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Deductivism – the fundamental flaw of mainstream economics

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> "Confusion of sign and object is original sin coeval with the word"
> W. v. O. Quine

Introduction

In science one could argue that there basically are three kinds of argumentation patterns / schemes / methods / strategies available – deduction, induction and abduction.

In this paper it will be argued that the failings of the mainstream modelling strategy are related to how mainstream economics (mis)uses the first two of these three modes of inference and – with severe negative analytical consequences – to a large degree disregard the third one.

Fixation on constructing models showing the certainty of logical entailment – *realiter* simply collapsing the necessary ontological gap between model and reality – has been detrimental to the development of a relevant and realist economics. Insisting on formalistic (mathematical) modelling forces the economist to give upon on realism and substitute axiomatics for real world relevance. The price for rigour and precision is far too high for anyone who is ultimately interested in using economics to pose and (hopefully) answer real world questions and problems.

The deductivist orientation is the main reason behind the difficulty that mainstream economics has in terms of understanding, explaining and predicting what takes place in our societies. But it has also given mainstream economics much of its discursive power – at least as long as no one starts asking tough questions on the veracity of – and justification for – the assumptions on which the deductivist foundation is erected. Asking these questions is an important ingredient in a sustained critical effort at showing how nonsensical is the embellishing of a smorgasbord of models founded on wanting (often hidden) methodological foundations.

The mathematical-deductivist straitjacket used in mainstream economics presupposes atomistic closed-systems – i.e., something that we find very little of in the real world, a world significantly at odds with an (implicitly) assumed logic world where deductive entailment rules the roost. Ultimately then, the failings of modern mainstream economics has its root in a deficient ontology. The kind of formal-analytical and axiomatic-deductive mathematical modelling that makes up the core of mainstream economics is hard to make compatible with a real-world ontology. It is also the reason why so many critics find mainstream economic analysis patently and utterly unrealistic and irrelevant.

Although there has been a clearly discernible increase and focus on "empirical" economics in recent decades, the results in these research fields have not fundamentally challenged the main deductivist direction of mainstream economics. They are still mainly framed and interpreted within the core "axiomatic" assumptions of individualism, instrumentalism and equilibrium (cf. Arnsperger and Varoufakis (2006)) that make up even the "new" mainstream economics. Although, perhaps, a sign of an increasing – but highly path-dependent – theoretical pluralism, mainstream economics is still, from a methodological point of view, mainly a deductive project erected on a foundation of empty formalism.

If we want theories and models to confront reality there are obvious limits to what can be said "rigorously" in economics. For although it is generally a good aspiration to search for scientific claims that are both rigorous and precise, we have to accept that the chosen level of precision and rigour must be relative to the subject matter studied. An economics that is relevant to the world in which we live can never achieve the same degree of rigour and precision as in logic, mathematics or the natural sciences. Collapsing the gap between model and reality in that way will never give anything else than empty formalist economics.

In mainstream economics, with its addiction to the deductivist approach of formal mathematical modeling, model consistency trumps coherence with the real world. That is sure getting the priorities wrong. Creating models for their own sake is not an acceptable scientific aspiration – impressive-looking formal-deductive (mathematical) models should never be mistaken for truth.

- **Deduction**

Premise 1: All Chicago economists believe in REH
Premise 2: Robert Lucas is a Chicago economist
——————————————————————
Conclusion: Robert Lucas believes in REH

Here we have an example of *a logically* valid deductive inference (and, following Quine, whenever logic is used in this essay, "logic" refers to deductive/analytical logic).

In a hypothetico-deductive reasoning – hypothetico-deductive *confirmation* in this case – we would use the conclusion to test the law-like hypothesis in premise 1 (according to the hypothetico-deductive model, a hypothesis is confirmed by evidence if the evidence is deducible from the hypothesis). If Robert Lucas does not believe in REH we have gained some warranted reason for non-acceptance of the hypothesis (an obvious shortcoming here being that further information beyond that given in the explicit premises might have given another conclusion).

The hypothetico-deductive method (in case we treat the hypothesis as absolutely sure/true, we rather talk of an *axiomatic-deductive* method) basically means that we

- Posit a hypothesis

- Infer empirically testable propositions (consequences) from it
- Test the propositions through observation or experiment
- Depending on the testing results either find the hypothesis corroborated or falsified.

However, in science we regularly use a kind of "practical" argumentation where there is little room for applying the restricted logical "formal transformations" view of validity and inference. Most people would probably accept the following argument as a "valid" reasoning even though it from a strictly logical point of view is non-valid:

> Premise 1: Robert Lucas is a Chicago economist
> Premise 2: The recorded proportion of Keynesian Chicago economists is zero
> ————————————————————————————————
> Conclusion: So, certainly, Robert Lucas is not a Keynesian economist

How come? Well I guess one reason is that in science, contrary to what you find in most logic text-books, not very many argumentations are settled by showing that "All Xs are Ys". In scientific practice we instead present other-than-analytical explicit warrants and backings – data, experience, evidence, theories, models – for our inferences. As long as we can show that our "deductions" or "inferences" are justifiable and have well-backed warrants, other scientists will listen to us. That our scientific "deductions" or "inferences" are logical non-entailments simply is not a problem. To think otherwise is committing the fallacy of misapplying formal-analytical logic categories to areas where they are pretty much irrelevant or simply beside the point.

Scientific arguments are not analytical arguments, where validity is solely a question of formal properties. Scientific arguments are *substantial* arguments. If Robert Lucas is a Keynesian or not, is nothing we can decide on formal properties of statements/propositions. We have to check out what the guy has actually been writing and saying to check if the hypothesis that he is a Keynesian is true or not.

In a *deductive-nomological* explanation – also known as a *covering law explanation* – we would try to explain why Robert Lucas believes in REH with the help of the two premises (in this case actually giving an explanation with very little explanatory value). These kinds of explanations – both in their *deterministic* and *statistic/probabilistic* versions – rely heavily on deductive entailment from assumed to be true premises. But they have preciously little to say on where these assumed to be true premises come from.

Deductive logic of confirmation and explanation may work well – given that they are used in deterministic closed models! In mathematics, the deductive-axiomatic method has worked just fine. But science is not mathematics. Conflating those two domains of knowledge has been one of the most fundamental mistakes made in the science of economics. Applying the deductive-axiomatic method to real world systems, however, immediately proves it to be excessively narrow and hopelessly irrelevant. Both the confirmatory and explanatory ilk of hypothetico-deductive reasoning fails since there is no way you can relevantly analyze confirmation or explanation as a purely logical relation between hypothesis and evidence or between law-like rules and explananda. In science we argue and try to substantiate our beliefs and hypotheses with reliable evidence – propositional and predicate deductive logic, on the other hand, is not about *reliability*, but the *validity* of the conclusions *given* that the premises are true.

Deduction – and the inferences that go with it – is an example of “explicative reasoning”, where the conclusions we make are already included in the premises. Deductive inferences are purely *analytical* and it is this truth-preserving nature of deduction that makes it different from all other kinds of reasoning. But it is also its limitation, since truth in the deductive context does not refer to a real world ontology (only relating propositions as true or false within a formal-logic system) and as an argument scheme, deduction is totally non-ampliative – the output of the analysis is nothing else than the input.

Just to give an economics example, consider the following rather typical, but also uninformative and tautological, deductive inference:

> Premise 1: The firm seeks to maximize its profits
> Premise 2: The firm maximizes its profits when marginal cost equals marginal income
> ————————————————————
> Conclusion: The firm will operate its business at the equilibrium where marginal cost equals marginal income

This is as empty as deductive-nomological explanations of singular facts building on simple generalizations:

> Premise 1: All humans are less than 20 feet tall
> Premise 2: Robert Lucas is a human
> ————————————————————
> Conclusion: Robert Lucas is less than 20 feet tall

Although a logically valid inference, this is not much of an explanation (since we would still probably want to know why *all* humans are less than 20 feet tall).

Deductive-nomological explanations also often suffer from a kind of emptiness that emanates from a lack of real (causal) connection between premises and conclusions:

> Premise 1: All humans that take birth control pills do not get pregnant
> Premise 2: Lars Syll took birth control pills
> ————————————————————
> Conclusion: Lars Syll did not get pregnant

I guess most people would agree that this is not much of a real explanation.

Learning new things about reality demands something else than a reasoning where the knowledge is already embedded in the premises. These other kinds of reasoning may give *good* – but not *conclusive* – reasons. That is the price we have to pay if we want to have something substantial and interesting to say about the real world.

- **Induction**

> Premise 1: This is a randomly selected large set of economists from Chicago
> Premise 2: These randomly selected economists all believe in REH

———————————————————————

Conclusion: All Chicago economists believes in REH

In this inductive inference we have an example of a logically non-valid inference that we would have to supply with strong empirical evidence to really warrant. And that is no simple matter at all, as Keynes (1973 (1921): 468f) noticed:

> "In my judgment, the practical usefulness of those modes of inference, here termed Universal and Statistical Induction, on the validity of which the boasted knowledge of modern science depends, can only exist—and I do not now pause to inquire again whether such an argument must be circular—if the universe of phenomena does in fact present those peculiar characteristics of atomism and limited variety which appear more and more clearly as the ultimate result to which material science is tending...
>
> The physicists of the nineteenth century have reduced matter to the collisions and arrangements of particles, between which the ultimate qualitative differences are very few...
>
> The validity of some current modes of inference may depend on the assumption that it is to material of this kind that we are applying them... Professors of probability have been often and justly derided for arguing as if nature were an urn containing black and white balls in fixed proportions. Quetelet once declared in so many words— 'l'urne que nous interrogeons, c'est la nature'. But again in the history of science the methods of astrology may prove useful to the astronomer; and it may turn out to be true— reversing Quetelet's expression—that 'La nature que nous interrogeons, c'est une urne'."

But even though induction is more demanding in terms of justification than deduction, we should not draw the conclusion that it is no inference at all:

> "Now it might be charged that moving from such facts as that F's have always been followed by C's, to the claim that F's obtaining is a good reason for expecting C, – that this is not an inference at all; not when one's only defence consists in citing more facts, namely the specific meteorological, botanical, and biological data which support the general claim that F has regularly preceded C. Entailment it may not be, granted. But inference it certainly is, as must be every case of drawing reasonable conclusions from evidence." N. R. Hanson (1971:242)

Justified inductions presupposes a *resemblance* of sort between what we have experienced and know, and what we have not yet experienced and do not yet know. Just to exemplify this problem of induction let me take two examples.

Let's start with this one. Assume you're a Bayesian turkey and hold a nonzero probability belief in the hypothesis H that "people are nice vegetarians that do not eat turkeys and that every day I see the sun rise confirms my belief." For every day you survive, you update your belief according to Bayes' Rule

$$P(H|e) = [P(e|H)P(H)]/P(e)],$$

where evidence e stands for “not being eaten” and $P(e|H) = 1$. Given that there do exist other hypotheses than H, $P(e)$ is less than 1 and a fortiori $P(H|e)$ is greater than $P(H)$. Every day you survive increases your probability belief that you will not be eaten. This is totally rational according to the Bayesian definition of rationality. Unfortunately – as Bertrand Russell famously noticed – for every day that goes by, the traditional Christmas dinner also gets closer and closer…

Or take the case of macroeconomic forecasting, which perhaps better than anything else illustrates the problem of induction in economics. As a rule macroeconomic forecasts tend to be little better than intelligent guesswork. Or in other words – macroeconomic mathematical-statistical forecasting models, and the inductive logic upon which they ultimately build, are as a rule far from successful. The empirical and theoretical evidence is clear. Predictions and forecasts are inherently difficult to make in a socio-economic domain where genuine uncertainty and unknown unknowns often rule the roost. The real processes underlying the time series that economists use to make their predictions and forecasts do not confirm with the inductive assumptions made in the applied statistical and econometric models. The forecasting models fail to a large extent because the kind of uncertainty that faces humans and societies actually makes the models strictly seen inapplicable. The future is inherently unknowable – and using statistics and econometrics does not in the least overcome this ontological fact. The economic future is not something that we normally can predict in advance. Better then to accept that as a rule “we simply do not know”.

Induction is sometimes a good guide for evaluating hypotheses. But for the creative generation of plausible and relevant hypotheses it is conspicuously silent. For that we need, as noted already by Peirce (1931:§145), another – non-algorithmic and ampliative – kind of reasoning.

- **Abduction**

Premise 1: All Chicago economists believe in REH
Premise 2: These economists believe in REH
———————————————————-
Conclusion: These economists are from Chicago

In this case, again, we have an example of a logically non-valid inference – *the fallacy of affirming the consequent*:

p => q
q
———
p

or, in instantiated form

∀x (Gx => Px)
Pa
———
Ga

But it is nonetheless an inference that may be a strongly warranted and *truth-producing* – in contradistinction to *truth-preserving* deductions – reasoning, following the general pattern

> Evidence => Explanation => Inference.

Here we infer something based on what would be the best explanation given the law-like rule (premise 1) and an observation (premise 2). The truth of the conclusion (explanation) is nothing that is *logically* given, but something we have to justify, argue for, and test in different ways to possibly establish with any certainty or degree. And as always when we deal with explanations, what is considered best is relative to what we know of the world. In the real world all evidence has an irreducible holistic aspect. We never conclude that evidence follows from hypothesis *simpliciter*, but always given some more or less explicitly stated contextual background assumptions. All non-deductive inferences and explanations are *a fortiori* context dependent.

If extending the abductive scheme to incorporate the demand that the explanation has to be the *best* among a set of *plausible* competing/rival/contrasting potential and satisfactory explanations, we have what is nowadays usually referred to as *inference to the best explanation* (IBE). In this way IBE is a refinement of the original (Peircean) concept of abduction by making the background knowledge requirement more explicit.

In abduction we start with a body of (purported) data/facts/evidence and search for explanations that can account for these data/facts/evidence. Having the best explanation means that you, given the context-dependent background assumptions, have a satisfactory explanation that can explain the fact/evidence better than any other competing explanation – and so it is *reasonable* to consider/believe the hypothesis to be true. Even if we do not (inevitably) have deductive certainty, our abductive reasoning gives us a license to consider our belief in the hypothesis as reasonable. The model of inference to the best explanation is, as Peter Lipton (2000:184) writes,

> "…designed to give a partial account of many inductive inferences, both in science and in ordinary life... Its governing idea is that explanatory considerations are a guide to inference, that scientists infer from the available evidence to the hypothesis which would, if correct, best explain that evidence. Many inferences are naturally described in this way... When a detective infers that it was Moriarty who committed the crime, he does so because this hypothesis would best explain the fingerprints, blood stains and other forensic evidence. Sherlock Holmes to the contrary, this is not a matter of deduction. The evidence will not entail that Moriarty is to blame, since it always remains possible that someone else was the perpetrator. Nevertheless, Holmes is right to make his inference, since Moriarty's guilt would provide a better explanation of the evidence than would anyone else's.
>
> Inference to the Best Explanation can be seen as an extension of the idea of 'self-evidencing' explanations, where the phenomenon that is explained in turn provides an essential part of the reason for believing the explanation is correct... According to Inference to the Best Explanation, this is a common situation in science: hypotheses are supported by the very observations

> they are supposed to explain. Moreover, on this model, the observations support the hypothesis precisely because it would explain them."

Accepting a hypothesis means that you consider it to explain the available evidence better than any other competing hypothesis. The acceptability warrant comes from the explanatory power of the hypothesis, and the conscious act of trying to rule out the possible competing potential explanations in itself increases the plausibility of the preferred explanation. Knowing that we – after having earnestly considered and analysed the other available potential explanations – have been able to *eliminate* the competing potential explanations, warrants and enhances the confidence we have that our preferred explanation is the best – "loveliest" – explanation, i.e., the explanation that provides us with the greatest understanding (given it is correct). As Sherlock Holmes had it (in *The Sign of Four*): "Eliminate the impossible, and whatever remains, however improbable, must be the truth". Subsequent confirmation of our hypothesis – by observations, experiments or other future evidence – makes it even more well-confirmed (and underlines that all explanations are incomplete, and that the models and theories that we as scientists use, cannot only be assessed by the extent of their fit with experimental or observational data, but also need to take into account their explanatory power).

This, of course, does not in any way mean that we cannot be wrong. Of course we can. But as Alan Musgrave (2010:94) writes:

> "Quite so – and so what? It goes without saying that any explanation might be false, in the sense that it is not necessarily true. It is absurd to suppose that the only things we can reasonably believe are necessary truths.
>
> What if the best explanation not only might be false, but actually is false. Can it ever be reasonable to believe a falsehood? Of course it can... What we find out is that what we believed was wrong, not that it was wrong or unreasonable for us to have believed it.
>
> People object that being the best available explanation of a fact does not prove something to be true or even probable. Quite so – and again, so what? The explanationist principle – 'It is reasonable to believe that the best available explanation of any fact is true' – means that it is reasonable to believe or think true things that have not been shown to be true or probable, more likely true than not."

Abductions are *fallible* inferences – since the premises do not logically entail the conclusion – so from a *logical* point of view, abduction is a weak mode of inference. But if the abductive arguments put forward are strong enough, they can be warranted and give us justified true belief, and hence, knowledge, even though they are fallible inferences. As scientists we sometimes – much like Sherlock Holmes and other detectives that use abductive reasoning – experience disillusion. We thought that we had reached a strong abductive conclusion by ruling out the alternatives in the set of contrasting explanations. But – what we thought was true turned out to be false. But that does not necessarily mean that we had no good reasons for believing what we believed. If we cannot live with that contingency and uncertainty, well, then we're in the wrong business. If it is deductive certainty you are after, rather than the *ampliative* and *defeasible* reasoning in abduction – well, then get in to math or logic, not science.

What makes the works of people like Galileo, Marx, or Keynes, truly interesting is not that they describe new empirical facts. No, the truly seminal and pioneering aspects of their works is that they managed to find out and analyse what makes empirical phenomena possible. What are the fundamental physical forces that make heavy objects fall the way they do? Why do people get unemployed? Why are market societies haunted by economic crises? Starting from well known facts these scientists discovered the mechanisms and structures that made these empirical facts possible.

> "Newton pressed on; Einstein, DeBroglie, Schrödinger, Heisenberg and Dirac pressed on – for explanations, which no amount of statistical repetition or deductive ingenuity could ever supply … From the observed properties of phenomena the physicist reasons his way towards a keystone idea from which the properties are explicable as a matter of course. The physicist seeks not a set of possible objects, but a set of possible explanations" N. R. Hanson (1965:88).

The works of these scientists are good illustrations of the fact that in science we are usually not only interested in observable facts and phenomena. Since structures, powers, institutions, relations, etc., are not *directly* observable, we need to use theories and models to *indirectly* obtain knowledge of them (and to be able to *recontextualize* and *redescribe* observables to discover new and (perhaps) hitherto unknown dimensions of the world around us). Deduction and induction do not give us access to these kinds of entities. They are things that to a large extent have to be *discovered*. Discovery processes presupposes creativity and imagination, virtues that are not very prominent in inductive analysis (statistics and econometrics) or deductive-logical reasoning. We need another mode of inference. We need inference to the best explanation.

Inference to the best explanation is a (non-demonstrative) ampliative method of reasoning that makes it possible for us to gain new insights and come up with – and evaluate – theories and hypotheses that – in contradistinction to the entailments that deduction provide us with – *transcend* the epistemological content of the evidence that brought about them. And instead of only delivering inductive generalizations from the evidence at hand – as the inductive scheme – it typically opens up for conceptual novelties and *retroduction*, where we from analysis of empirical data and observation reconstruct the ontological conditions for their being what they are. As scientists we do not only want to be able to deal with observables. We try to make the world more intelligible by finding ways to understand the fundamental processes and structures that rule the world we live in. Science should help us penetrate to these processes and structures behind facts and events we observe. We should look out for causal relations, processes and structures, but models – mathematical, econometric, or what have you – can never be more than a starting point in that endeavour. There is always the possibility that there are other (non-quantifiable) variables – of vital importance and although perhaps unobservable and non-additive not necessarily epistemologically inaccessible – that were not considered for the formalized mathematical model. The content-enhancing aspect of inference to the best explanation gives us the possibility of acquiring new and warranted knowledge and understanding of things beyond empirical sense data. Arguably, realism in its different guises ultimately rests on inference to the best explanation to found the existence of such unobservable entities.

Outside mathematics and logic, scientific methods do not deliver absolute certainty or prove things. However, many economists are still in pursuit of absolute certainty. But there will always be a great number of theories and models that are compatible / consistent with facts, and no logic makes it possible to select one as the right one. The search for absolute certainty can never be anything else but disappointing since all scientific knowledge is more or less uncertain. That is a fact of the way the world is, and we just have to learn to live with that inescapable limitation of scientific knowledge.

> "Traditionally, philosophers have focused mostly on the logical template of inference. The paradigm-case has been deductive inference, which is topic neutral and context-insensitive. The study of deductive rules has engendered the search for the Holy Grail: syntactic and topic-neutral accounts of all prima facie reasonable inferential rules. The search has hoped to find rules that are transparent and algorithmic, and whose following will just be a matter of grasping their logical form. Part of the search for the Holy Grail has been to show that the so-called scientific method can be formalised in a topic-neutral way. We are all familiar with Carnap's inductive logic, or Popper's deductivism or the Bayesian account of scientific method.
>
> There is no Holy Grail to be found. There are many reasons for this pessimistic conclusion. First, it is questionable that deductive rules are rules of inference. Second, deductive logic is about updating one's belief corpus in a consistent manner and not about what one has reasons to believe simpliciter. Third, as Duhem was the first to note, the so-called scientific method is far from algorithmic and logically transparent. Fourth, all attempts to advance coherent and counterexample-free abstract accounts of scientific method have failed. All competing accounts seem to capture some facets of scientific method, but none can tell the full story. Fifth, though the new Dogma, Bayesianism, aims to offer a logical template (Bayes's theorem plus conditionalisation on the evidence) that captures the essential features of non-deductive inference, it is betrayed by its topic-neutrality. It supplements deductive coherence with the logical demand for probabilistic coherence among one's degrees of belief. But this extended sense of coherence is (almost) silent on what an agent must infer or believe" (Psillos (2007:441)).

Explanations are *per se* not deductive proofs. And deductive proofs often do not explain at all, since validly deducing X from Y does not *per se* explain *why* X is a fact, because it does not say anything at all about *how* being Y is connected to being X. Explanations do not necessarily have to *entail* the things they explain. But they can nevertheless confer warrants for the conclusions we reach using inference to the best explanation. The evidential force of inference to the best explanation is consistent with having less than certain belief.

Explanation is prior to inference. Inferring means that you come to believe something and have (evidential) reasons for believing so. As economists we entertain different hypotheses on inflation, unemployment, growth, wealth inequality, and so on. From the available evidence and our context-dependent background knowledge we evaluate how well the different hypotheses would explain these evidence and which of them qualifies for being the best accepted hypothesis. Given the information available, we base our inferences on explanatory considerations (noting this, of course, does not exclude that there exist other, nonexplanatory, factors that may influence our choices and rankings of explanations and hypotheses).

Where did economics go wrong?

If only mainstream economists also understood these basics. But most of them do not. Why? Because in mainstream economics it is not inference to the best explanation that rules the methodological-inferential roost, but deductive reasoning based on logical inference from a set of axioms. Although – under specific and restrictive assumptions – deductive methods may be usable tools, insisting that economic theories and models ultimately have to be built on a deductive-axiomatic foundation to count as being economic theories and models, will only make economics irrelevant for solving real world economic problems. Modern deductive axiomatic mainstream economics is sure very rigorous – but if it's rigorously wrong, who cares?

Instead of making formal logical argumentation based on deductive-axiomatic models the message, we are better served by economists who more than anything else try to contribute to solving real problems – and in that endeavour inference to the best explanation is much more relevant than formal logic.

> "The weaknesses of social-scientific normativism are obvious. The basic assumptions refer to idealized action under pure maxims; no empirically substantive law-like hypotheses can be derived from them. Either it is a question of analytic statements recast in deductive form or the conditions under which the hypotheses derived could be definitively falsified are excluded under ceteris paribus stipulations. Despite their reference to reality, the laws stated by pure economics have little, if any, information content. To the extent that theories of rational choice lay claim to empirical-analytic knowledge, they are open to the charge of Platonism (Modellplatonismus). Hans Albert has summarized these arguments: The central point is the confusion of logical presuppositions with empirical conditions. The maxims of action introduced are treated not as verifiable hypotheses but as assumptions about actions by economic subjects that are in principle possible. The theorist limits himself to formal deductions of implications in the unfounded expectation that he will nevertheless arrive at propositions with empirical content. Albert's critique is directed primarily against tautological procedures and the immunizing role of qualifying or 'alibi' formulas. This critique of normative-analytic methods argues that general theories of rational action are achieved at too great a cost when they sacrifice empirically verifiable and descriptively meaningful information" (Habermas (1988:48)).

Science is made possible by the fact that there are structures that are durable and are independent of our knowledge or beliefs about them. There exists a reality beyond our theories and concepts of it. It is this independent reality that our theories in some way deal with. Contrary to positivism, the main task of science is arguably not to detect event regularities between observed facts, but rather, to identify the underlying structure and forces that produce the observed events.

From that point of view, it could be argued that the generalizations we look for (often with statistical and econometric methods) when using inductive methods (to say anything about a population based on a given sample) are abductions. From the premise "all *observed* real-world markets are non-perfect" we conclude "all real-world markets are non-perfect". If we have tested all the other potential hypotheses and found that, e.g., there is no reason to believe that the sampling process has been biased and that we are dealing with a nonrepresentative non-random sample, we could, given relevant background beliefs / assumptions, say that we have justified belief in treating our conclusion as warranted. Being able to eliminate / refute contesting / contrastive hypotheses – using both observational *and* non-observational evidence – confers an increased certainty in the hypothesis believed to be "the loveliest".

Instead of building models based on logic-axiomatic, topic-neutral, context-insensitive and non-ampliative deductive reasoning – as in mainstream economic theory – it would be more fruitful and relevant to apply inference to the best explanation, given that what we are looking for is to be able to explain what's going on in the world we live in. The world in which we live is – as argued by e.g. Keynes and Shackle – genuinely uncertain. By using abductive inferences we can nonetheless gain knowledge about it. Although inevitably defeasible, abduction is also our only source of scientific discovery.

Most mainstream economic models build on a theory that is abstract, unrealistic and presenting mostly non-testable hypotheses. One important rational behind this kind of model building is the quest for rigour, and more precisely, *logical* rigour. Formalization of economics has been going on for more than a century and with time it has become obvious that the preferred kind of formalization is the one that rigorously follows the rules of formal logic. As in mathematics, this has gone hand in hand with a growing emphasis on axiomatics. Instead of basically trying to establish a connection between empirical data and assumptions, "truth" has come to be reduced to, a question of fulfilling internal consistency demands between conclusion and premises, instead of showing a "congruence" between model assumptions and reality. This has, of course, severely restricted the applicability of economic theory and models.

Unpacking premises and relationships within a consistent model is not enough in empirical sciences. In empirical sciences we do also have to be concerned with the truth-status of the premises and conclusions *re* the world in which we live.

In their search for the Holy Grail of *deductivism* – an idea originating in physics and maintaining the feasibility and relevance of describing an entire science as (more or less) a self-contained axiomatic-deductive system – mainstream economists are forced to make assumptions with often preciously little resemblance to reality. When applying this deductivist thinking to economics, mainstream economists usually set up "as if" models based on a set of tight axiomatic assumptions from which consistent and precise inferences are made. The beauty of this procedure is of course that if the axiomatic premises are true, the conclusions *necessarily* follow. The snag is that if the models are to be relevant, we also have to argue that their precision and rigour still holds when they are applied to real-world situations. They (almost) never do. In the positivist (Hempelian, deductive-nomological) tradition, explanation is basically seen as deduction from general laws. In social sciences these laws are non-existent, and so, *a fortiori*, are the deductivist explanations. When addressing real economies, the idealizations necessary for the deductivist machinery to work simply don't hold.

> "The thrust of this realist rhetoric is the same both at the scientific and at the meta-scientific levels. It is that explanatory virtues need not be evidential virtues. It is that you should feel cheated by 'The world is as if T were true', in the same way as you should feel cheated by 'The stars move as if they were fixed on a rotating sphere'. Realists do feel cheated in both cases" Musgrave (1999:68).

The one-eyed focus on validity and consistency makes much of mainstream economics irrelevant, since its insistence on deductive-axiomatic foundations does not earnestly consider the fact that its formal logical reasoning, inferences and arguments show an amazingly weak relationship to their everyday real world equivalents. Searching in vain for absolute and deductive knowledge and "truth", these economists forgo the opportunity of getting more relevant and better (defeasible) knowledge. For although the formal logic focus may deepen our insights into the notion of validity, the rigour and precision has a devastatingly important trade-off: the higher the level of rigour and precision, the smaller is the range of real world applications. Consistency does not take us very far. As scientists we can not only be concerned with the consistency of our universe of discourse. We also have to investigate how consistent our models and theories are with the universe in which we happen to live.

To understand and explain relations between different entities in the real economy the predominant strategy is to build models and make things happen in these "analogue-economy models" rather than engineering things happening in real economies. This formalistic deductive modeling strategy certainly impresses some people, but the one-sided, almost religious, insistence on axiomatic-deductivist modeling as the only scientific activity worthy of pursuing in economics, forgets that in the realm of science it ought to be considered of little or no value to simply make claims about the model and lose sight of reality. Although the formalistic tractability of deductivist mathematical modeling method makes conclusions follow with certainty from given assumptions, that should be of little interest to scientists, since what happens with certainty in a model world is no warrant for the same to hold in real world economies.

> "Mathematics, especially through the work of David Hilbert, became increasingly viewed as a discipline properly concerned with providing a pool of frameworks for possible realities...
>
> This emergence of the axiomatic method removed at a stroke various hitherto insurmountable constraints facing those who would mathematise the discipline of economics. Researchers involved with mathematical projects in economics could, for the time being at least, postpone the day of interpreting their preferred axioms and assumptions. There was no longer any need to seek the blessing of mathematicians and physicists or of other economists who might insist that the relevance of metaphors and analogies be established at the outset. In particular it was no longer regarded as necessary, or even relevant, to economic model construction to consider the nature of social reality, at least for the time being...
>
> The result was that in due course deductivism in economics, through morphing into mathematical deductivism on the back of developments within the discipline of mathematics, came to acquire a new lease of life, with practitioners (once more) potentially oblivious to any inconsistency between the ontological presuppositions of adopting a mathematical modelling emphasis and the nature of social reality. The consequent rise of mathematical deductivism has culminated in the situation we find today" Lawson (2015:84).

Theories and models being "coherent" or "consistent" with data do not make the theories and models success stories. To have valid evidence is not enough. What economics needs is *sound* evidence. The premises of a valid argument do not have to be true, but a sound argument, on the other hand, is not only valid, but builds on premises that are true. Aiming only for validity, without soundness, is setting the economics aspirations level too low for developing a realist and relevant science.

In science, nothing of substance has ever been decided by just putting things in the right logical form. Those scientific matters that can be dealt with in a purely formal-analytical matter are only of second-order interest. The absurdity of trying to analyse and explain (necessarily "non-Laplacian") real world systems equipped with analytical rather than substantial scientific arguments, becomes clear as soon as we become aware that this is fundamentally a denial of the *field-dependent* character of all science. What counts as a justified inference in economics is not necessarily equivalent to what counts in sociology, physics, or biology. They address different problems and questions, and – *a fortiori* – what is considered absolutely necessary in one field, may be considered totally irrelevant in another. In the case of substantial arguments there is, as Toulmin (2003:163) notes,

> "…no question of data and backing taken together entailing the conclusion, or failing to entail it: just because the steps involved are substantial ones, it is no use either looking for entailments or being disappointed if we do not find them. Their absence does not spring from a lamentable weakness in the arguments, but from the nature of the problems with which they are designed to deal. When we have to set about assessing the real merits of any substantial argument, analytical criteria such as entailment are, accordingly, simply irrelevant ... 'Strictly speaking' means, to them, *analytically* speaking; although in the case of substantial arguments to appeal to analytic criteria is not so much strict as beside the point ... There is no justification for applying analytic criteria in all fields of argument indiscriminately, and doing so consistently will lead one (as Hume found) into a state of philosophical delirium."

Bayesianism

Bayesian statistics has during the last couple of decades led a substantial school in the philosophy of science to identify Bayesian inference with inductive inference as such. However, there is really very little to warrant that belief.

Neoclassical economics nowadays usually assumes that agents that have to make choices under conditions of uncertainty behave according to Bayesian rules (preferably the ones axiomatized by Ramsey (1931), de Finetti (1937) or Savage (1954)) – that is, they maximize expected utility with respect to some subjective probability measure that is continually updated according to Bayes theorem. If not, they are supposed to be irrational, and ultimately – via some "Dutch book" or "money pump" argument – susceptible to being ruined by some clever "bookie".

Bayesianism reduces questions of rationality to questions of internal consistency (coherence) of beliefs, but - even granted this questionable reductionism - do rational agents really have to be Bayesian? Actually, there is no strong warrant for believing so.

The "problem of induction" is usually described as a problem of how we can learn things about a population from knowledge of a sample (spatial version) or how the past may give us information and help us to decide what to believe about the future (temporal version). In both cases Bayesians think they solve the problem through application of probabilistic calculus (especially with the help of Bayes Theorem).

This is however wrong, since from a Bayesian point of view *any* prior probability distribution is "as good as any other", which means that the probability calculus actually does not rule out anything. Anything goes. The sample does not tell us anything about the population. And the past does not – as argued by e.g. Max Albert (2009:55) – tell us anything about the future:

> "Keeping to the Bayesian recipe, then, cannot, by and in itself, help us make better decisions. It just burdens us with a lot of calculations... From a Bayesian point of view, any beliefs, and consequently, any decisions are as rational or irrational as any other, no matter what our goals and experiences are. Bayesian rationality is just a probabilistic version of irrationalism... Any conclusions result from the choice of the prior probability distribution, but Bayesianism does not help us in choosing this distribution."

In many of the situations that are relevant to economics one could argue that there is simply not enough of adequate and relevant information to ground beliefs of a probabilistic kind, and that in those situations it is not really possible, in any relevant way, to represent an individual's beliefs in a single probability measure.

Say you have come to learn (based on own experience and tons of data) that the probability of you becoming unemployed in the US is 10%. Having moved to another country (where you have no own experience and no data) you have no information on unemployment and a fortiori nothing to help you construct any probability estimate on. A Bayesian would, however, argue that you would have to assign probabilities to the mutually exclusive alternative outcomes and that these have to add up to 1, if you are rational. That is, in this case – and based on symmetry – a rational individual would have to assign probability 10% to becoming unemployed and 90% of becoming employed.

That feels intuitively wrong though, and I guess most people would agree. Bayesianism cannot distinguish between symmetry-based probabilities from information and symmetry based probabilities from an absence of information. In these kinds of situations most of us would rather say that it is simply irrational to be a Bayesian and better instead to admit that we "simply do not know" or that we feel ambiguous and undecided. Arbitrary an ungrounded probability claims are more irrational than being undecided in face of genuine uncertainty, so if there is not sufficient information to ground a probability distribution, it is better to acknowledge that simpliciter, rather than pretending to possess a certitude that we simply do not possess.

I think this critique of Bayesianism is in accordance with the views of Keynes, *A Treatise on Probability* (1921) and *General Theory* (1936). According to Keynes we live in a world permeated by unmeasurable uncertainty – not quantifiable stochastic risk – which often forces us to make decisions based on anything but rational expectations. Sometimes we "simply do not know". Keynes would not have accepted the view of Bayesian economists, according to whom expectations "tend to be distributed, for the same information set, about the prediction of the theory". Keynes, rather, thinks that we base our expectations on the confidence or "weight" we put on different events and alternatives. To Keynes expectations are a question of weighing probabilities by "degrees of belief", beliefs that have preciously little to do with the kind of stochastic probabilistic calculations made by the rational agents modelled by Bayesian economists.

There is also a kind of bias toward the superficial in Bayesian thought, which to Richard Miller (1987:325) is an example of:

> "…real harm done in contemporary social science by a roughly Bayesian paradigm of statistical inference as the epitome of empirical argument. For instance the dominant attitude toward the sources of black-white differential in United States unemployment rates (routinely the rates are in a two to one ratio) is 'phenomenological.' The employment differences are traced to correlates in education, locale, occupational structure, and family background. The attitude toward further, underlying causes of those correlations is agnostic... Yet on reflection, common sense dictates that racist attitudes and institutional racism *must* play an important causal role. People do have beliefs that blacks are inferior in intelligence and morality, and they are surely influenced by these beliefs in hiring decisions... Thus, an overemphasis on Bayesian success in statistical inference discourages the elaboration of a type of account of racial disadvantages that almost certainly provides a large part of their explanation."

And as Henry E. Kyburg (1968:56) writes (emphasis added) in perhaps the ultimate takedown of Bayesian hubris:

> "From the point of view of the 'logic of consistency' (which for Ramsey includes the probability calculus), no set of beliefs is more rational than any other, so long as they both satisfy the quantitative relationships expressed by the fundamental laws of probability...
>
> *Now this seems patently absurd*. It is to suppose that even the most simple statistical inferences have no logical weight where my beliefs are concerned. It is perfectly compatible with these laws that I should have a degree of belief equal to 1/4 that this coin will land heads when next I toss it; and that I should then perform a long series of tosses (say, 1000), of which 3/4 should result in heads; and then that on the 1001st toss, my belief in heads should be unchanged at 1/4. It could increase to correspond to the relative frequency in the observed sample, or it could even, by the agency of some curious maturity-of-odds belief of mine, decrease to 1/8. *I think we would all, or almost all, agree that anyone who altered his beliefs in the last-mentioned way should be regarded as irrational.*"

The standard view in statistics – and the axiomatic probability theory underlying it – is to a large extent based on the rather simplistic idea that "more is better". But as Keynes argues in *A Treatise on Probability* – "more of the same" is not what is important when making inductive inferences. It's rather a question of "more but different".

Variation, not replication, is at the core of induction. Finding that p(x|y) = p(x|y & w) doesn't make w "irrelevant". Knowing that the probability is unchanged when w is present gives p(x|y & w) another evidential weight ("weight of argument"). Running 10 replicative experiments do not make you as "sure" of your inductions as when running 10,000 varied experiments – even if the probability values happen to be the same.

Keynes argued that it was inadmissible to project history on the future. Consequently we cannot presuppose that what has worked before, will continue to do so in the future. That statistical models can get hold of correlations between different "variables" is not enough. If they cannot get at the causal structure that generated the data, they are not really "identified".

> "A major, and notorious, problem with this approach, at least in the domain of science, concerns how to ascribe objective prior probabilities to hypotheses. What seems to be necessary is that we list all the possible hypotheses in some domain and distribute probabilities among them, perhaps ascribing the same probability to each employing the principal of indifference. But where is such a list to come from? It might well be thought that the number of possible hypotheses in any domain is infinite, which would yield zero for the probability of each and the Bayesian game cannot get started. All theories have zero probability and Popper wins the day. How is some finite list of hypotheses enabling some objective distribution of nonzero prior probabilities to be arrived at? My own view is that this problem is insuperable, and I also get the impression from the current literature that most Bayesians are themselves coming around to this point of view" Alan Chalmers (2013:165).

Econometrics and randomized experiments

Bayesianism has its root in statistics – and within economics, more specifically, in the statistical application of inductive reasoning in the form of econometrics.

Firmly stuck in an empiricist tradition, econometrics is only concerned with the measurable aspects of reality, But there is always the possibility that there are other variables – of vital importance and although perhaps unobservable and non-additive not necessarily epistemologically inaccessible – that were not considered for the model. Those who were can hence never be guaranteed to be more than potential causes, and not real causes.

When causal mechanisms operate in real world social systems they only do it in everchanging and unstable combinations where the whole is more than a mechanical sum of parts. If economic regularities obtain they do it (as a rule) only because we engineered them for that purpose. Outside man-made "nomological machines" they are rare, or even non-existent. Unfortunately that also makes most of the achievements of econometric forecasting rather useless.

The increasing use of natural and quasi-natural experiments in economics during the last couple of decades has led some economists to triumphantly declare it as a major step on a recent path toward empirics, where instead of being a deductive philosophy, economics is now increasingly becoming an inductive science.

In defence of this view, the works of Joshua Angrist and Jörn-Steffen Pischke are often apostrophized, so let us start with one of their later books and see if there is any real reason to share the optimism on this 'empirical turn' in economics. In *Mastering Metrics,* Angrist and Pischke (2014:xiii) write:

> "Our first line of attack on the causality problem is a randomized experiment, often called a randomized trial. In a randomized trial, researchers change the causal variables of interest... for a group selected using something like a coin toss. By changing circumstances randomly, we make it highly likely that the variable of interest is unrelated to the many other factors determining the outcomes we want to study. Random assignment isn't the same as holding everything else fixed, but it has the same effect. Random manipulation makes *other things equal* hold on average across the groups that did and did not experience manipulation. As we explain... 'on average' is usually good enough."

Angrist and Pischke may "dream of the trials we'd like to do" and consider "the notion of an ideal experiment" something that "disciplines our approach to econometric research", but to maintain that "on average" is "usually good enough" is an allegation that is rather unwarranted, and for many reasons.

> "RCTs… fail to demonstrate any form of universal causality. They show us that by the use of the law of large numbers, we can describe the average characteristics of a large population and changes over time, by appropriately studying a small sample drawn from the population. RCTs do this extremely well, though even here one should add the reminder that average characteristics are not the only pertinent features of populations" Basu (2014:461).

It amounts to nothing but hand waving to *simpliciter* assume, without argumentation, that it is tenable to treat social agents and relations as homogeneous and interchangeable entities. When Joshua Angrist and Jörn-Steffen Pischke in an earlier article of theirs (Angrist & Pischke (2010:23)) say that "anyone who makes a living out of data analysis probably believes that heterogeneity is limited enough that the well-understood past can be informative about the future," I really think they underestimate the heterogeneity problem. It does not just turn up as an *external* validity problem when trying to "export" regression results to different times or different target populations. It is also often an *internal* problem to the millions of regression estimates that economists produce every year.

> "Like us, you want evidence that a policy will work here, where you are. Randomized controlled trials (RCTs) do not tell you that. They do not even tell you that a policy works. What they tell you is that a policy worked there, where the trial was carried out, in that population. Our argument is that the changes in tense – from 'worked' to 'work' – are not just a matter of grammatical detail. To move from one to the other requires hard intellectual and practical effort. The fact that it worked there is indeed fact. But for that fact to be evidence that it will work here, it needs to be relevant to that conclusion. To make RCTs relevant you need a lot more information and of a very different kind" Cartwright & Hardie (2014:ix).

It is hard to share the enthusiasm and optimism on the value of (quasi)natural experiments and all the statistical-econometric machinery that comes with it. Guess we are still waiting for the export-warrant.

In econometrics one often gets the feeling that many of its practitioners think of it as a kind of automatic inferential machine that solves the problem of induction: input data and out comes casual knowledge. This is like pulling a rabbit from a hat. Great – but first you have to put the rabbit in the hat. And this is where assumptions come in to the picture.

As social scientists – and economists – we have to confront the all-important question of how to handle uncertainty and randomness. Should we equate randomness with probability? If we do, we have to accept that to speak of randomness we also have to presuppose the existence of nomological probability machines, since probabilities cannot be spoken of – and actually, to be strict, do not at all exist – without specifying such system-contexts.

In his book *Statistical Models and Causal Inference: A Dialogue with the Social Sciences* David Freedman (2010:14) touches on this fundamental problem, arising when you try to apply statistical models outside overly simple nomological machines like coin tossing and roulette wheels:

> "Regression models are widely used by social scientists to make causal inferences; such models are now almost a routine way of demonstrating counterfactuals. *However, the 'demonstrations' generally turn out to depend on a series of untested, even unarticulated, technical assumptions.* Under the circumstances, reliance on model outputs may be quite unjustified. Making the ideas of validation somewhat more precise is a serious problem in the philosophy of science. That models should correspond to reality is, after all, a useful but not totally straightforward idea – with some history to it. Developing appropriate models is a serious problem in statistics; testing the connection to the phenomena is even more serious...
>
> In our days, serious arguments have been made from data. Beautiful, delicate theorems have been proved, although the connection with data analysis often remains to be established. And *an enormous amount of fiction has been produced, masquerading as rigorous science."*

Making outlandish statistical assumptions does not provide a solid ground for doing relevant social science.

A popular idea in quantitative social sciences is to think of a cause (C) as something that increases the probability of its effect or outcome (O). That is:

$$P(O|C) > P(O|-C)$$

However, as is also well-known, a correlation between two variables, say A and B, does not necessarily imply that that one is a cause of the other, or the other way around, since they may both be an effect of a common cause, C.

In statistics and econometrics we usually solve this "confounder" problem by "controlling for" C, i.e. by holding C fixed. This means that we actually look at different "populations" – those in which C occurs in every case, and those in which C doesn't occur at all. This means that knowing the value of A does not influence the probability of C [P(C|A) = P(C)]. So if there then still exist a correlation between A and B in either of these populations, there has to be some other cause operating. But if *all* other possible causes have been "controlled for" too, and there is still a correlation between A and B, we may safely conclude that A is a cause of B, since by "controlling for" all other possible causes, the correlation between the putative cause A and all the other possible causes (D, E, F, …) is broken.

This is of course a very demanding prerequisite, since we may never actually be sure to have identified *all* putative causes (cf. Basu (2014:460)). Even in scientific experiments may the number of uncontrolled causes be innumerable. Since nothing less will do, we do all understand how hard it is to actually get from correlation to causality. This also means that *only* relying on statistics or econometrics is not enough to deduce causes from correlations.

> "If the assumptions of a model are not derived from theory, and if predictions are not tested against reality, then deductions from the model must be quite shaky...
>
> In my view, regression models are not a particularly good way of doing empirical work in the social sciences today, because the technique depends on knowledge that we do not have. Investigators who use the technique are not paying adequate attention to the connection – if any – between the models and the phenomena they are studying...
>
> Causal inference from observational data presents may difficulties, especially when underlying mechanisms are poorly understood. There is a natural desire to substitute intellectual capital for labor, and an equally natural preference for system and rigor over methods that seem more haphazard. These are possible explanations for the current popularity of statistical models.
>
> Indeed, far-reaching claims have been made for the superiority of a quantitative template that depends on modeling – by those who manage to ignore the far-reaching assumptions behind the models. However, the assumptions often turn out to be unsupported by the data. If so, the rigor of advanced quantitative methods is a matter of appearance rather than substance" David Freedman (2010:56).

Conclusion

Abduction and inference to the best explanation show the inherent limits of formal logical reasoning in science. No new ideas or hypotheses in science originate by deduction or induction. In order to come up with new ideas or hypotheses and explain what happens in our world, scientists *have to* use inference to the best explanation. All scientific explanations inescapably relies on a reasoning that is, from a *logical* point of view, fallacious. Thus – in order to explain what happens in our world, we have to use a reasoning that *logically* is a fallacy. There is no way around this – unless you want to follow the barren way that mainstream economics has been following for more than half a century now – retreating into the world of thought experimental "as if" axiomatic-deductive-mathematical models.

The purported strength of modern mainstream economics is that it ultimately has a firm anchorage in "rigorous" and "precise" deductive reasoning in mathematical models. To some of us, however, this "strength" has come at too high a price. Perhaps more than anywhere else can this be seen in macroeconomics, where an almost quasi-religious insistence that economics has to have microfoundations – without ever presenting neither ontological nor epistemological justifications for this patently invalid claim – has put a blind eye to the weakness of the whole enterprise of trying to depict a complex economy based on an all-embracing representative actor equipped with superhuman knowledge, forecasting abilities and forward-looking rational expectations. How can we be sure the lessons learned in these models have external validity, when based on a set of highly specific assumptions with an enormous descriptive deficit? To have a deductive warrant for things happening in a closed model is no guarantee for them being preserved when applied to the real world.

The urge to view all inferences as more or less deductive and equating good arguments with logical entailment of the "All Xs are Ys" kind, has led mainstream economics down the wrong path. The more mainstream economists insist on formal logic validity, the less they have to say about the real world. And real progress in economics, as in all sciences, presupposes real world involvement, not only self-referential deductive reasoning within formal-analytical mathematical models.

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