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Towards a theory of ignorance

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Abstract: The paper develops an argument for the criteria that a theory of ignorance should meet. It starts from the distinction between instrumental and non-instrumental action. Usually, the latter is considered irrational and the former rational as being based upon known cause-effect relations whilst the latter is not. I argue that the former requires a reasoned basis in predictive knowledge of cause and effect, without which good council is either for inaction or non-instrumental action. The argument proceeds by exploiting mainstream statistical methods to explore an example of a 'metric of advised ignorance' to guide explicit reasoned choice allowing rejection of instrumental action in favour of inaction or non-instrumental action. The argument then explores a case study of how such rejection is disallowed by official requirements in International Development Assistance (aid) that contexts must always be believed predictive and so action organised as instrumental. This shows the basic irrationality of mainstream policy rationality. The paper then discusses wider social epistemological issues of this irrationality and concludes with a list of criteria a theory of ignorance should meet.

Keywords: Agnotology, policy advice, predictive ignorance, methodology, scientific method, non-instrumental action

Introduction – the hidden crisis of instrumental rationality

The collapse of instrumental rationality as a reasonable basis for state action

> 'If an anomaly is to evoke crisis, it must usually be more than just an anomaly.' (Kuhn 1970, p. 82)

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> 'An engineer is a man who can do for ten shillings what any fool can do for a pound.' (Shute 1954, p. 66)

The basic rationality of contemporary state action, within the rich club of OECD (Organisation of Economic Cooperation and Development) nations, as presented normatively, generally asserts its rationality in creating and maintaining a social order through its ability to generate and deploy policy, to secure stated ends through mechanisms linked to that policy. It is, and has long been, asserted to be instrumental in its logic. By this is meant, that policy acts *X* are instruments that create change leading to outcomes *Y*, and that the relationships between *X* and *Y* are reliably known before and afterwards. They are therefore, clearly, said to be based upon predictive knowledge that *X* will lead to *Y*. This basic concept includes the issues of reliability of measurement, and reliability of prediction, and protects the notion of instrumental policy rationality by saying that whilst knowledge is essentially predictive, it may be uncertain. This of course evokes Plato's famous vision of the relationship between forms and the shadows cast on a wall by a fire.

This is the case across a wide range of situations, whether the social market economy ideas articulated by Ludwig Erhard, Keynesian notions or the free-market liberalism of Milton Freedman. Yet, within Marxist analyses, interpretations of the nature of commodity production argue that, due to the operation of processes such as commodity fetishism, actual concrete social relations become abstract, and so the underlying conditions of capitalist reproduction are hidden: Marx's theory of value is, therefore, not a theory of observable price (Fforde 2011). It follows from this interpretation of Marx that the appearance of mainstream policy rationality is nothing of the sort, and its rationality is, in its proclaimed self-identity, fake. And that, therefore, political orders need to be analysed in ways that take account of this.

Current tensions in world politics, perhaps marking the highly-contested end of a period of neo-liberal fluency, suggest that there is a declining authority of, that is belief in, the basic rationality of 'rich country' state action, manifest in many areas. This decline can be seen in the increasing evidence that important beliefs are not empirically founded. Full discussion of this issue would require, as it is part of puzzles going back to at least Plato, space well beyond a single academic article, and this is not my goal here. It seems self-evident, for example, that macroeconomics is not a predictive science, capable of predicting economic growth based upon contests between predictive deductions from competing theories. At the end of the day, the empirics of macroeconomics are to do with the ways in which different theories do or do not fit past data, which is essentially different. Not least, this encounters

the problem of confirmation bias (Fforde 2016). The lack of predictive power is compounded by the presence of powerful beliefs that certain things are 'known', when there is persuasive evidence that they are not. Such beliefs often entail the assumption that the causal logic of a theory, perhaps supported by empirics, but not predictively tested, maps powerfully enough to what is observed for it to be treated as possessing predictive power, and so a reasonable basis for instrumental action. Such beliefs entail formidable overreach, and much effort can be found in natural sciences to avoid basing action upon theory that has not been tested predictively.

Two examples are, first, that the causes of variations in economic growth, globally, are known, when powerful research shows that there are almost no robust relations between such variations and possible candidates for causes in that variation (Levine and Zervos 1993; Fforde 2005), and second the widespread assertion of the necessity of industrialisation for poor countries to grow, when the data shows that this is both what is asserted, and that it is contrary to the facts (Fforde 2018). Other pointers are the evidence from investigations of method that much social science, including economics, does not require (unlike standard accounts of natural science) that theories be tested predictively. This can then be used to investigate international development practice, where important norms require belief in predictive power (Fforde 2010, 2017). My most basic point is that policy as instrumental action requires, conceptually, predictive reliability, and we have good reasons, both empirically and in terms of how knowledge is constructed, for thinking that there is little reason to believe that this is present. This is arguably what has been learnt from the massive investment in 'policy science' since the mid-20th century (Fforde 2010, 2017).

In this paper I pose questions about how, given the empirical tensions created by the assumption of rational instrumental policy logic, other, non-instrumental, logics may appear, so that state actions be experienced as linked to positive experiences. More broadly, in that political authority normatively should rest on some persuasive rationality, the decline in the power of assertions of instrumental rationality to generate belief implies a need, for progressive forces, to search for other, non-instrumental, acts to secure change. Since such acts definitionally lack a basis in predictive knowledge, this require a better understanding of ignorance, in other words, *theories of ignorance*.

Instrumental rationality

The dominant conception of collective action is certainly instrumental: here, based upon known cause-effect relations, acts 'X' are defined as the deployment of instruments, and these should and will then lead to the desired goals. Cutting interest rates leads to increased demand for loans; deregulation leads to greater and better use of markets which lead to increased economic efficiency; compulsory vaccination leads to lower incidence of epidemics, and so on. In fact, there is very little attention in the relevant literatures to non-instrumental action, which is usually treated as irrational, rather than a reasonable response to a situation where predictive knowledge seems absent but important purposes remain valid. [1] Inaction is of course also a reasonable response. [2]

This conception of action underpins mainstream assertions about how state actions are to be conceived. We see it in economics, and, as it is basic to policy logic, in other areas of social science. But much contemporary popular mistrust of 'policy and policy eggheads' stems from a lack of belief that such predictive knowledge is actually present, although experts may assert that it is. This mistrust has been compounded by the arguments of post-modernists and others that knowledge production is best seen as a social process rather than a search for reliable predictive power. This scepticism is however, as I discuss below, supported by some important statistical exercises that suggest that important policy areas such as the causes of variations in economic growth are not, in fact, robustly known predictively. Within policy studies, though, there is a strong sceptical strand arguing that policy work is *not* about applying predictive knowledge to social problems. [3] Yet, the OECD, through its Development Advisory Committee, requires its members to organise aid interventions as though there were predictive knowledge, and also to evaluate them as though it was easy to establish without doubt what had led to what. Some argue by contrast that repeated evaluations of the same intervention lead to a range of accounts, without convergence (Fforde 2017, p. 46 citing Tilley 2000, p. 4).

Again, I state that the point of this article is not to rehearse the great range of arguments that can be referred to here, though I have already mentioned the issues of empirics and the issues of method – that predictive criteria are lacking from the various criteria used to assess theories (Fforde 2017). For me, is it self-evident that it is quite unwise to assume that social science statements about reality, including those by economists, are predictive; they might be, but we have good reasons for being sceptical. Part of this is the simple point that it is usually very hard to argue that we are predictively ignorant, and so we should not use instrumental logic, and

explore alternatives. This is supported by the fact that use of non-instrumental logics, the logical destination of this line of thinking, is almost entirely un-studied. Theories of ignorance that would support such study (and practice) are hard to find.

Underlying all these tensions, therefore, is the issue that the question – are we predictively ignorant - is rarely posed. A clear answer requires some theory of ignorance. Rather, the presence of predictive knowledge is loudly asserted. However, if there are conditions of ignorance, logically we must be sceptical: avoiding instrumental action and choosing either inaction or non-instrumental action. [4] But what we find in the mainstream is a contradictory presence of empirics that suggests that predictive knowledge as a basis for instrumental action is 'in fact' lacking (combined with knowledge production rules that do not require predictive testing of knowledges), with assertions that such knowledges are present and necessary, as they must be if action is to be instrumental. Essentially, what is lacking are theories of ignorance, that will explain and help identify situations where predictive knowledge is deemed lacking. Logically, it must be the case that predictive knowledge sometimes be lacking (perhaps because it is too expensive to acquire), and such situations then must be thought through.

An illustrative example may be useful here.

Aeroplanes are allowed to fly, and to be insured, because the knowledge associated with their construction and testing is accepted as reliably predictive. One aspect of this is the speed with which cracks propagate in metal that is repeatedly stressed, for this in the past led to accidents and deaths. So far is known, this predictive power is entirely based upon trial and error, highly systematic, and there is no analytic theory that is accepted as working (Fforde 2017 Chapter 5 and NDT Resource Center 2013). The basis for the social context that allows aeroplanes to fly, in terms of knowledge, is entirely based upon an accepted level of predictive power, and in this area of human activity this is unexceptional. By contrast, any student of development will know about policies that turned out to have unexpected outcomes, often damaging, sometimes beneficial, but justified theoretically.

Whilst general literature searches (such as via Google Scholar) show a lack of interest in non- action as a reasonable basis for purposeful acts, they also show a bias against the study of ignorance. This is I think commonplace. By way of illustration, therefore, it is not surprising that we find recent attempts to create a field of knowledge – agnotology [5] – specifically to study ignorance, and these are

revealing, in that they show, not an interest in developing a theory of ignorance per se, but in the study of the construction of ignorance as false knowledge. [6]

Agnotology

Ignorance as outcome or conscious choice?

Whilst my focus is upon situations where it can reasonably be asserted that predictive knowledge is *contextually lacking* (that is, absent in a particular context), the new field of agnotology by contrast seeks to explain the creation of ignorance, such as the debates over links between tobacco and cancer, or hydrocarbon emissions and climate change. The difference is, I think, important. One can reasonably choose to be ignorant, rather than it being assumed that one has been somehow manipulated into being so. Therefore, a theory of ignorance may be used to explore, not how ignorance is created and maintained, *but how in a given context ignorance is chosen – 'here' we assert that we decide that, avoiding instrumental action, we lack predictive knowledge.* In the agnotology literature, preserving the tendency to assert the possibility of genuine knowledge, the focus is upon the former.

Consider:

> Epistemology has largely confined itself to analyzing *knowledge* and what is necessary for knowledge, such as *epistemic justification*. It has paid relatively little attention to what one might think of as the opposite of knowledge, namely *ignorance*. (Peels 2017, p. 2)

> ... epistemology studies the operations of knowledge *with the goal of eliminating ignorance* ... The epistemology of ignorance is an examination of the complex phenomena of ignorance, which has as its aim identifying different forms of ignorance, examining how they are produced and sustained, and what role they play in knowledge practice (Sullivan and Tuana 2007, p. 1 – stress added)

And:

> ... agnotology, or the study of ignorance, contributes to a better understanding of commercially driven research and its societal impact, showing the ways in which industrial interests have reshaped the epistemic aims of traditional scientific practices, *turning them into mechanisms of ignorance production* (Pinto 2017, stress added)

It is striking that a sub-field that defines itself largely as the study of ignorance appears to have limited its focus to the study of false knowledge, as these quotes suggest, for this implies a belief that there is accurate knowledge there to be had, but access to this has been prevented. This maintains an optimistic rather than a sceptical position. It thereby shifts attention away from the possibilities of non-instrumental action, thus preserving the position of instrumental action, and so avoids the question as to whether we can establish criteria for judging whether or not we are contextually predictively ignorant – for which some theory of ignorance is needed. If we accept that we can reasonably decide either not to act, or to act non-instrumentally, then a theory of ignorance should have goals other than 'eliminating ignorance' (Sullivan and Tuana 2007, p. 1).

This is shown both by the quote from Pinto above and by how Weiss (2012) 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 ... (Weiss 2012, p. 96).

He goes beyond this to pose what he calls 'the agnotological question: How can we convey what we do not know?' (Weiss 2012, p. 96).

Since it is logically obvious that mainstream empirical justification of instrumental action must also include the possibility of advising against it, then research using those same techniques can and should be able to establish for a given context whether the conclusion should be to advise predictive ignorance. Thus, mainstream techniques, typically not sceptical, can in fact be adapted (next section) to be able reasonably to council either not acting instrumentally, or acting non-instrumentally, which is not the same as asserting a knowledge of 'what we do not know'.

A striking aspect of this literature is therefore that it is not generally *sceptical.* Rather, as my quotes suggest, it tends to treat ignorance as *incorrect* knowledge, created in some way, and so it preserves the optimism that proper knowledge can be found.

Establishing conditions of ignorance and relevant procedures for abandoning instrumental rationality in a given context

The paper proceeds as follows.

First, to make the point that mainstream statistical techniques allow for conclusions of, in a given context, predictive ignorance, I overview a statistical technique which allows for coherent justification of an assumption of predictive ignorance. This simply shows how, whilst empirical work is biased towards the apparent construction of predictive knowledge, it is easy to develop a technique that can support reasonable conclusions of ignorance. This shows how a theory of ignorance may be empirically founded. I do not assert that this is the only possible method, and there are certainly many others.

It is based upon another technique devised by economists who were concerned with the results of econometric analysis. In the analysis of economic growth, these results tended to consist of a series of empirical studies each of which argued for a cause or causes of variations, but these studies generally conflicted, without converging (Fforde 2005). The creators of the technique were therefore concerned to see whether results were robust, or dependent upon pattern-finding in a context – their data – that in fact lacked adequate regularity. Given my remarks about the tendencies within the agnotology literature to argue that ignorance is the result of the actions of others, this clearly shows how an empirical technique can ground ignorance in choice.

The main choice made in their technique is not that between different models that seek to explain variations in economic growth, but between variables deemed causal and those deemed to be effects. The technique I have built upon this then treats as variables the familiar confidence limits used in statistical analysis, which are conventionally treated, not as variables but as set - at 5% or 10%. This technique can be used generally to guide and elucidate choice between instrumental action, inaction and non-instrumental action.

This shows that there are procedures that offer reasonable grounds for asserting that a context is suited to inaction or non-instrumental action. It is not that it is impossible to do so, but that generally, with bias towards instrumental action, the choice is avoided. Again, this offers an empirical basis for a theory of ignorance that entails choice (to decide that one is ignorant in a context).

Second, I discuss an illustration of what may happen when government or governments conceive of policy work in predictively known instrumental terms: that it is known what will lead to what, and why. I use the norms required by the Development Advisory Committee (DAC) of the Organisation for Economic Cooperative and Development (OECD) to show this. The DAC is the vehicle through which the members of the OECD define norms for how International Development Assistance (aid) should be conceptually organised and evaluated. As such, given that the OECD contains most *rich* countries, it expresses the views, which have normative power, of developed countries on how change should happen – in other words, how relevant intervention agencies should act (Fforde 2017). OECD countries' total spending on aid is large and subject to the general audit and other oversight systems of their governments. It is therefore a useful and rather representative case study.

Since there is considerable evidence that development processes are not wisely judged as in general predictively knowable (Levine and Zervos 1993), any such forced assumptions must often be unwise. I show that the norms of the DAC force precisely such beliefs.

The question then arises as how we might understand this situation, to provide a contingent foundation for a theory of ignorance. I therefore, in the following section, offer possible explanations for the apparent reluctance of important actors and social institutions to accept the importance of a theory of ignorance in the terms I am using here: a theoretical basis for the right to choose, reasonably, that a context be deemed unpredictable and so instrumental action ill-advised. The section concludes that there are persuasive and accessible explanations for prejudices, such as those of the DAC, that require instrumental action.

The penultimate section then discusses what this all implies for a theory of ignorance and its implications for non-instrumental action. The paper then concludes and summarises the implications for some of the criteria a theory of ignorance should meet.

Mainstream empirical techniques as a basis for asserting ignorance

This paper now argues that there are statistical tools ready to hand that can be used to judge whether a given context (here accessed through a numerical dataset) is

best treated as regular enough to support instrumental action. This offers a metric of advised ignorance as a basis for reasoned judgements. It platforms on statistical work that shows, contrary to mainstream belief, that the instrumental causes of variations in economic growth, globally, are not known.

The example here considers how confidence levels (or their equivalent) that are treated as fixed in standard statistical work (here, robustness-testing algorithms), can instead be treated as variables. This then offers a way for researchers and practitioners to gauge, for given datasets and such statistical practices, the point at which it is reasonable – that is, a judgement that is rigorously and empirically based - to advise predictive ignorance and so non-instrumental action or inaction. Other ways surely exist (I present but one) to gauge whether a given context exhibits too little robust regularity, so the point made here through this example is a general one: we can deploy reasonable and empirically based arguments for and against instrumental action in a given context. If the judgement is against instrumental action because the context is judged too irregular and so unpredictable, some theory may then be adopted to allow the situation to be understood and be a platform for action. Advisedly, though, that theory should not be treated as a predictive guide to action, no matter its plausibility and causative logic(s) (Fforde 2011, 2013).

To advise on the regularity of a given context, both absolutely and relatively, we need to construct a statistic, calculated through a repeatable method to a given set of data that describes the context. The fact that the method is repeatable means that judgements based on it are reasonable.

This can be done by adapting the confidence level concept so that it is not taken as a given, but varied to construct a metric, which is the level of the confidence level at which the given algorithm shows *no* regularity. In statistical analysis the confidence level is conventionally set at 10% (*easier*) or 5% (*harder*), so that if we end up with a confidence level setting that is *very easy* then the dataset clearly exhibits very little regularity and the advice should be *not* to use instrumental rationality to engage with that context.

Leamer's Extreme Bounds Analysis (EBA) is a tractable and specific example of how to construct a metric of advised ignorance using confidence intervals as suggested above (Leamer and Leonard 1983; Leamer 1985). It was used by Levine & Zervos (1993) to report, *using conventional confidence limits*, that there were, for their dataset, *almost no robust relations between policies and outcomes, the latter being economic performance*. This result, applied to a global dataset, of itself offers

a strong caution against assuming predictability, that is, that policy X will reliably lead to outcome Y, the basis for advocacy of instrumental action.

This method entails minimal theory – no more than an assertion that the data contains a division within it between exogenous and dependent variables; that is between variables thought to be causes, and others thought to be effects. When we used the EBA with *different* settings of the confidence level, seeking a statistic to measure conditions of ignorance, we found that we could calculate a metric of advised ignorance for the dataset used in Levine & Zervos (1993). This meant finding the confidence level beyond which there were no robust relationships reported, and this was our Statistic.

We also applied another statistical technique to the same database, Bayesian model averaging, with similar conclusions Again, one could find the boundary at which the technique found no regularity in the dataset.

In looking for evidence to support a conclusion that causal variables are not to be judged linked to outcomes, both techniques offered a way to establish a metric of advised ignorance. This shows how to construct an empirical basis for a theory of ignorance.

Both techniques give us the deserved statistic, each a *metric of ignorance*, and this gives power to such judgements. For example, the following graph (Figure 1) shows that as we increase the confidence interval (*alpha*), for a dataset, [7] using the EBA we find a greater number of robust relationships. Intuitively, the lower the hurdle the less sceptical we define ourselves to be, and the more willing to propose, in general, instrumental rationality.

This graphic tells us that with this dataset, and of the seven variables thought to be casual candidates to explain house prices, only five at most can do so. Increasing the required confidence interval (1 minus alpha), *squeezes* the algorithm though, as the graph shows, in this context we never reach the boundary level at which the algorithm first reports no robust variables. This would be the level at which the analysis advises adoption of non-instrumental action, and so instrumental action is advisable for this dataset or context. *The point is that this choice is now founded empirically, is repeatable and can be compared with that for other datasets.*

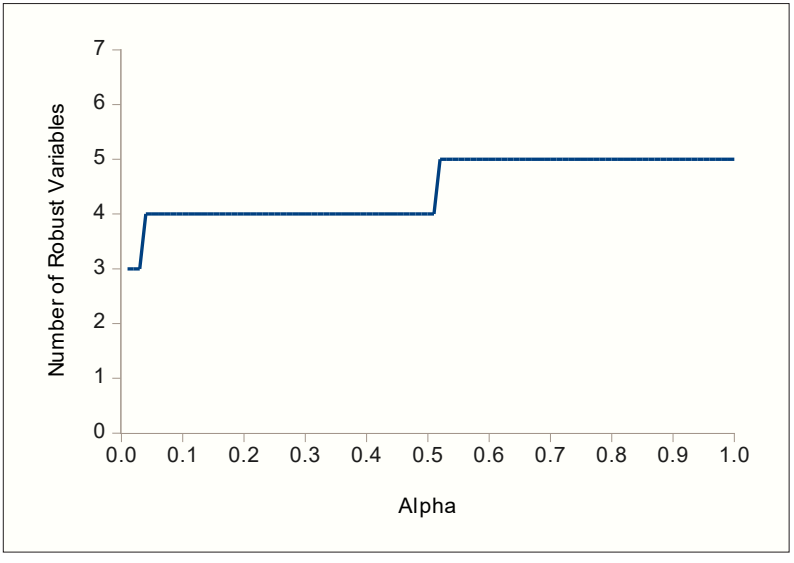
As an adaptation of mainstream techniques with their well-known biases towards assertions of knowledge (Turri 2015), we can note that both the adaptation of the Leamer EBA and the Bayesian framework work by exploiting their different but

equally conventional gauges of what regularity must be found to support statistical conclusions about likely relationships between variables in the datasets (in the former, confidence intervals). Though usually treated by researchers as robust and valid, these gauges are in fact conventional and contingent Cowles and David (1982).

Authority for empirical results, as is well known, often assert that the cause-effect relations of analyses are real and true, which resonates with the sense that in instrumental action they form a real basis for organising change. This is illuminated by Cohen (1994). Here he makes lucid points about the contingent and judgemental nature of statistical inference:

What is wrong with NHST (Null Hypothesis Significance Testing)? 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] 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. (Cohen 1994, p. 997, emphasis added)

Figure 1



Note: For sources, see endnote [7].

The point as, McCloskey (1985) argues, is often ignored. This is that such empirical work is not about obtaining non-contingent truth (though many want it to be). Rather it is about judgements, and these are inherently related to social convention and practice.

> 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.* (McCloskey 1985, p. 118 stress added)

My discussion thus shows by example (others could be used) that mainstream statistical techniques can be used to underpin a theory of ignorance. A statistic can be obtained to guide decision-making: at some values, instrumental rationality is reasonable and empirically justified; at others, the reasonable choice is only between inaction and non-instrumental action. This shows that the empirical basis of a theory of ignorance is contingent upon the particular application of the technique (such as the sample size, the data used, the degree of certainty required ...), but the technique itself must be able to conclude that it is better to council predictive ignorance, and so the avoidance of instrumental action.

The epistemological basis of international development practice (aid work)

Developed countries spend large amounts of money on aid, and within the OECD they coordinate their normative positions on how this should be organised and evaluated through the Development Advisory Committee (DAC). Aid, being tax-payers' money, is subject to the same auditing and evaluator procedures as other government spending. Therefore, it is a valuable case study of the ideas that underpin how this is meant to happen – its social epistemology (Fforde 2007).

International development practice is part of wider structures that include knowledge production and ways of linking knowledge to interventions. Most University courses on Development Studies include subjects such as 'Theories of Development' that exposit to students the history of different theories and current positions, and these are generally expressed in terms of different instrumental logics (e.g. Willis 2005). The underlying position is that this situation, of multiple

truths, is one where each different truth, with its cause-effect logic, can be and has been assumed to be a valid predictive guide to instrumental action. Students are not usually asked to question this fundamental assumption, rather to consider the co-existence of a range of locally (for each development organisation) valid truths as a fact of life. If they gain employment they will usually be required to buy-in to the local truth – usually that of their employer (Fechter and Hindman 2011).

Yet, given the evidence that economic development is an area of predictive ignorance (see Levine and Zervos (1993), as discussed in the previous section; also, Fforde (2015)), the norms of international development practice, as stated authoritatively by the DAC are striking. This is because they *require* instrumental action, and, even more strikingly, *require* that evaluations *ex post* also assume that there is sufficient contextual regularity to justify instrumental action (Fforde 2017; AusAID 2005; OECD n/d). There is also no DAC procedural requirement placed upon knowledge production that would justify asserting (as they do) that there actually is, in a context, sufficient predictive regularity to justify instrumental interventions. The DAC simply requires that interventions assume predictability, and its norms do not require testing that a theory or account maps well enough to act as a *predictive* guide.

This implies that the DAC norms are epistemologically flawed (Fforde 2017; 2015), yet they are authoritative. In consequence, discussion of inaction and non-instrumental action is constrained and inhibited, but pushes in two directions:

First, given that there are good arguments that many contexts are in fact best seen as irregular and chaotic are nevertheless required to be deemed predictively knowable, *much experience of non-instrumental action would be 'under the radar' as part of the lived experiences of many people, such as aid practitioners.*

From personal experience this contrast between informal and formal aid practices is the case. There is much evidence of this in the literature (e.g. Rottenburg 2009). There is also evidence that many aid workers are often uncomfortable with these tensions. For example, in a study for a major aid donor (Sweden's Sida), Bakewell and Garbutt (2005) used their personal contacts to ask a range of international NGOS just how they used the log-frame or Logical Framework Approach (LFA) - a development management tool that requires that interventions be expressed in terms of known causes and effects [2]. Strikingly, their interviewees required guarantees of anonymity to speak openly:

> [W]e conducted all interviews on conditions of anonymity. Since our major concern is about how logical frameworks are actually used, rather than the stated corporate position of how they should be used, we felt this would help to enable people to speak more freely (Bakewell and Garbutt 2005, p. 2).

What this quote shows is a picture of mismatch between what the log-frame is meant to be and what happens. Sweden remains part of the DAC. The alternative narrative is that the LFA is used only because it is required as part of their DAC-endorsed accountability norms by the official agencies (such as Sida) that fund INGOs:

> Although some NGOs have internalised the use of the LFA, for most the original driving force came from donors, some of which have insisted that those receiving grants should adopt an LFA. One organisation which supports a range of local NGOs in preparing logical frameworks for donors, stated that these organisations only ever use it because it is a requirement, 'it is never used voluntarily or because the client thinks it is a good idea'. Another INGO which does not use LFA internally, stated that 'staff are free to use it if they find it useful, but most do not' (Bakewell and Garbutt 2005, p. 6).

Thus, and secondly, there is likely to be far more experience of non-instrumental action than we might imagine. Yet this has not been formally studied that much (Fforde 2017), apart from a small (compared with interest in instrumental action) number of studies that tend to define it as 'irrational' (because instrumental rationality is privileged) [8]. As already mentioned, the results of Levine & Zervos (1993) suggest that the causes of variations in economic growth are not robustly known, yet most citations ignored this (Fforde 2005). Yet, the reality of economic growth is a large part of the experienced realities of large parts of the world's population.

So, if the preceding section showed that mainstream empirical techniques can be adapted to provide an empirical basis for a theory of ignorance – a reasoned choice to assert ignorance, the international development case study shows that the actual informal practices of aid workers can be used as a basis for exploring how a theory of ignorance is deployed in practice, though informally and so not fully thought through.

Towards a theory of ignorance

Whether one looks at procedures for knowledge production, or discussions about how the resulting knowledges are to be used, the picture is surely one where an instrumental action is likely to have been constructed on weak empirical foundations. [9] Being not what it says it is, this invites use of analytical frameworks that explain it as part of hegemonic discourses, structures of power or the main thrust of agnotology already discussed – the construction of ignorance as false knowledge. Further, the contrast between assertions of the value of instrumental action (and so its prescribed use) simultaneously with the absence of any requirement that instrumental action be based upon predictive knowledge of cause and effect is vivid.

Is, indeed, what appears as instrumental action best seen as such? If instrumentality – genuine links between causes and effects - is not actually known, then what is going on? If, as Levine and Zervos (1993) imply, variations in economic growth rates are not robustly correlated with pretty much anything, then what are we to make of the classic international development policy story that explains success in terms of good policies? What is a consistent understanding, then, of what is widely meant by instrumental actions, if, as much evidence suggests, they are *not* generally founded on predictive knowledge? Surely part of the issue here is the mainstream identification of rational action with rational use of known cause-effect relations to secure defined goals. Non-instrumental action is then dismissed as not being rational. This in many ways is an absurd position. If it is reasonable to argue that an actor faces conditions of ignorance, which clearly is inherent in the idea that cause-effect relations may be known but not always, then inaction or non-instrumental action is not irrational.

There are other conceptions of action. For example, Stoecker (2007):

> ... it is not surprising that the philosophers did not succeed in finding a satisfactory answer to the question what actions are, since the whole question is due to a misunderstanding. ... [T]he mistakes lies in the implicit assumption that ... sentences like 'He did it' are descriptive sentences, while in fact they are ... *ascriptive sentences*, sentences ascribing responsibility to a person (Stoecker 2007, p. 35).

This refreshing view suggests that what the DAC is really up to is enforcing certain norms that fit with styles of government and power relationships, so that countries that 'do well' can have ascribed to them a correct and praiseworthy application of

what the DAC says is a good idea. This fallacy, clearly, provides conceptual space for the actual deployment of a theory of ignorance into fact-based practice.

Work on what aid workers *actually do* reaches similar conclusions. Aid work appears as instrumental action, but on closer examination seems very different (Fechter and Hindman 2011). If predictive power is often lacking, this is hardly surprising. Pressman and Wildavsky (1972), in a classic text, reach similar conclusions.

Anthropologists have often argued in ways resonant with both Kuhn and Stoecker that statements that purport on the surface to be reporting the predictive foundations of instrumental actions are better seen quite differently. Shore and Wright (1997) view such statements rather as part of belief sets that give meaning, through notions of policy, to individuals in their social activities. '[P]olicy increasingly shapes the way individuals construct themselves as subjects. ... From cradle to the grave, people are classified, shaped and ordered according to policies, but they may have little consciousness of ... the processes at work'. (Shore and Wright 1997, p. 4)

These literatures point the way to an understanding of the tensions identified above. They also point to the prejudice against inaction and non-instrumental action manifest in empirical justifications for instrumental action.

Further support for deployment of a theory of ignorance may be found in consideration of knowledge production practices and norms. Knowledge production may be said to draw upon two different sets of criteria to judge the acceptability of an explanation or theory (Fforde 2017). Some criteria, such as those offered by Nisbet (1969), or Held et al (1999) [10] quoted above, allow for multiple truths because they contain no criterion requiring as a matter of procedure that theories be tested against each other. It is obvious that in such a world of multiple truths the absence from the list of acceptability criteria of a criterion, equivalent to prediction, that requires that theories be comparatively tested, suggests that which theory wins out is a consequence of factors outside the method or procedure adopted by knowledge producers (thus Kuhn's statement that it is not enough just for there to be an anomaly). Students of Theories of Development I believe often suspect that shifts in theories over time and variations between donors are not best explained by changes in the realities of poor countries. Rather they accompany the changing interests and views of those who control and finance aid donors of different types, as the OECD controls the DAC (Willis 2005).

Nisbet offers a historical account of how acceptable accounts of change are, and have for centuries, been metaphorical, not predictive:

> ... 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. (Willis 2005, pp. 8-9]

Tellingly, he adds that: '... what we have brought into conceptual existence, we are prone to believe has actual existence'. (Willis 2005, pp. 241)

Dominant analyses of social change (such as those in the realm of aid that are required by the DAC norms) are therefore, he is arguing, generally metaphorical, not predictive, so that their use to organise interventions predictively has no sound empirical basis and is indeed not actually meant to have any. For a theory or analysis to pass the requisite criteria it does not have to be predictive, and the list from Held et al above gives a neat summary of what peers and knowledge consumers are likely to expect.

These metaphors advance an account of the nature of change (in Nisbet's phrase they are 'natural histories') and are generalisations. In a more modern language, they are simply (untested) theories. In a common-sense way of putting it, the more the purpose matters, the less it is wise to base instrumental action on theories that have not been predictively tested. Where unpredictively tested theories are used as the foundations for instrumental actions, one should be wary of just how the dominant theory gets chosen.

On the other hand, what have been called 'natural science' criteria do by contrast include a comparative criterion – prediction - which requires that theories be tested against each other on so-called predictive grounds (Crombie 1953). This advocates an approach to knowledge that seeks a single truth.

There is of course an important debate about why theories change over time. Whilst scholars such as Kuhn (1970) and Popper (1977) seem to hold to a belief that knowledge progresses, others, such as Said (1978) or Escobar (1995) are more interested in questions about how and why knowledges determine or influence their own subject matter, with little sense of 'progress'. (Fforde 2013, Chapter 5). For a

fascinating historical account of how economics dealt with the massive increase of empirical potential for testing theories in the 1930s see Yonay (1998).

The presence or absence of a predictive criterion in the procedural norms of knowledge production practices is something with which we are familiar. In fact, quick reflection will suggest that the central issue is not a matter of prediction per se, but rather the presence or absence of the practice of treating cause-effect accounts as known single truths and valid foundations for instrumental action. Thus, whether a theory with predictive power is accepted because its predictive accuracy is 'high' or 'low' depends upon the context (as the quote from Shute at the start of the paper suggests). Whether it needs to be right 51% or 91% of the time depends on what the theory is used for. But the criteria for deciding which the best theory is are internal to the procedures of knowledge production.

This discussion then, like that of the preceding section, points to an epistemological space for theories of ignorance.

Conclusions

If, as seems to be the case, social change seems often unpredictable, but we are not meant to spend much time empirically assessing when this is the case, the outlook seems bleak. The DAC, part of the OECD, requires formally that in aid work instrumental action be used, and this requires also that it be believed (or at least there be a pretence of a belief) that social change is always predictable.

The paper has provided a basis for a re-theorisation of relationships between data, purpose and action that sees advocacy of inaction or non-instrumental action as empirically justifiable and so warranting far more attention. If my arguments are even partly correct, then in fact very many acts deemed instrumental are in fact non-instrumental, and this can be researched. Deciding that a given context is best treated as unsuited to instrumental action may be based upon solid empirical reasoning. Further, there are accessible and for me persuasive explanations for why mainstream accounts of social change are vulnerable to being treated as predictively valid.

A theory of ignorance, I conclude, should:

First, seek empirical foundation in a reasoned process of empirical investigation that can (but need not) conclude that good council is for an assertion that the context is unpredictable and so instrumental action unwise.

Second, seek principles of organisation in the existing practices of those involved in purposeful activities – such as aid work – that are (wrongly) required to assume predictive knowledge, but find ways of working and thinking that correspond to realities that are not experienced as predictive, and so involve non-instrumental action.

Third, be informed by the results of scholarship that explains and elucidates the epistemological practices that assert fake predictive knowledges.

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Endnotes

[1] Search using Harzing's Publish or Perish (which platforms on Google Scholar) produced just 161 papers with 6260 citations (11/5/2020) for the phrase *non-instrumental action*, but a search on *instrumental action* produced over 980 papers with nearly 200,000 citations. Most of the papers on non-instrumental action treated it as irrational.

[2] Important military organisations (to my knowledge German and Israeli) as a matter of doctrine have at times asserted that the context – combat – is chaotic and organise accordingly (Fforde 2017, Chapter 8).

[3] Widely known *sceptical* works include Lindblom (1959), Pressman and Wildavsky (1973) and Bridgman and Davies (1998 and later editions). There is an extensive discussion amongst development workers and scholars about related issues, which I do not discuss here, but see below. The *do no harm* principle in medicine attempts to drive home to practitioners that an intervention, combined with an attractive but predictively untested theory, may encourage reckless behaviour damaging to the patient.

[4] Belief in the validity of the predictive instrumental action model can be somewhat shrill. Thus: '[I]t is reasonable to induce that we will make further progress towards the ... synthesis or reconciliation of the physical and social sciences. ... The alternative, to assume that causal links and consistency ... is impossible, leads fairly rapidly in rather dismal nihilistic cul-de-sacs' (Talbot 2010, p. 5, stress added). Talbot's stance supports blanket assertion of knowability (with enough resources) and so a strong bias against inaction and non-instrumental action. I think it is reckless. I thank an anonymous reviewer for pointing me to the issue of rational ignorance addressed within public choice theory and plead issues of space in not following this up.

[5] For an overview of the field see Proctor and Schiebinger, eds. (2008).

[6] 'Voluminous behavioral research shows that it is more complicated and effortful to represent negation than affirmation' (Turri 2014, p. 14).

[7] Data comes from Hill et al. (2010) and are used in his exercise 5.13, p. 205. This data contains details about 1,080 houses sold in Baton Rouge, Louisiana, during mid-2005. Details are available from the author.

[8] There is, as already mentioned, a growing literature in International Development that grapples with the issues raised here. However, the DAC position remains unchanged.

[9] We may note that the thrust of modern policy handbooks such as Bridgeman and Davies 1998 and its later editions is to argue that good government comes from good procedure. See also Perl et al 2018 for a discussion of the main ways of understanding policy work in the contemporary 'post-fact' world and whether that scepticism devalues them; they conclude not. Fforde (2019) replies that their conclusion faces in the wrong direction, as the central political issue is the value of policy rationality as a rationality, part of a politics based upon persuasion rather than force.

[10] '... 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.' (Held et al. 1999, p. 14) There is nothing here about predictive power, or how to compare theories exhaustively.

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