017c) to revive philosophy of history in the analytical tradition evince an attempt to take the distinctive features of the social sciences seriously, moving beyond the constraints of natural-science-orientated models to focus on narrative explanation as the legitimate prerogative of historiography. Roth's account centres on epistemic aspects of narratives rather than on their formalistic features (White, 1975), i.e. on narratives as justifications offered 'under a description' (Anscombe, 1979; White, 1979; Roth, 2002; Sharrock and Leudar, 2003). He proposes that such justifications are governed by the interconnected principles of 'non-detachability' from the narratives they are embedded in which, in turn, are non-aggregable since, finally, they cannot authoritatively fix a standardized form of description. According to Roth who is following Danto (1965) and Mink (1968), this fact allows for the endless proliferation of re-descriptions, narratives and explanations. In Mink's account this is shown in the manoeuvre Danto introduces of separating an explanatum which is a general re-description of the more specific explanandum. Now, evidently, this is an issue which is central not only to history but to all the social sciences, being tied to the turning of an action or event as ordinarily described, through re-description, into a theoretical object for explanation. Consider, for instance, the following:

The summum of the art, in social science, is, in my eyes, to be capable of engaging very high "theoretical stakes" by means of very precise and often mundane empirical objects. We tend too easily to assume that the social or political importance of an object suffices in itself to grant importance to the discourse that deals with it. What counts, in reality, is the rigor of the construction of the object. I think that the power of a mode of thinking never manifests itself more clearly than in its capacity to constitute socially insignificant objects into scientific objects. (Bourdieu & Wacquant 1989: 51)

Yet what both Roth and Bourdieu do not appreciate is the fact that there are important limits to re-description or, what amounts to the same thing, theoretical construction of the object, limits which point to the core of the issue of how social science can pose well-formed problems through re-description. If the record of social science achievements is anything to go by (cf. Davis, 1994; Mouzelis, 1995; Weinstein, 1997; Savage and Burrows, 2007; Hollands and Stanley, 2009; Manza et al., 2010) it seems that there is an important gap in understanding of this matter. I submit that Wittgenstein's angle (e.g. 2005: Big Typescript Section 98, §406-7) on the posing of philosophical problems can forcefully bring to the fore the difficulties sociology faces in this regard.

Using a common diagnosis for philosophy's and sociology's ills rests on much more than superficial similarities. How this is so can be seen by following a two-step procedure. First, taking it that a discipline's activities are captured to a significant extent in the kinds of problems that it deals with and, second, locating a kind of problem that two disciplines have in common, i.e. saying that sociology shares a kind of problem with philosophy as, for instance, Winch (2008[1958]) argued with regard to conceptual problems. Apart from centring discussion on the issue of correct description, the import of Winch's arguments is to emphasise how problems are frequently misrecognised and paired with methods unsuitable to their nature (Hutchinson et al., 2008; Tsilipakos, 2016; Pleasants, 1999; Gunnell, 2011;). Following this line of reasoning while also taking into account the facts of disciplinary practice, which present sociology as a shelter for a varied group of problems, worries and concerns, leads to the realisation that the mis-pairing may apply to these kinds of problems too. The suggestion is made that sociologists might be pursuing ethical, political, conceptual and other kinds of problems by inappropriate means.

Thus, moving further away from abstract discussion, and turning to look at social scientific reasoning, I will end by examining particular problem posing strategies in the sociology of emotions (Barbalet, 2001; Burkitt, 2014; Bericat, 2016) which, as the appellation indicates, attempts to get an explanatory programme off the ground by turning 'the emotions' into a sociological object on which research questions can be posed. I will show how a central tension is generated through problems of re-description, demonstrating the importance of posing well-formed problems prior to asking what kinds of explanations may be appropriate.

Walter Veit - University of Bristol

Who is afraid of Model Pluralism?

Economic models are often criticized for being false or at least unrealistic, relying on a variety of highly abstract and idealized assumptions. Despite this, economists continue to show confidence in their models and their ability to explain real-world phenomena. Philosophers, hitherto, may have been exceedingly critical of such confidence in highly idealized models by solely focusing on the relationship between a single model and its relationship to the real world. Only recently have philosophers of science started to shift

their focus to sets of models, rather than single models as such (see Weisberg, 2007; Ylikoski & Aydinonat, 2014; Aydinonat, 2018; Grüne-Yanoff & Marchionni, forthcoming). In the following, I am going to argue that even though the roles multiple models play has been starting to get recognized, philosophers still underestimate the full explanatory potential of model pluralism.

In the ensuing new focus on multiple models in the philosophy of science literature, Grüne-Yanoff and Marchionni (forthcoming), similar to Aydinonat (2018) enthusiastically greet and attempt to strengthen Dani Rodrik's (2015) call for multiple models in his recent book on economics. Rodrik, an economist, argues that economic models due to the complexity of social phenomena have several limitations and hence calls for a kind of pluralism, where a different purpose or modelling goal may require a different model with a better fit to a specific modelling goal. For anyone familiar with modelling in biology, this thesis is anything but new. In an article as old as 1966, Richard Levins argued that among multiple goals one may have in the creation of a model, himself focusing on generality, realism and precision, only two can be maximized. Due to inherent trade-offs between different modelling goals, there cannot be an all-purpose model, perhaps not even for one specific research question, at least not one understandable by cognitively limited agents as us. We will often be better off with a set of models illuminating different aspects of the phenomena. Weisberg (2007) refers to this strategy of using multiple models as "multiple model idealization" arguing that it provides a more accurate picture of model-based science, especially when it comes to modelling complex phenomena. The latter reading is what I take to be the takeaway Aydinonat (2018) draws from Rodrik, with sets of simple models perhaps even outweighing the explanatory power of particular complex and more realistic models. If Rodrik (2015) is taken as a criticism of contemporary economic practice and a suggestion for improvement, then it is a lesson that could have been learnt at least 60 years ago by looking at insights from biological modelling practice. In my talk, I am going to focus on lessons we can learn from evolutionary game theory, where the use of multiple models is typical, suggesting that model pluralism is nothing to be afraid of.

Though enthusiastic about the explanatory power of using multiple models, Grüne-Yanoff and Marchionni (forthcoming) warn of an "embarrassment of riches" when the multiplicity of models makes model selection for specific purposes difficult. The selection of the appropriate model is an important question, but I am going to argue that it is not a specific challenge to a pluralistic account of models. If anything it is a source of strength calling for the use of multiple models where there is no clear answer as to which models would serve a specific modelling goal better. Furthermore, my defence of model pluralism exceeds even the "cluster of models thesis" by Ylikoski and Aydinonat (2014). While they focus on the semantics of what scientists mean when they refer to a particular model by including the 'offspring' of a particular model, I focus on the question how competing explanations can be supported by sets of highly diverse and even incompatible models. I argue that there can be large sets of models without common origin and perhaps even completely different structure, but with a similar causal interpretation of the results. After all, modellers often independently create similar models from scratch complementing each other. A common origin, then tells us nothing substantial, except perhaps how to categorize and call particular sets of models. Referring to them as a "family of models" might then be rather misleading. Instead of competing models, one should focus on competing explanations, each supported by a set of highly diverse and often completely unrelated models, support that can rise the

explanatory power of a hypothesis often substantially. Model pluralism is nothing to be afraid of.

Louis-Étienne Villeneuve - Université Paris 1 Panthéon-Sorbonne / Université du Québec à Trois-Rivières

Cultural Historiography and Information Theories

The main goal of this presentation is to look over recent cultural historiography practices in the light of information theories, following Aviezer Tucker's philosophical framework set in *Our Knowledge of the Past: A Philosophy of Historiography* (2004). This survey highlights three problems:

- i. Information-transmitting chains studies do not appear to be the best model available to understand how cultural historians work when they try to explain past cultures.
- ii. Neo-bayesian probabilistic evaluation of cultural hypotheses is conditional to background acceptance/denial of cultural determinism, which reduces (or even nullifies) the possibility to make cultural/non-cultural explanations compete.
- iii. Theorizing on cultural configurations leads either to poor cognitive value on Tucker's simplicity-complexity/accuracy-scope scale, or, even worse, to non-informative and unnecessary conceptualizing.

These three problems are considered here good reasons to believe that cultural historiography, when it comes to explain past cultures and not just to describe its historical manifestations, cannot fit the "science of the past" categorizing suggested by Tucker. I will try then to make the point that this is rather a problem for cultural historiography than for information theory-based philosophy of historiography. This may even entail that cultural historians are not justified to argue with others for any of their claims when it comes to explain past cultures, even if they subscribe to non-relativist constructivist epistemology, like Junni-Matti Kuukanen's postnarrativism (2015). The point here is that without the possibility to build a solid information theory where you can trace the relations between culture and evidences, cultural historiography is condemned to be, as an explanatory discipline, nothing more than an ongoing essay on history.

Andreas Vrahimis - University of Cyprus

Philosophy, Social Science, and the Myth of the Given: Neurath's reply to Horkheimer

Recent interest in the history of the divide between analytic and continental philosophy has tended to focus on a particular continental school, namely the European phenomenological tradition. Exemplary of such an approach is Michael Dummett's (1993) attempt to define analytic philosophy. Despite the many problems faced by Dummett's effort to define analytic philosophy, the idea that phenomenology has an exceptional role to play in relation to the analytic-continental divide is not arbitrary. From Frege to Carnap and Ryle, there is a

long line of early analytic criticisms of 'continental philosophy' which were directed against work undertaken within the phenomenological tradition. It is thus no wonder that many works which followed Dummett's account have tended to refine, rather than radically challenge, Dummett's image of early analytic philosophy gradually diverging from phenomenology.

One example of such refinement is that undertaken by Michael Friedman's *A Parting of the Ways* (2000), which positions Neo-Kantianism as a mediator between these two poles. In particular, Friedman has shown that an attempt to formulate a response to the purported decline of Neo-Kantianism underlies the clash between Heidegger and Carnap in the 1930s. In Friedman's account, Ernst Cassirer's defence of the Neo-Kantian legacy is the middle ground between the two extremes marked by Heidegger and Carnap. Nonetheless, despite the additional refinement of Dummett's picture provided by Friedman, this new account still culminates in what his title names a 'parting of the ways' between analytic philosophers and phenomenologists, leaving an open question as to how this applies to a generalisation about so-called 'continental' philosophy.

A hint of how to put the pieces of the puzzle together has been provided by accounts of the troubled relationship between early analytic philosophy and an altogether different strand of 'continental' philosophy, namely the Frankfurt School. Dahms (1994) as well as Uebel and O'Neill (2004, 2018) have been among the few scholarly attempts to investigate the intriguing history of the failed attempt at collaboration between the Vienna Circle and the Frankfurt School during the 1930s. The attempt, led by Max Horkheimer and Otto Neurath, had resulted in Horkheimer's well-known critique of positivism in his 1937 'The Latest Attack on Metaphysics'. Here, Horkheimer launches a vehement polemic against what he conceives as logical positivism (or rather, as this paper will demonstrate, a caricature thereof).

Horkheimer's attack relies on a conception of a divide in his contemporary philosophy quite different from that invoked by subsequent depictions of the analytic continental divide.

Horkheimer sees philosophy at his time as divided between two poles. He names these two poles 'metaphysics' and 'scientivism'. He traces both elements back to Descartes' dualism, which he sees as an attempt to disenchant scientific enquiry into the *res extensa* while at the same time retaining a mental excess which cannot be disenchant. The lineage which derives from Descartes is seen by Horkheimer as culminating in an impasse in his contemporary philosophical scene's division between metaphysics and what he calls 'scientivism'. Scientivism, which is the main target of Horkheimer's critique in 1937, is supposedly exemplified by Logical Positivism. In brief, scientivism for Horkheimer is equivalent to a metaphysical position according to which there is no possible object of knowledge outside strictly speaking empirically acquired knowledge. Horkheimer proceeds to argue, against this strawman, that positivism buys into a version of the myth of the given according to which the givenness of facts cannot be further questioned.

Horkheimer draws far-reaching conclusions from this caricature of positivism. For Horkheimer, it means that positivism is bound to a type of conservatism prohibiting the radical questioning of appearances. From there, Horkheimer leaps to an identification of positivism with the silencing of critical Reason, and even to the declaration of the compatibility of positivism with authoritarianism. The 'conservative' nature of positivism is contrasted by Horkheimer with his own conception of critical Reason. Horkheimer calls for a kind of meta-scientific theoretical endeavour that is free from the limitation of being derivable from the given, and thus able to approach the social praxis that conditions givenness. In 'The Latest Attack on Metaphysics', this is conceived as being both able to subvert authoritarian metaphysics and also to critically react to empirical science.

Having presented Horkheimer's attack, this paper will focus on discussing Neurath's unpublished reply to Horkheimer. The reply remained unpublished due to Horkheimer's refusal to publish it in the *Zeitschrift für Sozialforschung*. In it Neurath's develops a defence of 'Logical Empiricism and the Unity of Science' (Neurath p.4). The primary task at hand is to analyse the arguments employed in Neurath's little-discussed response to Horkheimer. Neurath's defence helps dispel some of Horkheimer's misconceptions of positivism. Neurath shows that once Horkheimer's objections are reformulated in empirically-minded terms, they point towards a genuine problem concerning science's relation to social praxis. For Neurath, the problem can be addressed within empirically-minded investigations in the history and sociology of science (rather than, as Horkheimer would have it, from a standpoint 'above' science).

Neurath's debate with Horkheimer over the relation between social science and philosophy constitutes a significant, though generally overlooked, precursor to subsequent polemics between the Frankfurt School and analytic philosophy. Horkheimer's *The Latest Attack on Metaphysics* had introduced the critical rejection of positivism as a central element in what he would come to call 'Critical Theory'. Proponents of the latter, such as Adorno and Marcuse, would later follow Horkheimer in an increasingly vehement opposition to what they considered to be positivism, leading to their polemical exchange with Popper's 'Critical rationalism' in the *Positivismusstreit* during the 1960s. Thus Horkheimer's article, Neurath's reply, and their failure to collaborate, constitutes a significant, though often ignored, step in the development of the history of the analytic-continental divide.