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Article

Induction and Epistemological Naturalism

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Abstract: Epistemological naturalism dismisses the notion that epistemology is a basis for the empirical sciences. In particular, it rejects the demand for a general justification of induction. Making inductive generalisations is a basic habit among humans. There is no such thing as a logic of inductive inference. The role of induction in science is heuristic; it is our way of inventing new theoretical predicates and developing theories. We discover new laws by applying inductive thinking; but this is not any kind of inference which can be evaluated as more or less rational.

Keywords: induction; naturalism; evidence and justification; epistemic norms; induction and concept formation; induction and discovery of laws

1. The induction problem in the naturalistic perspective

All sciences except mathematics and logic apply inductive reasoning when drawing general conclusions from observed phenomena.[1] Such inferences are ampliative; the conclusion is logically stronger than its premises. By contrast, a logically valid conclusion from a set of premises is not ampliative; the conclusion does not say more than its premises.

Rules for logical reasoning have been studied already since antiquity and there is almost universal agreement about at least the basic logical laws encapsulated by first order predicate logic; disagreement concerns the law of excluded middle and extensions to second order logic and modal logics.

Many philosophers have in a similar vein tried to formulate basic rules for inductive reasoning. The idea has been that, whereas a logically valid inference ends in true statements whenever the premises are true, inductive inferences result in conclusions which are probable to some degree whenever the premises are true. Hence the hope has been that probability theory may be used for this purpose. But, alas, efforts in this direction have been in vain; no theory to this effect have so far survived reasonable criticism. So one is prone to ask; is there really any inductive logic to be found? Is it possible to formulate general and formal rules by which we can justify inductive reasoning?

Hume stated the problem clearly: There are two possible ways for justifying a proposition, either to point out that it follows logically from other propositions held true, or that it is supported by experience. Neither can be used in a general justification of using induction: if we argue that past experiences show us that inductive reasoning quite often is successful and therefore continued use of induction is justified, our reasoning is circular. Neither can logic provide any justification, and since there are no other options, inductive reasoning cannot be given any justification at all.

Many philosophers have tried to rebut Hume's skeptical conclusion, without success in my view. In particular, using probability theory in the efforts to formulate an inductive logic is of no help; one may easily recognise that Hume's original argument still applies when one tries to use probability theory, for the simple reason that probabilities are based on previous experiences.

Popper claimed [1] that science should be based on rationally justified methods, and since induction fails this criterion, science should do without induction. Instead we should adopt falsificationism as the only rational scientific method; we should do our best to try to falsify our

[1] Mathematical induction is not a species of induction, but an axiom in number theory.

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hypotheses, and if the attempts fail we could say no more than that our hypothesis is corroborated. In Popper's terminology the word 'corroborated' means no more than 'so far not falsified'; hence, if we denounce inductive reasoning we cannot say that a corroborated hypothesis is more probable than before, or that we can attribute any degree of belief to this hypothesis.

However, Popper has been criticised for being inconsistent, since he further argued [2, 1192-3] that we have reason to think that of two hypotheses, neither of which is falsified, the one which is better corroborated, i.e., has survived more tests, is closer to truth, have more truthlikeness, than the other. In short, corroboration is a sign of truthlikeness. What is the argument for this thesis? It is no doubt easy to accept that if theory A is closer to truth than theory B, it follows (given some reasonable auxiliary assumptions) that A has higher chance to survive the next test than B. But the converse argument is a species of induction, as Newton-Smith once pointed out. Popper admitted that it is a 'whiff of inductivism' and Newton-Smith retorted 'This is a full-blown storm.' [3, 68]. I cannot but agree with Newton-Smith.

For my own part I have no problem accepting that degree of corroboration is a sign of truthlikeness (supposing that we can give a better analysis of truthlikeness than Popper's failed attempt), but Popper cannot coherently take this stance since he rejected inductive reasoning tout court. It is a telling fact that not even Popper succeeded in formulating a scientific methodology totally devoid of inductive reasoning.

The induction problem is still with us; we use a form of inference, which we see no way of defending. Quine once characterised our situation with his characteristic witty: "The Humean predicament is the human predicament." [4, 72].

As with many other predicaments, the solution is, I believe, to reconsider the tacit presuppositions at work when formulating the problem. My suggestion is to start by asking: why do we want a general justification of induction? The usual answer is that it is the business of epistemology to provide a foundation for the sciences. Science is that kind of human activity, which should fulfil the highest standards of rationality, and that means that we, ideally, should be able to justify our scientific methods. A lot of specific methods are species of induction, hence we need a general justification of induction. Such a stance is part of a common view that philosophy, in particular epistemology, is more basic and fundamental than the special sciences. This view is motivated by the idea that scientific knowledge should be as certain as possible and since certainty cannot be had without a certain foundation of the general principles, epistemology should provide such certainty. But this train of thought is, I think, erroneous. It is based on a rationalistic outlook, the notion that we humans are able to know, a priori, something about the relations between human minds and the external world. I can't see how such a priori knowledge is possible.

Epistemology has traditionally tried to answer the question what we *ought* to believe, not merely to describe what we in fact believe; it has a normative component. It results in recommendations how to proceed in scientific thinking. But do scientists care? No, often not; it seems to me an obvious fact that in cases where philosophically motivated epistemic principles conflict with actual scientific practice, the latter usually wins. Scientists most often take little notice of what philosophers say, and practising scientists never care about the induction problem. Science proceeds well without any justification for induction, which indicates that the search for a priori foundations for knowledge is a mistake.

Established inductive inference principles sometimes result in false conclusions. That is unavoidable, but it naturally triggers the question how to diminish the frequency of mistakes. One obvious example is the recommendation to use double blind tests as much as possible in statistical testing. This has no doubt decreased the risk of making errors when inferring from samples to populations. And it is obvious that the recommendation to use double blind test is based on inductive reasoning. In other words, inductive reasoning is improved by applying inductive reasoning at a meta-level. Rationalist philosophers are prone to criticise this form of reasoning as circular. But I'm not impressed; it seems to be to be a good circle, not a vicious one.

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Should we then stop doing epistemology as a pointless enterprise? I think not. Instead we should reconsider our picture of the relation between philosophy and empirical science. Like many present day epistemologists, I suggest that we adopt a naturalistic stance, which means, as I interpret the term 'naturalism', to view epistemology as part of our scientific and empirical study of the world; epistemology is the study of how the cognitive apparatus of humans works and under what conditions the resulting cognitive states represent real states of affairs. In such an endeavour no factual a priori knowledge is needed.

The old-age problem of rebutting total skepticism could just as well be left aside as a purely internal problem for philosophers; no scientist is bothered, and in fact, not even the skeptics themselves. They continue to live their lives unreflectively acting as if they believe most of the same things as the rest of us.

Traditional epistemology results in epistemic norms. The critic might now claim that, as an empirical study, naturalised epistemology cannot entail any norms and so it cannot do its work. My reply is that it can result in statements of normative form ('Do so and so!'), but that does not entail the existence of a kind of entities, NORMS. We may well accept a declarative sentence as true without accepting that that sentence refers to a FACT and similarly, we may well accept the validity of a normative statement without accepting the existence of any NORMS.

Epistemic principles have the form 'do so and so in order to obtain knowledge', or maybe 'do so and so in order to minimise the risk of drawing false conclusions'. Such statements can be reformulated as conditionals, such as 'if you want to get knowledge, do so and so'. Such a sentence could be the conclusion (an inductive one!) of empirical investigations of our cognitive faculties and earlier failures. Now, our goal of obtaining knowledge are often left unsaid as a tacit condition, since in many contexts it is obvious, and we follow Grice' rule of not saying obvious things. Hence we just utter the consequent, which is a sentence of normative form. Thus, the normative form of epistemic principles can be explained as the result of a transformation from a conditional to a categorical statement with a tacitly presupposed condition 'We want to know'.

Many norms have this character. For example the social norm 'Do not play music loudly if you live in a flat' could be interpreted as tacitly presupposing that people normally want to have good relations with their neighbours and in order not to jeopardise that goal, they should avoid disturbing them. And the conditional is based on an inductive inference from ones own and others' experiences.

In the naturalistic view epistemology is fallible and revisable as all our knowledge. There is no vantage point of view from which to judge whether a particular method is bad or good; such a judgement must be made from within the sciences. The conditionals we believe and express as sentences of normative form, leaving the condition tacit, are the results of empirical investigations and every day experiences.

Naturalists oppose transcendentalism, i.e., the Kantian philosophy of stating a priori conditions for empirical knowledge. Kant was right in insisting that (propositional) knowledge presupposes concepts, but wrong is assuming that the mind and its functional structure could be studied from a transcendental point of view, as if it were something outside the natural world. The naturalist move is to say that our formation of concepts is a natural process, which can be studied by science in the usual way; that is done in cognitive science. (I do not claim, for certain, that naturalists have explained the intentional character of mental concepts; but the naturalist sets his task as precisely doing that.) We humans are part of the natural world and our cognitive capacities are our means of interacting with our environment. And knowledge about our cognitive capacities, for example our ability to form useful concepts and to apply induction, have no a priori status; these capacities are functions in the natural world which we may inquire into by ordinary scientific means.

2. Justification in the naturalistic perspective

Propositional knowledge consists of true, justified beliefs, according to the standard definition. Justification is a relation between beliefs or statements expressing these beliefs; one statement can

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contribute to the justification of another statement. No matter how we analyse the relation, it is obvious that a general demand for justification will result in an endless regress; if B justifies A, one will immediately ask for a justification of B etc. In practice we must stop somewhere and epistemological foundationalists have thought that the endpoints must be some kind of a priori and self-justifying statements.

But is there any a priori ground for empirical knowledge, a set of basic and self-evident statements? I think not; any statement can be doubted; even the simplest observation or the most obvious logical principle can and has been doubted. Now, the sceptic attacks; from the statement 'Any particular statement can be doubted', it follows, he claims, 'All statements can be doubted'. But this is an invalid inference. The premise can be paraphrased as 'For all x, if x is a statement, it is possible that x is false' and the conclusion renders 'It is possible that for all x, if x is a statement, x is false'. This is an invalid inference, no matter which modal logic you adopt. The same point has been made, in a different context, by Davidson:

> Yet, it has seemed obvious to many philosophers that if each of our beliefs about the world, taken alone, may be false, there is no reason why all such beliefs might not be false. The reason is fallacious. It does not follow, from the fact that any one of the bills in my pocket may have the highest serial number, that all the bills in my pocket may have the highest serial number, or from the fact that anyone may be elected president, that everyone may be elected president. [5, 192]

So it is perfectly consistent to say that none of my beliefs are beyond doubt, that anyone might be false, and at the same time hold that most of my beliefs are true. Doubts about a particular belief are based on other beliefs not in doubt.

But how can there be starting points in chains of justification which are not justified? This is, certainly, a problem for traditional epistemology. But in the naturalistic perspective we do not ask for ultimate *justification*; instead we look for intersubjective agreement of observation reports; such agreements make up the empirical basis in the empirical sciences, the endpoints where chains of justification begin. And this fact is the reason why I now change from talking about beliefs to talking about statements/sentences.

We humans are normally able to agree about shared observations. When several people at the same spot and talking the same language observe an event, they normally agree on at least some descriptions of it, so long as no intentional notions are used. Since any observed situation may be described in many different ways, they may disagree about what should be called the most salient description of what happens, but that is another thing. Some descriptions of observed events are agreed to be true.

This does not mean that agreement is a guarantee for the truth of the sentences agreed upon. But it is a basis for empirical knowledge in the sense of a starting point in an ongoing discourse. Rejection of a previously agreed sentence is possible, if coherence arguments, emanating from our background knowledge, against it are strong enough. But this in turn depends on agreement about the truth of other observation sentences.

Intersubjective agreement is not a species of justification, since justification is a relation between beliefs and between sentences expressing these beliefs, whereas agreement is not a species of belief; it is a kind of collective action.

We ask for justification when we doubt a certain statement made. In cases when two or more people at the same spot are able to observe something and agree on the observation, the demand for justification has come to an end. If several people standing in front of, say, an elephant, none would ask for any justification if someone uttered: Look, an elephant! Others would simply agree. If someone disagrees, he would not ask for reasons for the statement made, but simply reject it.

Such intersubjective agreements function as implicit determinations of the extensions of the predicates used in observation reports. This fact is most clearly recognised when we reflect on how

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infants learn language. For example, we learn a little child words for colours by pointing; we point to a number of hues of e.g., blue and say ' This is blue' (if we speak English). Learning to use 'blue' correctly requires repetition, situations where we point at blue things and say 'blue'. After some time the child can correctly identify blue things. No one will ask for reasons. We have learnt the child correctly to use the predicate 'blue'. In other words, we have learnt it the (approximate) extension of this predicate.

The extension of the predicate 'blue' is somewhat vague. How would a child classify a hue between blue and green, if it has only learnt the words 'blue' and 'green'? It depends on its internal dispositions for similarity among colour hues. If the unclear case by the child is perceived as more similar to blue than to green, it will call it 'blue', otherwise 'green'. Thus classifications of perceived objects is determined by spontaneous perceptions of similarities. This is a point made by [6].

One might wonder to what extent people agree on what is more or less similar. Small differences are to expected, but it is a fact about us humans that when it comes to colours people in general agree. (I myself, however disagree sometimes because I'm colourblind, which is a genetic defect affecting almost 8% of men.)

In the first stages of learning one's mother tongue such learning of predicates is common. Wittgenstein argued this point at least at two places in his oeuvre. The first is in §§143-202 of *Philosophical Investigations*, where we find his famous discussion about the notion 'to follow a rule'. He discussed a simple rule of arithmetic, addition, and considered the possibility of explicitly stating rules for its application in particular cases. When so doing we get another rule and the application of this in turn requires another rule. Very soon we find that we just do things without any justification. Wittgenstein arrives at the conclusion in §202: 'And hence also "obeying a rule" is a practice'. The point with this remark is, I believe, that the request for general justification cannot be met and the search for it is a misconception of the task of philosophy.[2]

The second place is remark 150 in [8]:

> 150. How does someone judge which is his right and which his left hand? How do I know that my judgment will agree with someone else? How do I know that this colour is blue? If I don't trust myself here, why should I trust anyone else's judgment? Is there a why? Must I not begin to trust somewhere? That is to say: somewhere I must begin with not-doubting; and that is not, so to speak, hasty but excusable: it is part of judging.

To judge, to express one's beliefs, is to apply predicates. I interpret Wittgenstein as saying that those beliefs/statements which we hold true without justification function as criteria for use of the predicates used in such statements, i.e., as partial implicit definitions of these predicates. And the same applies when we agree on descriptions of what we observe. Observation sentences agreed upon may be viewed as having the function of (partial) implicit definitions of the predicates used in these descriptions. Asking for justification of such sentences is to misunderstand their function.

Every chain of justification ends in such partial implicit definitions; at every moment we unreflectively hold some beliefs while doubting others. This holds true even in logic; if we for example try to justify modus ponens we find ourselves using modus ponens, as is nicely shown by Lewis Carroll in the famous dialogue 'What the tortoise said to Achilles' [9]. The discussion is about a certain inference in Euclidian geometry. Achilles asks the Tortoise to accept the conclusion Z upon the premises A and B:

A: Things that are equal to the same are equal to each other.

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[2] There is an enormous debate about this famous passage in *Philosophical Investigations*. To me it is obvious that Wittgenstein's point is that language usage is open-ended and based on habits. The demand for ultimate definitions of meanings of linguistic expressions is a modern version of the rationalists' demand for fundamental justification of knowledge, a demand that Wittgenstein totally rejects. And we empiricists agree; the demand for ultimate justification is a modern version of rationalism.

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B: The two sides of this Triangle are things that are equal to the same.
Z: The two sides of this Triangle are equal to each other.
The tortoise accepts A and B but do not yet accept the conclusion Z. Achilles and the Tortoise agree that in order to accept Z one need to accept A, B and the hypothetical,
C: If A and B are true, then Z must be true.
So they agree to make this completely explicit by writing in a notebook:
A: Things that are equal to the same are equal to each other.
B: The two sides of this Triangle are things that are equal to the same.
C: If A and B are true, then Z must be true.
Z: The two sides of this Triangle are equal to each other.
Achilles now maintains that logic tells us that Z is true. However, Tortoise still expresses doubts about Z and Achilles then repeats the move. He asks the Tortoise to accept:
D: if A, B and C are true, then Z must be true.

Tortoise now accepts A, B, C and D, but he still expresses some doubts about Z. Achilles once more repeats his move and the dialogue continues infinitely.

The point Lewis Carroll wanted to make, was, I think, that we cannot really say that the general rule modus ponens *justifies* its instances. Rather, the inference rule modus ponens must be seen as a *description* of how we in fact use the if-then-construction. The naturalist has only to add that this is our way of thinking and talking.

If someone would fail to use the if-then-construction correctly the only thing one can do is to give examples of its use; fundamental rules cannot be proved. Hence, by explicitly accepting modus ponens as a valid inference is the same as accepting it as an implicit definition of the sentence operator 'if..... then.....'. Similarly, many basic beliefs, when expressed as sentences held true, function as implicit definitions of predicates occurring in these sentences.

In science we often introduce new predicates, i.e., scientific terms, in this way. To my knowledge, the earliest example is the introduction of the concept of mass. In *Principia* Newton explicitly introduced the word 'mass' as short for 'quantity of matter'. This expression in turn was 'defined' in the very first sentence of *Principia*: "The quantity of matter is the measure of the same, arising from its density and bulk conjointly." But this formulation is, I believe, a rhetorical move against Descartes, who held that quantity of matter is volume, for one is immediately prone to ask how Newton defined 'density'; obviously he cannot, on pain of circularity, define density as mass per volume unit.

The real empirical basis for the introduction of the term 'mass' is the discovery of conservation of momentum made by John Wallis, Christopher Wren and Christiaan Huygens almost 20 years before the publication of *Principia*. Newton extensively rehearses their findings in the first *Scholium* (after Corollarium VI) in *Principia* and it is clear that this is the empirical basis for the introduction of the predicate 'mass'.

Wallis, Wren and Huygens had, independently of each other, found that two colliding bodies change velocities in constant proportions such that an increase in velocity of one body is proportional to a decrease of velocity in the other body. Thus $\Delta v_1/\Delta v_2 = constant$, which can be written as $k_1\Delta v_1 = -k_2\Delta v_2$. One only need to chose a body as the mass unit in order to attribute a definite mass to each body. Consequently, Newton introduced the quantity of mass as a constant attributable to each body. So our formulation and acceptance of the law of momentum conservation applied to two colliding bodies, i.e., $d/dt(m_1v_1 + m_2v_2) = 0$ is at the same time a generalisation of observations and an implicit definition of mass.

Those basic beliefs, which constitute endpoints in instances of chains of justification, concern many sort of things, such as observable objects in the vicinity of the speaker, the meaning of sentences uttered by other people, common opinions about nature and society etc. Popper, by the way, was fully aware of this; in his methodology he introduced *basic statements* as a set of truths taken for granted

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for the time being in a particular case of testing a hypothesis. But a basic statement can be doubted if other things appear more safe and consistency requires that something is given up.

These basic beliefs are mostly taken for granted as part of the background to our discourse and interaction with other people. For example, it is a non-negotiable fact about us humans that we take it for granted that ordinary things in our vicinity, including other people, have roughly the same properties from one moment to another and will respond roughly in the same manner. We also take it for granted that other people in general mean something with their words and we do not really doubt that our fellow humans think, have feelings and experiences more or less similar to ours. We follow a large number of rules, linguistic as well as non-linguistic, in every moment of our waking life without really justifying them. This is our way of being as animals with linguistic competence.

I see a resemblance between Carroll's and Wittgenstein's stance on ultimate justification. And, of course, the idea traces back to Hume's position in *Treatise* when he discussed the skeptic's doubt about the veracity of our immediate experiences of external objects. Hume concluded that a convincing argument cannot be given, but it does not lead to doubts about the existence of external objects:

> Thus the sceptic still continues to reason and believe, even tho' he asserts, that he cannot defend his reason by reason; and by the same rule he must assent to the principle concerning the existence of body, tho' he cannot pretend by any arguments of philosophy to maintain its veracity. ....We may well ask 'What causes induce us to believe in the existence of body?' but 'tis in vain to ask Whether there be body or not? That is a point, which we must take for granted in all our reasonings.' [11, 238]

Thus Hume did not aspire to justify that our experiences are caused by external objects. Instead he stated that it is an empirical fact about us that we do believe that our perceptions are perceptions of external physical objects and we do believe that these objects may cause each other's motions. It belongs to our nature to assume that external objects exist and cause our impressions. One may say that, in Hume's view, someone who claims to be skeptical concerning the existence of external objects and other mundane things is not serious; he professes skepticism, but that is just empty talk. Hume's stance is the first exposition of epistemological naturalism.

The most explicit proponent of naturalism is Quine. The common trait in Hume's and Quine's position is the stance that justification of beliefs from a vantage point outside the realm of empirical knowledge is impossible. The difference between Hume and Quine is that Quine thinks it possible to give a scientific explanation of the interaction between our mind and the external world, whereas Hume is satisfied without such an explanation, he just notes that certain ways of thinking belongs to our nature.

The attempted scientific explanation of the interaction between the external world and the mind is by Quine seen as epistemology in a new key. Epistemology is thus not the foundation for empirical knowledge, but an integrated part of it; as Quine puts it, 'a chapter of psychology and hence of natural science.' [4, 82]

Had Hume endeavoured to justify the veracity of our impressions by somehow arguing that they reflect the real nature of things, he would have ended in rationalistic metaphysics, thereby contradicting his own empiricist principles. For my own part I would say that naturalism is the logical development of empiricism.

3. Epistemology without foundation

Epistemological foundationalists argue that there must be endpoints in chains of justification, statements that we accept as certain without them being justified by something else. In older times some such statements were called 'self-evident', but this label has come into disrepute; there is, for example in the history of mathematics examples where we now dismiss as false statements once held to be self-evident. (One example is Euclid's axiom that the whole is greater than any of its parts.)

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It is obvious that there must be endpoints of justification, but foundationalists' mistake is to conceive these endpoints as certain *knowledge* in the sense of justified true beliefs. As already indicated, one may reasonably hold that in many cases those endpoints are statements that function as implicit definitions of predicates. Wittgenstein's insight in *On Certainty* was that such statements are not knowledge in the traditional sense of this word, while we nevertheless are certain about their truth. When we accept as true a statement which lacks any kind of justification we in fact treat it as an implicit definition. This is the core point of *On Certainty*

This is true both in ordinary and scientific language. But we should not assume that these endpoints of justification forever will be conceived as such. It is possible to change what we treat as definition and what we treat as empirical statements. This was, I think, one of the points Kuhn wanted to make in his [12].

By viewing certain statements as implicit definitions of terms used in these statements we also shift focus from the individual to the communal perspective. I think it has been a big mistake to focus the epistemological discussion about reasons for *beliefs*, i.e., whether an *individual* has, or may have, reason for his/her beliefs. Both in science and in ordinary life we interact by talking to each other expressing our beliefs. What we may discuss is whether intersubjectively available things, *utterances*, are true or not. Epistemology is fundamentally a social endeavour; we search for items that can be agreed upon as evidence. The fundamental question is which *sentences* we may agree upon and take as basis for further discussions; talking about beliefs is a side issue. Davidson expressed a somewhat similar criticism of much of epistemology in his *Epistemology Externalized*.

When we decide to accept a certain statement as true we at least sometimes do that after considering what other people say about the matter. The ultimate evidence for any theory consists of its empirical consequences found true after comparisons with observation reports. Empirical consequences are statements derived from the theory and describing observable states of affairs; these empirical consequences are compared with observation reports agreed upon by those which in fact observed the outcome of the experiments, for example the pointer states of measurement devices. Such observation reports are neither self-evident, nor justified by other sentences. But they are agreed upon by several observers; that's why they are counted as evidence for, or against, theories.

Some might claim that naturalism leads to a vicious circle in epistemology: we gain knowledge about our own knowledge process through precisely that very knowledge process we are describing. I agree that it is a circle, but it is not a vicious one. It is more like the hermeneutic circle: by an ever-increasing inquiry we constantly widen the circle in order to make it as vulnerable as possible to empirical constraints.

4. Evidence and justification

A core problem in epistemology has always been to analyse how the external world relates to our beliefs. This is but one aspect of the mind-body problem. Sellars has formulated it as how to relate the space of causes (i.e., the external world) to the space of reasons, our internal world. It seems indeed reasonable to say that external states of affairs may cause some or our beliefs. But it is immediately obvious that we must *recognise* a state of affairs before it can cause any belief, and since the term 'recognise' belong to the space of reasons, it describes mental acts, we have not really made any progress.

The term 'evidence' is often used to relate items in the space of causes to items in the space of reasons; an object, a state of affairs, a data set, all may be called 'evidence' for the content of a belief and the sentence expressing that belief. (I'm not claiming that using 'evidence' is the solution to the epistemological problem; I'm just describing how we use this term.). Typically, we say that things observed constitute evidence for observation reports. An observed amount of DNA found on a knife by which a person has been murdered, is strong *evidence* for the proposition that the murderer has that DNA. We believe it certain that the person having this DNA has touched the knife, and the sentence 'the person with this DNA has touched this knife' then *justifies* the sentence 'that person

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was the murderer'. (Most murderers are men.) We see here that usage has it that 'justification' relates things that are truth-apt, whereas 'evidence' also may relate a fact or state of affairs to something that is truth-apt. Thus the two-place predicate 'justifies' relates things that are truth-apt, i.e., sentences, propositions or contents of beliefs, while 'evidence'. also may relate something not being to truth-apt to something being truth-apt. Hence if A justifies B, then A is evidence for B, but the converse is not always true. The claim that something, an object, a data set or an observation, is evidence is defeasible; another piece of evidence may overrule it.

How do we know that A is evidence for B in those cases where A is an object with a certain property and B is a sentence? Just as in the case of learning colour words, it is a matter of learning to use the word 'evidence'. My observation of a person being at a particular place and time is evidence for the statement that that person was at that particular place at that particular time; This may be taken as a partial implicit definition of 'evidence'.

By paraphrasing Lewis Carroll's dialogue about modus ponens I indicated that the situation is similar in logic, i.e., no ultimate justification is to be found. Even logicians are sailors on Neurath's boat.[3] However there is a difference between deductive and non-deductive reasoning. Deductive principles are valid independent of context of application, whereas no such general principle applicable for all cases of non-deductive reasoning can be found. Non-deductive reasoning can only be appraised case by case, in specified contexts. (We need not here distinguish between induction and abduction, both are non-deductive forms of inference.)

5. Induction in the naturalistic perspective

In the naturalistic view the problem of induction is thus *not* that of justifying induction in general. But it is obvious that we do not consider all instances of inductive thinking equally good; we have strong intuitions that some conclusions are much more reliable than others. Hence, we should reformulate the induction problem as the task of describing more thoroughly our inductive practices and to give an account of the methodological role of induction in our scientific work. We should try to explain why we think that certain inductions are more trustworthy than others.

This is roughly Goodman's way of viewing the matter in his [13]. More precisely, he asked what kind of predicates is used in (normal) inductive reasoning. To illustrate the problem he construed the artificial predicate grue, defined as true of things examined before some time in the future, AD 3000 say, and found to be green, or examined after AD 3000 and found blue. All emeralds so far examined are thus both green and grue. Without further constraints simple induction tells us that we have equal reason to assume that the first emerald to be examined after the year 3000 will be green as well as grue, i.e. blue. One prediction, at least, will ultimately fail and we all believe that emeralds will continue to be green. But why? This is the induction problem in the new key.

Goodman's formulation of the problem is that some predicates are projectible and some other not. Obviously, we need to know the conditions for a predicate being projectible. Goodman suggested that the notion of entrenchment could be used in order to distinguish between projectible and non-projectible predicates. But why do some predicates become entrenched? Goodman gave no answer.

The naturalist will here propose an evolutionary explanation: we humans have in the course of time evolved certain cognitive habits, viz., those that have made us more apt for survival and reproduction. We have invented concepts, which we use in predictions of future events, and sometimes this is successful. Let us consider colour concepts; they are not a priori in the sense of being innate, independent of any experience. They are not even universal; different cultures divide the colour spectrum differently, see eg. [14] and [15]. There are languages which have two, three, or four colour

[3] Cf: 'Wie Schiffer sind wir, die ihr Schiff auf offener See umbauen müssen, ohne es jemals in einem Dock zerlegen und aus besten Bestandteilen neu errichten zu können. Otto Neurath: Protokollsätze. In: Erkenntnis. Band 3, 1932/33.

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words only. Consider for example people talking a certain language which do not distinguish between green and blue, (and there are such people according to [14]); they have one colour word for all hues from blue to green, let us call it 'tribegrue'. How should we express Goodman's problem in their language? One option is to translate all three colour words, 'blue', green' and 'grue' as 'tribegrue', in which case Goodman's point is lost. Hence, Goodman's point may be interpreted as showing that predicates are cultural phenomena and what from one background appears artificial from another background appears natural.

Even though Goodman did not answer his own question, his analysis is a step forward, because he replaced the quest for general justification of induction with the more empirical question 'under what conditions can a particular instance of induction be expected to be successful?' However, stating the problem in terms of the distinction between projectible and non-projectible predicates, taken one at a time, is not satisfying. Goodman overlooked a crucial component in describing the situation, viz. the identification of the referents of the singular terms used in our observation statements. When we for example ask which predicate to use in generalisations about emeralds, green or grue, we should also consider the rules we follow in identification of emeralds. The question is thus not which single predicate, green or grue, to use in a particular case of inductive reasoning, but the correct pairing of predicates. In the sentence 'This emerald is green' we have two predicates, 'emerald' and 'green'. Obviously, we use a predicate, 'emerald', in the identification of the referent of the noun phrase in the sentence.

In general, any inductive conclusion has the form 'For all x, if Ax, then Bx', hence the real question cncerns the relation between the predicates 'A' and 'B'. The induction problem can now be reformulated as: for which pair of predicates A and B is it reasonable to expect that if an object satisfies A, it also satisfies B?

In the case of grue or green emeralds it is rather simple. We know that emeralds consist of the mineral beryl contaminated with chromium. This metal makes the mineral green, according to fundamental physical laws. The necessary and sufficient condition for something to be an emerald is that it is a gem made up of beryl containing chromium. The same condition entails, via scientific laws, that it is green, independent of time. Hence if something satisfies the predicate 'emerald', it satisfies also the predicate 'green'.

I rely here on the concept 'physical law' and on the fact that laws are justified empirically, by being generalisations of observations. Hence the argument depends on previous inductions. This is no vicious circle as already pointed out.

Speakers of a language which do not distinguish between green and blue will of course say that all emeralds are tribegrue and this inductive conclusion is correct, if the conclusion that all emeralds are green is correct. So long as they have no practical need of distinguishing blue from green objects, their use of 'tribegrue' is successful and in no need of improvement.

Perhaps members of this community at some time will find it useful to make finer colour distinctions. History of science is full of conceptual developments of this kind, the most obvious cases are perhaps introductions of finer distinctions between diseases. Two diseases may have similar symptoms but very different aetiology, in which case it is necessary to keep them apart as different diseases if one wants to prescribe cures.

Suppose we have observed a regularity in nature: So far, all observed objects are such that if they satisfy a predicate A, they also satisfy another predicate B. Let us assume that both predicates are expressions taken from our vernacular without using scientific theory. We thus have two options: either to assume that the regularity so far observed is a mere coincidence, i.e., an accidental generalisation, or else to assume that it reflects a hidden structural feature. Taking the first option is to guess that sooner or later will we hit upon a counterexample. The second option is to guess that the generality 'for all x, if Ax, then Bx' is true. If this is correct, we have found a natural law.

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Suppose we have found a natural law by inductive reasoning. Isn't the existence of such laws a bit astonishing: Why is it the case that an indefinite number of objects satisfy two logically unrelated predicates? Is not the most reasonable assumption that the probability for such a state of affairs is zero?

History of science suggests two ways of explaining such regularities. The first possibility is to derive the regularity, or some version close to it, from a set of more fundamental and independently acceptable principles. A telling example is the general law of gases. This law began life as Boyle's observation that the product of pressure and volume of a portion of gas is constant. Later, Jacques Charles in 1787 and Joseph Lois Gay-Lussac in 1808, found that this constant depends on temperature and still later the complete general law of gases was formulated when the concept of mole was available. For some time this law appeared to be an empirical regularity, a brute fact. However we now know that it can be derived from the principle of energy conservation, given the identification of absolute temperature as mean translational kinetic energy among the particles making up the gas. So it is not just an empirical fact that the two open sentences 'x is a gas' and 'the pressure, volume and temperature of x satisfies the equation pV=nRT' are both satisfied by the same objects. It follows from a basic principle, given some auxiliary assumptions.

This brings us to the second way explaining the remarkable fact that an indefinite number of objects all satisfy two unconnected predicates. Many scientific predicates start their lives as part of our vernacular, 'energy' and 'force' are two obvious examples. As science advances vague notions are sharpened and changed into scientific predicates with explicitly defined criteria of application. And, of course, many new predicates are introduced by implicit or explicit definitions. The crucial point is that in this process of conceptual development a well-established regularity is normally not given up. Suppose we have such a well-established generality, 'for all x, if Ax, then Bx', and hit upon a putative counter example, an object which satisfies A but not B. Logically we have two options; either to drop the regularity and accept it being falsified, or to change the criteria of application of the predicate A so that the putative counter example can be excluded.

A simple example of the latter is the history of the concept of fish. Aristotle had observed that dolphins have lungs, that mothers gave birth to living offspring and fed them with milk, hence he clearly recognised that they were not fishes. (He classified dolphins, porpoises and whales in the genus *cetacea*.) But his insights were forgotten and for a long time these mammals were classified as fishes. But fishes have gills, while cetaceans have no gills, so how to resolve this conflict? It was John Ray (1627-1705) who in his [16] finally recognised that dolphins, porpoises and whales are not fishes. Thus our predecessors did not give up the generality 'all fishes have gills', instead dolphins were reclassified as not being fishes. The intuitive criteria for being a fish, 'animal swimming in the seas with mouth, fins and eyes', or something of the kind, were sharpened by additional clauses.

Another example is provided by the atomic theory and in particular the law of definite proportions. This law says that all elements have atomic weights which are integer multiples of the atomic unit, equivalent to the weight of a hydrogen atom. However, soon after the formulation (beginning of 19th century) of this law it was found that the atomic weight of chlorine is 35.5, indicating that chlorine in fact do not consist of a certain number of atomic units. But the law of definite proportions was not given up; instead one guessed, correctly, that chlorine samples extracted from naturally existing compounds is a mixture of two isotopes with different masses, Cl-35 and Cl-37, hence naturally existing chlorine is not really one single substance but two and the average weight of chlorine in naturally occurring mixtures is the weighted mean of Cl-35 and Cl-37. Thus, identification of substances were improved.

These are two examples of a possible and sometimes reasonable strategy, viz., to keep the regularity and redefine the criteria of application of the predicate in the antecedent. New counter examples might trigger new adjustments of criteria of application of predicates. The logical endpoint of this process is reached when the set of necessary conditions for satisfaction of the predicate in the consequent is a subset of those for the predicate in the antecedent; in such a case no further counter

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example is possible, in which case we have arrived at a fundamental law. Hence, a fundamental law is a (partial) implicit definition of one of the predicates in the law sentence.

One may observe that I here use 'fundamental law' in an epistemic sense. In an axiomatic exposition of a theory quite other laws may be chosen as fundamental, since such a choice may result in the most elegant exposition of the theory in question. This might be called the logical sense of 'fundamental'.

Inductive reasoning is intimately connected with theory development, but both inductivists and falsificationists have told a distorted story. The inductive process also involves concept development. Inductive reasoning is our way of finding out the structure of the world. The success of empirical science and in particular the usefulness of induction is explained fundamentally in the same way as other evolutionary processes; it is the result of adaptation and competition, in this case adaptation of concepts to the way the world is and competition among theories.

Summarising the argument, the answer to the question above is that the predicates are in fact conceptually dependent on each other, or can be so made; either by deriving the regularity from fundamental laws or else are the criteria of application for the predicate in the consequent a subset of those for predicate in the antecedent. This argument applies, of course, not to ordinary language, only to a well structured scientific theory. So called 'laws' expressed in ordinary language are most often not strict regularities.

It is obvious that this procedure of refinement of criteria for application of concepts, when repeated on more and more abstract and general levels, ends in a set of fundamental laws and principles which are accepted as fundamental without being derived from other principles. Now the same question recurs, with still stronger force: how is it possible that an indefinite number of objects satisfy two logically unrelated predicates? And the answer is still; these fundamental laws function as implicit (partial) definitions of theoretical predicates.

6. Induction as a heuristic device

The picture emerging from all this is that induction should not be seen as particular form of reasoning for which one need independent and non-empirical justification, but as an heuristic device in theory construction. We observe in a number of cases a regularity using two more or less well-defined predicates. Sometimes we believe that the observed regularity reflects a structural feature in nature. This naturally induces the scientist to try to invent a theory which reflects this structural feature and the goal is reached when the theory entails the empirical regularity or some formulation reasonably close to it.

What I have just said resembles to some extent what Aristotle claims in *Posterior Analytics*. According to Hankinson [17, 168], Aristotle's word '$\epsilon\pi\alpha\gamma o\gamma\epsilon$ (epagoge) which usually is translated 'induction', should not be interpreted as an inference principle, but rather as a causal term:

> The method in which we arrive at first principles is called by Aristotle 'epagoge'. Starting from individual perceptions of things the perceiver gradually, by way of memory, builds up an experience (empeiria), which is 'the universal in the soul, the one corresponding to the many' (Posterior Analytics 2.19.100a6-8); and it is this which provides the arche, or first principle:
>
> 'These dispositions are not determinate and innate, or do they arise from other more knowledgeable dispositions, but rather from perception, just as when a retreat take place in battle, if one person makes a stand, another will too, and so on until the arche has been attained.' (2.19.100a9-13)
>
> This process gives us universals (such as 'man') without which we cannot utter assertoric sentences, which in turn lead to higher-order universals, such as 'animal' from particular species. (2.19.100a15-b3). It is described in causal, not inferential terms (which is why 'induction' is misleading): the world simply impresses us in such a way that we come to

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> internalize ever wider and more inclusive concepts. We are by nature equipped to take on form in this way; if we are diligent and unimpaired, our natural faculties will see to it that we do so. Thus in a relatively literal sense we just come to see that Callias is a man and, ultimately by the same process, what it is to be a man.

Hankinson here in fact says that Aristotle was a naturalist in the sense here given, and it seems to me that he has evidence for this interpretation. Furthermore, Hankinson's remark that 'we just come to see that Callias is a man and, ultimately by the same process, what it is to be a man', is another way of saying that endorsing the truth of the sentence 'Callias is a man' is to hold that that sentence is a partial implicit definition of the predicate 'man'. It is thus clear that no justification for this sentence is needed, or indeed possible.

Who is to say, in advance, that a particular inductive conclusion is justified or unjustified? In retrospect we can say of a particular inductive step and the resulting theory that it was successful or unsuccessful and hence in a sense justified or unjustified as the case may be. But we cannot decide that in advance. In this perspective, to ask for a general rule for accepting/rejecting an inductive generalisation would amount to assume that we could know a priori the structure of reality and the future development of a scientific theory. A traditional metaphysician might think that that is possible, but a naturalist does not.

A critic might say that all this presupposes what should be proved, viz. that nature is regular and not completely chaotic. The account makes only sense if there really are regularities to be found. I agree that a general faith in the regularities is presupposed, but that is also part of the naturalistic view-point. If nature were not sufficiently stable over longer periods of time, no biological evolution could have taken place and we would not be here to ask questions. The problem is not to justify the general assumption of regularity, since the demand for such a justification, again, is precisely what the naturalist rejects. Instead, the task is to discover which *particular* regularities there are in nature; that there are such regularities can be inferred from the fact that we human beings are here asking these very questions. Answering these questions is precisely the task of natural science.

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