Induction, complexity, and economic methodology

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12 January 2009

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Abstract.

This paper focuses on induction, because the supposed weaknesses of that process are the main reason for favouring falsificationism, which plays an important part in scientific methodology generally; the paper is part of a wider study of economic methodology. The standard objections to, and paradoxes of, induction are reviewed, and this leads to the conclusion that the supposed 'problem' or 'riddle' of induction is a false one. It is an artefact of two assumptions: that the classic two-valued logic (CL) is appropriate for the contexts in which induction is relevant; and that it is the touchstone of rational thought. The status accorded to CL is the result of historical and cultural factors.

The material we need to reason about falls into four distinct domains; these are explored in turn, while progressively relaxing the restrictions that are essential to the valid application of CL. The restrictions include the requirement for a pre-existing, independently-guaranteed classification, into which we can fit all new cases with certainty; and non-ambiguous relationships between antecedents and consequents. Natural kinds, determined by the existence of complex entities whose characteristics cannot be unbundled and altered in a piecemeal, arbitrary fashion, play an important part in the review; so also does fuzzy logic (FL). These are used to resolve two famous paradoxes about induction (the grue and raven paradoxes); and the case for believing that conventional logic is a subset of fuzzy logic is outlined. The latter disposes of all questions of justifying induction deductively.

The concept of problem structure is used as the basis for a structured concept of rationality that is appropriate to all four of the domains mentioned above.

The rehabilitation of induction supports an alternative definition of science: that it is the business of developing networks of contrastive, constitutive explanations of reproducible, inter-subjective ('objective') data. Social and psychological obstacles ensure the progress of science is slow and convoluted; however, the relativist arguments against such a project are rejected.

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Introduction.

This working paper is part of a larger investigation into the problems of the methodology of economics, considered as a science: what should its foundational assumptions be; what should be its scope and purpose; what should count as valid evidence; what should count as a significant anomaly; and what should give us confidence in any extensive, coherent body of economic ideas (such as neo-classical economics). Induction is relevant to this in a number of ways.

Philosophers have long regarded induction as objectionable in principle. Concerns about induction contribute to the low status accorded to empirical evidence by economists [1] (although they are certainly not the only factor responsible). Secondly, the desire to sidestep the 'problem of induction' motivated the single most widely advocated scientific methodology, falsificationism, which is the approach most commonly espoused by those few economists who do concern themselves with methodological issues – although even among them, it is honoured more in the breach than in the observance [2, 3]. Thirdly, dealing with induction draws in techniques and perspectives (including fuzzy logic and a richer concept of rationality) that have important implications for scientific methodology in general; restoring the respectability of induction will inevitably imply other changes. I hope to show that the supposed problem of induction is a false one, an artefact of an exclusive adherence to one particular style of logic, referred to here as conventional logic[2] (CL).

Most of the general philosophical discussion of induction focuses on induction by simple enumeration. (Strictly speaking, the process of inferring theories, rather than the properties of members of a class is abduction, but the same considerations apply to both.) In this, we have a known species – a mineral, such beryl, or a biological species, such as *Corvus corax*[3] – and we see successive instances of it. It is alleged that if, at a certain point in time, every emerald we have seen is green, every raven black, and so on, then we will believe that 'all emeralds are green', 'all ravens are black', etc, with a degree of conviction that increases with the number of positive instances encountered (presumably in an asymptotic manner). Those philosophers who are worried by 'the problem of induction' are concerned that we cannot possibly know that *all* cases will fit the observed rule; but they also fret about the lack of a respectable basis for our ability to apply this 'improper' pattern of reasoning selectively.

[2] This term refers here to two-valued logics, in which the laws of contradiction and excluded middle apply, and in which the principle that a contradiction implies the truth of any other proposition (this last qualification excludes variants such as relevance logic [4]).

[3] Emeralds are a variety of beryl, coloured green by impurities; *Corvus corax* is, as every schoolchild knows, the raven. Both figure in the famous paradoxes introduced later.

(As the logician Susan Haack points out, if you had seen only philosophers who wear beards, you would probably not feel that you can reasonably infer that all philosophers wear beards.)

The framing of this supposed problem contains a dangerous assumption. It assumes that we have no problem in assigning new instances of anything that comes along to pre-existing, well-defined categories – i.e., that we have somehow been provided with a ready-made, totally reliable classification system. This does not depend on our previous observations of the distribution of colours and other properties of minerals, birds, etc[4], and whose categories are crisp, excluding the possibility of overlaps, 'grey areas', etc. Some readers will have already reached for the blunderbuss of commonsense to deal with this, sensing – rightly – that the apparent problem is something to do with *natural kinds* of objects, and their inherent or essential properties. Those who do are in good company: John Stuart Mill noted the logical problems, set them firmly aside, and went on to develop his principles of induction, still an important part of scientific method [5]; but one consequence of this sturdy commonsense reaction is that the sceptical attitude to induction keeps re-appearing, like a thistle growing up through a neatly-paved yard.

2. Induction in Practice.

In one exceptionally striking example of induction, Dmitri Mendeleev (expanding on the earlier efforts of others) detected some interesting patterns in the properties of the chemical elements when arranged in order of atomic weight[5], and, in constructing his *Periodic Table of Elements*, drew the conclusion that this pattern reflected something fundamental. On this basis, he correctly forecast the existence and properties of new elements (e.g., germanium) that corresponded to 'holes' in the table. His forecasts of their physical and chemical properties were strikingly accurate. He even amended the ordering of elements by atomic weight, where the pattern suggested that there might be an error in the observations, or some unknown disturbing factor at work. In each case, his judgment was eventually vindicated [6].

Mendeleev had, in effect, inferred from 'all known elements, from an extensive sample, conform to the pattern *P*' that '… therefore, all elements conform to *P*'. But there is no legitimate way in conventional logic (CL) of quantifying 'all known instances, from an extensive sample': you can only have 'all', 'none', or 'some'. 'Some' has a very different meaning from 'all known, reliable instances from what we think is a thorough sample', but

[4] It also assumes that our system of classification assures us that 'minerals', 'birds', etc are valid, crisp categories, of course.
[5] Strictly, this should be 'relative atomic mass'.

this important difference cannot even be expressed in CL, which treats inferences such as Mendeleev's as identical with this one:

- some four-legged animals [e.g., rabbits] are easy to kill and delicious in a casserole;
- therefore all four-legged animals are easy to kill and delicious in a casserole

The dangers of the over-enthusiastic use of this pattern of reasoning are fairly obvious, even to us non-philosophers, and this very construction has been used to argue that evolutionary forces will ensure that we all learn to use CL (except that we do not, of course.) We will see later that the problem lies in the belief that truth should be determined by the form of a chain of reasoning, and never by its technical content, *in all possible domains of subject matter that we might need to reason about*. Patrick O'Sullivan, in a manner typical of this school, says that induction is a 'logical fallacy', 'which must ultimately fail when arraigned before the court of Reason [*sic*]'; but he then goes on to say that its use may be 'reasonable', but can never be 'rational', apparently not noticing the dual use of 'reason' [7]. I hope to show this view of induction is deeply mistaken, and that richer, more differentiated concept of rationality exists, one that reconciles 'Reason', 'rational', and 'reasonable'. The confusion here arises from the historical tendency of many philosophers to ignore – in their professional capacity – the possibility that we may need to reason about subjects that may not comply with the conditions required by CL. Since the middle of the 20th Century, this situation has changed considerably [8], but many philosophers still recognize only conventional, deductive logic; and this has led to an enormous (but ultimately pointless) body of literature concerned with whether induction can be given a sound basis in *deductive* logic. A lot of the more recent material of this type has attempted to resolve two famous paradoxes about induction, which are considered next.

In what follows, examples from a range of sciences are treated as relevant to the basic issues of induction; however, using such examples is not intended as an endorsement of scientific monism in the conventional sense. Monism asserts that there is only one scientific methodology (that supposedly in use in the natural sciences). Dualism asserts that the human sciences require a distinct methodology of their own; this is often associated with claims that humans have especially trustworthy 'a priori' insights into human behaviour, and that theorems deduced from these insights need no empirical verification [more on this in the main study, where this 'apriorism' is rejected].

2.1 The Two Paradoxes of Induction.

Goodman's Grue Paradox. Nelson Goodman developed invented the term 'grue', and applied it to emeralds. (Both paradoxes are dealt with at length in [9].) An emerald is grue if either it was examined at or before a particular (future) instant in time, *T*, and found to be green; or it is first examined after *T*, and will then be found to be blue. The rationale for this peculiar construction is the supposition that we feel further confirmed in our belief that 'all emeralds are green', by every additional green emerald we encounter. However, every green emerald seen at or before *T* also confirms 'all emeralds are grue', and just as strongly. Accordingly, we should believe both that emeralds are green, and that they are grue – but the predictions from these inferences contradict each other, as to the colour of any emerald that is first inspected after *T*. Goodman thought that the possibility that induction could lead to contradictions could be resolved by introducing a new concept, *projectibility*: a predicate such as green is projectible, whereas grue is not.

Nobody is very happy with this. First of all, 'projectibility' seems to be circular, in that something is projectible if and only if it has been projected successfully in the past. Then, it seems odd that the predicate, rather than the subject, should control what sort of inferences may be drawn. (What about predicates that can legitimately be projected for some subjects, but not others, e.g., bearded billy-goats *versus* bearded philosophers?) It would have been at least as sensible to outlaw inductions about as-yet unobserved phenomena; in what follows, I hope to show that the idea of projectibility is redundant.

Hempel's Raven Paradox. Assuming that black is not part of the definition of 'raven'; *and* assuming that we can identify and safely exclude 'abnormal' specimens, such as albino ravens; *and* assuming that competent observers do in fact apply confirmatory new instances in the way that some philosophers suggest; *then* each new black raven should strengthen our belief in the proposition 'all ravens are black'. So far, so good … but in CL, all logically equivalent propositions should have the same significance, and 'everything that is not black is not a raven' is logically equivalent to 'all ravens are black'. Therefore, an encounter with a yellow banana, a pink gorilla, etc should all have the same impact on our convictions about the essential blackness of ravens as actually seeing another black raven. This offends against almost everybody's understanding of induction. The usual response has been built around the idea of 'natural kinds': it is safe to apply induction to natural kinds, such as 'ravens', but the complement of a natural kind (e.g., non-black non-ravens) is generally not itself a natural kind, and therefore outside the legitimate scope of induction. Although this makes sense, I will argue later that the idea of natural kinds is not needed to resolve this paradox (although it has other important applications, see below).

3. The Domains of Reason.

The perceived ‘problems’ or ‘riddles’ of induction arise from a failure to recognize that the matters we may need to reason about belong to four distinct domains, and that the stringent conditions required by CL are met in only one of these. (The reasons for this failure seem to be purely historical and cultural; for many centuries, this was the only logic formally taught in the West [10]; however, Aristotle was aware of its limitations, see below.) These domains are:

3.1 Domain I: The Heartlands.

This consists of subject matter in which every contrast set is completely divided into mutually exclusive, non-overlapping categories, with precise boundaries. Where this is the case, the laws of excluded middle (LEM) and non-contradiction (LNC) apply: nothing can be neither *X* nor *not-X*, and nothing can be both *X* and *not-X*. In this artificial context, a raven cannot be both black and non-black, and it is forbidden to be neither black nor non-black. Here, we always know with certainty the properties of any item we are presented with, and how to classify it. (The term ‘crisp’ distinguishes such categories from fuzzy ones, see below.) Within this domain, truth is indeed formal, depending solely on the syntax of a chain of reasoning, and not at all on its technical content; a valid pattern of reasoning is one that is incapable of generating falsehood, given true inputs.

In this domain, the pursuit of absolute truth and the total elimination of error are both reasonable aims, and there is no reason even to wish for a mechanism to do what induction does, i.e., provide new, additional grist for the logical mill. This grist consists of propositions that we are prepared to set up, perhaps provisionally, as postulates or axioms; and propositions that are analytic, and therefore necessarily true. Deductions from propositions of the first kind are, of course, conditional on the truth of the axioms. As defined by Kant (see Box 1), the analytic category is quite capacious, but faith in the existence of non-tautologous analytic propositions has declined drastically [11]. In terms of the argument presented below, the central problem is that anything of this kind has to be derived from knowledge of the external world, and virtually nothing we can know about that conforms to the standards required for the application of CL (see below).

Box 1: The Concept of Analyticity.

When Kant introduced the synthetic / analytic distinction, he said that something was analytic if the concept of the predicate was contained in the concept of the subject (as ‘not being married’ is contained in the idea of ‘bachelor’), and is therefore necessarily true; and that every other proposition is synthetic. Kant also distinguished between an *a priori* frame of reference – including space and time – which he believed to be inbuilt into the human mind, and which was necessary for us to be able to make sense of the external world, and *a posteriori* knowledge, which could only be derived from experience. In his view, our perceptions are therefore a joint product of our internal frame of reference, and the stimulus we receive from external objects; Lindsay [12] gives a clear exposition of the two distinctions, and explains some of the confusion and inconsistency in *The Critique of Pure Reason*. The two distinctions give rise to three combinations (nothing can be both analytic and a posteriori, of course):

- Analytic & *a priori*: we know things such as ‘material objects have extension in space’, but cannot give a reason why it is so.
- Synthetic & *a priori*: this includes geometry – our *a priori* framework makes us aware of space and some very basic concepts, but ideas such as ‘circle’ also have a contribution from experience (e.g., from the construction of geometric figures). When von Mises and Robbins [13, 14] try to assert that the fundamental truths of economics are ‘a priori’, they are actually placing them in this category.
- Synthetic & *a posteriori*: this sort of knowledge is, of course, the subject matter of all the technical, scientific, and humanities disciplines.

There are a number of problems with this formulation. First, there is no evidence that the a priori framework actually exists, or that it is not learnt through some combination of direct experience and cultural transmission (so that it could be otherwise, and is therefore not analytic) – this certainly seems to be he case for our perceptions of space, time, and the way that material objects occupy space. Secondly, ‘contained in’ is a much looser specification than ‘true by definition’, and Kant gave us no test for deciding when some quality of a particular predicate was contained in its subject; obviously, unrecognized assumptions can enter through this gap. These weaknesses are illustrated by Kant’s belief that Newtonian absolute space and time are a priori, and that there was legitimate ‘synthetic’ data that could be combined with it to prove that Newtonian mechanics had to be so, in the same way that Pythagoras’ Theorem has to be true for plane triangles [15] – despite the fact that we actually live in a relativistic universe (even if it is not apparent with everyday masses and velocities). Similarly, it is difficult to square the idea of an a priori frame of reference with many quantum phenomena, so much so that explaining them challenges our idea of what ‘understanding’ means [16].

3.2 Domain II: The Frontier.

Nothing in the physical or social worlds can meet the standards for full citizenship in the Heartlands. In this second domain, we know neither all the properties of the objects we have to deal with, nor, indeed, how we should classify them. This is not a trivial problem: in chemistry alone, the lack of a classification system that distinguished mixtures from compounds held back the development of the atomic theory for many decades, and the early chemists' lack of a scheme of classification that would have enabled them to recognize that there are three states of matter, solid, liquid, and gas, contributed greatly to the difficulty of moving beyond the phlogiston theory of combustion, and impeded the discovery of oxygen [17]. In psychology, the ways in which individuals construct, inductively, their own, individual systems of classification – relating to social relations, work, and so on – have huge effects on their levels of well-being and achievement [18].

This limitation makes the possibility of error inseparable from action or investigation in the real world, and it forces us to treat all our hypotheses and theories as provisional ones. A typical sequence comes from the history of medicine. Scientists investigated a sequence of infectious diseases, found that each fitted a causal explanation that revolved around microscopic living agents, and concluded that all infectious diseases are bacterial in origin. Then they found other disease-causing agents (the viruses) that are not living, but consist of a body of genetic code (wrapped in a minimal delivery and protection system) that reproduced itself by hijacking the cellular apparatus of its victims. This seemed to show that all infectious diseases rely on transmission of the genetic blueprint of the causal agent – until someone discovered that some (the prions) rely on a direct transmission by contact of a pathological version of the *shape* of certain proteins. Now, even this list seems too short [19].

In every case, we have to infer the proper way to classify objects from what we observe; we do *not* infer the properties of members of *given* classes (such as ravens, or emeralds, or swans) by observing instances of them. This process is inevitably iterative, and involves a lot of bootstrapping. The scientific systems of classification that we now use with (occasionally excessive) confidence rest on centuries of such effort. Our rough, preliminary attempts at classification have led us into error, and will inevitably do so again. 'Failures' of this kind will force us to revise both the divisions that we make, and our choice of which aspects of the objects we examine to use as the basis of our judgments about classification; massive changes have taken place in biological classification over the last thirty years in precisely this way. (Because classification is such a central issue, Box 2 examines it in more detail.)

Box 2:

Classification and Natural Kinds.

Science is interested in explanation across a wide range of contexts. Accordingly, it focuses on explanations that are (i) contrastive, explaining why a specimen of X does *this* rather than *that;* and (ii), constitutive, explaining in terms of the sort of thing it is, and its history.

Electronic structure does this for chemical species; and genetic relationships do the same for biological organisms. In the social sciences, good classifications enable us to say things like ‘this group responded by doing *X*, while that one responded by doing *Y*, because ….’ Henry Mintzberg’s classification of organizations on the basis of the internal mechanism of coordination and their external relations with key stakeholders is a good example [20]; Bailey’s classification of the membership of political groups into *core* and *following* is another [21]. Classification and theorizing interact, of course.

In all these cases, the reason that there are distinct kinds is that what an individual specimen does and how it reacts depend upon a complex internal structure, which cannot be changed in an arbitrary, piecemeal fashion without destroying it [22]. Much philosophical discussion of problems of classification seems quite footling (e.g., the discussion of the problems of classifying objects which are various combinations of round, red, and wooden, cited in Quine’s contribution to [23]), precisely because it ignores the significance of internal complexity in determining *natural* kinds.

Classification also has implications for how instances confirm inductive generalizations. Often, initial observations on a particular phenomenon make little impact; then a single critical finding breaks a conceptual log-jam, requiring only a relatively small number of successful attempts to replicate it, before it is accepted. (The distinction between replication – i.e., making sure that the original observation is not a product of technical error or self-deception, or even fraud – and confirmation is an important one.) For example, the role of DNA in genetics was rapidly accepted, without extensive confirmation in a range of species, because the nucleus, chromosomes, and their biochemical components were already accepted as natural kinds.

This contrasts with, say, the slow recognition of the importance and generality of what were originally known as heat-shock proteins. Following their original discovery in the early 1960s, it took a long series of wide-ranging investigations to convince the relevant community that they represented a fairly universal phenomenon – not because there was any intrinsic resistance to the idea, but because protein shape was not then perceived as the basis for a ‘natural kind’ [24, 25].

By the time that we have uncovered one of these ‘failures’, we have already paid the price of our ignorance (by, for example, applying medical treatments that could not work in an unrecognised but distinct sub-category of infectious diseases). Remedying it is then an achievement of rational activity, not a penalty for fallacious reasoning. What some philosophers have taken to be the products of fallacious reasoning are actually the very things that science seeks: interesting and informative failures of induction. (The situation in applied fields is more complicated, because what counts as a failure there is *culpable* failure, which is defined by the community involved.)

If there were some better *attainable* standard of knowledge for this domain – one that gave, perhaps, the level of certainty attainable in the heartlands of CL – it would make sense to speak of a ‘problem of induction’. But there is little point in treating as a problem that needs solving our inability to accomplish something that is, by definition, impossible; and it is nonsense to speak of a ‘problem of induction’ or ‘riddle of induction’. Induction is the only way to widen our understanding of the situation in which we find ourselves.

3.3 Domain III: The Badlands.

Outside the previous domain, there lies another, in which the problem of not having an independently-guaranteed system of classification of the phenomena around us is aggravated by weakness of the functional links between antecedents and consequents: we cannot say with certainty, ‘if it is an *X*, then it will do *Y* in situation *Z*’. Even when we can classify individual situations reasonably accurately, and perhaps even measure all the variables that our existing knowledge suggests are important, their dynamics may be such that they can evolve in surprising ways. We find, with hindsight, that the numbers (or other descriptors) did not mean what we thought they meant at the time, i.e., they are ambiguous; this is very common with macroeconomic data, for example. (The problem is emphatically not one of stochasticity.)

Induction, in the accepted sense, does not work here. Aristotle – more famous for his work within the confines of CL – was already aware of this problem [26] in relation to biology and ethics. In the latter context, he wrote:

> *... in discussing [these] subjects, and arguing from evidence, we must be satisfied with a broad outline of the truth; that is, in arguing about what is for the most part so, from premisses which are for the most part true, we must be content to draw conclusions that are similarly qualified.*

How we can reach a stage of 'contentment' about our conclusions, in which we are reasonably confident that we have made the best use of our input information without going beyond what it warrants, is the concern of fuzzy logic (FL). This is a rigorous calculus of reasoning, which differs from CL in the following ways:

(i) The object of DL is to derive absolutely reliable conclusions from data that are known to be true. The object of CL is to formalize two important aspects of practical reasoning. It enables us to extend the knowledge that we have about well-defined sets to related ones (e.g., to interpolate between our knowledge about honest administrations and wholly corrupt ones). And it helps us to derive, from data that are quantitatively vague, qualitative, or ambiguous, conclusions that limit the amount of harm that can result from mis- or over-interpreting the data. Ross [27] describes the details, and gives a very thorough analysis of the relationship between CL and FL.

(ii) CL uses only two truth-values, true and false, to determine, with absolute certainty, *whether or not* some item belongs to a given, crisp set. Fuzzy logic uses membership values that can take on any value from zero to one inclusive, which say *how typical* a particular item is of a given fuzzy set. A membership of zero indicates that the item is a non-member, and one, that it is a perfect, typical member. The sources of fuzziness are outlined in Box 3.

(iii) The sets in CL are, by assumption, given; those in FL are determined by membership functions, which are empirically determined[6]. A particular fuzzy set consists of all those things to which its 'parent' membership function assigns a non-zero membership value. Membership functions convert 'readings' about the situation (say, indicators of the level of market confidence in economics) to a membership value in one or more fuzzy sets, such as <u>*low confidence*</u>); the input is from a *support scale* (or scales), which may be quantitative or qualitative.

(iv) In FL, but not in CL, an item can be a member (to varying extents) in different classes within a contrast set. In CL, a contrast set such as 'sentient beings' might be divided into 'mortals' and 'immortals' (gods, etc); and it would be assumed that the dividing lines are absolutely precise, so that nothing can be 'somewhat' immortal. (Thus we get the famous expository example: All men are mortal; Socrates is a man; therefore Socrates is mortal.) In FL, these dividing lines are imprecise.

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[6] Membership functions can be determined from: special cases about which we have some theoretical knowledge; from historical data; by calibrating the rules implicit in the actions of experienced operators; or by optimisation through some simulated learning process.

(v) Accordingly, FL has neither a LNC nor a LEM (although all the other rules and theorems of CL are carried over into FL). If we believe we have good evidence that red-haired people are hot tempered, DL forbids us to draw any conclusions from this about the temperament of anyone whose hair is merely 'reddish'; FL does not. This enables FL to handle approximate knowledge; it can, for example, process qualitative information about system dynamics that can be described qualitatively, but not expressed as a mathematical function (for an example from economics, see [28]).

Box 3: The Sources of Fuzziness.

The key variables of some systems are inherently qualitative, and cannot be assigned a numerical value, but neither are they discrete qualities. Flavours, scents, and physical textures are good examples [29]. Many other systems have qualitative aspects that can be assigned a numerical value, but only at the cost of severe loss of information, or distortion of meaning; the extent to which an administration is corrupt, the extent to which a particular line of RD is promising, or the balance of success and failure in an ongoing conflict are examples.

The distortion-of-meaning effect is illustrated by the current UK rules for assessing the adequacy of a pension fund to meet its future obligations. Although the rules generate a number that determines trustees' statutory obligations, what sum is *actually* needed is also strongly affected by qualitative, uncertain (not merely risky) factors: the choice of discount rate for the calculation, the interaction of the recent historic trends of increasing life-expectancy and increasing obesity-related morbidity – and of course, the forecast performance of bonds and shares. The classic case of this effect is the relative body-count measure of military success during the US/ Vietnam conflict, which appeared to show that the former was winning the war, almost up to the final, desperate evacuation of the embassy in Saigon in 1975 [30].

Variables that are ostensibly numerical may also be fuzzy, if they are generated by a process that is sufficiently nonlinear to ensure that historical data give a poor and misleading indication of future events [31]; many applications of fuzzy logic concern systems with variables of this kind [32]. It is, of course, an error to assume that, when someone makes a choice, that choice implies specific values for any underlying fuzzy variables; this is only true if the mapping from those variables to actions is one-to-one and invertible, and this is not normally the case for ill-structured and wickedly-structured choices (see Section 4, below).

(v) In CL, truth is formal/ syntactic, depending only the form or pattern of the reasoning; in FL, it depends upon the membership functions, which are empirical, and express our knowledge about the situation. Box 4 looks at an aspect of truth and fuzziness in more detail.

3.3.1 The Implications of Fuzziness.

In the context of methodology, the recognition of fuzziness transforms a number of issues of fundamental importance. For example, it exposes as a fallacy the claim that it is 'logically' impossible to compare the subjective impacts of different economic actions (purchasing goods, paying taxes, etc on different individuals (*cf* [33] with [34]). More basically, fuzzy logic collapses into deductive logic if all the classes (input and output) involved in propositions are discrete [35], i.e., deductive logic is a special case of fuzzy.

This is relevant to the grue paradox. If 'previously unseen emeralds are green when first inspected at or before *T*, but blue thereafter' is rewritten as a fuzzy rule, the input membership functions have to be based on *time*, not colour (since grue and green are currently identical in that respect), and there are two of them, (upto-and-including-*T* and after-*T*), neither one fuzzy. Similarly, the corresponding output membership functions are both single points, specifying a single, defined, non-fuzzy colour (Goodman ignored the possibility of shades of green.) But this represents a scheme of inference from conventional deductive logic!

Goodman wanted to demonstrate that induction can give rise to contradictions, but he did not even manage to escape from the domain of CL into one in which induction is relevant. All he has given us is a piece of legitimate but hypothetical deductive reasoning showing that, if emeralds are grue, then the first emerald newly-inspected after noon tomorrow will be blue. Well, I'll go to the foot of our stairs[7] …

[7] This is a Mancunian expression, indicating that the speaker is only minimally impressed.

Box 4: Truth and Fuzziness.

Haack refers to this method of inference as the *base logic* of FL in her book *Deviant Logic, Fuzzy Logic*, and sees nothing objectionable in it [36]. She argues that fuzzy models, inputs, and the standard processes of fuzzy inference are valid, but takes issue with those exponents of FL who claim that it is *truth* that is fuzzy. However, 'truth' can be used to label the outcome of particular test ('is this less than one metre in length?'), or to define a process ('truth is established by empirical verification'): the results of comparison with a standard, versus the comparison process itself. In the former case, in principle, all statements about the real world are fuzzy, except those that that involve discrete variables; in the latter, we would be saying that the comparison process itself is fuzzy. Can this ever make sense?

I believe that it can. In a later work, *Evidence and Inquiry* [37], Haack makes a convincing case for believing that neither foundationism nor coherentism on its own can justify our confidence in a body of knowledge. Foundationism seeks to derive utterly reliable conclusions by deduction (using CL, of course), from foundations whose truth is guaranteed. The different versions of foundationism take different guarantors for their foundations. These include: revelation; the supposed reliability of certain kinds of empirical data; and the supposed reliability of some introspection. Its appeal is the apparent strength and integrity of the knowledge structure it would give rise to. However, in the context of science, all versions of foundationism are vulnerable to the fact that real world phenomena do not meet the requirements for reasoning with conventional logic, i.e., the potential strength of any chunk of foundationist knowledge is undermined by the inherent weakness of the mortar used to construct it (and the guarantors can always be disputed, of course).

The main established alternative, coherentism, claims that our confidence in a body of knowledge should depend on the *coherence* of the evidence in its favour. Unfortunately, coherence is not easy to define; it has to include scope, to avoid triviality; and the links cannot be constructed with conventional deductive logic, since that would mean that the uncovering of a single error would invalidate the whole. The latter is not such a problem, if you accept the validity of FL, but full-blooded coherentism does also carry the obvious risk of validating internally-consistent fairy-tales. Instead, she suggests that we should adopt a hybrid, 'foundherentism'. In this, we accept that our confidence in the diverse elements of a body of knowledge may vary; different elements may give each other mutual support; different sorts of evidence may carry different weights (either as confirmation or anomaly); and we may have to tolerate, *pro tem*, inconsistencies of the quantum/ relativity kind (e.g., if there is more than one sort of heavily-weighted, 'privileged' evidence). Foundherentism cannot be made operational in a specified field of study, without using FL [an attempt to do this is made in the investigation of economic methodology, of which this Working Paper forms part]. But consider an applied version of foundherentism that relied on both correspondence with certain sorts of empirical data, *and* some strong theoretical beliefs about what is necessarily the case. This will need to use different sub-criteria, with different weights, based on qualitative criteria, at different times; here, the actual process of comparing assertions with the (complex) standard does seem to be fuzzy.

The case of the raven paradox is even simpler. In the domain to which FL applies, the law of the excluded middle does *not* apply. There may be, for example, overlaps between ravens and non-ravens, and between black and non-black. (Ravens might belong to a ring species, as do some of the gulls[8], where there are intergrades between certain apparently well-defined species. And 'black', in the real world, is quite variable – my cat's black fur is brownish-black at certain seasons, and this is more apparent in some lights than others.) As a result, in this domain, we cannot argue that 'all *X* are *Y*' is equivalent to 'all *non-X* are *not-Y*'; here, you cannot even set the paradox up. Presumably, Hempel saw brushing-aside of the difficulties of constructing examples that are both realistic and tractable (under CL) as an innocent approximation, but it is actually quite pernicious. The very difficulty of constructing such examples should, perhaps, have been taken as a warning that the subject matter involved might not always fall into the crisp categories required by CL.

3.4 Domain (IV): The Howling Wastes.

In leaving the heartlands of logic, we entered two domains (the frontier and the badlands, Domains II and III) in which we could no longer be sure of being able to classify accurately the things that we need to reason about; where, indeed, exploring the relevant systems of classification is a major part of gaining an understanding of the world about us. In neither of these domains is there any fundamental disagreement about how to classify entities, or their behaviours and properties. However, in all sciences in times of radical change, when paradigms are being replaced (see below), these matters are disputed. The same is true (most of the time) in the human sciences, and wherever scientific positions have implications for the distribution of obligations, authority, material rewards, or status – as there are in economics, for example.

In such cases, 'rational' thought or behaviour clearly cannot mean, 'that which is sanctioned by conventional logic' – with the restrictions that implies on admissible information. This revives an issue that we met with earlier, the supposed distinctions among Reason, rational, and reasonable (see Section 2, above). In the next Section, I hope to convince you that there is no justification for these distinctions, because 'rational' in the sense of thought and behaviour that comply with CL, and 'reasonable' behaviour in domains where CL is not applicable, are both part of a continuum defined against a scale of increasing

[8] In Britain, herring gulls and lesser black-backed gulls are 'good' species: they look different, and do not interbreed. However, if you trace them around the North Pole, at about the same latitude, you find that there is a series of similar-looking, interfertile sub-species that link the two.

complexity and ambiguity. ('Reason', in O'Sullivan's sense of an embodiment of the very sprit of CL, is an irrelevance.)

4. Problem Structure and the Concept of Rational Action.

Dethroning conventional logic means that we can no longer use it as the touchstone of what is right, reasonable, or rational. This section looks at what we should put in its place. In all spheres of practical activity, it is from time to time necessary to take decisions without sufficient data to make it possible to identify a unique, optimal course of action. In economics, it is widely recognized that most entrepreneurial activity is of this character; and a minority of writers (E.g., JM Keynes, Frank Knight) recognize the distinction between risk and uncertainty proper, and regard choice in both these spheres as irrational or arational [38]. Most decisions in basic research, and many in strategic research are also of this kind; and there is a direct link to the topic of induction, through the necessity to decide which areas to explore with the available resources. (The economists' response has been to ignore the problem, and proceed as though all such questions have identifiable, optimal answers, even where this requires implausible quantities of data [39].)

Nobody believes that, in the real world, such decisions can only be made at random (though some are, of course), but alternatives are thin on the ground. I am claiming that there are styles of conduct, which I will call 'rational', that we can expect to perform significantly better than random choice or intuition in circumstances which are outside the scope of logic and 'economic rationality'[9]; there is a considerable body of management science literature on this topic [40, 41]. The key to the whole issue is Ian Mitroff's concept of problem structure [42].

Mitroff distinguishes three cases. In *well-structured* situations, we are faced with problems whose nature is clear and undisputed, for which there are accepted methods of solution, and which are not affected by value or paradigm differences. Domain I is well-structured. Here, effective conduct lies in finding the best option,

[9] You are economically rational (ER) if, when confronted by a choice, you take the option that promises the maximum net benefit to yourself, taking into account only the current transaction. It is only possible o be ER where hazards arise as quantifiable risk, but not from any more radical kind of uncertainty. If you advocate being ER as an ideal, you are assuming that actions have no repercussions.

and implementing that; and, accordingly, methods for making a selection from a *given* list of options form an important part of the decision-maker's toolkit in this context. We can extract the maximum possible satisfaction from such circumstances through the successful solving of problems, without sacrificing our longer-range goals, or compromising our values.

Mitroff defined *ill-structured* situations as those in which there is sufficient uncertainty and ambiguity that it is possible to dispute the nature of any problem, how to solve it, and what counts as a good solution. This corresponds to Domains II and III above. Here, our effectiveness depends to a large extent on our skill in framing the problem at hand in a potent way, in applying techniques for creatively expanding the range of possible solutions open to us – and, equally important, in managing our choice well, for here, the likelihood of unforeseeable consequences arising from our actions is always relatively high. Smith [43] describes some of the techniques available for this purpose in an R&D context; these are surprisingly similar in function to the measures proposed, in a deliberately anarchistic metaphor, by Paul Feyerabend in his attack on the concept of scientific method, *Against Method* [44][10]. Here, extracting the maximum available satisfaction from the situation may involve the sacrifice of short-run projects, and the modification of longer-range goals. (Very occasionally, attempting to do this may force us to reflect on our values, as well.)

Domains II and III represent complex situations; but there are also hyper-complex situations, in which political conflict is an added ingredient, i.e., there is active dissent about which paradigm and set of values should determine the general pattern of activity in a community, and the allocation of rights, duties, and benefits within it. Such situations are undoubtedly found in science, where changing circumstances have produced a crisis of relevance within a scientific institution (see the work cited in Note [43]). Hyper-complexity corresponds to Mitroff's wickedly-structured domain, where the political skills of building coalitions, neutralizing opponents, gaining control of resources, gaining control of critical posts and functions, moving disputes into more congenial forums, and moulding public opinion, are fundamental to effectiveness. The participants gain satisfaction through two

[10] In attacking a particular method for approaching scientific problems, he ended up putting forward a method of his own.

channels, advancing their systems of values, and advancing their factional interests; short-run objectives and medium-term goals and strategies may well be abandoned or deeply modified in this process [45], and if things go badly wrong, the only source of *any* satisfaction may be through achieving survival, regardless of sacrifice.

5. Defining Science In an Inductive World.

5.1 Defining 'Science'.

The most commonly advocated scientific methodology is falsificationism. Karl Popper [46, 47] said that, as induction is reprehensible and logically dangerous, we should avoid it by being bold and creative in putting forward hypotheses, but that those hypotheses had to be capable of implying (*sensu stricto*) testable consequences. They had to have some consequence of the form, 'if we do *X*, then we shall observe *Y*'. If Y is not observed in the test, the parent hypothesis has to be abandoned; and objective knowledge is supposed to grow by the accumulation of hypotheses that have been *corroborated* by surviving such tests. The more of them, and the more stringent they have been, the greater should be our confidence in the relevant hypothesis. Under this scheme, there can be no such thing as 'proof' (but that is true of all science, anyway). According to Popper, only hypotheses or theories that support such testable implications are scientific.

Falsificationism has a number of very significant drawbacks (see Box 6): it is largely unworkable, and it ignores one real advantage of the much-maligned (but heavily used) confirmationist approach. If induction is rehabilitated, neither falsificationism nor its associated definition – that science consists, exclusively, of the production and testing of falsifiable hypotheses – has any rationale. Science can be better defined as the activity of constructing theories, composed of networks of explanations of reproducible phenomena, through a process of inference to the best explanation (IBEx) [48]. ('Explanation' is, by definition, contrastive: an explanation says why *this* is the case, rather than *that.* Mechanisms that could equally well generate the opposite of what is observed are not explanations [49].) These explanations are based on data that are 'objective' data, i.e., not determined by the whim of individual observers in comparable circumstances. 'Theories' implies that some as-yet unobserved entity exists, that would explain the observations.

Box 6:

The Limitations of Falsificationism.

Apart from the 'Domains' issue identified in the text, the primary problem is that no scientific hypothesis consists of a single, crisply-formulated proposition: the substantive hypothesis itself will always be associated with a complex of other propositions, describing the other elements of the particular science on which the former depends. When some implication of the hypothesis is falsified, there is no way of knowing which parts of this complex are actually at fault. This limitation was strongly emphasized by Kuhn, and Popper was aware of it. (As a logical objection to testing propositions by falsificationism in general, it goes right back to the work of Pierre Duhem, at the beginning of the 20th Century.) At first sight, it might seem that a possible falsificationist response would be to determine the logical structure of the main hypothesis, and attempt to corroborate each part of it, to determine the point of failure. However, since each of these parts is only corroborated, not proven – and not necessarily strongly corroborated at that – this will not work. (There is an additional difficulty: some of the supporting propositions may represent unrecognised assumptions.)

In addition, falsificationism seems to exclude many propositions and activities that would normally be taken as scientific, on the grounds that they are inherently incapable of yielding falsifiable consequences: evolutionary theory, patterns such as that in the Periodic Table, and propositions such as 'all ionic compounds have sharply-defined melting points' are all labelled as unscientific. The line that falsificationism draws between scientific and unscientific seems, for all normal purposes, arbitrary – it even excludes the process of formulating hypotheses.

Finally, the fact of natural kinds suggests one reason why, in reality, scientists prefer confirmationism to falsificationism, despite Karl Popper's confident strictures against the former. Suppose we have what appears to be a uniform population, e.g., a population of species of fungi, all of them parasites afflicting crops; and that we have the hypothesis that all of them contaminate the seed of their hosts, as their sole means of propagation. (This would have important practical implications, and substantial theoretical interest, whether confirmed across a wide sample of species, or shown to have systematic exceptions, *cf* [50].) There is no falsificationist route forward here; the only way is to examine as many different fungus/ host combinations as possible; but seeking confirmation – necessarily by looking at a *wide variety* of cases – will tend to identify any significant sub-categories within what we hypothesized (perhaps with reservations) to be a uniform group. Popper *had* to assume we are always working with a known, pre-existing, crisp classification, in order to be able to apply his chosen 'calculus of rationality', *viz*, conventional logic.

At any one point, there will almost always be alternative explanations for a given set of phenomena, and the choice of which is 'best' will inevitably involve historical and psycho-social factors. Paradigms, research traditions, and research programmes (see Box 7) all represent attempts to explain how they operate. Similar entities have, in particular contexts, been called thoughtworlds, worldviews, weltanshauungen, mindsets, and professional imageries; nor do the theories of the physical and social worlds revealed by ethnographic techniques seem to be of a fundamentally different kind from these pre-scientific structures. Even political factors play a part, either within disciplines [51], or between a group of disciplines and the wider society in which they are embedded [52].

This definition implies a position that I am calling 'constrained realism'. This holds that there is 'truth' 'out there', i.e., some parts of what we perceive as external to the core of our own minds are independent of what those minds think should be there [53]; this is a proposition for which there is strong inductive evidence. Because of the factors outlined above, and because the rate at which a given science can grow depends upon its current size, its coverage of its chosen area, and its current stock of techniques and concepts, the process of gaining a scientific understanding of the world will be a slow and tortuous one. Because it is necessarily based on provisional assumptions, any of which may be overthrown – the Heavens revolve around the Earth; humans belong, mentally, to the genus *Homo economicus*, etc – it may also be marked by major, temporarily disruptive revisions. All this implies that science will not always be able to give conclusive guidance on practical matters. It should not, for this reason, be given a 'final arbiter' status in ill-structured policy choices (and will not be permitted that status in wickedly-structured ones by the actors involved) – although that is, in effect, what has been given to mainstream economics in relation to many important political choices.

It should also be said that some social scientists reject the idea of objective truth, and this position, relativism, is dealt with in the final section.

Box 7: Socio-Psychological Effects In Science.

Thomas Kuhn first gained general recognition of such effects, with *The Structure of Scientific Revolutions* [54]. He showed that, in the various physical sciences, there had been extended periods during which the ruling theoretical model suffered from a growing list of known anomalies; and these were accommodated by increasingly unsatisfactory compromises. Eventually, the pressure of accumulated anomalies and the arrival of a potent new model led to the overthrow and replacement of the incumbent model; it is impossible to compare the state of knowledge before and after one of these revolutions, because the definition of the nature of the discipline has changed so radically (e.g., before and after the Copernican revolution). He explained the persistence of core models, despite such accumulations, as the effect of *paradigms*, visions of the 'true nature' of the phenomena in the particular field, transmitted by the social processes by which scientists are inducted into their profession, that blind their inhabitants to alternative ways of seeing the phenomena in their field, or drawing its boundaries. These visions determine what are taken to be reasonable questions, sound evidence, etc. Anomalies that might undermine the vision are ignored, or neutralized, e.g., by denying the validity of certain sorts of evidence. This tends to give each discipline a protected core of ideas.

Opaque Kuhnian paradigms are a real feature of the scientific scene; see [55]. However, they are not the only entities of this kind. Those who challenge such a vision of reality are putting forward something rather more in the nature of a Lakatian research programme [56]: a vision of a sequence of problems, placed in the order in which they need to be addressed. (The challengers can see the defects of the ruling paradigm, but not the one within which they themselves operate.) This ordering, together with a set of techniques, forms the *positive heuristic* of a RP; each RP also has a protected core, a set of founding assumptions protected by a *negative heuristic*, a set of foundational beliefs that, if there is a conflict, take priority over empirical results. Like Kuhnian paradigms, Lakatian research programmes also do exist in the real world. Most (sub-) disciplines are demarcated by a paradigm – which their members take as a direct view of the very bedrock of reality – and one or more research programmes which their members share. The difference between a paradigm and one of these programmes is that Imre Lakatos did not share Kuhn's belief in the impossibility of individual investigators escaping from their discipline's shared, communal vision of the true nature of its subject matter: Lakatos' programmes (RPs) are adopted, consciously and voluntarily, by the individual researcher, who may switch back and forth between them. He also drew attention to the two types of criteria that working scientists use in making that choice: those they apply to established RPs (solidity and significance of actual achievements), and those they apply to new RPs (promise of solving intransigent anomalies).

Larry Laudan made two further significant contributions to the debate [57]. Firstly, he pointed out that anomalies may be tolerated, if they persist long enough, and if there is no alternative that is capable of doing all that the incumbent theory can do, *and handling the anomaly as well*. Secondly, he presented cases in which a dominant paradigm – and its image of reality – simply faded away, when changes in the wider intellectual culture (in which science is always embedded) eroded the credibility of that image.

6. Objections to Relativism.

I am primarily concerned here with the doctrine that the beliefs of all communities are of equal standing, none being more well-founded than others. (This cuts across many field of interest, not just the sciences, of course.) Relativists assert that we have no right to claim that (for example) the modern geological account of the origin of the major features of the Earth is more true, or arrived at by more rational methods, than accounts that attribute them to conflicts among gods. If some of us justify our preferences for explanation in terms of validity or truth, it is simply that that is how our culture defends its preferences (relativists often ignore the requirement for explanation to be contrastive).

It is, of course, true that what we choose to focus on, and how we interpret it, are strongly influenced by the conceptual structures that we have called paradigms, research programmes, etc. However, to suggest that we can never get beyond these limitations is preposterous: in many areas, we have no strong theoretical preconceptions to direct us; and sometimes, we are confounded by observations that contradict our expectations, or even show us that they were based on a misconception of the nature of the situation; for examples, see [58]. Social forces, whether exclusively within science (e.g., resistance to the prion concept), or linked to the wider social milieu (as seems to have been the case with the virtual taboo on research into group selection and altruism [59]), are strong; but so are those that drive radical change in science. The pioneer who overturns some deeply entrenched belief often reaps substantial professional rewards that more than compensate for the initial obloquy (Stanley Prusiner of prion fame being a fairly recent example), although it is fair to say that many are drawn on by the intense curiosity that is an important (if sporadic) component of human nature. Also, few modern scientific cultures are monolithic: for every entrenched minority view, there is a minority waiting to welcome its overthrow.

Relativists offer three main types of argument. The first is linguistic, and it claims to have demonstrated two principles: that our classification of the world is determined exclusively by the cultural fact of language, and is not based on any independent reality; and that the problems of translating between cultures that use different languages are such that there can be no conclusive debate on what is real and fundamental. The second argument is that formal logic[11] is itself local and culture bound[12]; and the third, that because what we choose to pay attention to and how we interpret it is determined by our culture, there cannot

[11] I am not aware of any relativist literature that attempts to extend this argument beyond CL.

[12] It is, of course, true that culture and history have made an important contribution to the status accorded to CL.

be any core of basic facts and concepts that all informed observers will agree on. A widely-quoted paper by Barnes and Bloor [60] set out these arguments in detail; the first aspect of the linguistic argument is, of course, the Sapir-Whorf hypothesis (which, unfortunately, is widely taken as confirmed fact).

Linguistic relativism (i) The contrast between the sort of constrained realism adopted here on the one hand, and relativism on theory other, is strongly influenced by different views about language. Many years ago, Benjamin Whorf, the anthropologist, wrote that:

> *We dissect nature along lines laid down by our native languages. The categories and types that we isolate from the world of phenomena we do not find there because they stare every observer in the face; on the contrary, the world is presented in a kaleidoscopic flux of impressions which has to be organized by our minds—and this means largely by the linguistic systems in our minds. We cut nature up, organize it into concepts, and ascribe significances as we do, largely because we are parties to an agreement to organize it in this way - an agreement that holds throughout our speech community and is codified in the patterns of our language. The agreement is, of course, an implicit and unstated one, but its terms are absolutely obligatory; we cannot talk at all except by subscribing to the organization and classification of data which the agreement decrees.* [61]

If this is true, it would, of course, eliminate the concept of 'natural kinds'; and because of the reciprocal relationship between classification and theory – what we see is affected by what we think should be there, how we theorize depends on what we think we see – it would undermine any claim to objective theory. However, Whorf is begging the question: it is only justifiable to assume that our ability to parse the world into distinct components gets no help from the fact that there are many distinct kinds of objects out there, provided that you already know that there is no such external (or, at least, independent) reality. Secondly, he is making a major assumption. Learning about social entities and constructs may not all be based on language: we infer a lot about social facts inductively, by our own observations (although we almost certain do not do that in such a regular, accurate way that we all share precisely the same notions, norms, etc).

Linguistic relativism (ii:) the translation problem. As a corollary to the Sapir-Whorf thesis [62], that the languages of different cultures embody such divergent views of the world that communication between them is impossible, Barnes and Bloor ('B&B', from here on) put forward the translation problem. This is the alleged impossibility of knowing that we have successfully translated the meaning of an utterance, document, etc between two languages. But what sort of thing would it be, that could not be translated? It would have to be something that has no concrete manifestations whatsoever (and that includes behaviour and speech), so that it is impossible to establish the fact that, for example, some of what we put in category *X*, they identify as belonging to *Y* and *Z*. As soon as we can do that, we can start to apply inductive methods to build our theories of what 'they' think they are doing, and why – explaining both in our own language. (Indeed, Sapir himself frequently does exactly that [63], for example, in explaining how the Navajo perception of time differs radically from the Western one.) Of course we may always commit an inductive error in the course of this, but that danger is inescapable from investigation in the real world. Relativists seem to be complaining that rationalists cannot prove that they have a guaranteed method of getting perfect knowledge, which, as far as the real world is concerned, does not exist, of course. There are a couple of holes in that particular bucket, however – and the relativists themselves put one of them there, when they threw out (conventional) logic.

Logic. B&B claim that logic is esoteric and *ad hoc*. One of their examples is 'implication', which (in CL) is defined in such a way that a contradiction implies any other proposition, true or false. They go on to state that, if there are any informal intuitive methods of reasoning, they are *ipso facto* without reasoned justification; and that anything that can be justified with such reasoning will therefore not be universal, but merely local in its credibility. To make this work, you have to accept that B&B's original observation, '[some] logics are esoteric and *ad hoc*' supports the proposition that all logics are share that weakness[13]. But this is not the case: for example, fuzzy logic is designed to formalize natural reasoning, and it does so without introducing esoteric new operations or concepts. There are a number of other assumptions here: that only deductive logic (which they are trying to reject!) can provide 'justification', and that there are no universally used sound, informal, and intuitive methods of reasoning.

Lack of a 'core'. Relativists do seem to accept that there are certain basic things that can be learnt in a unique way by induction, so that everybody, everywhere agrees on them (B&B talk about navigation skills, not falling into rivers, etc); and no relativist, as far as I know, has ever claimed that culture can overwhelm the real-world factors that teach us all that (subject to some easily-stated physical conditions) fire burns flesh, for example. Realists (including constrained realists) see such consensus on such matters as a potential bridge for communication between different communities. Relativists deny this possibility, saying that such skills are irrelevant to this bridge-building, because they are shared with other species

[13] Note the important difference between induction based on 'some' (of an extensive sample) and induction based on 'some' (from a sample of one).

that do not possess language, and that it is wrong in principle to mix unverbalized learning with what is communicated by language.

But why should we accept this? In many areas of life, there is continuity across this supposedly impassable barrier between the linguistic and non-linguistic. The most telling counter-example is provided by our ethical behaviour and knowledge. Some of our ethical behaviour does seem to be learnt by 'embedded' induction: the inhabitants of complex systems are under a lot of evolutionary pressure to develop accommodative and even altruistic patterns of interaction with their fellows [64]. This adaptive response to living as part of a complex system is now built into our physiology at a sub-cognitive level: we have the same neural responses – diluted or modified by our closeness to the other – when we see another undergoing a painful, joyous, frightening, etc experience, as when we undergo that sort of experience ourselves. And, having the same neural responses, it is reasonable to infer that we have similar mental responses [65]. In addition, we absorb standards of behaviour; in some cases, this happens through individual learning, and is a matter for cognitive psychology; but some is indeed cultural – and, of that some at least has been absorbed unconsciously. Certainly, some has also been absorbed through language (and other systems of symbols). But where is the impassable barrier between the biological and the social/ linguistic realms here?

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the descriptor 'fuzzy') came out about the same time; and, since then, there has been a whole series of works whose authors no longer accept the unique, near-sacred status of CL, and who are much more interested in how we can think about reality, than what we can discover by CL an introspection alone. These include the works by Haack and Kornblith cited below. The trend is not, of course, universal; O'Sullivan's book represents the earlier tradition.

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[55] Behaviourism and personal construct theory provide the classic example of two paradigms of the same field, whose adherents have no basis for productive debate. Behaviourists (see BF Skinner's *About Behaviourism)* only accept data about observable phenomena as scientific; this excludes intentions, mental states, etc, and thereby automatically identifies PCT theorists as unscientific, and therefore irrelevant. The latter insist that mental states, intentions, etc, are the true subject matter of psychology, so that behaviourism is, equally, irrelevant (see GA Kelly's *A Theory of Personality: Psychology of Personal Constructs*). For an insider's account of psychological impacts of a paradigm shift (in neurophysiology), see Marco Iacoboni's *Mirroring People: The New Science of How We Connect With Others*, p 10*ff.*

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