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Randomization and process tracing

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

For decades, ‘data mining’ has been used in econometrics as a pejorative phrase. In the econometrician’s view it refers to the practice of searching datasets for relationships between variables and only reporting those that are found to be statistically significant. This amounts to covert multiple hypothesis testing, rendering the results of reported statistical tests incorrect. It also undermines the role of theory, with theoretical explanations being concocted ex post – an approach fundamentally at odds with the Popperian-inspired model of empirical testing that many economists claim to subscribe to.

The economist view of data mining contrasts with other disciplines, such as computer science and engineering, where the concept is deemed entirely legitimate. There appear to be three main Reasons for this difference: other disciplines are not wedded to the simplistic Popperian model favoured by economists; data mining is implemented in these other fields in a transparent and systematic manner; causal relationships are less important, or less in question, in disciplines that use data mining.

Recently, however, influential econometricians have begun to research, develop and use methods that correspond to ‘data mining’ in other disciplines, but referred to as ‘machine learning’ in economics. An obvious explanation for this development is the increasing 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 have championed RCTs.

Experimental and quasi-experimental methods in economics were intended to address the lack of credibility of previously-dominant methods for causal identification. 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. RCT-based approaches are therefore consistent with the prior, negative view of attempts 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 championed experimental methods is a puzzle; machine learning methods overlap with data mining techniques. How can this be explained?

I argue that machine learning presents advocates of experimental 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 are also subject to forms of specification searching that undermine the credibility of published results. The most common such 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. The related literature on estimating ‘heterogenous treatment effects’ provides the link that explains why researchers who previously may have been labelled ‘randomistas’ are now proponents of machine learning methods.
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 a 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 atheoretically for heterogenous treatment effects, which in turn can be used to shore-up claims of external validity.

Judith Favereau. From "Economists as Plumbers" to Economists as Surgeons

Randomized field experiments (RFEs) have been massively promoted and implemented in development economics, mainly through the Jameel Abdul Latif Poverty Action Lab (J-PAL) at the Massachusetts Institute of Technology (MIT). The main argument for such a promotion is that RFEs enable to produce evidence of development programs efficacy. J-PAL’s researchers consider that neither international aid agencies nor researchers know “what works” to fight poverty. Therefore, evidence is needed. According to J-PAL’s researchers, RFEs would allow to produce such missing evidence, thanks to their strong internal validity. Esther Duflo is a leading figure of RFEs’ promotion in development economics, and also the co-founder of the J-PAL -- jointly with Abhijit Banerjee.

Recently in her presidential address to the American Economic Association, Duflo claimed that economists should be like and act as plumbers. Such affirmation moves explicitly the metaphor of economists as an engineer one step further. According to Duflo, economists as engineers rely on theories and assumptions to design policy or institutions. In contrast, economists as plumbers do not rely on any theories or assumptions. For Duflo, theories and assumptions often offer bad guidance for policy-making, especially in the field where many unexpected effects arise. Therefore, the economists as plumbers should proceed in three steps. First, they intervene directly in the field through randomized experiments. Second, they observe what result from such an intervention. Finally, they tinker in order to render development programs efficient.

The aim of this paper is to build an epistemological analysis of Duflo’s metaphor of economists as plumbers in order to scrutinize its implications for development economics. The analysis of metaphor has a long tradition in economics, especially thanks to the work of Deirdre McCloskey. McCloskey argues that metaphors play a central role in the rhetoric of economics. According to McCloskey, using metaphors helps to persuade that the argument is ethically neutral and confer to it scientific authority. The claim of the paper is that RFEs in development economics do not enable to tinker but rather to observe. In order to properly analyze RFEs, we thus propose to move from the plumber metaphor to the one of economists as surgeon. Indeed, RFEs in development economics do not enable economists to be like plumbers but rather to be as surgeons of the 19th century. Surgeons of the 19th century observe through medical intervention (the surgery), they highlighted symptoms instead of applying cures.

The paper shows that RFEs in development economics have actually produced symptoms of poverty rather than evidence of effective remedy (e.g. development program). In that sense, economists do not tinker but rather observe through the experimental intervention. Sticking with the plumber metaphor masks such an aspect and therefore the possible methodological alternatives that could allow RFEs to produce efficient intervention on reducing poverty. That is why, finally, the paper proposes alternative
methods to complement RFEs in order for development economists to be more like surgeons of the 21st century. That is to say methods that would allow the surgeon not only to observe but also to propose remedy.

The paper proceeds as follows. Section 1 defines Duflo’s metaphor as well as its rhetorical motives. Section 2 shows that RFEs do not enable economists to be as plumbers but rather like surgeons of the 19th century. Section 3 highlights methodological alternatives that could complement RFEs in order for development economists to be like surgeon of the 21st century. Section 4 concludes.

Chad Harris. An alternative to the qualitative redemption of comparative process tracing

The problem of extrapolating causal effects, or external validity, continues to bedevil researchers in the fields of evidence based policy and econometrics. Comparative process tracing is a method for making predictive extrapolations or external validity inferences put forward by Daniel Steel (Across the boundaries: Extrapolation in biology and social science. Oxford: Oxford University Press. 2008). While this is one of a handful of promising methods for making predictive extrapolations, it is not without its limitations. For one, its genesis as a response to extrapolation problems in biology and the natural sciences means that it is often difficult to reconcile comparative process tracing with external validity problems in other disciplines, particularly in the social sciences.

Recently Daniel Khosrowi (Journal of Economic Methodology, 26:1, 45-58, 2019) proposed that comparative process tracing can be rendered more suitable to predictive extrapolation in econometrics and evidence based policy if it can be bolstered by qualitative, instead of quantitative, observational evidence from target environments. In this talk I suggest an alternative approach to rescuing comparative process tracing, an alternative that is not underpinned by qualitative observational evidence from the target environment.

Institutions: unification or pluralism? (Symposium)

Theories of institutions are rather diverse. Many conceive of them in terms of regularities in behavior; many others in terms of rules for action. A number of hybrid theories have been proposed that seek to integrate different aspects of institutions. Some focus on behaviors, others on their normative dimension, and yet others on their cognitive or symbolic functions. This symposium addresses the question what the advantages and disadvantages of a unified theory are as compared to pluralist approaches. Two contributions defend unification, while focusing on normative and symbolic features of institutions. Two other contributions favor pluralism instead.
Frank Hindriks. Unifying Theories of Institutions: Why Integration Is Better Than Reconciliation

Explanation has been equated with reductive unification. But unification is neither necessary nor sufficient for increasing the explanatory power of a theory. Furthermore, it need not be reductive. I identify two kinds of non-reductive unification, only one of which aims at explanatory unification. First, two apparently rival theories can be reconciled by specifying their explananda in a complementary manner. The goal of a reconciliation is to find a way of holding on to two important ideas that are in tension with one another. Second, they can be integrated by resolving the tensions between them. Integration establishes complementarity. However, it also tends to decrease the number of explanatory factors and thereby to increase the explanatory power of the integrated theory. I illustrate these two kinds of unification in terms of theories of institutions, which are alternatively conceptualized as equilibria and as rules. This case study reveals two things. First, the integrated theory does indeed increase the explanatory power of the theories. Second, the tensions between theories cannot always be resolved by means of a reconciliation, which means that complementarity sometimes requires integration as well.

Brian Epstein. Real-world Heterogeneous Institutions in Economic Theory

Economists have long observed that social institutions matter for the development of nations. Acemoglu and Robinson (2012), for instance, examine macroscopic institutional structures, distinguishing the effects of “extractive” and “inclusive” institutions for economic growth. Prevailing theories of institutions regard them as made up of cognitive structures, such as sets of rules or attitudes, and they hold that institutions generally have the function of affecting cognitive structures, such as incentives, payoffs, and choices. I argue against both of these, describing more heterogeneous building blocks of institutions than just cognitive structures, and more general functions that institutions have than affecting such structures. In this paper, I argue against this approach to institutions. If we are to construct a micro-level foundation of institutions to explain and design large-scale institutions, it will have to be different basis than the prevailing one. I argue instead for a heterogeneous treatment of the ontology of institutions. I argue further that the way that both institutions and technologies are modeled in economics reflects a mistaken understanding of the causal dynamics of economic systems, and propose approaches for incorporating the heterogeneity of institutions and technologies in economic models.
Valuation and policy

Rebecca Livernois. Contingent Valuation and the Problem of Externalities

Environmental problems are largely viewed as externality problems in economics. Activities like pollution are overproduced because they are unpriced. An externality problem is therefore solved by setting a price on the activity at the value of the externality in equilibrium. Contingent valuation is a survey-based method that is used to estimate the value of an externality. In its simplest form, this method asks individuals to report their willingness to pay for some environmental good. These reports are then used to estimate a demand schedule for the good, and consequently the optimal price of the good, with the intention of recommending an associated policy response.

Contingent valuation tends to be used in government or applied economic research to value local environmental externalities like the damages caused by irrigation on a river ecosystem and, perhaps most famously, the damages caused by the Exxon Valdez oil spill in 1989 (Venn and Quiggin 2007, Maas and Svořenčík 2017). Nevertheless, this method is generally dismissed by mainstream economists (Banzhaf 2017). Jerry Hausman (2012) argues that contingent valuation is “hopeless” for three reasons, all of which center on the inability of surveys to elicit information about ‘true’ preferences: what individuals say they will do in a survey is different to what they will actually do when they are faced with the real prospect of purchasing a good; individuals’ willingness to pay is significantly different to their willingness to accept in a survey context, which is inconsistent with economic theory; and individuals cannot distinguish between their willingness to pay for the entire environmental good or a portion of it. In a revealing statement about the mainstream opinion of contingent valuation, Timothy Haab et al. (2013) reveal that their motivation in publishing responses to Jerry Hausman’s (2012) criticisms is to “urge the community of economists to recognize that the intellectual debate over contingent valuation is still ongoing” (593).

In this paper, I argue that the criticisms leveled against contingent valuation are largely misdirected. The problems that are associated with contingent valuation, which is a procedure of measuring externalities, are really problems with the characterization of externalities (Cartwright and Runhardt 2014, McFadden 1999, Banzhaf 2017). Economists tend to characterize externalities broadly as unpriced spillover effects. Given the standard assumption that the satisfaction of actual preferences constitutes welfare, they conclude that any unpriced spillover effect in the world, like in the model, generates untapped gains from exchange and untapped welfare gains. These features give externalities their measurability and policy relevance, respectively. However, this view of welfare is mistaken. Welfare is not the satisfaction of actual preferences, and this is particularly apparent in the world where people make mistakes about what will make them better off. Instead, sufficiently ‘laundered’ or ‘purified’ preferences are evidence for welfare (Daniel Hausman 2012). This means that, in the world, gains from exchange that are based on actual preferences can diverge from welfare gains.

I argue that externalities are essentially policy-relevant, which means that they must feature untapped welfare gains. Therefore, if they are to be measured in monetary terms even when actual preferences are not a reliable guide to welfare, then they must be a measure of hypothetical gains from exchange determined by what people would be willing to pay if they had sufficiently laundered preferences. It is unclear, however, how this hypothetical willingness to pay can be estimated non-arbitrarily. I argue, using a case study of the problems researchers report in using contingent valuation to value culturally significant forests in Australia, that the fundamental problem with contingent valuation is that it attempts to estimate an externality, which requires constructing hypothetical willingness to pay
(Venn and Quiggin 2007). The problem is with what contingent valuation tries to measure, not how it does this. To address this problem, mainstream economics needs either a new conception of externalities or a new conception of environmental problems.

Maria Nordström and Sven Ove Hansson. What matters when time is all that matters

Putting value on time is a common practice in transport economics, where the practice of assigning value to time savings, which can be aggregated into a total benefit for many individuals over time, is a common approach to evaluate the value of time gain as part of evaluations of potential transport investments. There is often an underlying assumption of transferability between time and money, which arguably does not sufficiently take into account the specific features of time.

In this paper, we examine the account of having the disposition of time as the only ultimate source of utility (Zeckhauser, 1973) and its relation to underlying assumptions in mainstream transport economics. If one views temporal well-being as being how well off one is at a particular time, it seems intuitively reasonable to assume that what we do, i.e. the activity being carried out at a particular time matters to our well-being. However, as Zeckhauser points out, “[t]ime-related decisions involve complexities not encountered in a well-behaved, goods-oriented, neoclassical consumption model” (ibid, p. 670). We consider these complexities (or aspects) by first and foremost dividing them up into two categories: aspects due to the nature of time (as a commodity) and aspects due to the interconnections between activities and their temporal location.

In the first category, we place the characteristics of time: non-accumulativeness, irreversibility and non-transferability. These are innate characteristics that set time apart from other commodities and goods. In the second category, we consider the following five aspects:

(i) The notion of distinguishing between process and goal gains. Simply put, some activities are enjoyable to do while others are enjoyable to have done. Here, the process benefit is the intrinsic aspect of the activity, i.e. the value of carrying out the activity, while the goal benefit is the instrumental aspect of an activity, i.e. the value of the outcome.

(ii) The temporal division of activities. The second aspect is based on the claim that the grouping and duration of activities matter, with longevity of activities being something we intuitively value.

(iii) The order of activities. The third aspect is based on empirical evidence that suggests that context and reference points matter to how one values time allocated to an activity. It should be noted that in many cases, people also strive to allocate time to certain activities simultaneously with others. This reflects that the well-being obtained when doing a certain activity can differ depending on whether the activity is carried out in solitude or in the company of others.

(iv) Gossen’s ‘laws of pleasure’. It can be claimed that carrying out the same activity over time will, sooner or later, lead to a decrease in returns to scale. Yet, repeating the same activity after a (long enough) period of abstinence can restore the enjoyment of that activity.

(v) The notion of joint production. Lastly, the notion of ‘joint production’ refers to the possibility of carrying out multiple activities simultaneously.

In summary, whenever considering the value of time, it seems intuitive to not only look at the total amount of time spent on each activity but also the order of these activities and the pattern of how much time was devoted to each activity at each instance. In conclusion, we discuss the implications of the aspects discussed in this paper on models of time value. We argue that the complexities show substantial
differences between temporal and monetary value that should be considered in the context of transport economics and transport policy.

Merve Burnazoglu. Built-in Normativity in Tailoring Identity: The Case of the EU Skills Profile Tool for Integrating Refugees

In current European policy debates, integrating refugees is considered complex but achievable and beneficial in the long term. Policymakers often emphasize that acknowledging employment as a core part of socio-economic integration and taking measures to facilitate employment are very important to manage what is usually referred to as the refugee crisis. Identifying refugee skills and qualifications is one of the most significant preconditions for matching refugees and jobs and thus for labour market integration. In a meeting of the European Economic and Social Committee of the European Commission on November 6, 2017, experts and policymakers repeatedly expressed that one-size-fits-all measures would not allow for job matching for refugees. Rather, more flexible, customized, individualized, and tailor-made ways to identify skills and qualifications would be necessary. The EU Skills Profile Tool is one of the most recent tools created by the European Commission for making it easier to identify skills. In the meeting, participants discussed this tool but did not inquire into whether it is of a tailor-made type. The participants agreed on the tool’s potential benefits. It seems that what participants sought was the flexibility in the use of the tool; the tool itself, however, was considered neutral by design with respect to its role of in 'tailoring' refugees' profiles, that is to create a correspondence between refugees and their profiles.

This paper presents an analytical framework to investigate a tension between one-size-fits-all and tailor-made measures in policy making regarding the integration of refugees that was raised in the meeting of the European Economic and Social Committee. I investigate whether the EU Skills Profile Tool, which is said to be a powerful tool for making it easier to identify refugees' skills and qualifications, would be of a tailor-made type model. Employing diverse accounts of economic models, I argue that, throughout a process which I call 'tailoring', a built-in normativity (similar to Boumans' 'built-in justification, 1999) of the tool mediates between refugees and their representation. The tailoring process functions similar to the process of 'packaging' that Leonelli (2011) argued to involve in de-contextualization of (small) facts about model organisms in biology from their context of origin to travel and arrive at destinations to be re-contextualized for use in new research settings. The EU Skills Profile Tool de-contextualizes refugees' skills in accordance with the tailoring in the design of the tool and packages them to travel in the labour market. One-size-fits-all measures may be necessary for standardization in that they provide the benefit of common standards by giving refugees a 'familiar face' in this process of de-contextualization by tailoring based on which they can match with jobs. Nevertheless, tailoring is important for flexibility as one-size-fits-all measures can lead to only one kind of profiling, or, in Leonelli’s terms, packaging.

Tailor-made measures, however, are not easily applicable in the presence of standardizing tools of a one-size-fits-all type. I argue the standardizing tools are not (necessarily) neutral. Their design can create system dependency and limit tailoring after the use of the tool as the normativity is already built in the tool design and thus the tool categories cannot be changed afterwards. The built-in normativity of the tool, on the other hand, may also have normative implications in use such as to lead to social stratification by sorting refugees into certain profiles.
Extrapolation and external validity
(Symposium)

External validity and extrapolation are the central concepts in understanding where and why inferences from experimental and observational studies can and cannot be generalized. The amount of discussion on the issues of external validity and extrapolation in the methodology and philosophy of economics has steadily grown for the past ten years; put very generally, both concepts are used to address the epistemic and practical issues and the possibilities of generalizing (causal) inferences drawn from experimental and observational studies to new settings, times, and populations. Issues of extrapolation and external validity are pertinent in economics, but in other social sciences, public health, medicine, and psychology as well.

The extant literature in philosophy of economics stems from the contributions of Cook and Campbell (Cook and Campbell, 1957, 1979), and focuses mainly on analyzing the role of external validity in experimental methodology, as well as on building theoretical frameworks for extrapolation (Guala 2005, 2010; Jiménez-Buedo 2011; Steel 2008, 2010; Cartwright 2009; Cartwright and Hardie 2012; Pearl and Bareinboim 2013; Reiss 2018). On the whole, the level of success of the extant accounts of external validity and extrapolation is still an open debate. With the call for evidence-based policy growing stronger and stronger, there is an increasing need for a nuanced understanding of extrapolation, its limits, and its possibilities.

The major topics that the presentations in the symposium deal with are extrapolation and external validity, especially as the two are understood in evidence-based economic policy, as well as experimental economics and development economics. The presentations discuss extrapolation, both as a theoretical concept and as scientific practice; external validity as a concept that is too often linked to a deterministic view of the causal inferences that can be yielded from an experiment; the fact that external validity and extrapolation mistakenly understood as synonymous, and as a single, uniform “problem”; and the methodology of randomized field experiments and its issues with external validity.

The symposium continues and complements the discussion on the central themes in the existing literature with both theoretical analyses and case studies. The variety of theory- and practice-oriented perspectives from which the issues revolving around external validity and extrapolation are studied by the different presentations is illustrating the need for the continued rigorous treatment of extrapolation and the possibilities of generalizing causal inferences. Understanding the perspectives from which extrapolation and external validity can be investigated, as well as their points of tension, aids understanding the ways in which the concepts are and can be relevant for policy-oriented studies, but also causal phenomena and causal inference in general. The main contribution of the symposium is its focus on both theory and practice of extrapolation/external validity, bridging the gap between the philosophical literature and practice.

Donal Khosrowi. What’s (successful) extrapolation?

Various proposals have been offered for how to extrapolate causal effects from study to target populations, with some authors suggesting that the problem of extrapolation has been successfully solved (e.g. Marcellesi, 2015; Bareinboim and Pearl, 2015).
I argue that such conclusions are unwarranted, and indeed that the very question of whether the problem of extrapolation has been solved is misleading. Two basic questions need to be addressed first: 1) What is an extrapolation? 2) What constitutes successful extrapolation?

Addressing the first question, I argue that existing treatments employ overly simplistic notions of extrapolation that unhelpfully gloss over the considerable diversity in real-world problems of extrapolation, leaving unclear which strategies, if any, are likely to be successful. To improve on this, I propose a two-stage analysis of extrapolation.

The first stage highlights important differences in problems of extrapolation. I offer a framework that distinguishes different levels at which, and ways in which, obstacles to extrapolation can obtain, suggesting that problems of extrapolation are highly diverse and that some are significantly more challenging than others. Building on this, the second part distinguishes different kinds of extrapolative inferences along several dimensions, including the envisioned mode of inference and its fidelity, the type of causal queries at issue, the availability of background theory and knowledge, the epistemic risks involved etc. This analysis helps recognize that there is no single answer to whether the problem of extrapolation has been solved, but multiple answers that depend on contextual details.

In the second part, I focus on what constitutes successful extrapolation. Here, an important challenge is Steel’s (2008) extrapolator’s circle: the knowledge about the target required for extrapolation must not be so extensive that we can answer our question based on knowledge about the target alone.

Acknowledging the extrapolator’s circle, I argue that success in extrapolation involves three things: The first, justification, concerns how much empirical support an extrapolative conclusion enjoys. The second, accuracy, concerns how accurate our conclusion is. The third, relevance, incorporates the extrapolator’s circle: the less relevant the knowledge from the study setting is to our conclusion, the less successful an extrapolation is.

General success in extrapolation requires a good mixture of justification, accuracy, and relevance, with all being necessary and none being sufficient for overall success. At the same time, there are important tensions between these criteria: the more extensive our knowledge about the target used to justify an extrapolation, the more accurate our conclusions tend to be, but on pain of falling prey to the extrapolator’s circle. I illustrate how existing strategies for extrapolation fail to acknowledge this.

I conclude that we should be sceptical about whether the problem of extrapolation has been solved. First, there is no single problem, but a multiplicity of problems, some of which are easier to solve, whereas others remain unlikely to be solved. Moreover, existing strategies experience difficulties in responding to the tensions between different success criteria. They achieve successful extrapolation in some cases, but fall well short in many others.

Sofia Blanco Sequeiros and Luis Mireles-Flores. A contextual approach to external validity in economics

Philosophers typically take the problems of external validity and extrapolation as synonymous. Attempts to solve them tend to focus on conceptual and methodological analyses conducive to constructing theoretical frameworks for justified extrapolation (Guala 2005, 2010; Jiménez-Buedo 2011; Steel 2008, 2010; Cartwright 2009; Cartwright and Hardie 2012; Pearl and Bareinboim 2013).

We argue that the discussion on external validity has neglected the multiplicity of epistemic and practical issues related to it, and by trying to propose univocal frameworks as solutions, inadvertently suggested that there is a single and definite “problem of external validity” to be dealt with. External validity is an issue that applies to different experimental and observational methods in different ways; there is no
method that by itself produces externally invalid results, or to which problems of external validity exclusively apply.

We propose that extrapolation should be understood as the inductive process of making inferences about a phenomenon of interest in a target situation (about which there is a new unsettled question or hypothesis under evaluation), on the basis of evidential results from a model situation. Extrapolation always includes assumptions about the similarity of the populations, the homogeneity of causal structure or causal effects (cf. Westreich et al, 2018). External validity is not the process of generalizing or transporting inferences, but a concept used to evaluate whether the assumptions used to justify this process are valid. Distinguishing between the two concepts is beneficial, because what is necessary and sufficient for successful extrapolation is not the same as what is necessary and sufficient to determine external validity, but inferences being externally valid constitutes support for extrapolation.

The criteria according to which researchers evaluate the external validity of inferences vary according to the epistemic and practical aims pursued in a field of inquiry, because the demand for and means with which to produce externally valid inferences vary. In econometrics, external validity is understood as a measure of the accuracy of a prediction about the effectiveness of an intervention, whereas in experimental and behavioral economics, external validity is used as a general term for identifying the factors, conditions and parameters that affect the extrapolation of inferences to "other circumstances" in general or a single target in particular. Inferring external validity thus ranges from quantitatively evaluating the accuracy of the prediction that microloans affect poverty to justifying the representativeness of a sample of 50 Peruvian farmers, and assessing whether a dictator game accurately measures the reciprocity of voters in a Paraguayan municipality.

This means there is no “problem of external validity”, but a set of interrelated epistemic and practical concerns related to evaluating inferences' potential to be generalized. We look at the actual practice in a number of cases of empirical research to highlight the relationship between the domain-specific concerns and the general issue of extrapolating causal claims. We conclude that external validity is a useful, yet incomplete concept for assessing potential extrapolation, and that a contextual understanding of the varieties of extrapolation complements analyses of external validity.

Maria Jiménez-Buedo and Federica Russo. Experimental data, causal inference and background knowledge in the internal/external validity distinction

In this paper we build on previous conceptual analysis (Hammersley 1993, Jiménez-Buedo 2011), to argue that one of the sources of ambiguity and confusion regarding the notion of external validity stems from an often embedded interpretation of the Campbellian project: the assumption that every experiment comes with its own inference or set thereof.

That an experiment is univocally associated with a set of causal hypotheses (licensing some specific inferences) might be a viable assumption in some research programs. In particular, in those settings where the causal hypothesis is either remarkably simple, or very specific. This could be the case of an RCT testing whether an well-defined active principle, taken into a particular dosage and excipient causes a particular biomarker to reach a particular level in a given sample, known to be representative of the target population, where we want to know exactly that (whether the particular dosage etc...), because, for example, we already have a lot of background knowledge acquired through previous experimental or observational evidence). In randomized trials of this sort, we can expect that the relation between the results of the experiments and the relevant causal inference that it licences, will be relatively straightforward.
In contrast to this kind of randomized trials, there are also experiments of a more exploratory nature, where the treatment implemented is only one of the many ways in which one could operationalize the variable of interest to researchers, and where the output variable is only one of the dimensions of a broader phenomenon that we are interested in. Suppose, for example, that we are interested in knowing whether support for income redistribution is linked to feelings of self-efficacy or competence. A first approach to the question can come from an experiment where I manipulate participants’ feelings of self competence (through the administration of a test and posterior communication of results and ranking of individuals) and then have participants engage a redistributive task, such as the Dictator Game.

A common interpretation of the contrasting nature between these two experiments (the RCT, and the DG) would be in terms of their respective internal and external validity. We contend, instead, that this kind of interpretation ignores the role of background knowledge in licencing different causal inferences, and that depending on the researcher’s background knowledge and prior theoretical commitments, different causal inferences will be licenced from the same experimental data.

The paper explores systematically the role of background knowledge in experimental inference and distinguishes its function in the design stages from its function in interpreting causal claims after experimental data is produced.

Michiru Nagatsu and Judith Favereau. Escape from theory: The limits of evidence-based development economics and how to overcome them

In this paper, we critically examine the methodology of evidence-based development economics, which deploys randomized field experiments (RFEs) as its main tool. We describe the context in which this movement started, and illustrate in detail how RFEs are designed and implemented in practice. Several methodological criticisms of RFEs have been raised from a wide range of fields, such as development economics (Deaton, 2010; Ravallion, 2009; Rodrik, 2008; Acemoglu, 2010; Basu, 2014; Barrett and Carter, 2010), experimental economics (Harrison, 2011, 2013), econometrics (Heckman, 1992; Heckman and Smith, 1995; Heckman et al., 1997, 1998; Leamer, 2010), and philosophy of science (Cartwright, 2007, 2009, 2010; Cartwright and Hardie, 2012; Teira, 2013; Teira and Reiss, 2013; Davis, 2013; Guillin, 2013). Although they target different methodological, practical and technical aspects of RFEs, one can think of them as all reiterating the need to identify the mechanisms underlying the results of the experiments to facilitate their effective applications in policy. What is currently missing, however, is a detailed examination of how the methodological and theoretical problems figure in concrete RFE studies, and an articulation of ways to resolve them. We provide both first by examining a series of experiments by Pascaline Dupas and her colleagues on the use of bednets, saving, and governance in Kenya, and second by providing a methodological framework in which RFEs are complemented by other methods.

In our case study, we show that it is difficult to obtain clear policy recommendations from those RFEs as initially intended. Such difficulty relates to the lack of external validity of RFEs widely criticized in the literature. After examining two recent responses by leading figures of evidence-based development economics, i.e. machine learning and structured speculation, we propose an alternative, objectivist methodological framework. Our framework explicitly incorporates two sub-fields of economics, namely experimental economics and behavioral economics. The former provides a methodological guidance for experimental design, while the latter provides theoretical resources. Both complement RFEs significantly. We illustrate two examples that show that our proposal is consistent with and explicate some of the innovative experiments in development economics.
Epistemology of economics

Ivan Boldyrev and Alexander Libman. Dire Quasi la Stessa Cosa: ‘Cycles of Invention’ in Current Economics

Although hardly any result in any science can be considered entirely original, in economics it is sometimes the case that new research essentially rephrases the older insights (see e.g. Foss 1996). In this paper, referring to the history of the 20th century economics, we conjecture that there is peculiar dynamics revealing ‘cycles of invention’ (or ‘cycles of oblivion’) that most often follow the pattern of formalization and ‘heuristic progress’: very similar ideas come up first in ‘discursive’ form and only afterwards are reformulated with recourse to mathematical modeling.

Apart from the ticklish normative questions that should certainly be treated both inside and outside the discipline (can we consider a science re-iterating almost identical ideas for years or picking up something formulated long ago to be in a healthy state?), there are important ‘positive’ challenges. First of all, we need to reconsider the very idea of ‘novelty’ and ‘progress’ in economics in view of these phenomena (see also Boehm et al., 2002). What is perceived as new within the profession is key for its development, and understanding the reasons behind the standards of novelty thus sheds new light on both conceptual and social structure of the economics discipline.

Furthermore, in each case of reinvention it is also important to know whether the protagonists were aware of the previous work. While persistent ignorance prompts us to look more closely at the flows of information between economics and other fields (along the lines proposed, e.g., by Fourcade et al. 2015), but also within the discipline, an intentional denial suggests further important research questions.

We argue that in some scientific communities in economics the value of the language game used – that is, the set of rules for planning, doing, presenting, and evaluating research in a given community – is so high, that its members may consciously refrain from citing work done within a different language game, even if the latter comes up with similar arguments.

The importance of a particular language game can be explained by several factors: genuine belief in its methodological superiority, power relations within the discipline, the need to accept cognitive limitations (making it necessary, e.g., to avoid citing sources in other languages or to prefer more recent sources), or the need to create a clear yardstick for evaluating scientific work. One can hypothesize that in disciplines using less specific terminology (i.e., vocabulary used also by other fields), the need to protect the language game is higher. A particular language game is reproduced within discipline or community, with students advised to adjust their citation patterns to maximize the publication output, and senior scholars rejecting or simply ignoring the contributions made in the unfamiliar framework (see also Gans and Shepherd 1994).

To illustrate our ideas, we provide several case studies from political economics, institutional economics, and international trade drawing on the work of Gordon Tullock, Oliver Williamson, Oliver Hart, W. Brian Arthur, Giovanni Dosi, and Paul Krugman among others.

Our general hypothesis (on the very existence of the ‘cycles of invention’) is then tested using the citation depth and its variation in different (sub)disciplines. The highly variable average age of citations would then suggest the existence of the invention cycles, while more steep dynamics would be an argument against this hypothesis.
This year the Society of Artistic Research is celebrating its 10th Anniversary, an event in itself calling for the exploration of the meaning of “research” in the field of the arts and of how ‘research methods’ developed there could be employed in Economics – if at all. The exchanges between economics and the arts have been proliferating since the crisis of 2008-9 with many critical interventions on the debate on the causes of the crash and on how to fix the economy after it. These interventions raised also the question about the position of artists in the economy and how s/he could influence the debate inside the economic profession. In addition, the integration of Art Education in the university system following the Bologna process has established artistic research as an academic field and institutionalized art also as a process of knowledge production, creating the context for the discussion of the epistemological challenges that the arts raise for economics.

Despite all these societal causes that brought economics and the arts closer and encouraged a debate on the methodological problems and benefits of addressing economic questions form a different point of departure, artistic research still needs to justify itself, especially if one is to adopt a realist position; after all scientific method is considered a rigid and verifiable movement from causes to facts directed towards knowledge production and the development of scientific theories that are able to explain these causal relations. The question that this paper is trying to address is how artistic research can be relevant – if at all – for economic methodology, taking into account that the artistic domain so far has tended to continually transgress the principles of scientific method. Instead of explicating the relations between means to an end – that is how functional, or, “transparent”, methodologies conceive themselves – artistic research practices complicate this relation by drawing the attention to the opacity of artistic methodologies (Elo 2017). Living off the critical tradition of the arts, artists tend to assume a position against method(s) while being reflective, which means that they deliberately touch upon their own inability or unwillingness to formulate an epistemological paradigm.

The article will try to unpack the meaning of artistic research, by analyzing the epistemic relevance of aesthetics, focusing on the possible contribution of the arts in the “context of discovery” (Popper 1959). Aesthetics is one of the key domains where the conditions determining how things in the world become perceptible, knowable and controllable to human beings are at stake (Rancière, 2004). The negotiation of sense at the interfaces of arts and economics, concerns the registers of sense and the questions of whether, how, and why some of them are prioritized over others. Especially, when the medial embeddedness of experience is recognized, questions of whether and how there is an aesthetic moment inherent to all knowledge production seem ever more relevant (Elo 2017).

Against this background, the double-bind between aesthetics and epistemology, i.e. the processes of sense experience and knowledge production can be addressed in terms of transdisciplinarity and cross-pollination. As Rheinberger argued, artistic research could be understood as an epistemic attitude that transforms what is initially at hand (“stuff”) into an object of investigation (an “epistemic thing”), with the arts transforming into experimental systems and artworks into epistemic objects (Rheinberger 1997). Initially, the epistemic object should not be conceived as sign to a signifier, but as an empty object of the investigation. In this capacity, artworks could provide the aesthetic and material-technical foundation for scientific innovation and new theoretical objects. They could open up new exploratory spaces for perception, communication, and cognition, inviting previously invisible and unrepresented economic relations into the space of articulation.

The article will elaborate on this analysis about the relations between aesthetics and epistemology in economics, by referring to specific examples of artworks that relate to the phenomenon of money.
(Haiven 2018), and how these works could illuminate some of the theoretical debates about the nature and the meaning of money.

Tom Kayzel. The Emergence of Model-Based Economic Expertise in the Netherlands, 1950-1963

In the 1950s, the Dutch Central Planning Bureau (CPB), the most prominent scientific advisory council to the Dutch government on economic issues, started working on the development of a large-scale econometric model, intended for policy analysis and short-term forecasting. The development of this model proved instrumental in the operationalization of many of government’s economic policies; firmly establishing a macroeconomic way of thinking about the Dutch economy that would typify the government policies from the 1950s and 1960s.

This paper will track the initial ideas behind this model, describe its subsequent development, and show how the CPB took up a new mode of scientific expertise when applying the model for their policy advice. I will try to show that the model was not only the product of epistemic interests and problems but also of very practical concerns in policymaking and the internal organisation of the bureau. Understanding the model’s impact on policymaking, it should not be understood solely as a research or forecast technique, but much more broadly as an organisational and policymaking tool. Focusing on these functions, I attempt to explain what made the model so successful, both in the internal organisation of the CPB and in policymaking.

The context from which the model emerged was that of the wake of the European Recovery Program, in which the Dutch government was pushing for the stimulation of industry and moderation of consumption through central wage-setting. Developing a macroeconomic model, the CPB hoped to better understand how such policies interact with other measures and economic variables, allowing for better estimation of certain policy scenarios. These circumstances not only inspired what type of model the CPB was to develop, but it also informed specific choices within the modelling process itself. Building on the practice-based accounts of modelling by Marcel Boumans and Mary Morgan, I will try to show that the structure of the model, what variables were included and how, and how the relations were estimated, was as much a product of an epistemic process of modelling as well as the political context in which the model had to operate. Developing this model meant more than adopting new research techniques for the CPB.

In order to develop such a large model, the CPB had to be re-organised in a more hierarchical and structured fashion than was previously the case; making sure that all the research output from all the research departments could be integrated into the mainframe of the model. In that fashion, the model became an important management tool for internal coordination.

In combination with the bureau’s decision models, the econometric model became also an important organisational instrument for policymakers, often being central in the design for a new set of policies. I will argue that how the CPB’s econometric model functioned within policymaker circles can be understood with Dan Hirschman and Elisabeth Popp Berman’s notion of policy device (a riff on Michel Callon’s notion of market device). Operating as a policymaking instrument, the model started to form the framework from which policymakers would identify economic issues and design viable solutions. Thus through the use of the model, a cognitive space was created in which the Dutch economy became more and more imagined as a manageable policy object. Lastly, I will argue that, together with the expertise of the CPB, the model propagated and justified the macroeconomic logic that the government was using for their economic measures.
The role of psychology in founding some parts of economics is just as complex as the debate around its defining borders with economic schools, for instance the Austrian School of Economics, and pedagogic endeavors to spread rules, learning and rule-learning in various discipline-matters, including economics.

The Austrian school is an example of choice to illustrate the point that the paper will make. It was sometimes denominated a "Psychological School" by some of its own members, while some disciples of the founder Menger (like Ludwig von Mises) rebuked inputs from psychology in their own versions. In the early stage of the school, the words "Psychologenschule" were used to designate the new Marginalist school of economists in its confrontation with the "Historical School". Historicians also used the term to discard another school, the Classical school, that had been dominant until then. Some Marginalists like Menger turned to antique concepts like "psyche". Menger explicitly and conscientiously referred directly to Aristotle's *Nico-machean Ethics* and to the *De Anima*. Commentators like Oskar Kraus extended the term of psychological economics to the works of Menger, while Menger rejected that interpretation in his archives and to his disciples (Bohm-Bawerk, Wieser, etc.). In the end, was the new school a "psychologische Schule"?

Another facet of that inquiry is how one learns to be responsible, that is the Relevance of Rule-learning. Early writings by Karl Popper are illuminating in that respect and at present not yet translated into English: their relevance in Philosophy and Economics and Philosophy of Economics may be displayed on the basis of archives.

On both issues this is where philosophy and pedagogics may meet. This paper will further this reflection upon the basis of German-language archives in order to discuss Menger's stand on psychology and how Popper's discussion of subjective vs. objective inquiry made Popper turn to the logic of discovery (as naturally illustrates the publication of *Logik der Forschung* - but one forgets too easily to talk about previous works and publications, that we shall explore and endorse here).

Starting from texts unearthed from various archive centers, the presentation shall present a view of a work-in-progress to discuss perspectives in characterizing relationships of psychology and economics nowadays.

Welfare economics and behavioural welfare economics

Aleksandr Alekseev, Glenn Harrison, Morten Lau and Don Ross. Deciphering the Noise: The Quantitative Intentional Stance and the Welfare Costs of Noisy Behaviour
Stochastic choice has become an active area of economic research, motivated primarily by two considerations. First, a large body of empirical evidence shows that stochastic choice is a robust phenomenon. Second, models of stochastic choice provide researchers with econometric tools to estimate structural models in a broad range of applications. The primary interest in applying a model of stochastic choice is to recover the structural parameters of the deterministic part of a model, such as risk or time preferences. Little attention has been given, however, to the systematic economic interpretation of the parameter estimates of the stochastic part, which determine the magnitude of choice randomness. The interpretation of these parameters is important for understanding the economic value of choice randomness, which has implications for the quality of decision making and for welfare assessments of choices.

We study the economic consequences of stochastic choice by developing a method of translating the estimates of the stochastic part into economically tractable terms. The stochastic part of a model is parametrized by \( \mu \), often called the noise parameter. The noise parameter determines how sensitive choice likelihoods are to the maximization of utility \( U \) according to a given structural model. As noise tends to zero, an agent will almost surely choose the alternative with the highest utility. When noise goes to infinity, the agent will assign equal likelihoods to choosing each alternative regardless of their utilities. Higher values of \( \mu \) thus imply a higher magnitude of choice randomness in this specification.

Three issues arise with the interpretation of the estimates of this parameter. First, while the effect of \( \mu \) on choice likelihoods is clear, one cannot readily interpret a particular estimate of noise in economic terms. A monetary value assigned to a noise estimate, on the other hand, would provide clear information about the economic consequences of choice randomness. Second, since the noise parameter is unbounded from above, it is difficult to judge whether the randomness of an agent’s choices is high or low. A value defined on the unit interval would solve this problem. Third, the raw estimates of \( \mu \) are not well suited for interpersonal comparisons, since behavioral parameters \( \beta \) also change across people. Having choice randomness expressed in common units, such as money, and taking into account the interpersonal differences in \( \beta \) would help to overcome this issue.

We address these issues by converting an estimate of \( \mu \) into two intuitive measures. The first measure, absolute welfare cost (AWC), puts a dollar value on choice randomness. It shows how much money, in certainty equivalent terms, an agent would be allowed to “waste” compatibly with rationalization of her choices by an underlying structural model. The second measure, relative welfare cost (RWC), scales the absolute welfare cost by the monetary value at stake in a choice context. The relative welfare cost is thus defined on the unit interval. It shows what proportion of the total monetary value at stake an agent would be allowed to waste compatibly with rationalization of her choice by the model.

Our approach rests on a careful interpretation of the concepts of “noise” and “waste.” We follow the descriptive, structural literature on risk preferences by assuming a specific model of the manner in which choice randomness is rationalized. Choice is understood according to the Generalised Axiom of Revealed Preference (GARP). We adopt Dennett’s “intentional stance” toward the evaluation of an agent’s behavior. This philosophical perspective emphasises that preferences and beliefs are not fixed internal states of people, but are rationalizations of choice behaviours that people rely on to interpret one another. This applies mutatis mutandis to self-interpretation. Preference and belief attributions pick out real patterns in choice behaviors, and these patterns, which typically involve some noise, are the basis for assessing people’s goals, and hence, for economics, their welfare.

We demonstrate our approach by using it to derive welfare estimates from noisy choice data we obtained from a 2016 study of Danish adults.
Mario Rizzo and Malte Dold. Old Chicago Against Static Welfare Economics

Most economic analysis is rooted in a tradition of static welfare economics with the assumption of stable preferences. Although behavioral economics has criticized the empirical accuracy of this assumption, it supports the normative view that welfare-increasing choice presupposes stable and context-independent preferences. We argue that this position neglects important features of decision making processes that were essential to the thinking of Frank H. Knight and James M. Buchanan ("Old Chicago"). Knight and Buchanan embrace the idea that individuals do not simply want to satisfy their current preferences, but are capable of evaluating the quality of their preferences against value-based reflections to create “better” preference rankings over time. We illustrate their view, discuss methodological and normative implications and present some empirical evidence.

Ivan Mitrouchev and Guilhem Lecouteux. The "View from Everywhere": Reconciling Normative and Behavioural Economics with Respect to the Plurality of Values

The problem of finding a normative approach consistent with the findings of behavioural economics has been recently labelled the ‘reconciliation problem’ and is now a recognised research program that entails many different propositions about how to reconcile normative and behavioural economics. Various alternative normative criteria have been proposed by some behavioural economists in response to the standard preference satisfaction criterion, but no consensus has been reached on a general and unifying approach for normative analysis. We argue that behavioural economics must accept the plurality of normative views, and should stop looking for 'the' unifying criterion. We advance a normative framework (‘the view from everywhere’), derived from the second-person standpoint that takes into account this plurality, and discuss the implications of this framework in various practical cases.

Jack Vromen. Social preferences in behavioral welfare economics: the double-counting argument

The paper examines whether double-counting arguments can be effectively leveled to support the conclusion that social preferences should be excluded in social welfare economics. In one version of the argument, one that had some prevalence in standard welfare economics (cf. Diamond 2006, Hammond 2018), social preferences should not be included in individual welfares because doing so would lead to double-counting of the welfare of beneficiaries (i.e., those who would benefit from other people expressing their social preferences). But this argument presupposes, rather than establishes, a view on how individual welfares ought to be counted (or "weighed") with respect to one another. (The typical, but by no means self-evidently right, view taken on this in standard welfare economics - one that also underlies standard cost-benefit analysis - is that individual welfares should be counted equally.)
This only shifts the focus to the issue of who, and on the basis of what sorts of considerations, is to
determine how individual welfares are to be weighed. If one holds that this should reflect as much and as
best as possible the distributional moral values of individuals involved, then this could be taken to imply
that they should reflect the individuals’ relevant social preferences as much and as best as possible. Put in
the terms of the “Bergsonian-Samuelsonian” social welfare function framework, this would imply that the
functional form of the social welfare function that is to be adopted should reflect the individuals’ relevant
social preferences as much and as best as possible.

If this is done, another version of the double-counting argument comes to the fore (Harsanyi 1986;
Gibbard 1987): if one has settled on the right functional form of the social welfare function, no social
preferences (and indeed, more generally, no sort of preferences at all) should be included in the arguments
of the social welfare function – namely the individual welfares – that would imply that the right functional
form of the social welfare function is distorted. Thus understood, the double-counting argument
establishes exactly what it (according to its name) purports to establish, namely that such distributional
social preferences should not be counted doubly or twice. But the argument does not establish that they
should not be counted at all: they should be counted, but in a way that does not upset the chosen
functional form of the social welfare function.

Macroeconomics

Kobi Finestone. The Incompatibility of Rational Expectations and Knightian Uncertainty: A Limit of Economic Knowledge

A long running debate in the philosophy and methodology of economics considers the role and importance
of idealizations employed in theoretical and empirical models. Many authors question economics’ scientific
credentials on the grounds that the discipline employs idealizations which not only deviate from those of
the observed economic world, but moreover deviate in ways so problematic that the discipline loses all
real-world applicability. This challenge, closely linked with the project of determining what even constitutes
a scientific discipline in the first place, is often approached in highly general terms. Idealizations are often
treated as homogeneous in terms of their philosophical and methodological importance.

Instead of proceeding at this highly general level, we should examine particular kinds of
idealizations in order to determine to what extent, if any, they are an appropriate component of economic
methodology. In this paper, I consider the role of expectations within economic models. This is done in
order to determine whether the foremost form of expectations employed in the discipline, rational
expectations, are appropriate given the goals of researchers. This approach effectively tackles the problem
of idealization in economic modeling at a more particular level rather than the more general one.

I begin the paper by defending the thesis that expectations are a necessary component of
economic models of intertemporal choice. However, this is a relatively general thesis that fails to
differentiate different forms of expectations. A more fine-grained analysis reveals that rational
expectations are not necessary for capturing economic behavior. Despite this fact, I provide numerous
desiderata which highlight the importance of rational expectations in creating tractable dynamic equilibrium models.

I proceed to create a schema which allows one to determine the applicability of rational expectations to any particular economic model. In order to create this schema, I examine Knightian Uncertainty and contrast it with the commonly used concept of risk. This examination underscores the fact that Knightian Uncertainty is incompatible with rational expectations by definition. Thus, when we have good grounds to believe that the phenomenon being modeled involves Knightian Uncertainty, we have a principled reason to eschew the use of rational expectations.

This schema, in order to be of any use, requires an investigation into the prevalence of Knightian Uncertainty. The best way to identify Knightian Uncertainty is to consider cases where the structure of the economic phenomenon changes rapidly. This structural change causes agents to no longer be able to form stable expectations, resulting in Knightian Uncertainty. This structural instability can result from changes in economic agents, the economic environment, or both. Thus, the presence of structural change renders the idealization of rational expectations inappropriate for capturing a central feature of the relevant economic phenomenon.

This analysis explores two interrelated theses. The first holds that models incorporating rational expectations can reasonably capture an economic phenomenon whenever the target system is free from Knightian Uncertainty, which can be identified by the absence of a structural change. The related thesis deals with the best way to tackle the more general philosophical and methodological issue over the use of idealizations in economics. Rather than evaluating all idealizations with the same analysis, it is better to move from the general to the specific and evaluate particular kinds of idealizations. This involves determining what roles they play in economic analysis and to what extent they depend on or contravene other critical components of economic modeling.

Yair Barak. Is "Public Rational Inflation Expectations" a Scientific Theory or Just Speculation?

"Public Rational Inflation Expectations" theory was developed on the theoretical basis of hyperinflation analysis. The theory was proposed by Philip Cagan, in his article *The Monetary Dynamics of Hyperinflation* (1956). Economists view this article as a pioneering and classic explanation of hyperinflation. Cagan related to governmental fiscal deficits as the main generator of seven hyperinflation episodes in Europe, which occurred between 1921-1946. According to Cagan, public expectations of money depreciation nourished the inflation's momentum, caused by the fiscal deficits, turning inflations into hyperinflations. Cagan cautiously wrote that, "The changes in the balances preceding hyperinflation imply that the expectations make sudden upward shifts, presumably when the public first loses confidence in the prospect for stable or lower prices." This leads to the persistence of inflation, which then speeds up and increases. According Cagan, public expectations were only one factor that could explain hyperinflation, and it was not even the central one.

Thomas Sargent and Neil Wallace were, in their article *Rational Expectations and the Dynamics of Hyperinflation* (1973), particularly interested in building a version of Cagan's model in which public expectations are both rational and pivotal in generating high and hyperinflation. Rationality is central to their theory about inflation. They wrote: "We showed that conditions exist under which adaptive expectations are fully rational." Sargent and Wallace turned Cagan's assumption about "public's speculations" based on past experience into a way to understand past inflations and to engage in rational forecasting.
Sargent and Wallace had developed their theory and published it as *Some Unpleasant Monetarist Arithmetic* (1981). This article garnered over 3,000 quotations in the economic literature, which makes it an "economics icon." Due to this theory, in 2011, Sargent became the laureate of the "Prize in Economic Science in Memory of Alfred Nobel". Some weeks later Thomas Sargent and Joseph Zeira, from the Hebrew University, published an article, *Israel 1983: A Bout of Unpleasant Monetarist Arithmetic*, which was explicitly based on the original theory. This article concentrated on an Israeli episode: when an inflation rate of approximately 120% suddenly jumped to 400% in October 1983. The authors claimed that the October 1983 jump in inflation was caused by the announcement of a massive bailout of bank shares by the government of Israel, which promised to reimburse shareholders for the high values that their shares had fallen after five years. They contended that the public rationally expected that the futuristic bailout would cause a deficit, which would generate high inflation. The public’s rational prospects generated immediate inflation.

This article will be presented as a case study that casts doubt on Rational Inflation Expectations theory, questioning whether it is based on empirical evidences or is just a speculative theory. I will discuss whether it is a scientific theory or mystics which cannot be refuted by rational arguments or empirical facts. The basic definition of a scientific argument is that a scientific claim can be rationally refuted. My presentation is aimed at eroding the scientific status of the theory of Public Rational Inflation Expectations.

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**Kevin Hoover. Karl Brunner’s Philosophy of Science: Macroeconomics through the Lens of Logical Empiricism**

Best known as a monetary economist and prominent proponent of monetarism, Karl Brunner was deeply knowledgeable about the philosophy of science and attempted to explicitly integrate logical empiricist thinking, derived in some measure from his engagement with the work of the philosopher Hans Reichenbach, into his economics. His philosophical commitments are clearly reflected in his empirical work on monetary economics, his monetarist analysis, and in his critical approach to econometrics, microfoundations, and the New Classical macroeconomics. Broadly, Brunner was a supporter of both Friedman’s monetarism and his philosophy of science. In both cases, Brunner arrived at his positions independently and found Friedman’s positions lacking with respect to formal theory. He reworked Friedman’s methodology of positive economics in a manner more congenial to logical empiricism and advocated a monetarism that was critical of, but more closely engaged than Friedman’s, with theoretical developments in monetary economics – e.g., with the "New View" of money associated with the work of Gurley and Shaw and of Tobin – as well as with the work on empirical macroeconometric modeling associated with Klein, Modigliani, Ando, and others.

Subsequently, he developed substantive methodological criticism of the econometric practices of empirical macroeconomists. He occupied a middle ground in that he defended the empirical methods of the St. Louis monetarists, defended aggregate macroeconomics against demands for microfoundations as a sine qua non of acceptable macroeconomics, and attacked the overweening claims of real-business-cycle and dynamic-stochastic-business-cycle (DSGE) modelers to have discovered the only valid methodology. Brunner was skeptical of empirical progress in economics. Significantly, the skepticism was not of its possibility, but of the practices of economists seen through the lens of his philosophy of science. He believed that economists had paid too little attention to developing the kind of theories that were susceptible to critical resolution of empirical implications.
Values in economics


There is a tradition in some social sciences that seeks to ensure the scientific status of the discipline. In order to guarantee this status, it is often assumed that the social sciences should abstain from making any judgements based upon non-epistemic values. Thus, though it has long been acknowledged that such values might enter the scientist’s choice of a problem, the way that her results are used, or perhaps even her theory choice, the scientific procedure itself, including the theory used and its concepts, must be free from judgements based upon non-epistemic values. Economics has prominently upheld a distinction between positive and normative economics whereby only questions falling into the latter category – such as, for instance, welfare economics – allow for non-epistemic values. Positive economics, by contrast, is seen as value free and thus is comparable to disciplines in the natural sciences.

In this paper, we re-examine the viability of this two-fold distinction by exploring the role of ‘thick concepts,’ in economics i.e., concepts that have both an evaluative and descriptive aspect, as components of social scientific theories. More specifically, we explore an argument and its implications that was formulated by Hilary Putnam (2002). The challenge is mainly articulated in response to modern economics as a policy science. Putnam argues that thick concepts, i.e., "concepts [that] are both evaluative and descriptive" (Kirchin (2013), 2), undermine the supposedly sharp difference in kind between facts that are described and evaluative stances we take towards these neutrally described facts. Pursuant to this, if thick concepts play an ineliminable role in social scientific research, these theories also lack a sharp line between described facts and values.

We argue that although some economists attempt to rid thick concepts of their evaluative aspects, they often fail. Specifically, we investigate the attempt to remove the evaluative aspect of ‘thick concepts’ via explication and argue that such an explication strategy does not work. This is because, either, they are unable to remove the evaluative aspect of the concept of addiction or are no longer modeling addiction per se. Both consequences are unwanted from the economist’s point of view. As such, theories containing thick concepts often commit the economist to making a value judgement and as such undermine the positive-normative distinction. It follows that those theories cannot be value neutral.

To support our claim, we focus on a particular case study, namely the Theory of Rational Addiction proposed by economists Gary Becker et al. (1988, 1996) and the concept of addiction it contains. This case study is useful for our purposes for two reasons. First, although several authors have discussed the relationship between thick concepts and economics, their discussions tend to be rather coarse-grained (e.g., McCain, 2018) or tend to focus on the history of economics in relation to thick concepts (e.g., Davis 2016; Colander/Su 2015). By focusing on a case study from contemporary economics, we both complement this research and fill a lacuna in the literature. Second, Becker et al., and Chicago school economics more generally, has always been strongly committed to value-neutrality. Milton Friedman, for instance, famously states that "[p]ositive economics is in principle independent of any particular ethical position or normative judgment. As Keynes says, it deals with ‘what is' and not with ‘what ought to be’" (Friedman (2008), 146). Thus, a theory originating in the Chicago school tradition is an ideal testing ground to see whether economists use thick concepts, how they use them, and whether they might undermine their commitment to value-neutrality.
The point of departure of this proposal is the definition and the relationship between normative and positive economics as put forward by John Neville Keynes and popularized by Milton Friedman in his essay *The Methodology of Positive Economics*. 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.

This proposal aims at contesting Friedman’s arguments in favor 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 and Friedrich von Hayek. Accomplishing this task requires that we overcome two major difficulties. The first one is that 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 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*.

The second hurdle to overcome is related to the fact that in Hayek, too, we can find different epistemological positions, and the most famous one among them does not really help us to formulate a critique of positive epistemology as such, but only of certain forms of positive epistemology. This first position of Hayek, highly influenced by Mises, develops an a priori epistemology that is very different from Friedman’s positive epistemology, but which differs from normative epistemology in that it aims at theorizing “what is” and not “what ought to be”. Hayek’s main criticism of other positive epistemologies, when he was defending this position, had to do with the use of mathematics and the role of exact prediction. It will be shown that we can find in the later works of Hayek, mainly in the third volume of *Law, Legislation and Liberty*, the outline of a distinctly normative economic vision where it is no longer about proposing a formal theory of economic action or describing the spontaneous order, but about advancing a normative model for “the political order of a free people”.

Through a critical dialogue with Popper and Hayek, the final part of this paper will advance an alternative normative epistemology for doing economics. It will also try to offer a better explication of why using normative epistemology rather than positive epistemology is more reasonable in economics, and this without putting the scientific status of economics in jeopardy.
Kevin Leportier. Connecting political philosophy and normative economics views on freedom

Ideally, the division of labor between political philosophy and normative economics ought to be the following: political philosophy provides intuitions and principles about values relevant for economic evaluation, whereas normative economics defines measures of essential features of these values. But when it comes to freedom, the now sizable literature in normative economics and social choice devoted to define and characterize formal measures of individual freedom (which I will refer to as "Freedom of Choice Literature") rarely mentions the work of political philosophers on the topic.

A possible explanation for this lack of connection lies in a difference of focus. Philosophers have debated the role of power and interference in creating constraints destructive of individual freedom. By contrast, underlying much of the contributions to the Freedom of Choice Literature is the idea that the freedom at stake is simply an individual’s ability to choose an element in a fixed set of alternatives, with the implicit assumption that this choice is made independently from what others will choose and do. If this assumption is relaxed, however, issues of interference and power can no longer be ignored, as the decisions of some agents may prevent others from reaching their favorite outcomes, especially in a context where goods are scarce.

Several authors took that step, and tried to define measures of freedom in the interactive framework of game forms. Some suggested to rank game forms with respect to the maximum level of utility attainable in them, or to the minimum level which is guaranteed by playing a maximin strategy. Others advocated to define the rankings on the subset of outcomes of the game that the individual can access, whatever others do. But how do these different modelling choices relate to each other? Why select one of these measures of freedom over the other in order to rank social states? No answer is provided in the Freedom of Choice Literature. This diversity of solutions only demonstrates the lack of a unified understanding of what freedom means in an interactive context.

However, on political philosophy’s side, philosopher Christian List has built a general and formal framework for defining and distinguishing several substantive conceptions of freedom (2006: Republican freedom and the rule of law Politics, Philosophy and Economics, 5(2), 201-220). He also argues that conceptions of freedom in a social context are faced with a trade-off between two dimensions of freedom, which he calls "scope" and "robustness". The "scope" dimension of freedom concerns the size or richness of the set of options within which the individual can choose in a given situation. Intuitively, more options gives more "scope" to our freedom. The "robustness" dimension concerns the stability of a given set of options when some relevant features of the situation vary (typically, when other individuals change their preferences). Intuitively, more power endows us with a more "robust" freedom.

I propose to use this framework to set up a classification of possible rankings of game forms in terms of freedom which would account for each modelling choice in the literature and connect it to conceptions of freedom. First, I argue that a third dimension, which I call "diversity", is missing in List’s map of the conceptions of freedom. It bears on the variety of the outcomes that could have been reached by the individual if some aspects of the situation were different. Secondly, I show how a measure of freedom in game forms defined along the previous dimensions can encapsulate information about power and potential interference from other people. Finally, this leads me to assess List’s claim that there exists a trade-off between the "scope" and the "robustness" dimensions of freedom: it turns out that the structure of game forms makes the conflict particularly obvious. Another conflict between "robustness" and "scope" on one side and "diversity" on the other is also highlighted.
Beliefs, preferences and choice

Lukas Beck. On the Dispositional Conception of Preferences

There is a lively debate about the nature of preferences in economics (Hausman 2011, Dietrich and List 2016, Clarke 2016, Angner 2018). The debate aims at establishing a coherent conception of preferences that captures the actual use of the term in economics. Yet, it is also interested in a conception that can guide economic research, e.g. by informing us about how much economists must rely on psychological findings to build models of choice-behavior (see Clarke 2016). The focus has been on mentalism and behaviorism. The behavioristic conception of preferences identifies them with choice-behavior (Guala 2012). Mentalistic conceptions identify preferences with some kind of mental-states (see Hausman 2011). Many authors argued that identifying preferences with choice-behavior is untenable and concluded that some version of mentalism is correct (Sen 1977, Hausman 2000).

Yet, recently a third alternative has gained traction. The dispositional conception of preferences (DCP) identifies preferences as belief-dependent, multiply-realizable dispositions (Guala, forthcoming). According to DCP, preferences are not necessarily mental-states because dispositions can be realized by other entities than mental-states. Yet, preferences are also not identical with choice-behavior as they are dispositions that causally produce the relevant choice-behavior. Guala (forthcoming) points at the successful application of economic choice-theory to many domains beyond human individual decision-making (e.g. hermit-crabs (Elwood and Appel 2009) and firms). He then argues that this wide applicability of choice-theory requires an abstract conception of preferences like DCP.

While I agree that DCP better captures the actual use of preferences in economics than mentalism, I will argue that we need to augment DCP with additional commitments about preferences to arrive at an account that is able to guide economic research. Therefore, I will explicate how different additional commitments about preferences have different implications on the relevance of certain research from the psychology of decision-making for the applicability of economic models of choice-behavior to human agents. Hence, adequately responding to this research requires us to take the correct stance with respect to these additional commitments.

More specifically, the psychology of decision-making is often evoked to argue that standard economic models of choice-behavior fail to capture human choice-behavior (Simonsos 1989, Kahneman and Tversky 1979, Camerer 2008). However, how far such research can support this depends on whether the causal-base of human preferences can be entirely located within the body of human agents. Humans are usually separated from their familiar environments when studying their decision-making capacities in psychological laboratories. Yet, DCP is compatible with the causal-base of human economic preferences being partially realized by the agents’ environment and therefore partially located outside their bodies. If this is the case, separating humans from their familiar environments can alter the properties of their preference relations. Consequently, the preference relation realized by the psychological mechanism studied by the psychology of decision-making may not correspond to the preference relation human agents exhibit in their familiar environments. This would undermine the relevance of such research for economics. Given that the precise nature of the causal-base of human economic preferences matters for the relevance of certain psychological research for economics, DCP needs to be either paired with the idea that the causal-base of human economic preference can be partially realized by their environments or with the one that it is solely located within the agents’ bodies. Consequently, DCP needs to be augmented with further commitments to be able to guide research.
I first outline the current state of the debate and argue that DCP better captures the actual use of preferences in economies than alternative conceptions. Second, I give an overview of the psychology of decision-making to support that it is usually implicitly assumed that the causal-base of human economic preferences is solely located within the agent’s body. Third, I motivate the idea that the causal-base of preferences may also be located outside the agent’s body. Fourth, I highlight the importance of augmenting DCP with additional commitments about preferences to arrive at an account that can guide research.

John R. Welch. When Econs Are Human

Econs, who were famously contrasted with Humans in Thaler and Sunstein’s *Nudge* (2008), are the imaginary species whose Linnaean classification is Homo economicus. Econs are characterized by unbounded rationality and an aptitude for optimal choice. Nevertheless, this paper argues that, in a certain class of situations, Econs aiming to optimize would choose like boundedly rational Humans using simple heuristics.

Central to this paper are economic choices conditioned by Knightian uncertainty (1921). In such conditions, because the probabilities of relevant states of the world are unknown (if objective) or unspecified (if subjective), the standard decision-theoretic maneuver of ranking options by calculating expected utility is inapplicable. Still, Econs could optimize even in these numerically impoverished circumstances. They could do so by relying on leveraged applications of qualitative terms such as ‘low’, ‘medium’, and ‘high’. The leverage is provided by a comparative version of decision theory that takes inputs in the form of comparative plausibilities and desirabilities and produces outputs in the form of expected desirabilities. These outputs can then be compared in the imprecise but actionable terms of <, =, and >.

Katsikopoulos (2014) has pointed out that the investigation of bounded rationality has spawned two different cultures: idealistic and pragmatic. Idealistic studies such as prospect theory (Kahneman and Tversky 1979), rank-dependent expected utility theory (Quiggin 1982), and cumulative prospect theory (Tversky and Kahneman 1992) retain the aim of optimization from Econ culture, though they modify Econ axioms to attain greater psychological realism. But pragmatic studies carried out in the research program on simple heuristics (e.g., Gigerenzer et al. 1999, Hertwig and Herzog 2009, Gigerenzer et al. 2011, Todd et al. 2012) are inspired by Simon’s (1956) goal of satisficing.

The investigation of simple heuristics has identified a heuristic—variously referred to as equal-weighting models (Dawes 1979), tallying (Gigerenzer and Goldstein 1996), Dawes’ rule (Gigerenzer et al. 1999), or the 1/N rule (Gigerenzer and Sturm 2012)—that is especially relevant here. This paper shows that this heuristic can be applied such that comparative decision theory emerges as a special case. In conditions of Knightian uncertainty, then, an Econ optimizing with comparative decision theory would make the same decisions as a Human satisficing with this simple heuristic. This result opens up new perspectives on the relation between normative and descriptive approaches to rational choice. The paper illustrates its claims with an example drawn from life: a momentous decision by the Federal Reserve Board in the United States.
When we say that an individual exhibits too much confidence in their subjective beliefs about an event, what do we mean? Some have defined overconfidence to mean overestimation of one’s ability or performance; others have defined overconfidence to mean overplacement of one’s self relative to others; and yet others have defined overconfidence to mean excessive certainty about the accuracy of one’s belief (Moore and Healy [2008]). We focus on the last of these definitions, the notion of overprecision of beliefs. We present rigorous behavioral evidence on the bias and overconfidence of individuals. We seek to classify individuals into one of the four types illustrated in Figure 1, or as one of the types implied but not shown in Figure 1.

To provide a meaningful definition and measure of overconfidence in this sense, we need two things. First, we need to elicit the latent subjective belief distribution that an individual has for some observable event. Second, we need to define a metric with which to evaluate the precision of those elicited beliefs. Both of these steps entail complications that have not been fully addressed in the previous literature.

There are many ways to elicit subjective belief distributions, and we employ familiar scoring rules defined over continuous events or discrete, non-binary events. We take care to infer latent subjective beliefs from observed reports that are financially motivated, correcting for the effects of estimated risk preferences of the individual. Our behavioral interface does not place heavy demands on subjects, who simply place bets in accordance with their beliefs and see the implied payoffs if they are correct when the event is realized (Harrison, Martínez-Corra, Swarthout and Ulm, 2017). We then recover the latent beliefs of individuals, conditioning our inferences on estimated risk preferences for the individual (Harrison and Ulm, 2015). We allow individual risk preferences to be characterized by Expected Utility Theory or Rank-Dependent Utility.

We also consider inferences about beliefs that assume risk preferences that relax the Reduction of Compound Lotteries (ROCL) axiom. This seemingly technical extension is actually fundamental to any rigorous discussion of confidence. The reason is that an agent that behaves consistently with ROCL will treat any subjective probability distribution as equivalent to a subjective probability that is equal to the weighted average of that subjective probability distribution. In effect, confidence might not play a role for any agent that obeys ROCL.

Two broad applications of these tools are considered and implemented. One is to evaluate the descriptive validity of Bayes Rule, and the other concerns the elicitation of norms that rely on beliefs. To evaluate Bayes Rule we employ an experimental task that allows us to precisely calculate the posterior distribution that beliefs are being elicited over by applying Bayes Rule. This posterior distribution is used as the basis for identifying bias, defined as a statistically significant difference between the mean elicited belief and the mean of the posterior distribution. This posterior distribution also allows us to directly identify overconfidence or underconfidence, defined as a statistically significant difference between the variance of elicited beliefs and the variance of the posterior distribution. Remarkably, there are no studies that consider incentivized reports about beliefs, correct for the effects of risk preferences, and use a metric for evaluation of bias and overconfidence for a specific individual and event. Many of the pioneering experimental studies of Bayes Rule used incentivized methods for eliciting subjective probabilities for a binary event, but this does not permit any discussion of confidence with respect to a continuous or discrete, non-binary event; nor was that the goal of those experiments. Several recent studies of overconfidence in the psychology literature have clearly recognized the need for a metric that is specific to the individual and event for which beliefs are elicited, but do not employ incentivized reports about beliefs.

Our findings on Bayes Rule are simple and clear. In general subjects exhibit no statistically significant bias in relation to Bayes Rule, but exhibit statistically significant overconfidence. Hence subjects
in general are characterized by panel A of Figure 1. The overconfidence starts with the first samples received, and persists as additional sample evidence accumulates. There are also some identifiable demographic effects. The marginal effect of being older is to exhibit statistically significant bias as well as overconfidence (as in panel C of Figure 1, albeit with a different qualitative bias). Females have a statistically significant marginal effect of overconfidence with larger samples, although again in an unbiased manner (as in panel A of Figure 1). Finally, the marginal and total effect of having a small initial sample is associated with a statistically significant insufficient confidence with no bias (as in panel B of Figure 1), but these beliefs become better calibrated as larger samples accumulate over time.

The application to the elicitation of norms that rely on beliefs follows the approach of Cristina Bicchieri, as illustrated in Bicchieri and Xiao (2009), and extends it to consider the implications of confidence of beliefs distributions. Results are expected by June 2019.
De-idealization in economic modelling

Ekaterina Svetlova. On the uses of unrealistic models in financial markets

The paper discusses the traditional problem in the philosophy of science and economic methodology regarding how and for which purposes unrealistic models can be used. The focus is however not on a particular scientific discipline but on model use in the applied field of financial markets. Especially in the aftermath of the 2008 crisis, the “unrealisticness” of financial models became an issue: As abstract and unworldly constructs, they were accused of causing the turmoil or, at least, of failing to give advance warning. Thus, financial markets offer an interesting field of analysing the use of unrealistic models in a specific professional practice that principally differs from scientific modelling. The paper assumes the sociology of economics and social studies of finance perspective in discussing these issues.

First, the paper briefly reconstructs the major historical trajectories of the long-standing debate on unrealistic models in philosophy of science, STS, sociology of economics and social studies of finance. It primarily shows how this debate evolved from the focus on representation and the everlasting discussion about the connection between models and their target systems (e.g. isomorphism (van Fraassen 1980), similarity (Giere 2004, 2010) or partial resemblance (Mäki 2009)) to a pragmatic, practice-oriented view on models (Morgan and Morrison 1999; Giere 2004, 2010; Frigg 2006; Frigg et al. 2013; Morgan 2002, 2012; Knuuttila 2005, 2011 and Mäki 2017). The questions of “What qualities do models need to make them useful?” and “What functions do models play in science?” were considered to be more fruitful (Morgan 2012: xvi). For philosophers and STS scholars, models were no longer the purely theoretical and abstract entities, but rather “dirty” and insecure tools that must be manipulated and “made to count” in situ in order to produce knowledge. The pragmatic accounts argued that the gap between models and reality can be closed in the process of model construction and model use by means of pre-formulating the anticipated results, narratives, interpretations, power relations and the audience. I will show how social studies of finance emancipated from and built upon this discussion (MacKenzie 2006, Muniesa 2014).

At the same time, the paper points out to the major difference between modelling in science and model use in financial markets. This difference is that financial model users are directly involved in the markets and thus are a part of reality that is modelled. In contrast to scientists, they cannot tolerate the distance to the markets and need models that are not just “caricatures of reality” (Morgan 2012: 384) or “credible worlds” (Sugden 2000) but guide them through the world and enable them to decide and act. Models are de-idealized and “made to count” in situ of markets.

Based on numerous empirical accounts provided by social studies of finance scientists, including my own research based on qualitative interviews and participant observations, the paper demonstrates how exactly this de-idealization happens, and provides a classification and systematization of distinctive styles of model applications observed in modern markets. Particularly, it identifies three major patterns of model use in financial decision-making – “qualitative overlay”, “backing out”/“implied modelling” and models as “opinion proclaimers” – and highlight the major commonalities and differences between them. This systematization is an important novelty of the paper. The identified uses of unrealistic models include the on-going comparison of model results with judgment (commentary), reconciling of models’ parameters with subjective views and stories as well as “feeding” of users’ subjective views into the model that becomes a translator of opinions into numbers.
Finally, the paper demonstrates that financial models are applied not only to make decisions but also to provide legitimacy for decisions, perform impression management and reach a consensus. The article concludes with implications for further research.

Josafat Hernandez. Abstraction, idealizations and scientific laws in Karl Marx’s thinking

The notion of “idealizations” was popularized by the Polish philosopher Leszek Nowak, who made a reinterpretation of Marx’s method, exposed in the Grundrisse, where Marx said that the correct method of political economy must depart from the analysis of a concrete fact. In this method, each part of a concrete whole is decomposed and is isolated for a separate analysis. Then there is a coming back, from the abstract to the concrete, where each analyzed part is re-integrated in a systematic view which helps us to understand a concrete fact. The synthesis of a number of different determinations.

Nowak reformulated the Marxian view on the method using other terms: idealization and de-idealization. An idealization is a mental operation the scientist introduces by using idealizing assumptions. These assumptions could be exaggerations (e.g. vacuum assumption) which misrepresent a fact, in order to simplify the complex and make easier the mathematical and conceptual manipulation in a model. Nowak’s view is that an idealization seeks to neutralize the secondary properties of phenomena in order to isolate the primary properties, an essence. This essence could be understood as a scientific law which operates in “ideal conditions”, in “pure state”. Then, a process of de-idealization takes place, which consists in simply adding back the idealizations introduced, for being substituted for more realistic assumptions.

In this paper, I will make a reinterpretation of Marx’s notions of abstraction, idealizations and scientific laws in his book Capital, in order to evaluate Nowak’s view on idealizations and Marx’s method. From my point of view, the Nowak view could generate some methodological confusions about the role idealizations have in science. These confusions could lead us to have a poor notion of abstraction, to think that idealization and abstraction are two different mental operations and that the “de-idealization” or “concretization” process is simply removing and adding back idealizations. In contrast, as I will show, Marx made models where he used idealizations to understand how the capitalist system works. In Marx, idealizations are part of a wider process of abstraction, and the process of concretization (de-idealization) is a creative moment of the synthesis, which is complementary to the previous moment of analysis.

Jaakko Kuorikoski and Aki Lehtinen. Confirmation by realism

Macroeconomic DSGE models are routinely criticized for being based on fundamentally unrealistic assumptions. The perceived problem lies not just in the necessary and ubiquitous idealizations and omissions common to all models, but in that the core theoretical (structural) assumptions depicting life-time individual optimization with rational expectations are taken to be necessary for the use of the models in counterfactual policy analysis, but are seriously mistaken. If this is the case, any improvement in the empirical performance of these models is mostly due to accommodation of the increasingly complex models to available data, not to the models getting the underlying causal structure right.
The twist in this tale is that many improvements in the underlying theoretical assumptions of DSGE models seem to have led to improvements in their empirical performance (fit and/or predictive accuracy). If you de-idealize a particular assumption in a complex model, and this improves the empirical performance of the whole model, does this constitute extra confirmation for the rest of the structural (realistically interpreted) parts of the model as well? The intuition is the following: wouldn’t it be unlikely for an improvement in a particular theoretical assumption to lead to empirical improvement also in variables not directly related to the assumption in question, unless the other central theoretical assumptions also got the rest of the underlying causal structure more or less right? This possible extra confirmation is to be distinguished from merely accommodating the model to existing data by tweaking a particular assumption or a parameter – an exercise in curve-fitting without any necessary connection to the structural interpretability of the model.

We study whether and under what conditions such success in piecemeal theoretical de-idealization provides confirmation for the rest of the model – what we call confirmation by realism. We deploy the confirmation-theoretic framework of Gerhard Schurz (2014) to formulate conditions for confirmation by realism. Key questions to be answered are: i) When is the de-idealization sufficiently independent from the resulting improvement in fit, so that it does not count as mere accommodation? ii) What determines whether another theoretical part of the model is also relevant for the resulting overall empirical improvement, so as to warrant distributing confirmation to it?

Schurz uses the concepts of content parts and relevant deduction to explore whether confirmation spreads to “evidence-transcending” content parts of a model. Schurz argues that this is not determined by probability theory (priors or likelihoods), nor the content part’s role in the derivation of the confirmed result, but also by the procedure by which the result is derived. Schurz proposes that this extra confirmation requires that the content part is necessary for the increase in $P(\text{result} | \text{model})$, and that the model did not result from such an accommodation of the content part to the result that would have been equally possible for every possible “outcome” result (within a set of plausible and relevant outcomes). We apply this condition to a toy DSGE to ask whether “microfounded” modifications satisfy this condition and thus warrant confirmation by realism to the structural core of the model.

Pluralism in economics

Jonathan Perraton. Macroeconomics since the Crisis as a Case of Structured Pluralism

There was a variety of responses from economists to the 2007/08 global financial crisis on the future direction of macroeconomics. After a pre-crisis period of apparent emergent consensus in macroeconomics, the re-emergence of vigorous debate was initially seen as prefacing more a fundamental examination of the methodological basis of the discipline. Claims for the re-emergence of core divisions between ‘freshwater’ and ‘saltwater’ schools were seen as signaling the end of the earlier apparent consensus. A number of authorities drew parallels with debates of the 1930s and 1970s.

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More than a decade on, some stock-taking is required here. The picture since this initial debate is somewhat paradoxical. There have been various calls to ‘rethink’ macroeconomics. There has been a radical questioning of aspects of macroeconomics previously regarded as central to core models from the NAIRU and Phillips Curve models to the almost canonical use of Euler equation consumption models. Fields not previously regarded as central to macroeconomics have been incorporated into key models, most obviously in the case of modeling the financial sector but also in areas such as the treatment of inequality, effectively moving away from representative agent approaches. At the same time, the initial prospects for a broad reflection on macroeconomic methodology – let alone ontology – have receded, arguably in contrast to the 1930s and 1970s.

This paper interprets developments since the global financial crisis as a case of ‘structured pluralism’ (Dow, JEM, 2004). It delineates the evolution of various responses to the crisis over time – not just the explicit initial responses, but developments since then. Although there are clear adherents to particular approaches, in partial contrast to earlier periods this pluralism is less clearly a case of clearly defined competing schools of thought. In part this reflects broader developments that in key respects there is no longer a clear corpus of mainstream economics. In some respects this makes assessment more complex than in Dow’s earlier work where the existence of more clearly defined schools of thought with methodological precepts could be assumed.

This paper investigates the evolution of various macroeconomic approaches since the crisis, including adding previous neglected features, (qualified) defences of new Keynesian economics and various attempts to somehow link macroeconomics more closely to macroeconomic developments. As explicit methodological discussion has preceded and schools of thought remain unclear the paper sets out how structured pluralism has permitted degrees of effective communication between different approaches but has meant that questions of methodology and ontology became increasingly buried. It offers some suggestions for how methodological issues could be made more explicit in macroeconomic work and the concerns raised initially after the crisis could be more clearly addressed.

Ioana Negru and Alessandro Vercelli. Monism and pluralism in macroeconomic methodology: a critical appraisal

Macroeconomics has been widely questioned since the Great Recession. Its critics have pointed out in particular its unrealistic assumptions and, the over reliance on the acritical use of quantitative methods and, not least, a lack of pluralism. It is this last point that this paper engages with, attempting to sketch the evolution of pluralism within macroeconomics and to discuss its policy implications. Keynes established macroeconomics as an autonomous discipline in the 1930s as a consequence of a paradigmatic change supported by a methodological revolution, followed in the 1970s by a paradigmatic and methodological counterrevolution led by Lucas, Sargent and Barro. The development of macroeconomics has been a dialectical one, starting with the confrontation between Keynesianism and classical ideas, a dualism and parallelism that have continued throughout the evolution of macroeconomics.

In this macroeconomics context, what has been the role of methodological monism and pluralism? The first use of the concept of methodological pluralism in economics was in Beyond Positivism by Bruce Caldwell (1982, 1994). Bruce Caldwell, adept of the growth of knowledge tradition, believed in a non-linear conception of science, characterized by dynamism and flux, by bold conjectures and refutations, and in the impossibility of finding ‘universally logically-compelling’ methods of theory appraisal. The impossibility of finding ‘a valid single method’ motivated his conception of methodological pluralism. Caldwell outlines 4 stages of a program for methodological pluralists, involving:

a) A rational reconstruction of each research program;
b) An assessment and criticism of the methodological content of each research program;
c) A critical analysis of the strengths and limitations of each ‘rational reconstructed methodology’;
d) The recognition of an existence of numerous rival research programs and their critical evaluation.

The latter point is seen by Caldwell as reflecting the most helpful and contentious aspect regarding methodological pluralism. Caldwell elaborates on the link between the role and importance of criticism, freedom in science and the process of revealing strengths and limitations of various theories and methods. He advances a distinction between internal and external criticism of programs, illustrating the case of the fruitfulness of internal criticism with the help of Austrian economics. But one needs epistemological pluralism in order to understand and fundament Caldwell’s project (see Dow, 2012). Starting by Caldwell’s perspective on methodological pluralism, this paper attempts to discuss the role and forms of monism and pluralism within macroeconomics and the importance of a methodologically pluralist position in macroeconomics for research and policy. Does methodological pluralism represent an independent methodological stance and are there research consequences for adopting this approach? Does it establish certain standards?

Often, there is a degree of reductionism involved and pluralism of methods is conflated with methodological pluralism and methodological eclecticism. Dow (1997, 2012) makes a clear distinction between pluralism of method as a methodological stance that advocates the lack of definitive criteria for selecting methods for economic research and methodological pluralism that is a meta-methodological position that involves the study and analysis of various methodologies. The paper discusses further this distinction, assessing the status of mixed research methods in macroeconomics.

Mario Aldo Cedrini, Angela Ambrosino and John B. Davis. Unity of Science and Disunity of Economics

Unity of science has been a very popular topic in history. The twentieth century saw the development of a radical version of this vision, the project of unified science advanced by logical positivists of the Vienna circle, and a series of attempts to build unity on a model of theory-reduction. Critics of this strategy, however, have opposed such efforts – and successfully illustrated the somehow mythical nature of the underlying logic – by emphasizing the irreducible plurality of sciences and the resulting, fundamental disunity of science. The significant difficulties associated with defending convincingly any “unity-of-science” project on traditional bases (ontological, theoretical, or methodological reduction unifications) directed the attention of both supporters and critics towards more flexible versions of unity (such as the proposal of unification as a “regulative ideal”) and to the theoretical and practical foundations of interdisciplinarity – unity being seen in terms of integration between different disciplinary approaches.

Some recent proposals for unifying the social sciences have originated within the economics profession. Our aim is to investigate the reasons why economists are increasingly debating this issue, and the (theoretical) feasibility of such projects, in the light of the current state of the relationships between economics and contiguous disciplines. The paper proposes a general theoretical framework in order to distinguish a set of possible options for integration between social sciences. As pointed out by various philosophers of science, interdisciplinarity (in its general meaning) is old news, and even the most integrative forms of disciplinary interaction seem to emerge from established disciplines. We therefore use disciplines as units of analysis, and adopt the so-called “nation” metaphor in order to investigate, using the analogy with Dani Rodrik’s “world political trilemma” (whereby democracy, national sovereignty and global economic integration are mutually incompatible; it is possible to combine any two of the three, but never
have all three simultaneously and in full), three ideal types of disciplinary integration, or three different roads to the realization of the unity of social science (which we here call, respectively, “reductionism”; “federalism”, and “complexity”).

The framework is then applied to three main “integration” projects, with a view to throwing light on their theoretical underpinnings and expected outcomes: 1. David Colander’s (and others’) idea of a ‘transdisciplinary’ social science, based on cross-fertilization of methods and approaches; 2. arguments advanced by influential philosophers of social sciences and economics (like Don Ross, Harold Kincaid) that regard disciplinary adaptation (of economics to sociology and psychology, and vice versa) as a possible solution to the unsolved problem economics has with the “social” dimension; 3. Herbert Gintis’ proposal for a new theoretical framework using the evolutionary perspective and game theory as bridges for unifying otherwise incompatible disciplinary approaches to human behavior.

While throwing light on the features of disciplinary relations as conceived in each of these projects, we speculate about their “implicit” origins, which we generally identify in the disunity characterizing the current fragmented state of economics. We then discuss the potential impact of these integration projects by focusing on the issue of pluralism in both the social sciences – which some proposals explicitly want to counteract – and in particular within economics, at a time of pervasive specialization, continuous creation of scientific research niches and declining cross-science research programs.

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**Economics and cognitive sciences**

*Olga Koshovets. Neuroeconomics’ “interdisciplinary” strategies: unbridgeable conceptual distance or sciences speaking the same language of Nature*

Having arisen as a new radical natural science-based methodological program in economics, neuroeconomics is being developed as a study of the biological microfoundations, which underlie economic cognition and behavior. All these microfoundations are considered as biological mechanisms that influence rational decision-making and this enables neuroeconomics to give a promise to improve economists’ ability to forecast behavior [Glimcher, 2003; Camerer, Loewenstein, Prelec, 2005; Sanfey et al., 2006; Camerer, 2007]. This neuroeconomics’ ambition to do better economics by examining brains has been heavily criticized; some have charged that neuroeconomics is a brain-centric enterprise, and neuroscience and economics are fundamentally incompatible, others have argued that economic hypotheses cannot be falsified using neuroscience data and are irrelevant for economics’ studies of decision-making.

Both criticism from economists and huge debates over neuroeconomics’ contribution to economics seems to be inspired by a negative attitude to the neuroscience intervention to economic theory. Indeed, since the 1990s economics have experienced some kind of imperialism as other sciences’ attempts to export their different conceptual tools and experimental practices. Neuroscience appears to be succeeding in attacks to the disciplinary boundaries of economics. Yet the possible influence of other sciences on economics challenges economics’ postwar self-isolation and mainstream economics’ implicit conception of itself as a self-sufficient science. Thus, it is hardly surprising that much of debates on neuroeconomics
contribution to economics have been limited to the question of what tools, information and methods from neurosciences might be employed in economics.

Meanwhile, economics itself is well known for its imperialism. In the 1970s and 80s the economics’ imperialism was an attempt to export economics’ understanding of what good science required to other sciences [Lazear, 2000]. Over the last few decades these ambitions have been reinforced as the theoretical apparatus of mainstream contemporary economics, i.e. rational choice theory, is established as domain general and its formalism does not explicitly refer to economic phenomena at all [Rodrik, 2015]. Certainly neuroscience’s reverse imperialism toward economic theory is capable of undermining economics’ claim to be an exemplary science with self-sufficient theory, concepts, explanation and powerful formal tools. However, neuroeconomics is little by little becoming a very strong and unusual form of economic imperialism. Indeed, unlike behavioral economics, where some concepts and experimental methods from psychology have been imported over to economics to explain various individual “anomalies” in choice behavior, in neuroeconomics much of the intellectual traffic has gone in the other direction – economic tools are widely used to model psychological and neural processes. Eventually, mathematical models coming from economics are naturalized and spread across the disciplinary frontiers, transforming the subject-matters and ontologies of the different sciences into the ‘local versions’ (paraphrasing Glimcher) of economics. This is how neuroeconomics turns from biological reductionism to economic imperialism.

In my presentation I aim firstly at reviewing methodologies and epistemological grounds of neuroeconomics evolving in two different strands of “behavioral economics in the scanner” and “economics of neural activity”. I identify them preliminarily as versions of biological reductionism and economic imperialism, respectively. Next I will consider it more particularly as possible “interdisciplinary” strategies of the discipline advance. Possible combination or even synthesis between core elements of sciences involved in neuroeconomics projects poses a question, what means of research, conceptualization, explanation and representation are preferable. Then I will analyze the contribution of economics, psychology and neuroscience to neuroeconomic research focusing on ontology (axiomatic core), empirical basis and epistemology of each discipline and on possible (un)bridgeable distance between initially conceptually disparate areas.

In the next section I will address mainly to Paul Glimcher neuroeconomic project as it is being implicitly developed as a specific version of economic imperialism. I attempt to show that Glimcher intends to reinterpret and rewrite neuroscience and other life sciences in terms of economics arguing that the brain can be modeled using the principles of standard economic theory and seeking to create a unified science encompassing neuroscience, psychology, and economics. The attempt to build a “unified science” disregards (as “useless metaphysical arguments”) possible unbridgeable distance or even ontological gap between social and natural. Neuroeconomics “successfully” overcomes the gap arming itself with mathematics as the universal language of Nature.

In the last part I will turn briefly to some inevitable yet problematic consequences arising from adoption of concepts from other disciplines and their reinterpretation. In particular I will highlight that there is a big ontological issue, as by adopting an alien concept (choice) one does not only describe with it something new (certain neurons activity) in a metaphorical manner. Over time one really comes to think of neurons activity as a choice.

Chiara Lisciandra and Michiru Nagatsu. Beliefs & Beliefs: Asymmetries Between Behavioral Game Theory And Behavioral Decision Theory

This paper focuses on the asymmetries in the interdisciplinary exchange between economists and psychologists in behavioral game theory and behavioral decision theory. Such asymmetries concern both
the development of the two research programs and their impact in the scientific community. Our initial observation is that the axiomatic treatment of subjective expected utility theory has been modified substantially through the interactions between psychologists and economists; by contrast, the modification of Nash equilibrium has been left mostly to economists. In other words, the strategic side of rational choice theory has not prompted the same pattern of interdisciplinary exchange between economics and psychology; although it has influenced many other disciplines in different ways, game theory has not been modified by psychology like decision theory has. This is surprising because in many ways game theory shares important features with the formal treatment of expected utility theory: for both theories, the mathematical proofs of existence and uniqueness are foundational. Moreover, beliefs and desires are central in both theories and may lend themselves to psychological analysis.

We will argue that one of the reasons for this outcome is that the very notion of beliefs is interpreted and used differently in expected utility theory and in game theory. Our claim is that this may be explained by economists' specific way of doing equilibrium analysis of aggregate-level outcomes and by psychologists' reluctance to fully engage with such practice. We focus on the notion of belief that is embedded in economists' practice of equilibrium analysis, and argue that its difference from the psychological counterpart is one of the factors that makes interdisciplinary exchange in behavioral game theory more difficult. As a consequence, to these days behavioral game theory as a subfield of behavioral economics remains mostly economists' game.

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Blaz Remic. Intrinsic motivation: an active exchange between psychology and economic

How to bring the findings from psychology into the social sciences and thus provide a firmer behavioral foundation has been one of the key issues in recent economics. As I argued elsewhere (** & ***, 2018), the question is not simply about whether or not to combine economics and psychology, but what particular combinations are best suited to address particular problems. This paper will build on this argument and investigate the various ways in which the concept of intrinsic motivation has been developed within psychology and adopted in economics. As such, the paper will provide a detailed historical account of the complex interplay between the two disciplines.

In psychology, the term intrinsic motivation became prominent in the early 1970’s as a group of psychologists developed a critique of the behaviorist exclusive focus on external stimuli for studying human behavior. From the beginning there existed several strands within that group (see Lepper & Greene, 1978), whose differences would later take them in divergent directions, such as Deci’s cognitive evaluation theory (as a part of what later developed into the self-determination theory), Lepper & Greene’s (1973) overjustification theory (based on the self-perception theory), and Kruglanski’s (1975) endogenous-exogenous attribution (recently restated as a ‘structural model of intrinsic motivation’). Through the following decades, SDT developed into a major research program, recently culminating in Deci & Ryan’s magnum opus (2017), but the underlying conceptual debates remain alive (Kruglanski et al., 2018).

These different approaches are also recognizable within economics. There the concept was quickly picked up because of its relation to ‘the hidden cost of reward’ phenomenon - an empirical finding that incentives may under certain conditions result in less, rather than more effort, providing a possible refutation of the relative price effect. Three important historical episodes are of particular interest: Frey’s application of SDT into his ‘motivation crowding theory’ (Frey, 1997); Benabou & Tirole’s (2003) reconceptualization of the ‘overjustification’ account as a signal-extraction problem, and the further application of the crowding theory to the issues of prosocial behavior and social preferences (see Bowles & Polania-Reyes, 2012). All three episodes represent active processes of exchange, where concepts were not
merely imported but transformed, and in return these adaptations often influenced research within psychology (e.g. Ryan & Deci, 2017).

By studying the way in which theories and empirical studies of intrinsic motivation happened at the borderline between economics and psychology I aim to provide three contributions. Firstly, I aim to further complicate the standard narrative of a natural combination of economics and psychology. Secondly, I will show the complex process of disciplinary exchange. And thirdly, I will use these historical investigations to provide more conceptual clarity in the current debates over intrinsic motivation. This is particularly needed since under the single banner of intrinsic motivation issues as diverse as prosocial behavior, activities pursued for their own sake, the value of choice-autonomy, and the effects of monetary and non-monetary incentives are discussed.

Methodology

Reinhard Neck. Methodological Individualism – still a useful methodology for the social sciences?

In this paper, we explain the role of Methodological Individualism as a methodology for the social sciences. We briefly discuss its earlier forerunners in economics and sociology, especially in the works of Carl Menger and Max Weber. Also some comments on Karl Popper’s and other Critical Rationalists’ contributions are made. In the main part of the paper, we discuss some recent arguments against methodological rationalism, especially those presented in Zahle and Kincaid (Synthese, 2018), and provide counterarguments, mainly based on recent exemplary work by economists and sociologists. In particular, we show that evolutionary approaches to the explanation of social and economic phenomena, which accord with methodological individualism, exhibit it as a successful and progressive methodology for the social sciences.

Jose Ricardo Fucidji, Celso Neris Jr. & Rafael Almeida. The performativity thesis and the interactions between economic theories and social reality

Originally proposed by language philosopher John Austin, and later adopted in other social sciences, the performativity thesis has been, since Michel Callon’s interventions, known as a critique of the homo economicus conception, typical of mainstream economics. The idea that economics brings forth activities and markets in the economy (rather than merely describe or interpret them) is what generally one
understands by performativity of economics. The aim of this paper is to engage with several arguments for and against performativity as a useful way of thinking about the economics-economy relationship.

After a survey of the literature on performativity of economics, we put forward our interpretation, viz., an interactive approach to the performativity of economics. In our view, economics and the economy should be seen as interactive poles of a performative phenomenon. Following critical realist transformational model of social activity, we give privilege neither to economics nor the economy in social reality. We hint that this problem should be set in a temporal frame (the economy performs economics, which performs the economy, and so on).

In our view, after probing some theoretical arguments favorable to the proposed interpretation, the performativity thesis has the virtue of pointing out that theories not only impinge on, but also interact with, the economy (through the agents behavior), by creating devices that are incorporated into the institutions, and that are discursively transmitted to the public. The path from economic ideas to the public is a non-linear one, and moreover, a recursive one. Models developed within economics are used to make economic decisions operational in the individual level. And students of economics learn by pedagogical devices that reinforce the grip of the theories that occurs to be teach. This binding grip of mainstream economics deepens its embeddedness in the social reality.

Our interpretation also highlights that the performativity thesis has implications for the resilience of mainstream economics to the critique. Rather than just waiting for anomalies in empirical observations to cause cracks in the dominant theory, and so rearrangement of research and after all a paradigm shift – the performativity thesis points out that dominant theories can be far more resistant to change when they are embedded in the social reality. Theories can be part of social reality not because any criteria of truth as correspondence, but because they are part of the institutional power structures of economics. In case of counter-performativity, when the mechanisms in the social reality responsible for its actualization are no longer operative, theories fade out (as for example the Black-Scholes).

We claim that the performativity thesis is useful for economic methodology as it sheds light on the importance of scrutinizing the relationship between theories developed in the domain of economics and what Marshall called “the ordinary business of life”. Rather than investigating these domains separately, the performativity thesis intends to evaluate the interaction between economics’ actions, practices, and propositions on economy’s ideas, institutions and behaviors. Moreover, the performativity thesis can be a powerful critique of the conception of ontology adopted by the mainstream. If the economics subject-matter is a complex and changing social reality, mainstream economics often deals with it imposing regularities to economic behavior (substantial rationality and its anomalies) and to its systemic effects (equilibrium, whether unique or multiple). These assumptions are then (not necessarily intentionally) percolated into the dealings of economic activity and its institutions, by means of cognitive or discursive devices.

This way, we argue for the epistemological relevance of performativity for economic methodology, i.e. for understanding the relation between theories and models of economics, at one hand and the events in the economy on the other. Furthermore, we claim that just because they are embedded in social reality, mainstream economic theories are so resilient.

Mariusz Maziarz. Recalcitrant results in econometrics: causes and effects

The ‘emerging recalcitrant result’ phenomenon appears when econometricians use the same or similar dataset and arrive at models that support inconsistent conclusions about relations between two variables under consideration (Goldfarb 1995; 1997). Some instances of inconsistent results are influential not only within academia but also shape public policy. For example, Maziarz (2017) argues that the Reinhart-Rogoff
controversy instantiate the ERR phenomenon. Scientific disagreement is a topic of growing importance in the philosophy of science and epistemology (Matheson and Frances 2018). However, climate-change science and life sciences dominate the literature (e.g., Malnes 2008; Isaac 2014). A dominant approach is to focus on disagreements among theoretical models or different kinds of evidence. There is a very limited number of studies focusing on the disagreement among quantitative studies of observational data. An exception is Moosa’s (accepted) study. He argues in favor of a thesis that the results of econometric modeling result from methodological decisions rather than features of data. Despite the passing time, economic methodology lacks studies developing our knowledge on the actual factors that drives disagreeing econometric results and approaches to dealing with using inconsistent empirical literature as evidence for policymaking and informing theoretical discourse.

In my presentation, I want to address these two issues and study (1) the causes of the ERR phenomenon (i.e., the factors that make econometricians estimate new models being in disagreement with previous ones) and (2) its effects (i.e., the difficulty in drawing informative conclusions from empirical literature). In regard to the first question, I review the studies on the causes of ERR phenomenon present in the literature (cf. De Long and Lang 1992; Goldfarb 1995; 1997) and argue that using the usual methods of the philosophy of science such as case study or methodological analysis is not sufficient. These methods are unable to enlighten beyond the ‘stories’ described in econometric papers and only allow for delivering possible explanations that may differ from the actual ones. Considering that the day-to-day (crude) research practice differs from the textbook, idealized view (Granger and Yeon 2004; Reinhart and Rogoff 2010; Spanos 2012), the hitherto research may underestimate the role played by the social and non-epistemic factors in the creation of recalcitrant results. To deliver a solution to the second problem, I first argue that the hitherto methods of studying empirical literature such as systematic literature review or meta-analysis are not informative when the topic under consideration experiences the ERR phenomenon and second analyze possible ways of coping with inconsistent empirical studies.

New directions in economic research

Brendan Hogan. What is Economics For?

The methodological principles that have come to be enumerated as the fundamental starting points of neoclassical economics have been subjected to critique since their inception. The rational chooser, the utility maximizing character of their choices, and the methodological individualism that dovetailed so nicely with advancing methods of quantification have all been subjected to severe critique from a variety of quarters. These critics come from such areas of intellectual specialization as the philosophy of the social sciences, more humanist quarters outside of the sciences, and even within economics itself. The latter group of critics suffered increasing marginalization as the ascendancy of neoclassical economics married a positivist philosophical underpinning to the scientific pretensions and mathematizing tendencies of economics as a discipline.

However, as a now well-told story in philosophical accounts of economics has it, while positivism and specifically the philosophy of science embodied in logical empiricism fell upon hard times and was
bypassed, the mainstream practitioners of economics seemed to ignore entirely the failure of logical positivism to account for a variety of its own aims. These failures reproduced themselves in neoclassical economics in terms of explanation, the irrational choices agents make, and a consistent failure to generate models which would predict aggregate market behavior. Behavioral economics took these failures as their starting point in reintroducing irrationality into their understanding of human decisions and now stands as a major contender for explanatory adequacy in providing economics with more scientific grounds for its intellectual project.

However, the question of the relation of values as intrinsic to the practice of scientific description itself still remains outside the purview of these two major schools of economic thought. This in two ways. First, both schools basically accept that the end of human activity is the realization of individual preferences. On the neoclassical model, this involves the cost-benefit analysis of an internal algorithm that calculates action based upon given preferences and available information regarding means to satisfy those preferences. Behavioral economics wants to introduce paternalistic interventions to overcome the irrationality of individuals in the choices they make to satisfy their preferences. Thus both see action, and economically informed policy as a means for preference satisfaction. That is, whether you model practical reason as neoclassical economists do, on what might be characterized as a Humean desire-belief model, or you follow behavioral economists in eliciting the inherent cognitive biases which interfere with our preference satisfaction, you isolate the question of morality from the discipline of economic thought. This first shows up in both schools’ attempt to provide a value-free and predictive account of economic science.

In this paper I would like to offer as an alternative understanding of economics, one that is informed by a pragmatic account of social science. Under this understanding, it is not only the case that, as philosophers from CS Peirce to Hilary Putnam have argued, facts and values are inextricably intertwined in the practice of all science. Additionally, according to the pragmatic view on offer here, sciences take their cue from the Aristotelian dictum that we should order our method to the object we are trying to understand. Specifically, pragmatism offers a general theory of inquiry as problem solving. And it follows then that if the problems we are trying to solve are not just problems of physics, for instance, but also problems of human beings, they will take on a moral character. Thus, from the pragmatic view on offer in this paper, economics cannot help but be a moral science as issues of labor, distribution, inequality and scarce resources affect the flourishing of the human species, and much more broadly, the flourishing of species on the planet.

Once economics is redrawn in this way, and the pretensions of economics to be a ‘science’ modeled on the natural scientific goals of explanation and prediction are cast aside, the question ‘what is economics for?’ can be given a morally and epistemologically robust answer.

Antonin Broi. 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 altruist 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.

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Cristian Frasser and Gabriel Guzman. What do we call money? An appraisal of the money-or-nonmoney view

Part of the debate fueled by cryptocurrencies has revolved around the question of what we call money. This paper identifies two traditions in monetary theory that have tried to answer that question. On the one hand, the money-or-nonmoney view follows a strategy proposed by a certain form of philosophical essentialism in which there is a set of defining characteristics of money that make it categorically different from other things used in transactions. For this view, therefore, it is possible to draw a precise dividing line
between money and nonmoney. This is possible if there is a set of necessary and sufficient characteristics that can be regarded as the essence of money so that there is never a gradual transition between money and nonmoney. We do not provide a careful historical account of the origins and current echoes of this classificatory ambition in economics. However, we detail a way of reading Menger (1892) that exemplifies an early effort to clearly differentiate between those objects that are money and those that are not. We remark that for Clower (1967), a clear distinction between money and nonmoney was the natural starting point of monetary theory. Building on Friedman and Schwartz (1970), we also show that a number of economists who participated in the debate about the construction of monetary statistics sought to draw a sharp dividing line between money and nonmoney. Admittedly, they disagreed on the correct approach to deriving the dividing line.

On the other hand, the liquidity degree view holds that the multiple assets that circulate as means of payment differ in their degree of acceptability. Since there is not an absolute standard of liquidity, a precise dividing line between money and nonmoney cannot be drawn. We challenge the money-or-nonmoney view, arguing that there is nothing in the nature of money that can be interpreted as a natural kind essence through which money can be categorically separated from nonmoney. The liquidity degree view is, then, an alternative that deserves more attention. A historical work will be required to carefully reconstruct the evolution of the notion of liquidity in the history of economic ideas. However, we argue that for economists such as Keynes (1936), Friedman (1970), Schwartz (1970), Hayek (1976), and modern monetary economists, liquidity is not about the detection of a critical threshold of acceptability. Rather, liquidity is a characteristic—admittedly in different degrees—of all objects that are accepted as means of payment.

The implications derived from the liquidity degree view can be non-negligible. A practical implication of the liquidity degree view is that the question of whether bitcoin is or is not money should be abandoned. Bitcoin can be described as a means of payment with a poor degree of acceptability. Furthermore, the very notion of what can be called money is questioned, as is the attempt to draw an indisputable dividing line between money and nonmoney. Does this mean that economists cannot call a group of means of payment, for lack of a better word, money? No. Following the footprint of Friedman and Schwartz (1970), we think that an alternative is the creation of arbitrary dividing lines that are established as a matter of convenience depending on the purpose of their use. Test a hypothesis. Build a simplified model. Predict a phenomenon. Design a policy. In any case, the motivating purpose arises from the particular needs of people in charge of studying and managing monetary issues.

Wellbeing and decision theory

Tomasz Kwarciański. Beyond mere utility: Towards a morally enriched economics

Well-being is a central concept of economics. First of all, the main assumption of economic theories is that the economic agent is focused on what is “good for her” (is self-interested). Secondly, well-being is the primary criterion of evaluation of the state of affairs in normative economics (as a willingness to pay). And
third, economic progress of societies is evaluated in terms of social well-being measured by GDP per capita. All of this constitutes a specific idea of what it is to a person’s life to go well (well-being) and what it is for something to be good for a person (prudential values). Historically, economic well-being was interpreted in terms of personal utility. Bentham, Mill, and others focused on looking for pleasure and avoiding pain. However contemporary economists and some utilitarianists abandoned this substantial notion of utility and began to understand it as a preference satisfaction.

The paper aims to reconstruct the moral roots of economics embodied in the well-being concept. I claim that to build a morally enriched economics we have to go beyond the notion of mere utility and accept a broader meaning of well-being in economic theories. If such richer meaning of well-being will be accepted, the dichotomy of a self-interested and moral motivation of economic agents can be undermined. I want to demonstrate that to overcome this narrow utility, we can first refer to prudential values, and then link prudential values to moral ones. The moral values, like justice, freedom or dignity, are universal for all human beings treated as persons. On the contrary, the prudential values, concerning what is “good for” particular people and enhancing their well-being, are subject-relative. I want to indicate that among three different meanings of well-being, (1) utility notion, (2) well-being independent from moral values, and (3) well-being compatible with moral values, the last one has the greatest potential to serve as a concept significant for economists as well as policymakers and ordinary people.

The paper will critically scrutinize three indicated concepts of well-being. First, I will reject the utility notion. I am going to follow Amartya Sen’s (1985a) claim that due to the “physical conditions neglect” and the “valuation neglect” the utility notion (understood as a pleasure or desire fulfilment), is not a proper articulation of welfare (well-being). And things suitable to achieve this goal (prudential values). I will build on Kraut’s claim that when something (G) is good for somebody (S), the word “for” in this phrase “(…) refers to the conformability or suitability of G to S” (Kraut, 2007: 94).

Finally, I want to investigate two approaches to the relationship between moral and prudential values. Firstly, prudential values can be treated as distinct and separated from moral values. The supporters of value pluralism defend this view. Secondly, prudential values can be seen as inherently linked to moral values. I would call the defenders of this view value compatibilists. In particular, I want to highlight the following aspects of the relationship between moral and prudential values: (1) scope – moral value is a necessary condition of prudential value – something is good for somebody if it is morally good; the consequence of which is (2) asymmetry – something can be prudentially bad (bad for somebody) even if it is morally good, but cannot be prudentially good if it is morally bad; and (3) generality – moral values are general while prudential values are particular (have more content).

Till Gruene-Yanoff. From Preferences to Well-Being: The Need for Cognitive Mechanisms

Economists and philosophers today largely agree that preference satisfaction does not constitute welfare; rather, individual preferences under the right circumstances are assumed to provide evidence for that individual’s well-being (Bernheim & Rangel 2009, Hausman 2012, 93-101). One important reason for the denial of this constitutive relationship is that humans’ actual preferences are often defective, and that the satisfaction of those preferences would not contribute to well-being. Instead, only preferences laundered from these defects should be considered welfare-relevant (Harsanyi 1997, Rosati 2009); various laundering strategies, ranging from selecting preferences (Bernheim & Rangel 2009), procedural requirements on
preference formation (Arneson 1989) or procedures for translating defective into non-defective preferences (Bleichrodt et al. 2001).

What remains rather vague in all of these accounts, however, is what these preference defects consist in. The above-mentioned authors all gesture towards behavioral economics, arguing that these investigations revealed “systematic flaws in decision making”, including “overconfidence, exaggerated optimism, status quo bias, inertia, inattention, myopia, conformity, akrasia (weakness of will) and addiction” (Hausman 2012, 100). Yet this argument rests on a number of unjustified assumptions.

First, it assumes that the thus-identified “flaws” or “biases” were identified due to their normative deficiency. Yet this is not the case: most of these were identified as deviations from the predictions of the standard expected-utility model. They thus are characterized by their empirical, not their normative properties.

Second, the “preference defect” argument assumes that the thus-observed preferences can be shown to be normatively deficient, even if this was not the purpose of the original investigation. Here it needs to be stressed what actually was observed: behavioral effects in an experiment. The term “bias” as well as some of the names for these biases sometimes suggest that the cause or the mechanism of these behavioral effects were determined in these investigations, but that is actually not the case. Instead, these sets of behavioral observations can be normatively assessed only in the sense that they are shown to violate some normatively valid principle. But here the record is at best mixed: for some of these biases (e.g. inertia, inattention, myopia, conformity, akrasia, addiction) no clearly stated normative principle is available. For others, axiomatizations are available, but it isn’t obvious that the axioms they violate are indeed normatively valid.

Third, at least in some cases, the “bias” names insinuate the operation of particular mechanism that could be deemed irrational or flawed due to e.g. deficient information uptake or processing. Yet despite these insinuations, most investigation provide no evidence whatsoever for such claims. Instead, the observed behavior is typically compatible with multiple possible mechanisms, and at least some of these mechanisms either (i) describe rational information uptake and processing, or (ii) assign all the particularities of the behavior to valuational idiosyncrasies. As long as such mechanisms cannot be ruled out, the welfare-relevance of the observed behavior can thus not be ruled out.

I illustrate the above arguments at the hand of cumulative prospect theory (CPT). First, CPT was proposed, in the words of its inventors, “as a descriptive, not a normative, theory” (Tversky & Kahneman 1992). Second, CPT satisfies those rationality axioms that are uncontroversially normatively valid (e.g. GARP, stochastic dominance). There is no full agreement on how to axiomatize CPT (e.g. Chateauneuf & Wakker 1999), but the most significant difference to expected utility theory (EUT) seems to be located in the weakening of the independence axiom, itself a principle of EUT whose normative validity is often questioned (Mongin 1997). Third, many possible cognitive mechanisms generate CPT patterns. Some of these identify the particular valuations of the individual as the driver of this pattern (Harrison & Ross 2017, 157). Here it is difficult to see why such an underlying value should not be welfare-relevant. Others identify simple heuristics like Maximin or the Priority Heuristics as drivers of this pattern, and defend the ecological rationality of these heuristics for at least some types of environments (Pachur et al. 2018).

The case for the normative deficiency of such biases in general, and for CPT in particular, is thus undecided as of yet. Neither appeal to the context of identification nor to axiomatizations will solve this issue. Instead, more evidence is needed to decide which mechanisms actually produce these biases. Therein lies the key to determine whether a certain preference is welfare-relevant or not.
Voting procedures in social choice (SC) theory describe the manner in which the preferences of individuals are combined to produce a collective decision. A voting procedure is defined by two characteristics: 1) the type of a vote, and 2) the aggregation rule by which votes are counted to find the winner or a preference order. Voting methods are numerous, and using different methods the same group of voters can end up with different outcomes. The more candidates and voters in the system, the more complexities and discrepancies arise. Especially positional procedures are complex: voting outcomes can change when the amount of candidates are either added or dropped. Varying the choice of positional methods outcomes can become most contradictory despite fixed ballots. With some methods some alternatives win while with others they may be bottom-ranked. An election outcome not necessarily reveals the true preferences of the voters but moreover the choice of an election rule. As voting methods are prototypes of general aggregation rules, same kind of inconsistencies may occur in other disciplines – e.g. multi-criteria decision-making (MCDM), economics, statistics - as well.

The Saari (representation) triangle, contrived by the American mathematician and economist Don G. Saari, is a geometric profile representation (i.e. an equilateral triangle simplex). The list of the voter preferences is called a (preference) profile. With a choice set of three alternatives A, B, and C, there are six possible (strict) preference profiles for each mode: A > B > C; A > C > B; C > A > B; C > B > A; B > A > C; B > C > A. These profiles can be represented geometrically in an equilateral triangle, with each vertex representing a choice option. The triangle can be divided into six equally large (ranking) regions representing the specific profiles. The ordinal ranking of a point in the triangle comes from its distances to the vertices where “the closer the better”. The midpoint of the triangle represents a complete tie between the alternatives with equal share of votes for each. The median line initiating from any of the vertices dividing the opposite side of the triangle to parts of equal length, represents a tie between the two other alternatives. A positional election with three candidates A, B and C is defined by the (normalized) voting vector W(S) = W(1), W(2), W(3) = (1,S,0), where S, 0≤S≤1, is a specified weight for a second-ranked alternative (candidate). The positional rule with S=0 reduces to the plurality method W(PL)=(1,0,0). With S=1, the antiplurality method gives the result W(APL)=(1,1,0), i.e. against the 3rd-place alternative. The Borda count W(BC)=(2,1,0), with S=1/2, assigns 2 points for each 1st-place vote, 1 point for each 2nd-place vote - 0 points for 3rd-place votes.

Voting methods in SC and MCDM can be used interchangeably. Candidates (in SC) stand for alternatives (in MCDM) and voters (in SC) for criteria (in MCDM). The more alternatives and criteria the more interrelated dimensions there are to be fitted together to make reasonable decisions. Applying the Saari triangle a multifaceted example of three alternative places for the elderly in need of care to live in is depicted: homecare, supported residence, and health-center ward. The alternatives are ranked according to five criteria: the patient’s overall functioning, with the weight 5 (= 5 votes), cost-effectiveness (of the services), with the weight 4 (= 4 votes), quality of care, with the weight 4 (=4 votes), co-operation with the core family, with the weight 2 (=2 votes), working ability of the personnel, with the weight 2 (= 2 votes), and many other unspecified criteria, with a zero weight (=0 votes). As a consequence, the plurality, the antiplurality and the Borda rules rank the preferences differently. The pairwise rankings produce cyclic preferences, with no rational choice. The perplexing question remains: “Which one is the best choice?”
If, as Hands (2015) has suggested, economic methodology is an inferior good, the years of the last recession should have been a kind of golden age for the discipline. Indeed, much has been written about macroeconomics, particularly its capacity—or rather, incapacity—to model the economy accurately. Yet, the protagonists of these discussions have seldom been economic methodologists or philosophers of economics. The young discipline, though flourishing, seems to have missed a chance.

I argue that this situation is due to the project in which philosophers of economics, particularly those working on models, have embarked. This is mainly a philosophical project. That is, a project that satisfies philosophical curiosity, rather than one that aims at relevance for economic or, more generally, social scientific practice. I call this the attempt to solve the mystery of models. It aims to explain what makes models successful, despite their patent falsities. For the best part of the twentieth century, philosophers of science regarded theories to be what mattered in science: they were the vehicles of scientific knowledge. A good theory made models redundant, temporary. Progressively though, philosophers of science acknowledged that models have a much more important role than the heuristic one they were originally attributed. First, theories were demoted. They are not about concrete phenomena and thus unable to be explanatory (Cartwright, 1983). Then the tool-box view of science, in which theories are just one tool, among many was proposed (Cartwright, 1995). Finally, models were thought of as autonomous objects and the ones that mediate between theories and the world (Morgan & Morrison, 1999). Now philosophers agree that models are the vehicles of scientific knowledge. What is not agreed is why they are so, particularly given the false assumptions with which they are built. Philosophers debate with each other.

What about a philosophical project that aims at relevance for economic practice? I suggest that engaging in such a project involves addressing at least two aspects that, until now, haven't received much attention in the mainstream literature on modelling. First, the relation between theoretical and empirical models. Philosophers have been concerned with theoretical models because these are the ones that pose the mystery. Their relation with other models is thus not well understood. Here I suggest that models can complement each other vertically and horizontally. Verticality refers to different technical means through which a phenomenon is modelled and thus understood. Horizontality refers to a broader understanding of a phenomenon.

Second, a notion of model failure and its potential sources. Attention has been given to understanding what makes models (epistemically) successful. The foil, failure, is as ubiquitous in science and probably more important that it be understood—the consequences of failure can be dire. I suggest that understanding model failure requires delving into at least three practical aspects of modelling. First, the identity of the model user, specifically their incentives. Understanding these helps us determine the threshold of what is accepted as a valid result. Second, the non-epistemic goals of the modelling exercise. Some models are used for other purposes than learning about the world. Understanding the goals helps us determine the criteria used to assess the model’s validity. Finally, the historical context of the model. This helps us compare the model to its predecessors and determine what could possibly be expected from the model.
Travis Holmes and Randall Westgren. Demystifying Idealizations in Economic Modeling

The economics literature on idealization is predominated by a view we will call causal dogmatism (Maki, Weisberg, Hausman and Ylikoski). This view includes two related but independent core commitments:

1. All economic explanations are causal explanations.
2. All economic models are causal and ought to be veridical models.

Support for 1 is vast in the economics explanation literature. This is manifest in the widespread adoption of mechanistic, interventionist and difference-making accounts. These accounts imply that causal explanations involve the search for causal difference-makers. Good explanations consist of a minimal bundle of laws and initial conditions all of which jointly make a difference with respect to the explanandum. Notice that on these kinds of causal explanations, both 1 and 2 are non-problematically consistent with abstractions in models or explanations whereby abstractions, we mean the elimination of detail which is irrelevant to the production of the explanandum. The removal of details or laws which are not causal difference-makers for the explanandum is not only licensed but required on these causal views (Woodward and Strevens).

Concerning 2, the insistence that economic models are causal is undoubtedly motivated by acknowledging that economics is concerned primarily with modeling phenomena that are ontic or out in the world. Veridicality or truth would seem to follow from this as well. A model, if it models ontic processes and phenomena, ought to be veridical.

We believe both 1 and 2 are mistaken assumptions and by extension, that causal dogmatism is a mistaken view. We thus argue for a rejection of both assumptions and the adoption of a non-causal view of economic explanatory models. Particularly, assuming 2 complicates the issue of idealizations —whereby idealizations we mean false assumptions used to simplify an explanatory model— and this further compounds their mysterious nature. The simple mystery is, roughly, “how could explanatory models include false assumptions?” This simple mystery is compounded into a further, more complex problem when causal dogmatism is blended in: “how could economic models which are both causal and veridical contain false assumptions?”

We hold that rejecting both commitments of causal dogmatism and adopting a non-causal view of economic explanation includes two, weighty benefits: first, the problem of idealizations in economic modeling is demystified; second, the non-causal view we advocate enjoys greater parsimony than the other rival accounts which attempt to solve the problem of idealization.

In repudiating 1, we shall argue that there are economic explanations (equilibrium/optimality explanations and statistical, econometric explanations) which are best understood as non-causal explanations. We demonstrate this via examples of economic models, showing how the idealizations integral to them can be better accommodated from a non-causal perspective as well as why the model is best understood as describing a non-causal explanation.

Further, once 1 is rejected, much of the motivation for 2 drops out. The recognition that there are non-causal, economic explanations ushers in the possibility that some economic models, which are part and parcel of these explanations, are non-causal. And the question of how a non-causal, non-veridical model can explain is far less mysterious, helping to dissipate the earlier mystery. We claim that in addition to demystifying the problem of idealizations in economic modeling, a non-causal view of economic explanations also provides a more parsimonious account of idealizations than the rival accounts currently on offer. Pace the causal dogmatists, the non-causalist does not have to try to spin a yarn about how idealizations can be made veridical via a translation method (Maki, 2012) or refer to them as “useful
Robert Mróz. Value judgements and economic modelling - a Weberian perspective

Max Weber famously claimed (1904/1949) that in social sciences there is a need to distinguish and be explicit about normative and positive components of a given piece of research because normative components (i.e. value judgements) will impact policy conclusions. The possibility of any kind of strict fact-value distinction has been refuted by modern philosophy (e.g. Putnam 2004), and it has been also deemed impossible in economics (e.g. Sen 1970). This does not mean, however, that any distinction between facts and values is always impossible (e.g. Niiniluoto 2009). This topic is, therefore, of continued interest to social scientists and philosophers, economists and philosophers of economics among them.

What is curious, though, is that this strand of inquiry does not really connect to what is arguably the most prominent area in today’s philosophy of economics – research regarding economic models and modelling. Models are rightly considered the most important tool in the arsenal of modern economists – economics is a model-based science. But this means that, as far as it is productive to discuss the role of values in scientific practice in economics, it is also productive to connect this topic to the topic of economic modelling. In other words, when providing an account of what a model is, or how the modelling practice looks like in research, a philosopher of economics might want to consider the role of values in modelling.

Consequently, this paper argues that to compare economic models in a thorough way, one should include in such comparisons value judgements expressed or assumed in these models. So our “model of a model” should include these judgements, which is not common in the literature on economic modelling.

One of well-known accounts of economic models, by Uskali Mäki (2009, 2011, 2013), seems an appropriate basis for the inclusion of value judgements. Mäki’s “model of a model” goes beyond standard realist descriptions of models as tools for representing some parts of the world. It includes a modeller using the model for a particular purpose, and an audience the model is addressed to. The claim here is that there might be a need to add more detail to the description of the modelling agent – so as to include her value judgements inherent in modelling practice. The paper shows that an account based on Weber’s insights is well suited to become the basis of such action. It also explains that to proceed in this manner is different than to examine the purpose of the model or its intended audience.

At last, a case study is presented to show how some differences between the models in the Austrian Business Cycle Theory and the Real Business Cycle theory can be traced to the value judgements embedded in those models. These include the differing understanding of what the science of economics should look like, choices between realisticness and simplicity (which could be classified as methodological value judgements), as well as differences in political positions (which could be classified as evaluative value judgements).
Since its formulation by Samuel Bowles and Herbert Gintis in 1985, the labor discipline model has continued to motivate productive, and at times polemical, debate. The model has led to a convergence among heterodox schools of thought as well as a limited but meaningful congruence with mainstream economic analysis. Critical responses have tended to fall into one of two camps. Radical economists, while appreciative of the central role of unemployment in enforcing labor discipline, often criticize the model for its methodological individualism. Mainstream economists, on the other hand, welcome the commonalities of the model with the literature on incomplete contracts and efficiency wages, but they question whether the model offers any original insights.

I argue that these two central lines of criticism fail to acknowledge the model’s most important contribution: its attempt to integrate elements of heterodox economic thought within the mainstream of the discipline. Bowles and Gintis effectively propose to merge a Marxist theory of the production process and the employment relationship with a formal neoclassical account of rational choice and constraint optimization in the labor market. I suggest this synthesis of Marxist and neoclassical economics succeeds in so far as it provides a shared conceptualization of the inherent conflict in the capitalist employment relationship. The view of the labor market as a domain of contested exchange, as put forward by Bowles and Gintis, allows to think in terms of principal-agent problems at the micro level as well as to take into account class antagonism at the structural level.

While the labor discipline model brings to the fore the commonalities between heterodox and mainstream economists, it also helps to sharpen our understanding of two fundamental points of contention between the different schools of thought. First, the notion of power as defined by Bowles and Gintis based on the model. In their view labor markets are characterized by asymmetric power relations and the employers’ exercise of control over workers. Neoclassical economists, by contrasts, maintain that voluntary market exchange is non-coercive and mutually beneficial. This notion of power has also come under scrutiny from Marxist scholars who argue that, instead of demystifying the coerciveness of competitive market exchange, it falsely represents competition as the ultimate expression of individual freedom.

Second, at a more fundamental level, the reception of the labor discipline model speaks to a serious disagreement about the nature and aims of economic knowledge production. While methodological questions such as structure versus agency, or individualism versus holism are important, at the heart of discussions concerning the labor discipline model we find different conceptions of the purpose of and commitments to knowledge production. I frame this difference as a distinction between economic science and the critique of political economy. Mainstream economists, arguably, continue to be dedicated to the production of scientific knowledge. On the other hand, some heterodox economists -- in particular those drawing from Marx’s engagement with classical political economy -- emphasize the joint production of truth and critique, in what amounts to a direct confrontation of the presumptions of economic positivism.
Elizabeth Z Awomi and Christopher Shafuda. Women, the Voice of the future: Are Women Transforming the Practice and Study of Economics and business in India

The status of women has passed through numerous great changes over the past few years. India has seen a decline of women in the previous status of the ancient and medieval time, to the promotion of equal rights. In current Indian structure, women have held high offices including that of the President, Prime Minister, Chief Executive and Speaker of the parliament house (Lok Sabha). However, women in India continue to face numerous problems such as sexual assault, gender inequality and victim of dowry system. Furthermore, women become an object of being subordinated, face lack of inclusion to explore new areas and are subjugated by social mores. Mostly in the patriarchal set up there prevails the notion of gender biasness where women are mostly categorized as secondary to men. Thus, the objective of this study is to explore the journey of women’s transformation in India and the capability of women in the economic decision making world.

The study further explores whether women have been accorded the right to create power in their own lives, society and community by identifying gender specific outcomes. The study further analyses whether women in power are given the same respect as their male counterparts in the same positions. Also, the study examines whether the women who manage to climb the ladder to the top are helping their counterparts get to the top.

Extensive exploration research on the current and the previous status of women in India is done in order to determine weaknesses, strengths and threats of current policies on women’s empowerment, through observations and personal interviews with the concerned persons. Telephone calls and questionnaires were used as tools for collecting the required data in this study. Necessary and useful data were also collected from civil societies, government and non-governmental organisations as well as from the private sector in India. The study also explores documentations and reports on women’s participation in the economic activities as well as the previous literature on women’s empowerment, gender inequalities and the role of culture in gender.

The study found that with the predominance of ‘caucus’ known as ‘Women in Development’ that emerged in 1960s and 1970s and the later Gender and development (GAD) propagated by the feminist academics in efficiency approach of Women in Development (WID) has broadened women’s legal rights in many countries by not only doing away with the subjugated social mores but also by increasing their political representation in local and national assemblies. Thus, the challenges imposed against women’s subordination and lack of inclusion has enabled women to explore new areas and forms of analysis by acknowledging the differences among women and also in constructing of identities and various interests that has become the torch bearer in the complexity of gender biasness.

However, government at all levels (central, state and the local), the NGOs and the society as a whole must work together to progress women in the field of economics by combating the predicament of women’s empowerment and liberty. The study also revealed that poverty can be reduced through an indispensable tool of empowering women by creating power in individuals over their own lives, society and communities.
João Rodrigues, José Reis and Ana Costa. ‘The ground is dangerous’: debating the Portuguese economy in the turbulent seventies

In 1979, Paul Krugman and Jorge Braga de Macedo argued that: “In the last few years, a unique combination of drastic internal and external shocks has turned Portugal into a testing ground for social and economic theories. But the ground is dangerous; nations are not laboratories (...) if there are lessons to be learned from Portugal’ experience they must be based on a theoretical framework” (Krugman and Macedo, 1979: 454).

After the democratic revolution of the 25th of April, 1974, which ended the longest dictatorship in Western Europe, an opportunity opened up for more fundamental discussions about the future trajectory of the Portuguese economy among economists of radically different theoretical persuasions. This communication intends to frame and interpret those debates, in a period of intense and multiscalar turbulence, as an opposition between those economists of a neoclassical and/or neoliberal inclination and those of a more developmentalist bent, either structuralist or Marxist. Methodological divergences were never absent, while rarely at the forefront of the debates, given the political urgencies of the time. But they can and should be identified.

The first type of economists, most of which had just concluded their PhD’s in the USA, used the concept of the small open economy both as a descriptive and prescriptive term, emphasizing the need for policies that recognized economic interdependence and that opened-up the economy.

The second, more attuned to the economic goals of the socialist-leaning democratic Constitution emphasized the need to gain space for development through more attention to internal unbalances of the productive system and even a more cautious and limited economic integration.

We argue that in practice the space for these debates, in the public sphere and in academia, closed quickly in the early eighties due to a series of factors, both internal and external to the discipline of economics. Attention will be given to the possible overdetermination of national debates by international trends at the time.

Nudging

Ramzi Mabsout, Fadi Makki and Ali Osseiran. The ethics of nudge: An empirical investigation

We empirically investigate the ethics of nudge and their multiple characteristics by constructing a Nudging Ethical Index (NEI). Five ethical dimensions are identified from the literature on nudging (individualist—social, reflective—automatic, avoidable—unavoidable, means—ends, and transparent—non-transparent). Each dimension occupies a side in a pentagon. Nudge interventions can be located on the boundaries of the pentagon as the length of each side captures the gradation between two opposite ideals.
50 experts in behavioral economics and behavioral public policy were asked to score 15 nudge interventions across the five dimension of the NEI. At the same time, we submitted a survey to the general public to assess their acceptance or rejection of the 15 nudge interventions. We test whether the NEI has any power explaining the public’s approval or disapproval of the 15 nudge interventions. (We collected all the data and will soon start statistical analysis.)

Guilhem Lecouteux. From Mayo to nudges: psychology meets economics and policy

Traditional welfare economics assumes that individuals have stable and context-independent preferences, and uses preference satisfaction as a normative criterion. Behavioural economics has called this assumption into question, and thus raises fundamental problems for normative economics: if people’s preferences are likely to change over time, or to depend on apparently irrelevant features of the choice situation, can we still form normative judgments about people’s choices based on their revealed preferences? The main approach developed thus far to tackle this issue, behavioural welfare economics, consists in treating departures from conventional rational choice theory as mistakes, and uses the satisfaction of the ’true’ preferences of the individuals – the preferences they would have revealed, were they able to reason correctly – as a normative criterion. Humans are perceived as defective automata, seeking ‘help’ from the social planner to effectively maximise their welfare.

While the model of this ’inner rational agent trapped in a psychological shell’ is psychologically and philosophically problematic (Infante et al 2016), it has largely convinced behavioural economists, who regularly interpret deviations from rational choice theory as the manifestation of self-control problems. Pathologizing individuals by describing them as ’faulty Econs’ justified in policy discourses the rise of new forms of paternalism, such as Sunstein and Thaler’s libertarian paternalism (Mehta 2014). This rather limited interpretation of psychology is at the core of the emergent field of behavioural public policy, according to which we can use the results of behavioural sciences to design better policies.

Treating behavioural ’anomalies’ as mistakes, that should be corrected by a benevolent social planner, confers to economists the role of social designers, whose goal consists in designing optimal incentives and nudges so as to steer people’s behaviours in the ’right’ direction. Rather than considering individuals as citizens (to whom we should be accountable), we reverse the hierarchy of power by assuming that knowledge and expertise (in economics and psychology in particular) give us the legitimacy to impose our own normative criteria. An alternative approach to this technocratic solution would be to emphasise the democratic legitimacy of the individuals to choose for themselves the kind of lives they want to live. It falls to the agents – and not to behavioural economists – to decide whether the incoherence of their preferences matter or not.

The aim of this paper is to retrace the historical roots of this peculiar approach to normative analysis, and of the use of psychology as a tool of social control. The contrast between a democratic and a technocratic role of behavioural economists echoes the two perspectives on scientific management that emerges in the early 20th century. Unlike within the folk historical reconstruction of Taylorism and Human Relations (see Bruce and Nyland 2011), the Taylor Society actively participated in the defence of an ’industrial democracy’, against Elton Mayo’s defence of the power of the natural and skilled ’elite’ of businessmen. The risks of such a justification of expertise against people’s views are either the progressive legitimisation of authoritarianism (this was rather explicit in Mayo’s writings) or a violent backlash with the generalised reject of expertise (as is occurring in certain forms nowadays). This means that economists should probably reconsider their own practices when prescribing policy advices. Our role as economists
should not be to steer people’s behaviour in what we think is the best direction (from our perspective), but rather to actively participate in people’s education and democratic debates.

Petr Špecián. Thou Shalt not Nudge: Towards an Anti-Psychological State

Neoclassical economics views market failures as an uncompensated impact of one agent’s actions on the other agents’ well-being. The favored solution is the use of economic incentives like taxes and subsidies to correct these situations. Recently, the findings of behavioral economists have provided grounds for an argument that market failures should also comprise the cases where individuals harm themselves due to systematic mistakes they make (Sunstein 2014; Allcott and Sunstein 2015). At the same time, the calls for expanding the set of regulatory tools beyond economic incentives towards subtle manipulation of the choice architecture, i.e., nudging (Thaler, Sunstein, and Balz 2014), are growing increasingly influential.

I argue that both the admission of internalities as market failures that legitimize government intervention and implementation of nudging increase the arbitrary power of the government and make the liberal democratic institutions more fragile. While it is easy to muster intuitive support for the claim that exploitation of systematic mistakes in decision-making is an inherent feature of the free market exchange (Akerlof and Shiller 2015), no one has yet succeeded in establishing a coherent and practically useful notion of ‘true preferences’ against which these mistakes could be defined (Sugden 2018). Thus, the concept of market failure due to self-harm is vague. Therefore, the government interventions to prevent these failures lack a general theoretical framework and, where applied, proceed on an ad hoc basis. Moreover, as far as individuals’ choice is no longer to be taken at face value, the voters’ choices can be contested at least as easily as the consumers’ choices (Brennan 2016). Use of nudges instead of economic incentives to bring people’s choices closer to their nebulous ‘true preferences’ lowers the transparency of the intervention and increases the temptations to misuse it to strengthen the incumbents’ hold on political power (Schubert 2017).

To diminish these problems while not discounting the possibility that markets exploit limitations of human rationality in harmful ways, I propose a use of the government’s regulatory power to preempt the most dangerous manipulative techniques instead of engaging the government in them. Such ‘anti-psychological’ role has significant advantages. Regulation of the forms of commercial (and political) communication can capitalize on the scientific knowledge of human cognitive limitations, and yet avoids the necessity to establish in any specific terms which or whose preferences should be counted as ‘true’. It also takes a form of general rules which are much more transparent than measures needed to micromanage particular situations, e.g., in the form of a fine-tuned choice architecture.
Multiple models

Walter Veit. 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 argue that even though the roles multiple models play have begun to be 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 such 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 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 argue that it is not a specific challenge to a pluralistic account of models. If anything, it 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 of 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 raise the explanatory power of a hypothesis often substantially. Model pluralism is nothing to be afraid of.

N. Emrah Aydinonat. Multiple-model idealisation in economics

It is well known that models serve different functions, and they may be utilised in reaching several epistemic and non-epistemic goals (e.g., see Pielou, 1981; Wimsatt, 1987; Odenbaugh, 2005). Models help us explain, predict, explore model worlds, generate new hypotheses or find robust theorems. Some models serve as benchmarks, others as tools of measurement. It is obvious that for most of these goals one needs to use multiple models. In order to find out whether the results of a model is robust, or in order to explore the implications of a model's assumptions one needs to work with multiple models (Levins, 1966; Weisberg, 2006; Woodward, 2006; Wimsatt, 2007; Kuorikoski, Lehtinen and Marchionni, 2010). Similarly, building multiple models may help us generate new hypotheses, reject a preexisting hypothesis or change our confidence in it (Grüne-Yanoff, 2009, e.g., 2013). In order to fine-tune predictions one could use several models; if, for example, averaging the predictions of multiple models gives better predictions. Likewise, commonly multiple models can be used as benchmarks; as in the case of the model of a perfectly competitive market and the model of pure monopoly representing two extremes in economics. Even measurement may require multiple models. Different models could help us in measuring different aspects of the phenomenon of interest and describe it in a more comprehensive manner. Last but not the least, one could get help from multiple models in designing an experiment, engineering a product, or designing a market, etc. Finally, multiple models can be used to explain phenomena (Aydinonat, 2018). Although economists, like other scientists, develop a diversity of models and employ multiple-models for various tasks, philosophy of economics do not pay much attention to this. If we would like to get a better grasp of why economists develop multiple models and operate with a diverse set of models, we should start paying more attention to the uses (and misuses) of multiple models in economics.

In addition to highlighting the importance of multiple models, I also explore the various ways in which the use of multiple highly idealised models could be justified in economics. To do this, I use a Michael Weisberg’s account of multiple-model idealisation (MMI) as a starting point (Weisberg, 2007, 2013). Echoing Levins (1966) and Wimsatt (1987), Weisberg argues that multiplicity of models is a necessary counterpart of the complexity of the world, our cognitive limitations and the trade-offs between different modelling goals, such as generality, precision and simplicity. He argues that MMI can be justified in four ways: (i) minimal-model justification, (ii) trade-offs justification, (iii) robustness justification, and (iv) generality justification. A discussion of the limitations of Weisberg’s suggested justifications shows that to get a better grasp of why economists use multiple idealised models, one needs to look at these models within their appropriate historical context, which commonly includes a cluster of relevant models, explanations and arguments in economics. Although, philosophers of economics have long tried to make sense of the use of unrealistic economic models, they mostly focused on the relation between a model and
its intended target. I argue that zooming out from this one-model-one-target view, and taking the historical context into account will helps see the marginal contribution, or the value added of individual models.

Mickey Peled. Multiple models and Monetary Policy: The role of abduction

When contemplating the validity and manner of inference from a model to the world, the philosophy of economics literature often points to induction as the primary form of reasoning. In addition, the justification for the specific inference is based on certain intuitions, professional skills, experience and other characteristics that can be summed up in the term ‘craft’.

According to this common line of thinking, when asked whether a specific model fits the phenomena in question, or when asked to assess if a theoretical model can disclose an insight on empirical phenomena, an economist would apply his craftsmanship in order to give an answer. In his seminal work, Dani Rodrik expands this shared view and argues induction and ‘craft’ to be the primary tools for choosing the right model from multiple possible ‘shelf’ models.

While the combination of induction and craft might be the case in certain areas of economic practice, the paper will argue that it is not the case when macroeconomic decision-making is concerned. Following recent works in the philosophy of science and cognitive studies, the paper will argue that in the context of macroeconomic policy, a different mode of inference takes place. In these circumstances, a step-by-step observation of economists’ practice will reveal that the initial structure of inference from the model to the world and vice-versa is abductive.

To be precise, the kind of abduction to which the paper refers is not the kind coined by Gilbert Harman as ‘inference to the best explanation’ but rather the kind of abduction close to the original meaning of Charles Peirce as an inference of a plausible cause from the conjunction of the appropriate rule and the observation. More specifically, in the case of macroeconomic policy, this mode of abduction is used for the selection of a hypothesis and not for generating a new one. Accordingly, the purpose of craftsmanship shifts from the justification of the inductive inference to being a tool for justifying the ‘plausibility’ of the model (or combination of models), given the insights it can disclose on the observed empirical data, such as explaining, describing a mechanism, direction, causes, and more.

The argument for abductive reasoning will be demonstrated through the practice of the Monetary Policy Committee (MPC) of the Bank of England (BoE) - as a proxy for present monetary decision-making by central banks. The case study would focus on the first step that the MPC takes in order to make a policy decision, meaning the periodic construction of the economic outlook for the UK, from which a policy decision is derived. Using MPC meetings’ minutes, media interviews, public lectures as well as conversations with relevant economists, the paper would show how BoE economists use abduction as the tool to overcome the inherent complexity of the macro-level economic phenomenon. Members of the MPC use abductive reasoning first to sort out the wheat from the chaff and second to add ad-hoc supplements to the economic outlook, based on selecting a plausible model - from multiple possible models - to explain and predict economic data.

Emphasizing the role of abduction in monetary policy is in itself an interesting addition to the literature on economic methodology. Furthermore, it can assist in facilitating a broader stance on the practice of economists-as-regulators by demonstrating how models influence, sometimes even determine, how they see the world. In this sense, abduction describes the structure of the process that constitutes models as epistemic mediators in practical reasoning, which is a process of reasoning most common in the case of macroeconomic policy.
Sociology of economics II

Jack Wright. Hierarchy in research communities: the case of economics

What effect do hierarchical structures within research communities have on the output of those communities? In this paper I address this question by examining the way that research is organised and rewarded in economics.

In the last two years, the outsize role five journals—dubbed the ‘top 5’—play in determining success within economics has become a hot topic of discussion. In this paper I argue that the reification of five journals is just one of a number of features of how economists interact that reveals a steep hierarchy within the discipline. Although most academic disciplines are hierarchical in certain ways, the degree of asymmetry between the power, status, and influence of those at different rungs of economics’ hierarchy—the ‘steepness’ of the hierarchy in economics—is greater than in other disciplines. In this paper I collect empirical evidence from a variety of studies of economics to describe the ways that the discipline is hierarchical and then use ideas from social epistemology to discuss the epistemic and political implications of economics’ hierarchy.

In organisational terms, a hierarchy is an ordering of individuals along one or more socially important dimensions. I describe six features of the way that research is governed, evaluated, and rewarded in economics that illustrate the orderings that make up economics’ hierarchy. Economics places particular significance on journal rankings (and the ‘top 5’ journals in particular); networks in the discipline are dominated by ‘stars’; those that sit on powerful governing committees are drawn from a narrow subsection of economists; those at more highly ranked institutions are more likely to be editors of, and authors in, high-prestige journals; and, the rankings of institutions in which economists reside, get their PhDs, and get their first jobs play significant roles in career success. Taken together these features of how economists interact indicate that the research community in economics is steeply and consequently hierarchical.

There are two ways to respond to this. The hierarchy in economics, or certain aspects of it, might be justified, either as an unavoidable feature of something else desirable or by bringing positive benefits in its own right. Or, the hierarchy in economics might be taken to be a problem to remedy. In this paper I will argue against the first response and for the second. I will argue that there is little evidence that steeper hierarchical forms either motivate economists to be more productive or help to make the decisions and findings in economics more efficient and accurate. I will suggest three questions for evaluating hierarchies. Is the hierarchy facilitating or constraining productivity? Is the hierarchy able to react to the needs of those it should serve? Are those high up the hierarchy accountable? In answer to these questions, I will argue that the ways in which economics is hierarchical block avenues for epistemically productive feedback, make economics less responsive to certain interests, and make public scrutiny of the discipline more difficult.
The gap between economic beliefs and lay beliefs is often believed to be large (Garnett 1999; Caplan 2001; Dixon et al. 2014; Wobker et al. 2014), frequently leading to researchers claiming that consumers and voters are ignorant (Caplan 2011). Similarly, economists are often in agreement that economics has little influence on policy (Stigler 1982; McCloskey 1990; Klamer and Meehan 1999; Frey 2006). On the other hand, there is a widespread belief that economic ideas are more influential than ever in our day and age, both the Washington consensus and neoliberalism are believed to be the direct outcomes of the influence of economic ideas (Mirowski and Plehwe 2009; Burgin 2012). Prima facie it seems hard to square these two contrasting views. That is not for lack of trying, economists have argued that their ideas are not more influential, because of rent-seeking problems (for an overview see Rodrik 2014). People know better, but since these ideas clash with their interests they adopt a different belief. Another explanation might be that experts are in charge of policy-making so that we should examine the economic knowledge of experts.

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

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

From this analysis an alternative understanding of the gap between the knowledge of the economists and laymen is developed, which emphasizes the way in which economic concepts such as the market and values such as competition have been integrated in lay understandings. This suggests an alternative explanation of the empirical results on supposed economic ignorance. These are indeed true on the level of basic policy questions and basic knowledge of economic theory, but they miss the way in which economics has been influential in altering the lay perspective on the economy and society.
domains. As a specific example, I examine the case of economics. Unlike, say, physics that postulates electrons and other unfamiliar “unobservables”, economics does not make a radical ontological departure from the realm of “commonsensibles” such as preferences and expectations, households and business firms, wages and profits, contracts and prices, and so on. There is a (referential) sense in which economics is continuous with common sense.

Rather, “scientific economics” departs from “folk economics” in two other important (representational) ways. First, the commonsensibles of folk economics are modified by means of cognitive procedures such as selection, isolation, abstraction, idealization, exaggeration, projection, averaging, aggregation. Perfectly informed and rational homo economicus with complete and transitive preferences operating in perfectly competitive markets with zero transaction costs – this is a paradigmatic example of modified commonsensibles that makes the first-year economics student puzzled. This suggestion (Mäki 1993, 1996, 1998, 2012) has been both endorsed and challenged (Guala 2012, Hands 2012, Ross 2012); the challenges will be met in the present paper.

Second, scientific economics suggest a causal rearrangement of the relations between commonsensibles in folk economics. Folk economics conceives of the causal structure of the economic realm in terms of small-scale “sphere of intendedness” projected onto large-scale causation. Households and other small group systems are treated as model economies, and hence oikonomia as economics. The rule is that whatever happens, it is intended. A familiar slogan captures the outlook: Country = Company. By contrast, for scientific economics, Country ≠ Company. Large-scale causation is not to be modelled after small-scale, it is rather governed by more complex mechanisms beyond the limited sphere of (individual or collective) intendedness. Folk economics envisages the world as governed by visible-hand and hidden-hand mechanisms, while scientific economics explains economic phenomena and institutions by invoking various invisible-hand mechanisms, some of them backhand (cf Mäki 1996, 2013; Rubin 2003). This is the idea that economics often produces “counter-intuitive” results. I will briefly compare this to a similar situation in evolutionary biology; and to Marx’s criticism of what he called “vulgar economics”.

The boundary between folk economics and scientific economics is neither sharp nor fixed, but keeps moving, and their relations may be interactive (unsurprisingly, given that both deal with commonsensibles). Scientific economics keeps influencing folk economics, and scientific economics may be revised under the pressure from folk economics. On the one hand, folk economics is influenced by scientific economics by way of economics education, the overall economization / marketization / monetization / commodification of society, and the associated economic discourse penetrating society broadly. On the other hand, there is an ongoing pressure to revise theoretical notions such as that of rationality so as to bring it closer to “our intuitions” about rationality; and more generally, a typical enlargement of economic theory takes place by way of inclusion (and modification) in theory of everyday notions, such as trust and fairness. All of this is complicated further by the presence of rival theoretical accounts of the structure and dynamics of the economic realm.

The clash has scientific significance (how exactly to resolve it when doing scientific economics, for example to avoid inconsistency); educational significance (how to deal with students’ and the general public’s cognitive difficulties in adopting certain counter-intuitive conceptions); political significance (e.g. consequences for democracy under populist pressures); and philosophical significance (for philosophy of science, social epistemology, political philosophy, moral philosophy).
Adaptation and evolution in economics

John Davis. The Identity and Nature of Adaptive Individuals

This paper develops a framework for explaining the identity of individuals seen to be adaptive beings, as by institutionalist economists in the Veblenian tradition and behavioral economists who favor a Simon-Gigerenzer ecological economics. The main problem this conception encounters is: if such individuals continually adapt to the world, it is unclear why they aren’t then reducible to it and therefore are not distinct individuals at all. To address this issue – what I previously termed the individuation problem in the identity analysis of individuals (Davis, 2011) – first, I use a standard temporal distinction in economics between point-in-time and through-time measures to argue that these are two linked dimensions allow us to explain adaptive individuals in stock-flow terms. I discuss the advantages and problems with this representation, in the case of the latter, specifically, what sort of ‘stocks’ people might be and what the possible dissolution of a person as a stock might mean.

Second, the paper discusses how individuals understood in this way might adapt to the world and still function as distinct individuals by describing their behavior in terms of series of connected feedback and feedforward adjustment processes. Based on this, I characterize the identity of adaptive individuals as reflexive. Third, the paper turns to the issue of what type of reasoning such individuals might employ. The world they adapt to is characterized by uncertainty and ambiguity – Savage’s ‘large’ world (Savage, 1954). Following Binmore (2009), Bayesian reasoning is only available in his ‘small’ worlds, which ignore events such as ‘black swans,’ ‘unknown unknowns,’ etc. The paper then argues that adaptive reflexive individuals employ counterfactual reasoning in a decision-making and rely on inferences formulated as subjunctive rather than indicative conditionals. Finally, counterfactual reasoning is briefly linked to possible worlds theory, which allows us to explain how individuals have and manage multiple selves identities.

Tomi Kokkonen. What are biological markets and are markets biological?

The idea of markets has been applied to many phenomena beyond economy – including biological interactions. There are formal similarities between analysis of economic behaviour and the logic of evolution of behaviour, as evidenced by the interchange of models and mathematical tools between economics and biology through game theory. The basis for the applicability of the similar game-theoretical models is, however, different in substance. The “interactions” in behavioural evolutionary biology are on the level of fitness consequences, not on the level of observed behaviour. The biological markets approach, in contrast, refers to the patterns of proximate interactions. In the biological markets approach, organisms “trade” goods and these trading contexts are the selective environment for behavioural evolution. For the most part, this “trading” still involves evolved behavioural tendencies only – but some cases involve trading-related cognition and choice-making by the organisms themselves (e.g. primates). Furthermore, the
biological markets model has been applied to human cooperation and partner choice, too. This all raises a question: what are biological markets? Is there just an analogy between markets proper and biological markets, or is there a more substantial connection?

I will consider three possible ways to understand this connection: the weak analogy, the strong analogy, and the homology interpretation. In the weak analogy interpretation, the applicability of markets approach to some biological systems is based on superficial structural similarities, just like in the standard interpretation of the applicability of game theory in both economics and biology. This should probably be the default position, given the vast differences between economic markets and some of the target systems of the biological markets model, such as symbiosis.

But there are also reasons to think a stronger interpretation may be correct. In first of them, the strong analogy interpretation, market mechanism is a form of mechanism that is instantiated by any system that satisfies certain structural conditions. The structural similarities are not superficial, then, but deep. The mechanism itself is more general and it was simply first discovered in the economic context. This would make markets similar to natural selection – it, too, is a more general mechanism. It was discovered in the context of biological evolution, but the natural selection model can be applied to any process of change that has certain structural properties.

The homology interpretation, in turn, takes the economic markets to be a kind of biological market or a descendant of a biological market – in other words, the similarity is based on (partial) sameness. In this interpretation, biological markets are a general biological phenomenon and certain forms of human social interaction, including barter, are instances of it and can therefore be understood as biological phenomena. More sophisticated forms of trading and modern market economy would be either a sophistication of biological trading or a descendant system. I will suggest that both strong analogy and homology interpretation have some merit, and they are compatible.

Whatever the correct interpretation turns out to be, it has both methodological and wider philosophical consequences. The methodological consequences have to do with applicability. The basis for the applicability of markets ideas to biology in the first place (weak analogy based on superficial structural similarities, strong analogy based on deep structural similarities, and homology based on partial sameness in a strong sense) determines the core elements of the general model and the constraints of applicability. This informs on what kind of discoveries on one field should be expected to find a sibling target on the other field. The second consequence has to do with the nature of interdisciplinarity. In the weak analogy, approaching biological systems as if markets, is an example of scientific imperialism in a theoretical dimension, with potential detrimental epistemic consequences. In the strong analogy, biological markets are an example of theoretical unification. But in the homology interpretation, the tables turns around and markets become reducible to biology, with a possibility of a new biological extension to behavioural economics.

Jason Entsminger and Randall Westgren. The Organizational Species: An Application of a New Approach to the Taxonomic Classification of Firms

Classification is a fundamental cognitive activity; the ability to classify permits inference, explanation, generalization, and prediction about things in the world, whether physical objects or social ones. (Boyd 1999; Ereshefsky and Reydon 2015). At the limit, there is no other way to comprehend and study an infinitely complex physical world overlaid with an infinitely complex social world. Given this, systematic modes of defining categories and delimiting their boundaries have developed as a core scientific endeavor across many fields, seeking to identify methods and means that form groupings which consistently serve the purposes of inquiry.
Unlike natural sciences such as biology, in the social sciences, including economics, attention to the classification of objects of interest – markets, institutions, organizations, and agents – in such a systematized manner has lagged. Early works have focused on allegories to models of classification in the biological sciences, where groupings are formed in hierarchical orderings such that membership has predictive capabilities at some given level as well as explains origins or relations among kinds. (Bailey, 1994; Carper & Snizek, 1980; McKelvey, 1982; Rich, 1992) The focus of these works is on organizations, primarily firms qua economic agents, and forms the basis for what has come to be called Organizational Systematics. At the core is allusion to the biological species as the central unit of analysis for categorization and prediction.

In this paper we reignite attention to classification of firms. We integrate the concept of natural kinds taken from philosophers of science such as Boyd and Epstein with allegory to the ecological niche – an allusion already well-developed within the social sciences literature (Cattani, Porac, & Thomas, 2017; Popielarz & Neal, 2007; Astley, 1985; Astley & Van de Ven, 1983). We conceive an organizational species as being a natural kind of social organism, based on homeostatic property clusters, that tends to inhabit a particular niche of the social world. This operationalizes the species concept in a manner which accommodates the interests of an array of social sciences concerned with organizational forms and with making inferences about the structures, behaviors, resource allocation decisions, and other characteristics of firms.

We operationalize this concept through the application of cluster analysis to discover natural groupings of organizations. Using a multivariate cluster analysis approach, one is able to analyze cases under a polythetic lens to identify if natural structures of homeostatic property clusters are present within a population. If such groupings are present, and a given group tends toward filling an identified niche, then the boundaries which delimit the group form the basis for an organizational species.

This procedure is applied to investigate the presence of organizational species within the domain of Local and Regional Food Systems (LRFS) actors of the United States, specifically coordinating intermediaries known as “food hubs”. These actors represent an emergent organizational form that has been identified by key policy stakeholders as a means of “scaling-up” LRFS to achieve public policy goals related to economic, social, and environmental objectives. Implied by our organizational species concept is that new organizational forms arise at the intersection of evolutionary processes and ecological structures. Food hubs and their corresponding niche(s) exist within a dynamic agri-food sector. This new organizational form has developed with the advent of (social) environmental change within the sector. Social movements – particularly the local foods movement, but also allied movements of alterity such as farm-to-table restaurants, Slow Food, organic production, civic agriculture, and sustainability – have provided "strategic space" for the instantiation of food hubs. This phenomenon was identified by Weber, Heinze, and DeSoucey (2008) for the subsector that linked consumers to grass-fed livestock.

We use a US national database of food hubs for empirical analysis. These data allow us to investigate a pivotal question for organizational speciation: at what level of analysis does the species exist? Are food hubs themselves a species, or are they some higher order of a taxonomy?