

# Economics as a science: understanding its procedures and the irrelevance of prediction

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## Abstract

The paper clarifies economics' status as a science, using as an empirical base the most-cited textbooks in microeconomics and macroeconomics (Varian, 2010 and Blanchard and Fischer, 1989). To avoid the now sterile "positivist debate", it focusses on issues of method, citing two alternative accounts of scientific method – those of Crombie and Nisbet – and exploring which fits better the evidence implied by the two textbooks. It concludes that Nisbet, reporting a very long Western tradition requiring that accounts of social change be "natural histories" (empirically-founded metaphors), fits well the views found in the textbooks. Crombie's view, arguing that science requires management of scepticism by framing procedure in terms of inductive and deductive phases, with requirement for comparison between theories through use of a predictive criterion, fits badly. This suggests that decisions about which economic accounts are deemed correct are not defined by economists' methods, but rather outside economics. It concludes by suggesting that this supports arguments for a "right to scepticism" in both the creation and consumption of policy advice, because this allows judgements to better engage with forces attempting to deem certain accounts as "correct".

**Keywords** policy rationality, scepticism, economists' methodology, prediction, philosophy of science

## Introduction

It is self-evident that economics – what economists do – is both important in the creation of policy advice, and also that, as a procedurally-governed science, consumers of economists' accounts of the world should place trust in the validity and nature of economists' scientific procedures or methods as guiding what is deemed to be correct and so what good policy is. Yet, it is not as easy as it could be to establish precisely the methods that govern it.<sup>2</sup> This paper discusses these methods and argues that an examination of economists' scientific procedures suggests that, in the absence of a criterion within economics requiring exhaustive testing of accounts (such as predictive power), selection of the account deemed correct must, logically, occur *outside* economic method. It takes as exemplars of economists' normative views on procedure Varian 2010 and Blanchard and Fischer 1989, which are reportedly the most widely-cited microeconomic and macroeconomic textbooks respectively. Whilst on one metric these textbooks are the most-cited, of course there are other statements about economists' normative views, some of which are far harder and assertive in their prescriptions.

What is deemed to be correct policy, this paper argues, is better seen as not decided upon by economics, specifically through the scientific method of economics, but by something else. In

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<sup>2</sup> The author comes to the issue here as an applied economist with considerable experience in policy advice and a significant publications record. This paper is therefore in part a "reflective excursion" into matters of method and their relevance to action.

this, the paper argues, economics is far better understood as sharing, in its method, characteristics of other social sciences than how natural sciences are usually understood. In a time of Trump and Brexit, this perhaps helps explain low public trust in economists' assertions, and indeed in government based upon policy (Fforde, 2013; 2017).

Economics is a powerful presence in discussions of policy and governance, and I think it self-evident that it asserts that important parts of change processes are predictively knowable. Discussions, for example, of the pros and cons of austerity policies after the global financial crisis included forecasts of growth, tax revenues, state spending and fiscal positions. Yet, it is also self-evident that the predictive power of such accounts is extremely low, if not spurious, and examination of confirmation bias alerts us to the need for far better management of belief and scepticism alike (Fforde, 2016; 2017). This also means that students of economics and consumers of economists' ideas need, though they often do not get, some assistance in how they judge economics "as a science": what is meant by "as a science" and how can they form judgements about alternative answers? What method do economists use and what can be made of answers to this?

The paper throws light on this. For reasons of space and hoped-for utility, it focusses on presenting its own argument and therefore ignores much of the very large existing literature on the nature of scientific methodology in general and economic methodology in particular; this seems appropriate here and does not intend to suggest that this literature is unworthy, merely that the argument of the paper seems valid as it stands, and that it can be wise to be economical with words.<sup>3</sup> Its focus is upon method and statements of method.

The paper also offers a novel and useful interpretation of the meaning of prediction as a possible element of scientific procedure, of special significance for a highly policy-relevant "real world" science such as economics, but of more general potential value. This comes down to an explanation of why there is a tendency for forces or factors outside social science in procedural terms to be what determines "the truth of the matter". This is of great relevance to understanding how knowledge becomes policy, and here economics is a very useful example of wider and more general trends.

It first presents two statements, chosen for their relative simplicity and convenience, laying down which criteria are required to be met for theories or accounts within a science to be acceptable. They are quite different and clearly refer to distinct and alternative sets of criteria that may be used to judge a practice as scientific or not; in effect, they give two alternative "rules of the game". They may be, if one wants, labelled "natural science" and the other "social science", though this is unnecessary and perhaps confusing, and they draw upon the work of two scholars working in quite different fields who both share, however, a focus upon scientific method understood in terms of procedural *criteria*. I contrast these two statements in

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<sup>3</sup> A search for cited titles containing both "economics" and "method" using Harzing's *Publish or Perish* (which uses Google Scholar) "maxed-out" after returning 6527 citations to 1000 works (17<sup>th</sup> May 2015). Most of the highly-cited works are relatively old, and come from before 2000: 1<sup>st</sup> is Latsis 1976 (530 citations), 2<sup>nd</sup> comes Knight 1956 with 223 and 3<sup>rd</sup> Katouzian 1980 with 222. These arguably predate shifts in the centre of gravity of various ways of thinking about science epitomised by scholars such as Escobar 1995 and Said 1978. This "social epistemological", or relativistic, or linguistic, turn has of course deep roots, such as in Lakatos' stress on observation theory (Lakatos, 1970), not to mention Goedel's work on logical systems in the early 1930s. See also, however, Arndt 1981 for an early discussion by an economist of tensions inherent in the term "economic development" due to a frequent lack of clear distinction between transitive and intransitive uses of the verb "develop", which seems to me to be close to the nub of the matter (Fforde, 2013, Chapter 5).

terms of their different lists of acceptability criteria, intending then to use these lists as tools with which to examine economic science. I do so, therefore, in order to prepare a ground for what is to come, the main point of the paper.

I look for empirically-important discussions of method that can be argued to be particularly relevant to students and teachers of economics. I therefore identify and examine the most popular textbooks as defined by citations data to establish significant views of the criteria said by them used by economists to define their science. Examining these with the tools established in the first part of the paper then permits assessment of what we find. This shows that economics is best viewed as following the criteria loosely defined as “social science”, with some important implications – above all that it focuses upon providing insights and understandings, and in terms of method does not apply a predictive or indeed a comparative criterion. The problem the paper then turns to is to explicate what this means, and here the paper offers a novel insight. This is to suggest a re-interpretation of the nature of prediction, as a criterion with which accounts – theories – may be compared and judged, that is intended to help both economists and those who use the knowledge they create. This re-interpretation is that predictive power is usefully understood, not primarily as the ability of a theory to predict, but rather as a very particular potential member of the list of criteria applied to gauge and accept theories that would require their comparison and how it should be done. Awareness of the significance of the absence of such a criterion helps, I argue, better understand economics as a science.

There is of course a large literature on methodology. Beed 1991 attempts a summary of ongoing changes in natural science and concludes:

“... that the question of whether or not economics is a science, or makes progress, is indeterminate because of a widespread uncertainty about what science is” (p. 488).

This denies any sense that economics as a knowledge production practice exhibits patterns and as such cannot be itself researched, to analyse and present arguments as to what methods are explicitly or implicitly followed. This is denied by the presence of fascinating studies of “what economists do”, such as Yonay, 1998 and Yonay and Breslau, 2006. Such studies allow us to reflect on what their results suggest in comparison with representative studies of scientific methods. My focus here is upon method, as a core analytical focus, and I look for clear statements of method that I can use when examining the two textbooks. Here I deploy two. I avoid arguments as to just how correct or representative they are.

### **Scientific method # 1- Crombie and Grosseteste**

If we search for an accessible investigation of scientific method, a good idea is to look for an account of its historical origins, and a convenient one can be found in Crombie 1953. Crombie looks at a scholar called Grosseteste (c. 1168–1253) who taught Roger Bacon (1214-1294) to whom many histories of science refer. I take Crombie thus as a useful entry point to discussion rather than an established and accepted statement of the truth of the matter. Crombie himself, in the introduction to the second impression, expresses self-criticism in that his particular focus (upon the 12<sup>th</sup>-century scholar Grosseteste) led to his “writings {being} credited with too much influence on science, as distinct from logical and epistemological theory associated with science” (Crombie, 1953, p. v).

We can learn much from Crombie, and he offers the advantage of both historical distance and clear definition. The emphasis upon method is what I stress here.

Crombie argues that the most important aspect of what Grosseteste formulated was *procedural* (Crombie, 1953, p.1). Based upon a belief that science was about the finding of truth, grappling with “the conception of rational explanation contained in scientific texts recently translated from Greek and Arabic” (Crombie, 1953, p.1), what was done, Crombie argues, was to add to an Aristotelian view of procedure a requirement that deductions from theory be tested empirically. Aristotelian thought, it was believed, as a part of Greek science:

“...was dominated by the desire to discover the enduring and intelligible reality behind the constant changes perceived through the senses... and was brought into the realm of logical discourse through the idea of... demonstration or proof, the great methodological discovery of the Greeks which has occupied an essential place in all ideas of scientific explanation ever since. It meant, broadly speaking, that a particular fact was explained when it could be deduced from general principles which related it to other facts” (Crombie, 1953, p. 3).

This meant that, before Grosseteste (in Crombie’s account, viewed in terms of method and focussing upon Aristotle) “scientific investigation and explanation was a twofold process, the first inductive and the second deductive” (Crombie, 1953, p. 25). Regarding the first aspect of the process, the inductive one, Aristotle, according to Crombie:

“... gave a clear psychological account. The final stage in the process was the sudden act by which ... intuitive reason<sup>4</sup>... after a number of experiences of facts, grasped the universal theory explaining them, or penetrated to knowledge of the substance causing and connecting them” (Crombie, 1953, p. 27).

As an explanation, this was both positive about the power of “intuitive reason” and stressed the possibility of science apprehending links between the world of thought and the essential and natural aspects of reality, which are clearly considered knowable through and in this inductive stage. Deduction was then secondary and, in the main, simply showed-off the acquired knowledge. Thus:

“The investigator must begin with what was prior in the order of knowing, that is, with facts observed through the senses, and he must ascend by induction to generalizations of universal forms or causes which were most remote from sensory experience, yet causing that experience and therefore prior in the order of nature. The second process in science was to descend again by deduction from these universal forms to the observed facts, which were thus explained by being demonstrated from prior and more general principles which were their cause” (Crombie, 1953, p. 25).

Crombie then argues that the advances he reports, which he deems crucial, added experimentation to this duality, which implied that whilst the inductive aspect could lead the theorist to believe their theory was true, it was then necessary to relinquish this belief in some

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<sup>4</sup> That is, *nous*.

way and, now sceptical, assess their theory. Deduction then served empirical testing and the relationship between the two moments – induction and deduction – changed, with the latter given greater importance.

Inductive work would be seen as involving suspension of disbelief, a phrase fitting well with the language of theatre and metaphor, where what is obviously just theatre and metaphor can, through suspension of disbelief, be treated as real. We agree to pretend. What is crucial here, and why Crombie stresses *method*, is how belief and disbelief are managed and how they are treated as part of a social epistemology – whether what is done is deemed to be an example of good application of method or not; compliance with method validates what is and was done.

However, Grosseteste was a priest and Christian, who argued that in the process of induction “the mind was assisted by Divine illumination (Crombie, 1953, p. 57).”<sup>5</sup> Thus:

“The special merit of Grosseteste’s theory of science was that he recognized clearly that although causal theories of this kind could not be inferred from the facts they served to explain but could only be suggested by them, nevertheless they could be tested by deducing from them consequences not included in the original generalizations and then carrying out observations of experiments to see if these consequences did in fact happen” (Crombie, 1953, p. 72).

The reasons for this shift away from Aristotle’s position were, it appears, linked closely to Grosseteste’s Christianity and his belief that human reasoning could not, without reengaging with Divine order, find truth. This implies that in the inductive phase the theorist was seen as relatively distant from the Divine, and this needed reversal, hopefully through the deduction of empirically-testable predictions. Mediation – the relationship between theory and empirics – is here, as is surely the case throughout most Christian thought, linked to Christ’s presence in the world, as divine and human — both God and man. Theory therefore had to be tested for it to get closer to truth. Yet, believing that Divine illumination played a crucial role in theorization, in contrast to but not so different from Aristotle’s psychological metaphor (the power of *nous*), Grosseteste had confidence in the ideas he generated inductively. Theorising about optics, he did not bother to test his own theories experimentally. Thus, if Crombie’s account is to be believed, at the very historical origin of modern scientific method, we find the key contributor deciding that their theory “must be true”:

“Very simple experiments could have shown Grosseteste that his quantitative law of refraction was not correct. He was, in fact, a primarily a methodologist rather than an experimentalist... it was one of the basic principles of his theory of science that theories must be put to the test of experiment and that if they were contradicted by experiment then they had to be abandoned. In the next generation such natural philosophers as Roger Bacon and Petrus Peregrinus ... were to use this principle as the basis of some really thorough and elegant pieces of experimental research” (Crombie, 1953, p. 124).

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<sup>5</sup> Quoting Grosseteste “For in the Divine Mind all knowledge exists from eternity, and not only is there in it certain knowledge of universals but also of all singulars.... Intelligences receiving irradiation from the primary light see all knowable things” (Crombie, 1953, p. 73).

This perhaps evokes for a contemporary observer, in world far more secular, the powerful general attraction of theorisation, as a task and practice.

We can then view, using Crombie's account, prediction as a criterion that may or may not be present within a scientific procedure. It appears as a requirement that theory, having been created through a suspension of scepticism in an inductive phase, be confronted with a resumption of scepticism as deductions from theory are confronted with empirical testing. This framing means that a predictive criterion can be seen as essentially *procedural*, seeking to manage the relationship between theory and what it is meant to be about, rather than about prediction *per se*. This is in part because theorisation requires a belief that a theory being created "matters", let us say empirically, and this in turn requires some protection of the process of theorisation, which is removed when the theory is then deemed testable. Theorisation, as the quote above states, "must begin with what was prior in the order of knowing, that is, with facts observed through the senses" (Crombie, 1953, p. 25). One can reflect that what was "prior in the order of knowing" for Grosseteste, in other words possibly "what he saw around him", was thus procedurally deemed to be an inadequate empirical foundation for accepting a theory, and more was needed.

I now turn to a second and also powerful statement of scientific method, which offers a very different set of procedural criteria.

## **Scientific method # 2 – Nisbet and metaphor**

If Crombie's account goes back to the twelfth century, Nisbet's goes back to well before the start of the first millennium (Nisbet, 1969). His focus is upon the rules governing accounts of social change in the West, and he argues that analysis of these takes a long historical perspective. The key points to take from him are three.

First, much can be learnt from a historical discussion of accounts of social change. As Nisbet puts it in his Preface:

"Whatever novelty or originality may lie in the book comes from my having brought into single perspective ideas and themes which are ordinarily considered in isolation from one another. ... Nowhere to my knowledge are all of them united within a single frame of reference that is formed by their common assumptions in the history of Western social thought. This I have tried to do" (Nisbet, 1969, pp. vii, viii).

What Nisbet sees as underpinned by "common assumptions" is the "Western idea of social development" (ibid., vii). Like Crombie, he is examining the shared criteria applied to judge knowledge production. He argues that much can be learnt from digging deep into history to elucidate and map these assumptions, and he concludes that there is a shared pattern. His book goes back to the classical Greeks and forward to the contemporary (the 1960s).

His second point is that beliefs about social development have, over time, usually contained two distinct sets of ideas that are in mutual tension.

Third, that these two sets of ideas are, on the one hand, that social change is particular, contextual and real, and, on the other, that social change is best treated through *metaphor*.

His discussion of the second is a discussion of the rules applied to determine whether accounts are acceptable, that is, scientific, and is therefore a discussion of scientific procedure, equivalent to Crombie's but quite different.

“It is, however, the principal argument of this book that the metaphor ... {is} much more than adornments of thought and language. {It is} quite inseparable from some of the profoundest currents in Western thought on society and change. They were inseparable in ancient Greek thought and in the thought of the centuries which followed the Greeks; and they remain closely involved in premises and preconceptions regarding the nature of change which we find in contemporary social theory” (Nisbit, 1969, pp. 8, 9).

Nisbet stressed how standard accounts of social change in what he calls The West occurred in two different forms: first, detailed “histories” that offered contextual and contingent accounts of what happened; second, “abstract realities” that provided an understanding of essential common patterns in social change, which were, in the main, self-consciously quite different from the first form – natural histories – histories of the *nature* of change. These natural histories presented accounts of what were believed to be true and essential patterns of change. In the long period Nisbet considers (two and half millennia) most scholars understood that such accounts were essentially different from detailed contextualised historical accounts, with a sense quite different from that given to natural history nowadays. Nisbet argues, I think convincingly, that natural histories in Nisbet's sense have retained certain characteristics over this long period and are powerful, because their characteristics meet the criteria of foundational beliefs about what makes an account valid.

Nisbet calls these accounts of abstract reality – theories - *natural* histories. They are histories about the nature of things, for focussing on their nature is the main task for metaphorical accounts. He concludes that, in the broad cultural field he is studying (for him, The West), such accounts share specific attributes:

“For twenty-five hundred years a single metaphoric conception of change has dominated Western thought. Drawn from the analogy between society and the organism, more specifically between social change and the life-cycle of the organism, this metaphor very early introduced into Western European philosophy assumptions and preconceptions regarding change in society that have at no time been without profound influence on Western man's contemplation of past, present and future” (Nisbit, 1969, p. 211).

Nisbet lists the requisite characteristics of such metaphors (the acceptability criteria used to assess the validity of theories: their method) as follows:

“From the metaphor came the notion of change as natural to each and every living entity, social as well as biological, as something as much a part of its nature as structure and process. Second, social change – that is, natural change, was regarded as immanent, as proceeding from forces or provisions within the entity. Third, change, under this view is continuous, which is to say that change may be conceived as manifesting itself in sequential stages which have genetic relation to one another; they are cumulative. Fourth, change is directional; it can be seen as a single process moving cumulatively from a given point in time to another point. Fifth, change is necessary; it is

necessary because it is natural, because it is as much an attribute of a living thing as is form or substance. Sixth, change in society corresponds to differentiation; its characteristic pattern is from the homogenous to the heterogeneous. Seventh, the change that is natural to an entity is the result of uniform processes; processes which inhere in the very structure of the institution of culture, and which may be assumed to have been the same yesterday as they are today” (Nisbit, 1969, p. 212).

Such a list is deeply instructive. Consider the following, from a much-cited book in the field of international political economy [Held et al 1999] where the question is asked – “What is globalisation and how should it be conceptualised?”,<sup>6</sup> and they offer a list of criteria as follows:

“...any satisfactory account of globalization has to offer: a coherent conceptualisation; a justified account of causal logic; some clear propositions about historical periodization; a robust specification of impacts; and some sound reflections about the trajectory of the process itself” (Nisbit, 1969, p. 14).

Like Nisbet’s list, but unlike Crombie’s, this says nothing about how accounts or theories should be compared. What they focus on in the main is the (logical) form of the account, almost taking for granted that there is some empirical support for it. This is however very muted in both lists. Let us now consider significant statements about economic method.

## **Statements on economic method**

### **Statements**

As a science, a producer of knowledge, to be coherent economics must be governed by, and so explicitly or implicitly contain, rules that give scientists assessable criteria for judging candidates for knowledge, including the procedures that should be followed. The quote from Held et al above is an example. It is hard to imagine an economic account that was deemed illogical that would be accepted by economists as valid. There is thus an empirical question, which is what these rules are.

Study of such rules, how they change and how they are viewed, is familiar to many economists from the works of scholars such as Kuhn, 1962; Popper, 1959 and Lakatos, 1970. They may be less familiar with other scholars, such as Said 1978, Escobar 1995 and Foucault. One difference between these two groups is that the former tend to maintain a focus upon understanding scientific practices as in some sense progressive, in that they may be read as implying that science creates, on the whole, better knowledge over time, whilst the latter are more focussed upon issues such as the power implications of knowledges. What they share is an epistemological interest – in studying aspects of knowledge rather than knowledge itself: they are reflective. However, if we look at canonical texts in economics, we tend to find that matters of method are treated *ex cathedra*: that is, they are treated as given – perhaps to be stated, perhaps not, but not something meriting much reflection.

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<sup>6</sup> Harzing’s *Publish or Perish*, based upon Google Scholar, gives 7909 citations as of March 30<sup>th</sup> 2015, far more than either Varian or Blanchard and Fischer.



Here I treat textbooks as canonical: the place to look for normative statements of scientific method. Using as a citations metric the data from Google Scholar, the most highly cited microeconomics and macroeconomics textbooks are Varian 2010<sup>7</sup> and Blanchard and Fischer 1989.<sup>8</sup> I consider them in turn.

### **Varian**

Varian advances various scientific criteria for the validity of what he teaches. He states that his:

“... aim ... was to present a treatment of the methods of microeconomics”  
(Varian, 2010, xix).

That:

An analytical approach to economics is one that uses rigorous, logical reasoning (Varian, 2010, xix).

And that:

“The conventional first chapter of a microeconomics book is a discussion of the ‘scope and methods’ of economics. Although this material can be very interesting, it hardly seems appropriate to **begin** your study of economics with such material. It is hard to appreciate such a discussion until you have seen some examples of economic analysis in action ... Economics proceeds by developing **models** of social phenomena. By a model we mean a simplified representation of reality” (Varian, 2010, p. 1).

This is the only place in his text where the phrase “scope and methods” can be found. He does not return at the end of the book to discuss it - the final chapter is, like the others, about theory.

The book expositis the well-known body of microeconomic theory, and deploys powerful and elegant metaphorical argument. Thus:

The great virtue of a competitive market is that each individual and each firm only has to worry about its own maximization problem. The only facts that need to be communicated among the firms and the consumers are the prices of the goods (Varian, 2010, p. 627).

Searching on “facts” shows that this means for him the facts of theory (e.g. pp xix, 90, 162, 279 (where the phrase “mathematical facts” is used), 370, 398 and 479. Footnote 5 on p. 532, however, cites a Wall Street Journal article to support the assertion that “threat of retaliation then serves to keep all prices high”. There is but one mention of empirics (search under “empiric”), in a discussion of what the standard models say about the effects upon work of changes in wages:

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<sup>7</sup> Using Harzing’s *Publish or Perish* (8<sup>th</sup> April 2015) this work, dated 2010 – the 8<sup>th</sup> edition - but including citations of earlier editions when Varian had co-authors - had 3357 citations.

<sup>8</sup> Using the same source as before, Blanchard and Fischer had 4929 citations; running a close second and third were Romer 2011 with 4912 and Obstfeld and Rogoff 1996 with 4619.

“As the wage rate increases, people may work more or less. ... Why does this ambiguity arise? When the wage rate increases, the substitution effect says work more in order to substitute consumption for leisure. But when the wage rate increases, the value of the endowment goes up as well. This is just like extra income, which may very well be consumed in taking extra leisure. Which is the larger effect is an empirical matter and cannot be decided by theory alone” (Varian, 2010, p. 176).

His last sentence is a clear metaphor for the relationship between theory and reality. Theory captures the essence of reality, and beyond that empirical investigation is needed.

Varian is clearly following a scientific procedure. As he says, the core of this procedure is the construction of models that are “a simplified representation of reality”. Therefore, as for the accounts of natural histories Nisbet reports and analyses, empirical aspects of method are far less important than exposition of theory – he therefore, consistently, does not need to elucidate, for example, how economists should judge whether a representation of reality is a good one, other than that it be “logical”.<sup>9</sup>

### ***Blanchard and Fischer***

Turning to Blanchard and Fischer, we find again belief in the presence of shared and coherent procedure - that economics is a science. Thus they argue that the existence of “multiple truths” in macroeconomics does not mean that it is not a science:

On the surface, macroeconomics appears to be a field divided among schools, Keynesians, monetarists, new classical, new Keynesian, and no doubt others. Their disagreements ... leave outsiders bewildered and skeptical ... This is not our assessment ... We believe that macroeconomics exists as a science, an admittedly young, hesitant, and difficult one. Its inherent difficulties stem from the need to draw from all branches of microeconomics, deal with aggregation, make contact with data, and eventually make policy recommendations (Blanchard and Fischer, 1989, xi).

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<sup>9</sup> He has little to say, an issue shared by Crombie and Nisbet (and Held et al), about what exactly it means to be logical. Compare Kline 1980, arguing, for example, that as a believing Christian it was quite natural for Newton to believe what we could now call his intuition, but is better called his belief in a revealed or revealable natural order, leaving proofs of important steps in his formal argument until later as he pressed on to his conclusions. Also Priest, who, in a provocative and heterodox book argues for the possibility of true contradictions (Priest, 2002). For him, true contradictions are illustrated by the proposition that, standing in a doorway, somebody can be both in, and not in, the room. Varian would presumably disagree with Priest, asserting that Priest was illogical. This suggests that such statements and their acceptability would depend on what one means (in part, who one is) and how in that context meaning is interpreted, so that a procedural requirement that “one be logical” should also state what that means – what logic should be followed and how disputes about being illogical be resolved. Winch 1958 restates Lewis Carroll’s paper *What the tortoise said to Achilles* to conclude that “The moral of this, if I may be boring enough to point it, is that the actual process of drawing an inference, which is after all at the heart of logic, is something which cannot be represented as a logical formula ... Learning to infer is not just a matter of being taught about explicit logical relations between propositions; it is learning to do something” (p. 57). I conclude from all this that it would be better to describe such accounts as metaphors rather than theories.

And:

“We have written this book to ... present the common heritage, the conceptual framework and the set of models that are used and agreed upon by the large majority of macroeconomists ... {and to} present life at the frontier, showing the various directions in which researchers are currently working” (Blanchard and Fischer, 1989, xi).

Their stance regarding empirics is somewhat different from Varian. They start “with the basic facts that need to be explained, the existence and persistence of economic fluctuations and their characteristics” (Blanchard and Fischer, 1989, xi) and then exposit standard models used to explain them. But they point out up front that whilst these building blocks “shed light on the fundamental issues” (ibid., xi), they are essentially “equilibrium economics” (ibid., xii) and at this point there is disagreement amongst economists, who divide into the various schools they mentioned at the start. At this point, though, as strikingly as Varian’s remark that “Which is the larger effect is an empirical matter and cannot be decided by theory alone”, they argue that “Working economists, like doctors treating cancer, cannot wait for all the answers to analyze events and help policy. They have to take guesses and rely on a battery of models that ... have repeatedly proved useful” (ibid., xii).

The point here is that the gauge of a model, of an explanation, is for them linked strongly to the ability to use it to give policy advice. Yet, there is no discussion of the extent to which macroeconomics contains, in its method, either of two things: first, ways of comparing, procedurally, different models; second, whether it would or could be better, given the way in which models are empirically founded, to “do nothing”. Here, then, we find empirically-founded metaphorical accounts, expressed in terms of sophisticated models with varying degrees of econometric support, asserted to possess predictive power.

## **Discussion**

The power of economic ideas, especially in policy debates, clearly draws upon many things, and just how exactly they gain authority is far from certain, though what does seem clear from my exposition so far is that, like social science more generally, what is deemed to underpin a knowledge-based policy cannot, if we follow Nisbet, be understood solely in terms of scientific procedure.

Part of the story, however, surely is that audiences expect economists to be, in some sense, scientists, seeing economics as rule-governed. The question therefore examined here is, “what rules”? Audiences view economists, amongst other things, as producers of knowledge, and they expect economists to follow rules in doing so. Thus, as we have seen, these textbooks provide for students and others rules defining what is acceptable as microeconomic or macroeconomic theory, and these allow economists to refer to shared criteria that make theories acceptable, for without this discussion and debate would not only be chaotic, but lack the potential to gain audiences and so support for policy proposals. But of course many other factors come into play.

The three examples I have given (Held et al, Varian and Blanchard and Fischer, 1989) clearly all follow and share the same basic rule, which is that they offer accounts of what *should* be done to produce valid accounts, that is, scientific knowledge. These lay down the procedural rules that should be followed, and this is what we should expect. These are all, in equivalent

ways, statements about what a theory should be (for Varian, as for Blanchard and Fischer, acceptable models; for Held et al a satisfactory account). By implication, theories that do not meet their criteria are unacceptable. They are writing for audiences, so if you read or are taught any of these three books (Held et al, Varian and Blanchard and Fischer, 1989), then you learn that a theory or account that does not follow, in its production, the particular given criteria, is wrong, and should be rejected. If somebody uses different criteria they are wrong because they are not following the procedure that defines what science is.<sup>10</sup>

So, I ask what these criteria may be. It is clear from the way these texts are written (see the quotes above) that these are to a considerable extent taken by the authors as obvious, clear enough and not worth (at least in the texts) much deliberation. None of them, for example, give any citations as to the origins of their methodological statements, nor discuss alternatives, nor use citations to support their positions. This is striking, for, as I have discussed, there are choices being made because there are identifiable alternatives.

It is clear that the canonical economics texts discussed do not suggest following anything like the procedure reported by Crombie. No distinction, for example, is made between the empirics of, on the one hand, theorisation, and on the other use of deductions from theory to create assessable predictions. Theory is essentially metaphorical, showing the essence of what is happening, with deviations from it to do with the particular circumstance.

Microeconomics, *pace* Varian, is a statement of theory. Readers are therefore offered almost no discussion of empirics or facts, and data is referred to in order to provide passing support, in a manner reminiscent of what Crombie has to say about induction, to theory. There is no sense of a managed movement between a suspension of scepticism and then its resumption. There is no distinction between “things reliably known and things less reliably known”.

Searching through the text for references to “data” is illuminating. On pp. 83-84 data is presented to show how a utility function can be derived from data describing consumer behaviour. This is no more than a demonstration that a particular functional form, selected ad hoc, “fits” the data presented. The particular functional form used for this exercise is not theoretically justified (as, for example, an inverse square law is justified in Newtonian theories of gravitation). Of itself this suggests strongly that we dealing with a science of metaphor – that is with a production of “natural histories” that grasp and explain what is said to be essential. The etymology of the word metaphor appear to be “to carry beyond, or over”, which points to the status of theories and accounts as being related but somehow “beyond” something else, what is often called “reality”. The discussion here, drawing upon Nisbet, perhaps suggests that social science knowledge production is usefully seen as *essentially* theorisation, a rule-based production of theories that are usefully seen as metaphors, and only at great risk seen as truthful expressions of reality (and so reliable guides to prediction). Inductive methods that produce such metaphors, or theorisations, are not divorced from reality, they are empirically-based metaphors, *but that is all*. Nobody would trust (or be able to insure) an aeroplane whose design was based upon theory alone; what gets insurance is a judgement that risks are acceptable.

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<sup>10</sup> Obviously, and this is abundantly clear from practice, there is and can be extensive debate about the particular meanings of terms such as “accepted”, “procedure” and so on; but the point stands, as it is about social norms, not truth (or rather it is about the implications of the idea that the truth of a matter can be decided – sometimes a big ask). See the quote from Winch above.

As Varian states, however:

“We can estimate a utility function that describes their consumption patterns and then use this estimated utility function to forecast demand and evaluate policy proposal” (Varian, 2010, p. 85).

Data is also presented on p.126, again to show how demand varies, theoretically, with price. This data is constructed for this purpose only, to illustrate theory, for example, leading to the conclusion that it:

“... could not be generated by a consumer with stable preferences who was always choosing the best things he or she could afford” (Varian, 2010, p. 128).

Data here is used to see whether theory works, in terms of matching the data.

“Think, for example, of a household consisting of several people. Will its consumption choices maximize “household utility”? If we have some data on household consumption choices, we can use the Strong Axiom of Revealed Preference to see” (Varian, 2010, p. 130).

This is the empirics of inductive reasoning, in Crombie's sense. It seeks to manage empirical aspects of theorisation, not by deduction and prediction, but by continuing to believe in the theory. At root, *it seeks to defend the theory*. Data is used to support the theory; scepticism is suspended, disbelief is too. The scientific method applied is thus very different from that described by Crombie.

Similar considerations apply to Blanchard and Fischer. It is clear that, for them, macroeconomics is mainly to be defined as what macroeconomists do, and this is, essentially, to use a shared “conceptual framework and ... set of models that are used and agreed upon by the large majority of macroeconomists” (Blanchard and Fischer, 1989, xi). The main thrust is to do with “with the basic facts that need to be explained” (ibid., xi]. The word prediction is not to be found in their Index, nor is there an entry for forecasts or forecasting. Whilst some may argue that it is self-evident that macroeconomic modelling is not predictively powerful, more importantly, prediction is not important to its method.

What is important for Blanchard and Fischer is very similar to what Nisbet is reporting, and is the idea that an economic theory should offer an insight into the economic logic of what is observed. Like Varian, what we find here is a science of metaphor.

Consider the basic stance of microeconomics as Varian expounds it, and the role within it, well-known to any trained economist, of competition as modelled through comparative statics. Competition is seen as natural and the primary force of change, coming from forces within the economy, as theorised. In terms of his third criterion, change is cumulative, as competition pushes the economy to changes in levels of output and consumption. Change is directional, as competition pushes towards optimal outcomes, *unless inhibited by market failure*. What comes through particularly clearly is conveyed well by Nisbet's very particular use of the term “natural history”: economists' theories offer us accounts of an essential nature of social change, for example in microeconomics put in terms of deviations from competitive outcomes. After all, rents are “a gain or advantage that cannot be competed away” (Levy, 1995, p. 96).

Finally, reconsider the clear statement from Held et al about the criteria any account should meet to be deemed acceptable. Again, this omits the criterion of prediction, and indeed any criterion requiring comparison of competing theories or accounts; it states the rules that allow scientists to validate theorisation. In all three examples, therefore, we find a science of metaphor, close to what Nisbet reports and very distant from Crombie.

The remainder of the paper first draws together the discussion of the nature of economic science, and then offers a novel account, that greatly clarifies the situation, of how we should best view prediction as a criterion.

## **Economic science**

The discussion above relied for its empirical basis for discussing economic science upon textbooks. Although there is not much research that examines what economists do when they choose what to model, we can examine Breslau and Yonay, 2006.<sup>11</sup> They conclude:

“The truth of economic statements is ... the product of economists’ success in enlisting the support of other economists, data, whole economies, mathematics, and other agents, rather than adherence to an established and rule-based method” (Breslau and Yonay, 2006, p. 5).

Breslau and Yonay point out that whilst a model with the approved building blocks (statements about agents’ preferences, etc.) and an analytic solution may be challenged on the grounds of empirical plausibility, this is not a *predictive* criterion:

Referees and editors often cite implausibility as a reason for rejecting articles. They use their sense and knowledge of the economy to assess whether a model offers an important explanation of an economic phenomenon. Thus, an article can handle an important subject, be rigorously constructed, and still be rejected if the referees and the editor believe that it fails to address a main mechanism behind the phenomenon in question (Breslau and Yonay, 2006, p. 28).

This is clearly interpretable through Nisbet’s lens, as a deliberation on whether the theory captures empirically essential (“natural” in Nisbet’s word) processes that exist in reality.

I conclude that economic science, not following a methodology that includes something equivalent to a predictive criterion, is best seen as empirically-based theorisation that focusses upon the generation of models deemed to improve understanding. The absence of a comparative criterion from procedure is striking, and, as Nisbet suggests, this corresponds to scientific regulation that permits – has no formal criterion to prevent – the co-existence of “multiple truths”, any criterion for choice between which, if it happens, exist outside the rules scientists are following. Choice between theories – for example as part of debates about

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<sup>11</sup> They state – “[W]e want to ask how neoclassical economists themselves make the connection between their models and economic realities. . . . [O]ur goal is to elucidate the ‘epistemic culture’ of economists that guides their own routine work of model-building and their evaluation of their colleagues’ models. Such empirical studies of economics are strikingly missing, despite economics’ allegedly huge influence on economic policymaking, and consequently on the lives of us all” (Breslau and Yonay, 2006, p. 6).

public policy - must be made *outside* the rules followed by economists' that govern their endeavours as scientists.<sup>12</sup>

**Viewed as an element of scientific method, are tests for predictive power best seen as tests for the ability of a theory to predict?**

Whilst it may superficially appear clear, an alleged ability of a theory to predict is easily shown to depend upon a host of tangled factors, so things are not clear at all.

At an extreme, to start with, a theory that is right 51% of the time could feasibly be described as predictive, but is not likely to be. Yet if the point is to win bets placed very many times, then it could be thought of as predictive. Theories from physics, such as Newton's laws of motion, are widely felt to be predictive, but this is within certain bounds, about which quite a lot is known. On the one hand, for example, as velocities approach the speed of light, so mass, assumed constant, is thought to vary. Again, just as Newtonian space is conceptually made up of lines, with no presence outside one dimension, and points, with no presence at all, so mass is assumed to be something that can be situated at a single point, a centre of gravity. All of this can be understood to mean that the apparent clarity of Newtonian physics is not what makes it acceptable under some circumstance as a guide to action. The extent to which it matters that observables necessarily seem to flout the scientific metaphor involved – lines as measured have width, points in time have duration, forces cannot be directly observed - and are therefore associated with an ability to insure the resulting object (say, an aeroplane) depends on the local and social context. To develop this argument, if gun-laying was being done for "extremely inaccurate" riflemen in a war of accepted extreme levels of attrition (consider if the guns were aimed by cloned animals), then prediction that entailed a 51% accuracy rate could be, one can imagine, accepted, as it would arguably "win the war". There is no escape from the social context in which beautiful theory like Newton's might – or might not – be used.<sup>13</sup>

Further, as Lakatos 1970 pointed out, to make sense of data requires observation theories, and the accuracy of observation – whatever that means – likely has some bearing on the way in which terms within theory map to observables. Thus, whilst predictive power may seem clear, it is not. One is tempted to conclude that predictive power exists when it is said to exist; this is done by some community, with reference to all the complex tangles human communities generally seem to be able to manage. They will therefore likely often argue about it. If this conclusion is reasonable then what can be said about predictive power?

What comes from my discussion of the contrast between the different criteria defining the acceptability of theory that we find in Crombie and Nisbet is that prediction is most important

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<sup>12</sup> Such processes can be researched. Two studies that are striking for me are Yonay, 1998 and Rodgers and Cooley, 1999.

<sup>13</sup> As McCloskey 1985 puts it: "The numbers are necessary material. But they are not sufficient to bring the matter to a scientific conclusion. Only the scientists can do that, because "conclusion" is a human idea, not Nature's. It is a property of human minds, not of the statistics." (p. 112). And: "It is not true, as most economists think, that . . . statistical significance is a preliminary screen, a necessary condition, through which empirical estimates should be put. Economists will say, "Well, I want to know if the coefficient exists, don't I?" Yes, but statistical significance can't tell you. Only the magnitude of the coefficient, *on the scale of what counts in practical, engineering terms* as nonzero, tells you. It is not the case that statistically insignificant coefficients are in effect zero" (stress added p. 118). Quoted in Fforde, 2013.

in that it requires two things, and neither are to do with prediction *per se*, as it is generally understood (e.g. “getting a rocket to the moon”).

First is the requirement for comparison between theories *as a matter of procedure*. If, however, this is not part of scientific procedure and a single truth is required, then this choice is logically done outside of scientific procedure.

Second is explicit management of the shift between suspension of scepticism in theorisation (Crombie’s inductive phase, when theory is empirically-founded) and its resumption when theory can be, if the empirics suggest, abandoned. Following such norms, theory has to be protected, but not for ever, and it has also to be killable.

This view of the nature of predictability seems to me to be novel, and also to allow us to get away from somewhat fruitless debates. Economics as a science is about providing insights and improved understandings, and this is shown by its method.

## **Discussion**

The idea that, because social change is unpredictable, the notion of development, of intentional social change itself (based upon statements that policy X will lead to change Y), is particularly problematic in international development. Fforde, 2005 reported citations of the application of robustness-testing methods to studies of the causes of variations in economic growth globally (Levine and Zervos, 1993). Levine and Zervos concluded that in the data there were almost no robust relationships, in other words that the articles in large literature asserting that the causes of growth were known, and reporting statistical analyses to support this, were spurious. Citations examined in Fforde, 2005 showed that most economists dealt with this anomaly by ignoring it, though a minority did not. Kenny and Williams, 2001 suggested that these spurious statistical results stemmed from assumptions of ontological and epistemological universalism, in other words that the world was far more varied than economic theory and its language suggested. Fforde, 2017 points out that, in international development practice, this set of scientific assumptions, as development workers well know, leads to denial of voice and a well-publicised series of “horror stories” as, totally unsurprisingly, outcomes are unexpected and often perverse [e.g. Ferguson, 1997]. The tension between viewing intentional social change, such as the deployment of a given economic policy, as something that is both done and also happens, as intentional and also part of some predictively knowable process, was clarified by the work of Cowen and Shenton 1996, who argued that historically two apparent solutions had been deployed. Both were answers to the question: what is correct policy? Like Levine and Zervos, they imply that social change is unpredictable. Given this, they argue that both solutions preserved the stance that change was predictively knowable. One asserted that correct policy was simply policy that fitted with the logic of change (this they term the Marxist solution); the other that correct policy was simply what those in authority said was correct policy.

## **Conclusions**

The focus of this paper has been upon method. Economics is probably the single most important policy-relevant discipline in the social sciences. It is therefore important to understand matters of procedure – method – as they apply to economists’ knowledge



production. It is also important for students of economics and consumers of economists' ideas to have a clear understanding of the rules, sometimes implicit, that validate economists' judgements in that they "follow the rules" – this is what makes them acceptable to other scientists.

Based upon an examination of three important texts we find that all have much to say about method and procedure. This is to be expected. What we find, though, is that it is impossible to link their statements about procedure to an arguably canonical exposition, following Crombie, of a science procedure that entails empirical assessments of theories derived from deduction using inductively-derived theorisation that use a predictive criterion or an equivalent. This is not what they are doing nor is it what they think they should do. Rather, Nisbet's arguments about the criteria required for accounts of social change seem far more appropriate, and lead to an understanding of economic science as the production of empirically-founded metaphors. Nisbet's arguments elucidate what these economic texts say they do and what they think they should do, as economists.

This helps explain just why and how economics exists as a powerful "real world" source of policy-relevant knowledge and popular beliefs about social realities. To carry weight in such areas, where, if we agree with Nisbet the competition is between empirically-founded metaphors (rather than Crombian prediction), that is the type of knowledge that has to be deployed. Arguments about the value of competition and free markets, supported and informed by economic theory, sit well within what Nisbet has to tell us about the particular and deep-rooted beliefs he reports governing what is required for accounts of reality to be accepted – to be given a "seat at the table". For me, this very much helps explain the power of economics as a science.

But this was not linked, in my argument, to some notion that economics is "not a predictive science". Rather, prediction, I have argued in what I think is a novel contribution, is more usefully seen as a criterion present in some scientific procedures, but not in others. It is usefully seen as acting, I have argued, as a requirement that theories be procedurally compared, with the implication that if it or an equivalent is absent, and a single truth required, theory selection will be done by something *outside* scientists' procedure. From this point of view economic science (understood in the terms here, that is, a science that is following Nisbettian rather than Crombian procedural rules) is, not being so protected by its procedures from outsiders' influence, usefully seen as required to manage that influence, in ways this paper has not addressed, partly for reasons of space, as the literature is vast, but also as the point I am making does not require it.

Further, the analysis showed that, whilst the key point to grasp about prediction is not that "it tells you whether theory is right", but that it is absent or present in the different criteria adopted by different types of science, the key point about science method, as Crombie presented it, was the prescribed management of belief/disbelief during and after theorisation. Empirically-founded induction, or in a modern language theorisation, requires a suspension of disbelief in theory: a suspension of scepticism for the theorist to theorise and believe that theory, a metaphor, has an acceptable relationship to reality from which it is separated and to which it has somehow to be mediated. Predictive power, *in terms of its scientific method alone*, is therefore clearly not important to economic science. What is important, in terms of how we may interpret Nisbet, is the ability to generate understanding of economic aspects of social reality that makes sense in that it offers powerful metaphors about the nature of economic phenomenon, in a complex and confusing world.

Various obvious and important implications follow for many who sit within the policy – process. For those seeking to use policy to guide action, it follows that a right to scepticism is vital. People should be free to assert that in a particular context change is predictively unknown, and organise accordingly (Fforde, 2017). For those seeking to gauge policy analyses, such as politicians and their political advisers, my argument suggests that they wisely be keenly aware that it is they, not the procedures shared by the array of knowledge producers confronting them, that decide the “truth of the matter” – that is, which amongst competing theories will be used. This has important implications for accountability, as some modern democratic electorates may have realised, or be realising, in one interpretation of Trump and Brexit. Economics, in this framing, has to be sure - if it is progressive - just how its positions are deemed correct: that is, for and by whom, and in whose name.

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