Confirmation bias: methodological causes and a palliative response

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Abstract

The paper advocates for changes to normative aspects of belief management in applied research. The central push is to argue for methodologically-required choice to include the possibility of adopting the view that a given dataset contains insufficient regularities for predictive theorising. This is argued to be related to how we should understand differences between predictive and non-predictive knowledges, contrasting Crombie and Nesbit. The proposed direction may also support management practices under conditions of uncertainty.

Keywords: confirmation bias, research methodology, agnotology, unknowability

Introduction

Just what is confirmation bias: belief in belief

The phrase 'confirmation bias' is well-known to scholars. It is usually taken to mean the presence of a bias in interpretation of data that encourages support for pre-existing knowledge. The most-cited work containing the phrase in its title is Nickerson 1998:[1]

> As the term is used in this article and, I believe, generally by psychologists, *confirmation bias* connotes a less explicit, less consciously one-sided case-building process. It refers usually to unwitting selectivity in the acquisition and use of evidence [175]

Further, he reports that:

> Studies of social judgment provide evidence that people tend to overweight positive confirmatory evidence or underweight negative discomfirmatory evidence [180]

And that:

> People sometimes see in data the patterns for which they are looking, regardless of whether the patterns are really there [181]

This raises, for some, the question as to why people 'see things that are not really there',[2] which suggests, surely, that we should at least consider that beliefs we could label as superstition, or religious, are present. We can note from this Gillespie 2008

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[1] The metric used is from Harzing's *Publish or Perish*, which uses Google Scholar, and reported (25/3/2015) 1749 citations. Second was Mynatt et al 1977 with only 391.

[2] Thus Caballero 2010 treats such tendencies as an example of 'pareidolia', the familiar situation where the mind sees patterns (such as faces in clouds) that do not actually exist.

who argues that the modern secular world is far more influenced by its religious past than is perhaps thought:

> [T]he apparent rejection or disappearance of religion or theology in fact conceals the continuing relevance of theological issues and commitments for the modern age. . . . [T]he process of secularisation or disenchantment that has come to be seen as identical with modernity was in fact something different than it seemed . . . the gradual transference of divine attributes to human beings (an infinite human will), the natural world (universal mechanical causality), social forces (the general will, the *hidden hand*), and history (the *idea of progress*) [272–273, emphasis added]

> What actually occurs in the course of modernity is thus not simply the erasure or disappearance of God but the transference of his attributes, essential powers, and capacities to other entities or realms of being. The so-called process of disenchantment is thus also a process of reenchantment in and through which both man and nature are infused with a number of attributes or powers previously ascribed to God. To put the matter more starkly, in the face of the long drawn out death of God, science can provide a coherent account of the whole only by making man or nature or both in some sense divine. [274]

This is suggestive. It may be useful to consider that confirmation bias, viewed from a distance, reflects a culturally-specific 'belief in belief' explicable in terms such as belief in revelatory knowledge, in the very validity of searches for truth, etc. This is not to say that before Christianity everyone was a sceptic, but to suggest that confirmation bias (and here there is of course a vast literature) can be thought of as a belief that what is believed happens to be true, in some sense.

The bias, at the level of the individual, is towards confirming existing beliefs; at the social level, it supports shared beliefs that there is a knowable order, some specific knowledge of which is then to be confirmed. Consider Cohen 1984:

> What is wrong with NHST [Null Hypothesis Significance Testing – AJF]? Well, among other things, it does not tell us what we want to know, and we so much want to know what we want to know that, out of desperation, we nevertheless believe that it does! What we want to know is, "Given these data, what is the probability that H0 (the Null Hypothesis – AF) is true?" But as most of us know, what it tells us is "Given H0 is true, what is the probability of these (or more extreme) data?" These are not the same, as has been pointed out many times over the years. [997]

That what is taught in basic statistical courses – here the assumptions required for application of Hypothesis-testing methods - should so often be ignored arguably reflects the social value attached to confirmation of belief that there is a knowable order: a bias against judgement that it is better to assume unknowability in that particular context, or, to put it more strongly, that social knowledge production should be able to argue that we *know*, here, that there is an unknowable unknown, and that we act accordingly.[3] This bias can be dangerous. Reference could be made here to Keynes' discussion of uninsurable risk, which is not a matter of uncertainty, but of a view that 'here' we know that we do not know. This is not to take a philosophical view that, in general, we 'cannot know truth', but to seek and validate procedures that allow us to form and defend such views. The point of this paper is to provide an analysis of research method that then leads it to make proposals for changes in

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[3] Thus the American Statistical Association in March 2016 felt driven to release a statement on p-values and their misinterpretation (ASA 2016): ""Over time it appears the p-value has become a gatekeeper for whether work is publishable, at least in some fields," said Jessica Utts, ASA president. "This apparent editorial bias leads to the 'file-drawer effect,' in which research with statistically significant outcomes are much more likely to get published, while other work that might well be just as important scientifically is never seen in print."" [1]..

method, that come down to a *procedural* requirement that permits (and so also rejects) statements that 'here, we know that the unknown is unknowable'.

If knowability is associated with knowable cause-effect relations, then consider the design of structures facing risk of repeated stress leading to cracks that propagate fast or slowly, with high speeds of crack propagation being experienced as dangerous. It turns out that the social construction of knowledge that allows aeroplanes to be design, manufactured, sold and operated with reasonable levels of insurance relies upon predictive knowledge where no theory explains and predicts propagation speed:

> Aircraft fuselage structure is a good example of structure that is based largely on a slow crack growth rate design. …
>
> The rate of fatigue crack propagation is determined by subjecting fatigue-cracked specimens, like the compact specimen used in fracture toughness testing, to constant-amplitude cyclic loading. The incremental increase in crack length is recorded along with the corresponding number of elapsed load cycles acquire stress intensity (K), crack length (a), and cycle count (N) data during the test. [NDT Resource Center 2013]

In such situations 'confirmation bias'- if understood in terms of some theorised understanding - cannot operate. Such a bias seeks to defend belief in some knowable order, with order understood in terms of some theory expressed in cause-effect terms. Interestingly, in this example the social construction of knowledge does not extend to the social construction of an *analytical* knowledge in the sense of a theory or model that so articulates a cause-effect logic that it can be used to make predictions. Science here argues that whilst we know what will happen, so far we know that we knowably do not know, *in terms of a logical model*, why. Some find this surprising.

But what criteria make such theories acceptable? As an example, we find, in the widely-cited Held et al 1999 the question asked – "What is globalisation and how should it be conceptualised?". Their particular criteria are:

> "… 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" [14]

This is an example of criteria for theory acceptance that relies upon confirmation bias, avoiding scepticism. The bias against scepticism has as one consequence that such theories are too readily taken as guides to action, each suggesting that their "justified account of causal logic" maps to observed reality and so to action conceived in cause-effect terms. This stance encourages recklessness.

To build this argument, I frame it in terms of the need for researchers to treat ignorance[4] and knowledge – knowability and unknowability - as a duality, as ready to report the one as the other. I attack the 'belief in belief' that underpins confirmation bias, and to do this I examine different knowledge production methods *by looking at different sets of criteria for the acceptability of accounts*.

Palliative response

This paper has some suggestions for how confirmation bias arises and how palliative responses may be emerging. Palliative here implies measures that mitigate or ease

[4] In this paper I use the term ignorance to mean unknowable; alternative meanings are that it means not knowing that which can be, and perhaps should be, known. For me, then, the basic duality of ignorance/knowledge is that between knowability/unknowability.

costs, without actually solving the underlying problem; as the saying goes, 'like a blood transfusion for Keith Richard', it helps, but it does not solve the problem.

Philosophy and a social epistemological lens

It is perhaps self-evident that there is a difference between an account of something, and what that account might be said to be about. Distinguishing between the two is foundational to discussion of how things should be explained, predicted or whatever else an account is said to do. Indeed, a basic term we use to discuss accounts, that of the metaphor, etymologically links 'model and muddle' by suggesting that a model – a metaphorical account – must 'bear' or carry something across the gap between the two.[5]

Students may confuse reality and accounts of reality, but they are taught and learn that this can be unwise. The utility of accounts of reality is evidently varied, but an important value of accounts is that they enable us to think in those terms, rather than having to experience what it is said they are about. The value of theory is thus precisely that it is not reality, but about it, although just what we mean by 'about' varies. Some people find it useful to consider that accounts can be true, others that the very notion of truth is confusing and confused. My main point here is that use of an account of reality to think about reality requires the account, which is unreal, to bear a burden of belief. Thus disbelief has to be suspended if the account is to be a

[5] Thus most dictionaries tell us that the 'phor' in metaphor has the same etymological root as the second part of the two words euphoria and dysphoria, where reality is - respectively - happily or unhappily *borne*.

believable account of reality; put this way, the core issue here is the management of movements between belief and disbelief: how we manage accounts and their believability.

How do we generate accounts of how belief and disbelief are managed, socially? One useful point of view here is that, as *variation* in how belief is managed is evident, we can think of this in terms of social variation in the acceptability of accounts. Any foray into discussions of economics, for example, will rapidly show variation in method, in what is acceptable in seminar rooms or to reviewers of papers, pleasing to funding agencies and so on. Furthermore, in that particular knowledge may have implications for action and practice, beliefs about belief map into different practices, so knowing about them helps explain what people do.

For example, Hoffman, in a practitioner's account of Prussian and later German military doctrine argues that:

> The importance of acting … results immediately from the characteristics of war. War is the domain of uncertainty, friction and, often, sheer chaos. … When the unexpected occurs, those waiting for new orders will lose. … *auftragstaktik* is one way to call forth … quick and independent action [Hoffman 1994:3]

Hoffman offers an account of a practice – combat – and here reports that this practice asserts that belief that war is predictably regular is a mistake, and should be suspended: statements about reality in this context that assert their own predictive truth would not be accepted.

To take another example, discussion of ways in which belief and disbelief are managed and constructed is often associated with analyses of science practice – specifically, those elements of normative practice that state what correct practice is.

Thus Crombie, in a study of the historical origins of science, focusses upon *method or procedure*: recognisable social practices said capable of producing knowledge within that practice:

> The history of science shows that the most striking changes are nearly always brought about by new conceptions of scientific procedure [Crombie 1953:1]

Such conceptions of procedure reveal attitudes towards belief: here that better methods produce better statements about reality that should command acceptance and belief.

Nisbet 1969, by contrast, offers an account of Western theories of development and social change. He concludes that this broad tradition has consistently over time applied certain criteria: accounts of change here (which he says have dominated Western thought) are not intended to map reality closely, but rather offer metaphorical accounts of the essential aspects of change.

> 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 [8, 9]

By contrast, Crombie's historical study shows the origins of what he calls modern Western natural science. He argues that Robert Grosseteste (c. 1168-1253) was the source of a "strategic act {that} ... created modern experimental science {by uniting} … the experiment habit of the practical arts with the rationalism of twelfth-century philosophy' [10]. This -

took the double, inductive-deductive procedure described by Aristotle" [13] and built upon it to create a methodology that contributed to "the study of the relation between theory and experience, of the use of induction and experiment in scientific investigation, of the relation of mathematical to 'physical' and metaphysical explanations, and of the problem of certainty in the study of the world through the senses [14]

In Crombie's account of Grosseteste's reflections, however, two points stand out. First, the facts Grosseteste engaged with were the result of technological developments that were both considerable and largely free of underlying explanation – they were not theorised [White 1940]. The particular focus for Grosseteste was optics – in particular what was observed when light was guided through prisms – and this needed prisms and light sources created without theory. Second, that, as a cleric he both lived and worked in an environment permeated with belief that investigations were to do with the discovery of divine truth ("For in the Divine Mind all knowledge exists from eternity" Crombie:73 quoting Grosseteste). He believed that his theories, whose deductions he thought should be tested against evidence in the new scientific method he was founding, came from Divine illumination [131]. Thus it is not so surprising that he did not actually test his theories about optics empirically, which would have suggested, according to Crombie, that they were wrong [Crombie:124].

There is a striking contrast between the different criteria reported by Nisbet and those reported by Crombie. An important difference is in the issue of possible convergence and this illuminates what we mean by predictive power.

Box 1: Nisbet's seven criteria

"From the metaphor came the {first} 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" [212, underlining added]

Nisbet shows that the long-established criteria required for acceptance of metaphorical accounts of social change do not include any requirement that they be compared (see Box 1).

By contrast, Crombie's account of natural science method argues that the requirement of predictability, based upon deduction from theory derived from induction, requires different accounts to be compared. This implies that prediction can be thought of as a criterion internal to a knowledge creation practice that requires comparison between knowledges, which, if interactions with data support theory, leads to convergence. By contrast the criteria Nisbet reports lack such a criterion, suited to a world of multiple truths.

In re-examining *auftragstaktik* we thus find an example that requires belief that relevant reality is chaotic and unpredictable; Hoffman reports a situation where experts are required to suspend belief in the predictability of war (officers on training courses would fail if they asserted otherwise). In contrasting Nisbet with Crombie we learn how social norms involved in knowledge production may or may not contain a criterion to govern convergence. We may conclude that this is what prediction is all about: to understand, say, engineering theory it is far less necessary to understand factors outside the practice than, say, economics, and whilst the former is reliant upon predictive power, the latter clearly is not [Yonay 1998; Fforde 2013].

To recapitulate, statements of suitable method going back to Grosseteste (in Crombie's account) require movement between the inductive and deductive; between theorisation and confrontation of deductions from theory with facts. Since theory is not reality, but some account of it, disbelief is suspended to give suitable meaning to theorisation ('to theorise, you have to believe in your theory'). This allows theorisation to engage with facts (not yet predictively), for belief in the possible value and validity of theory encourages theorisation, just as it encourages confirmation bias. The distinction between induction, when theory is developed to match facts, and deduction, when theory is used to deduce assertions about reality – predictions - that can then be tested empirically, is the core of this. Crombie's point is that Grosseteste added empirical testing to a far older view (and procedure) which asserted how knowledge was to be created (that is, that the right criteria to apply to assess a candidate for knowledge creation) as psychological powers (*nous*) showed the theorist the path forward.

Underpinning method, though, is *belief that there is something knowable*: that there is something present in reality that makes theorisation viable. Applying Nisbet's criteria, this is simply production of an empirically-founded metaphorical account meeting the criteria. Applying Crombie's, theory must be predictively powerful. Obviously, there is no *a priori* reason to expect theory in a particular context to be able to do either of these things: the data may not exhibit suitable regularities and/or the theorist may lack whatever it takes. Theorisation may thus fail, but in these two accounts fail for very different reasons.

In Crombie's framing, the shift between induction and deduction is a shift between suspension of disbelief and its reinstatement. The obvious risk is that, belief in theory having being encouraged during the induction phase, belief may not then be suspended as the deductive moment requires. Therefore, it is permitted to believe in the truth of theory in the inductive phase, but the normative criteria are violated if the researcher does not suspend this belief as they deduce testable predictions. This starts to look like a story about confirmation bias.

One can think of an 'inductive box' into which theory-making must be put for it to then re-emerge so that theory can drive deduction and empirical engagement, to seek and (perhaps or perhaps not) gain status as more than 'just theory'.[6] In these terms, Grosseteste, his theorising soul warmed by proximity to his God (or so he believed) did not get out of the inductive box. Confirmation bias can be seen as the tendency to stay within the inductive box, which, as Crombie's history shows, was there very

[6] Here I avoid using the term 'inductive trap' as that may suggest that I am referring to the wide range of discussions, often associated with Hume, about whether solely inductive reasoning may or may not lead to truth. It should be obvious from my approach here, which views epistemological matters as inseparable from social practices, that I mean something quite different. By 'inductive box' I mean that first stage of the quite orthodox idea of the "double, inductive-deductive procedure" where, before theory is used deductively to produce testable statements, theory is developed, in some relation to its 'other' – a reality, perhaps – which is to do with 'induction', and whilst it is being developed has to be kept isolated, and I call this being 'in a box' – the 'inductive box'.

early on. Improved method here must therefore assert the value of scepticism. It must be socially acceptable, in terms of normative procedure, to believe that the particular context is unknowable (as crack propagation speed is, in terms of theorisation of cause and effect).

The analysis offers two insights:

- First, that predictability is a criterion within some but not all knowledge practices that requires comparison of theories, and pushes for convergence;
- Second, that the acceptability of non-predictive accounts is related to the shared criteria they meet (Nisbet's), which have a very long historical foundation and *are fundamentally metaphorical, concerned to offer an account of the essential aspects of what the account is about and meet criteria that allow co-existence of multiple truths*.[7]

Only if the data will carry it, and the theory created is good enough, will accounts add predictive power; otherwise, if we follow Nisbet and Crombie we expect that accounts meet only those criteria required of metaphorical accounts, and pressure for convergence lie *outside* the knowledge practice's own method. In a tolerant environment, perhaps they will exist in a world of multiple truths; if not, perhaps the Prince will decide on the truth of the matter.

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[7] Although I do not develop the argument here, the essentialist nature of theories of social change in Nisbet's account can be taken to imply that their terms do not have determinate meanings, viewed epistemically. Does this imply that such accounts necessarily entail ontological instability, so that for their accompanying statistical empirics sampling *cannot* be said to be from a single population, so that results are necessarily spurious?

Contemporary social science

Action and predictive social theory

The validity of a theory as a guide to action, when it has *not* been tested deductively, appears often to be high.[8] Consider Dunn 2000:

> In the case of the massive impetus towards economic liberalisation of the last two decades of the twentieth century . . . it is natural to see this [as] the discovery of ever clearer and more reliable techniques for fostering economic efficiency . . .
>
> This is very much the way in which a whole generation of economists actively engaged in public service have come to view it, just as their Keynesian predecessors a generation or so earlier saw the previous move in a roughly opposite direction. Viewed epistemologically, however, the sequence looks strikingly different. . . . [I]t was the increasingly evident falsity of one set of false beliefs, not the steadily growing epistemic authority of their replacements, which did most of the work. . . . The clear result is the negative result. [184–185]

The sense of negative here is that the hoped-for predictive power of what was replaced had been found to be absent (policy 'had not worked'). Though, recall that Nisbet concluded that Western theories of social change are judged by a range of criteria that do not include predictive power (see Box 1). Their relationship to reality, in a long historical tradition, is metaphorical. Such theories of social change, when,

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[8] What is meant here by 'tested deductively' follows the sense of the Aristotelian procedure of movement between induction and deduction, modified by Grosseteste to require that deductions be confronted with empirical testing. If chosen logical rules are followed, then the deductions from a given theory are true as logical deductions, premised upon the truth of whatever they rest on – they are relatively true; what is meant here, though, is that such deductions can be also be considered as untrue and their plausibility to be assessed empirically.

for example, they say that policy X will cause Y, are necessarily (unless by chance) bad guides to action.

Now, it is evident that belief that such social theories map easily to reality, in effect predictively, is nevertheless common [Friedman 2006 reflecting on Converse 1964]. Yet whilst metaphorical accounts in Nisbet's sense have empirical foundations, these are not predictive, and should not be expected to be so, given the contrasts between the different criteria of the different methods Nisbet and Crombie report.

Consider a recent development in philosophy: 'agnotology' – the 'study of ignorance'. This easily becomes the study, not of situations where there is suspension of belief in possible theory - a choice to believe in ignorance as unknowability - but rather where ignorance is seen when true knowledge is possible but obscured or prevented by the presence of an untrue set of beliefs.[9] Confirmation bias is present here, as the focus entails belief in knowability, and the issue in agnotology is to examine how knowability is stymied. Scepticism is devalued.

Weiss 2012 thus builds upon others' work to examine agnotology mainly as "how real-world facts can be manipulated or ignorance actually generated when information is distorted by obscured by special interests, as exemplified by tobacco companies' fight to prevent the evil leaf from being controlled …" [96]. He goes beyond this to pose what he calls "the agnotological question: How can we convey what we do not know?" [96].

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[9] Ignorance then means not knowing something that is knowable, which is not the general meaning I use in this paper generally.

Consider also the now common tendency to stress that truth is a social construct, ‘useful’ and seen as supportive of certain (knowable) social situations. In such approaches policy, for example, is viewed mainly as a way for social organisations to cohere, rather than as a means to attain predictively known ends [Shore and Wright 1997]. Thus Sullivan 2012, after a discussion of what she calls ‘Truth’ junkies in UK public policy, quotes Foucault as arguing that “we cannot exercise power except through the production of truth” [510]. She concludes herself that “the manifestation of evaluation as regulation … is an apt reflection of the way in which truth is constituted and described” [510].

If we look at Weiss and Sullivan, just what acceptability or truth criteria apply to these statements, and are they the same? Clearly not – if we ask the question ‘are these statements themselves true (or ‘apt’, to use Sullivan’s synonym, or ‘genuine’, to use Weiss’) there is no clear answer other than that the two authors clearly imply a range of answers. Both remain within their inductive boxes, asserting the validity of what they write. Their work is not without empirical foundation, but their methods do not follow Crombie’s procedure or method. It is striking how they follow those listed by Nisbet (see Box 1 above). As such, there is no way within the method to compare them; we have multiple truths.

The risk is clear: accounts following Nisbet’s criteria are metaphorical accounts of reality, containing references to facts, to causes and to effects. Arguing that they are stuck within inductive boxes says that they are insufficiently sceptical, and too ready to suggest that their accounts, their “justified account of causal logic” [Held et al 14], map predictively to reality. This I believe is profoundly unwise, as did Prussian and German generals when they required their subordinate officers to adopt a sceptical position – to act as though their context was chaotic. Policy as just metaphor is advisedly not used as a guide to action where cause is predictably meant to be linked to effect.

In the next section I use the case of econometric growth analysis to offer an account of what may happen as researchers chose to ignore scepticism, and remain in their inductive boxes when research suggests strongly that their data does not support their beliefs - in this example, in published academic work.[10] They continued to suspend disbelief, preserving belief in their theories.

Relevant here are two canonical discussions from inside the discipline – Solow 1956 and a Symposium organized by the World Bank [World Bank Economic Review 2001]. Solow argued that whilst –

> All theory depends on assumptions which are not quite true. That is what makes it theory. The art of successful theorizing is to make the inevitable simplifying assumptions in such a way that the final results are not very sensitive [65]

The paper has no empirical referents linked to the algebra deployed. His reference to “final results” is thus a remark about the theory, not its relationship to reality: it is a remark about the effects of variation in the algebraic formulation of the issue upon the results of the model in terms of the model alone: his article, seminal in the field, reports on theorizing.

[10] It would be easy but is not really necessary to present historical examples of application of allegedly predictively known economic relationships to policy, with poor results; recent ones would be the Asian Financial Crisis, the Global Financial Crisis and austerity policies.

Two generations later, at the World Bank Symposium, organized by an institution committed to the idea that it had expertise in knowing how to change rates of economic growth, Solow stated:

> I have been skeptical from the beginning about the interpretation of cross-country growth regressions [283]

Other contributors to the collection argued in terms of knowable but incomplete knowledge. After decades of research, no predictive knowledge had been found, yet theory, despite the scepticism of a key creator of it, remained valued as a basis for action - for policy advice. And, as I discuss in the next section, what is striking is the methodological inability to accept ignorance in the sense of their being nothing to know: unknowability.

The cross-country growth regressions literature: research as guide to action?

This paper arose originally from an interest in understanding belief in the knowability of change processes, in terms of cause-effect relations and the predictive viability of policy advice (that X will lead to Y), especially in the field of international development.[11] Econometric cross-country growth studies therefore purport to look

[11] This field is a very useful case study, suggesting that confirmation bias is deeply-rooted in mainstream governance practices [Fforde 2015] that assume a relative power over other poorer regions that is arguably absent [Seidel and Fforde 2015]. It can be argued that economists are particularly at risk, as their theoretical priors are particularly strong, perhaps compared with other disciplines such as anthropologists and sociologists. See Fourcade 2015 for a discussion. In the terms used in this paper

for reliable evidence for the causes of differing economic growth performance. However, examination will easily show a plethora of published but contradictory results: there is no apparent convergence, and this is a stable characteristic of the literature.

In this literature, lack of convergence in turn generated empirical investigation. Levine & Zervos 1993 applied techniques called formal robustness tests to a dataset typical of those used in the cross-country growth studies. They concluded that, based on their method, there existed extremely few – almost no - robust relations between economic growth and policy proxy variables. This challenged common beliefs both amongst consumers of policy advice and economic researchers that economics generates knowably reliable results in terms of known causes and effects. Fforde 2005 examined citations of Levine & Zervos and found the majority of those citing the study in various ways ignored this challenge: specifically, most authors did not move from belief that what causes growth is knowable and known with perhaps some uncertainty (whatever that particular belief may actually be in a particular place and time).

However, some authors did take on board the possibility that the data could be interpreted as advising that the situation was unknowable in its own terms. Kenny and Williams 2001 argued that it told us that the research was assuming ontological universalism – sampling from a single population – so that the evidence was

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and in Fforde 2016 forthcoming, economists are more inclined to be trapped in an ‘inductive box’, failing to return to disbelief in theory and adopt a skepticism as deductions from it are confronted with data.

suggesting that cause-effect relations between policies and economic performance were unknowable *in the terms of the dataset*. GDP here and GDP there were essentially different, but theory assumed they were not. This supported (though amongst only a minority) a sceptical position, mitigating the general tendency to confirmation bias. Thus Wood (1994):

> The value of this message is clear… None of us…will ever rely so casually and so heavily upon cross-country regressions. … I have a basic reservation about the methodology used … [67-8, quoted in Fforde 2005:74]

But the stance of the majority appears, in the terms I use above, as an example of a maintained suspension of disbelief – an inability to move, in the "double, inductive-deductive procedure" from the inductive box, where models are driven by a suspension of disbelief (belief that what is unreal is true), to a deductive stage when models are used to generate predictions and assessed empirically. The suspension of disbelief, here, becomes an empirically unwarranted belief in the soundness of the model as a *predictably* valid theory. Here the method used can be seen as a failure to follow a standard scientific methodology with its "double, inductive-deductive procedure". Clearly, the literature, published in high-status journals, follows rules for what is accepted as valid theories of social change (with this understood as economic growth), and also clearly the results are meant to be used to say that X will lead to Y. So what methodology are they following? What are the truth criteria being applied? It seems clear that Nisbet rather than Crombie offers a better guide.

Economic theories about change are algebraic in form and contain variables that refer to time – 't'. This is readily interpreted as meaning that the theory is potentially predictive. Equally clearly, however, the criteria applied are not those that seek predictive power. Economists engage with data in fairly rigorous ways, but their theories do not generate predictive power and it seems are not intended to, as their method does not require it. To quote researchers looking at what economists do when they model [Yonay and Breslau 2006]:

> What is distinctive about model-building in economics is the process that mediates between the microworld [the economic models] and the ostensible object of the research. Rather than involving scientific instruments or data-gathering procedures, this mediation is accomplished by vaguely defined but generally accepted conventions regarding the movement from reality to models. . . .
>
> There is no pretence that the model actually resembles reality. Rather, the concern with realism is a concern with the plausibility of the mediation between the reality and the model. [33–34]

The majority of economists stress the importance of a model's being based upon rigorous statements of the nature of, and the modelling implied by, their understanding of and belief in rational behaviour. From a Crombian perspective, there is nothing remarkable here - this is simply the chosen theoretical framework. A model, however motivated, that does not appear founded on such statements is therefore not highly valued; and the discipline - again unremarkably - polices its statements about reality by reference to such boundaries.[12]

Econometrics has a capacity to deal with vast volumes of data and create a sense, within the inductive box, of what patterns can be found. The basic theory assessed by

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[12] I am generalising, yet Yonay 1998 offers an intriguing historical account of what happened within the discipline in the 1930s when data became available to confront theory. He argues that 'those who won' tended to continue to believe that data was not very important to their belief in theory. Economics textbooks often assert the value of theory to students without much engagement with data [Fforde 2013].

econometrics remains a conceptual model of ‘equations plus error term’, with what I see as the underlying assumption, in some practices required, that the model be assumed true for the estimation to be legitimate (see the quote from Cohen above). This is highly evocative of the essentialism Nisbet reports as a long-established characteristic of Western theories of social change, if not Plato’s metaphor of the shadows on the walls of the cave.

Econometrics can thus be viewed as set up to estimate the parameters of an economic model *assumed true*; that is, *if* changes in X lead to changes in Y, as the model states, econometrics statistically estimates how much. Now, if the model is assumed true, it is clear that there will be tangles if statistical estimates of parameters are not significantly different from zero. Again, all this fits a view of theorisation within the ‘inductive box’. To quote McCloskey:

> The question of whether prices are closely connected internationally, then, is important. The official rhetoric does not leave much doubt as to what is required to answer it: collect facts on prices … and test the hypothesis. A large number of economists have done this. Half of them conclude that purchasing power parity works; the other half conclude that it fails. The conclusions diverge not because economics is arbitrary but because the disputants have not considered their statistical rhetoric [1985: 109—111].

She argues that many economic arguments confuse judgments about economic significance with judgments about statistical significance. As she 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. [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*. [118]

These arguments are suggestive. Centrally, as comparison between Nisbettian and Crombian criteria showed, the method of the former, unlike the latter, gives no way to decide between competing theories. This has to be dealt with outside the method. It is then made easier to report the finding of patterns, for different patterns may legitimately be found in the same data: a world of multiple truths. And this is just what we find in the cross-country growth econometrics.

We can add to this. For example, it is a knowable result in econometrics that the results of estimations can be *spurious*, in the sense that, for reasons to do with the nature of assumptions held to be true to generate them, they are best treated as neither true nor false, but empty of meaning. Granger 1990 and Granger and Newbold 1974 report positive correlations between variables derived from sets of random numbers. Patterns can be found using these methods where none exist.

I conclude that researchers may be thought of as treating belief *asymmetrically*: rather than choice between ignorance and knowledge – knowability and unknowability - being seen (and method requiring it be seen) as *necessary*, the socially normative focus (the method used) is upon the acquisition of knowledge, albeit perhaps with a degree of uncertainty associated with it. In the inductive box, whilst theorising, belief is needed; for Nisbet, viewed from a Crombie perspective, this is the end of it, for there is no criterion requiring any more to be done: in a nutshell, method entails confirmation bias. This would seem to be the case for any knowledge production process that lacks a criterion equivalent to prediction that requires comparison between accounts, and which, following Nisbet, is metaphorical.

What the growth literature case study shows us is therefore, first, that most practitioners do not question these assumptions, though they could. The issue is one of method; method can be changed, but it is not. I now develop an argument based upon existing statistical methodologies that offers a conceptual test for whether it is wise to believe that there is a knowable order, and so to step into the inductive box. This is a method for judging whether the data supports this or not. This offers an operationalization of the idea that ignorance and knowledge are part of a duality, so, as choice between them is inevitable, it is a good idea to work out how to manage it. It seems, though, that the cultural and one could say metaphysical predilections are towards belief in knowability, in the presence of a knowable order, so that such suggestions are not likely to be more than palliative. Whether this particular method is a good one or not, though, the point stands: in terms of procedure, it is a good idea to better manage pressure for confirmation bias by supporting in principle judgements of ignorance – of unknowability in particular contexts. There should be a right to scepticism.

How to avoid the inductive box?

Most statistical work, including that used in the growth economics literature, develops methods familiar from statistical hypothesis testing. Based upon a set of assumptions, an estimated value of a variable has an associated estimated probability distribution.

Thus, within certain limits and based upon certain assumptions, the researcher can make a judgement about the relationship between the estimated value of the variable and the population value that would be measured if the entire population were surveyed. These certain limits and assumptions include some metric that helps the researcher reach a conclusion and this is probably the familiar confidence level (5%, 10%). More generally, such procedures contain a 'hurdle', which is in integral part of the method by which the researcher draws their conclusions.

The quote from Cohen 1984 given above suggests that a researcher must predicate the truth of any conclusion upon the *assumption* that certain things are true. There must be confidence of enough regularity in the data to justify theorisation. More importantly for the argument here, researchers in practice assume that the confidence level is a given, conventionally at the 5% or 10% level.

Now, one can ask, for the given statistical procedure, an inverse question, which is at which confidence level should a researcher conclude that because there is not enough regularity in the data (robust relations between cause and effect variables) they should not try to theorise: they should be able to assert that any reality 'behind the data' is too unknowable. This question is not usually asked, is not part of standard procedure, and the choice is not guided empirically. Such choice can, however, be guided by the following considerations.

In the limited bounds robustness-testing techniques, such that used by Levine & Zervos, and in the application of Bayesian techniques [Fforde & Parker 2012], researchers' practices can be seen in different ways deploying a hurdle to permit them to gauge whether they should conclude that a relationship exists. The placing of this hurdle is not a property either of the data or of the model proposed. So we can ask the inverse question: for each practice, and dataset, at which hurdle height should a researcher report no known relationship to exist.

Researchers conventionally do not ask this question, which appears in part to be because of the assumption that knowledge – knowability - is, not part of a dual containing ignorance – unknowability - but a truth that researchers assume they may know, and whose parameters are to be estimated empirically, albeit with a degree of uncertainty. In other words it is assumed that theorisation is always worthwhile; by entering the inductive box (theorising) it has already been assumed, in this social practice, that there is a pattern to be found. If there is evidence (as we found in Levine & Zervos) that this is not the case, this is easily rejected and the choice made to maintain suspension of disbelief and assertion that the model works.

In the robustness testing techniques applied by Levine & Zervos, the hurdle is a *given* confidence limit used to gauge whether relationships between exogenous variables and others are as desired.

If we change the hurdle value to make it harder to find results, fewer results are found. Obviously we can then find the value at which there are (just) no robust relations. This then a valuable characteristic of the dataset, and we call this the Paine Index. The level of the Paine Index can be used to tell researchers whether there is adequate regularity in the dataset to justify theorisation – to getting into the inductive box.

What Hoffman is saying about *auftragstaktik* is equivalently that, based upon what they knew about war, the Paine Index or some equivalent was found to be at a level that implied that “War is the domain of uncertainty, friction and, often, sheer chaos.”

By contrast, datasets about iron smelting or crack propagation speed (see above) would generate values justifying theorisation as these were, on the contrary, domains of order and relative certainty.

Confirmation bias and belief in revelatory knowledge

Gillespie 2008 (quoted above) is like Nisbet a study of the history of ideas fundamental to Western culture, stressing the destructive effects upon various certainties of philosophical debates of the fourteenth century and the importance of these debates when considering modern ideas of progress and the advance of knowledge. Like Nisbet Gillespie sees cultural patterns. In the quote above Gillespie is focusing upon Western populations that have largely abandoned organized religious practice. In these particular religious practices, divinity is associated with the *absence* of contingency: with situations where meaning can be determinate, 'God-given' and revelatory – that is, that truth as Word can be knowable by us as humans, given divine intervention and/or presence (Grosseteste's 'Divine Illumination').

For contemporary social scientists such as economists studying data on variations in economic growth across countries and regions, belief in the validity of regression results based upon assumptions of ontological universalism plausibly reflects this legacy [Kenny & Williams 2001]. The evidence for the absence of regularities that we find in Levine & Zervos was probably obvious to statisticians once the datasets were available (through the 1970s, if not earlier), yet the analysis of citations of that article in Fforde 2005 shows a majority holding to their beliefs. These beliefs entail implicit choices about ontological stability that include beliefs that terms such as GDP, export-oriented growth policies and inflation have determinate meanings. As we have also seen, these beliefs flew in the face of evidence [Levine & Zervos 1993], in Crombian terms, and fit far more easily into Nisbet's criteria (Box 1). And, as we have also seen, at the historical birth of modern empirical scientific method, according to Crombie, Grosseteste, despite endorsing the method, did not actually test his theories about optics and, had he done so, he would have likely discarded them [Crombie:124]. For Grosseteste, suspension of disbelief during his theoretical ponderings was associated with his belief in Divine Illumination.

Gillespie's remarks offer a path into a conceptual discussion of the duality of ignorance and knowledge – knowability and unknowability - that is I think too often implicit in empirical practice. It offers a way to understand (and so better to deal with) how what I am calling the suspension of disbelief is powered and driven by belief in belief, which in my terms makes it too easy for practitioners to avoid the "double, inductive-deductive procedure" and remain in the inductive box. As is familiar from many areas, including international development practice [Fforde 2009, 2013], people appear very often to believe that what they happen to believe is true, and are happy with this despite evidence pointing the other way. They like confirmation bias despite the risks. This seems to be in part because the norms of their method give them reasons to believe in their theories and continue to believe in them as true, thus continuing to suspend disbelief in them (for they are but theories).

Yet a minority of the citations of Levine & Zervos reported in Fforde 2005 can be understood as shifting towards more sceptical ways of doing research, and so it appears that there is some hope that the effects of confirmation bias can be mitigated, even if its origins are so culturally-deep that a cure is far away.

Conclusions

My discussion of the nature of the conceptual underpinnings of empirical work suggests that framing judgements in terms of a duality of ignorance and knowledge informs us about how we may better manage belief. The core issue is how, when we fear we may lack predictive knowledge, we manage method. The Paine Index – or some equivalent - offers a tool to include in method to assess whether it is worthwhile, for a give dataset, to assume knowledge or ignorance, and so to avoid theorising. More generally, it shows how method can be changed to include a judgement that there is not enough regularity to justify suspending disbelief in theory and entering the inductive box.

We do not escape from our choices by privileging – by accepting confirmation bias - the acquisition of knowledge compared with the acceptance of ignorance. This is, I have argued, to choose to put ourselves in an inductive box and preserve a suspension of disbelief that is meant to be only temporary. It seems better to improve ways of avoiding the inductive box. With much policy guided by claims to predictive power, and where choices to believe are usually implicit, we risk much when our method of knowledge production include choices about what to believe that we are too often unaware of. Without realising this, in many areas of applied research it is too hard to assert ignorance, and behave in whatever ways suit that, and too easy to assert knowledge; this is risky and can be reckless and so unethical – and those seen as unethical risk loosing prestige and authority [1990, 1997].

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