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Induction and Epistemological Naturalism

Lars-Göran Johansson

Department of Philosophy, Uppsala University, 75126 Uppsala, Sweden; lars-goran.johansson@filosofi.uu.se

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Abstract: Epistemological naturalists reject the demand for a priori justification of empirical knowledge; no such thing is possible. Observation reports, being the foundation of empirical knowledge, are neither justified by other sentences, nor certain; but they may be agreed upon as starting points for inductive reasoning and they function as implicit definitions of predicates used. Making inductive generalisations from observations is a basic habit among humans. We do that without justification, but we have strong intuitions that some inductive generalisations will fail, while for some other we have better hopes. Why? This is the induction problem according to Goodman. He suggested that some predicates are projectible when becoming entrenched in language. This is a step forward, but not entirely correct. Inductions result in universally generalised conditionals and these contain two predicates, one in the antecedent, one in the consequent. Counterexamples to preliminary inductive generalisations can be dismissed by refining the criteria of application for these predicates. This process can be repeated until the criteria for application of the predicate in the antecedent includes the criteria for the predicate in the consequent, in which case no further counterexample is possible. If that is the case we have arrived at a law. Such laws are implicit definitions of theoretical predicates. An accidental generalisation has not this feature, its predicates are unrelated. Laws are said to be necessary, which may be interpreted as ' "Laws" are necessarily true'. 'Necessarily true' is thus a semantic predicate, not a modal operator. In addition, laws, being definitions, are necessarily true in the sense of being necessary assumptions for further use of the predicates implicitly defined by such laws. Induction, when used in science, is thus our way of inventing useful scientific predicates; it is a heuristic, not an inference principle.

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

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1. The Induction Problem in the Naturalistic Perspective

Inductive, i.e., non-deductive, reasoning is a core feature of both everyday reasoning and empirical science. Can it be justified? Hume famously answered NO. There are two possible ways for justifying a statement, i.e., to give arguments for its truth; either to point out that it follows logically from other statements 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 general justification at all.

Many philosophers have tried to rebut Hume's conclusion, without success in my view. So, for example, several philosophers have entertained the hope that probability theory could be used to overcome Hume's skeptical argument; but one may easily recognise that Hume's original argument still applies for the simple reason that probabilities are based on previous experiences.

Reliabilists have tried to evade the circularity critique by suggesting that meta-beliefs about first-level reliably produced beliefs can be reliably produced in part by the very processes that formed the first-level beliefs. However, how do we know that a certain belief forming process is reliable?

Normally we use induction when obtaining knowledge about probabilities, thus still using circular reasoning. The alternative is to rely on a priori knowledge about some probabilities, but that is not acceptable for any empiricist.

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 wittiness: "The Humean predicament is the human predicament." ([1], p. 72).

As with many other predicaments the solution is, I believe, to reconsider the tacit presuppositions at work when formulating the problem. The most basic presupposition when stating the induction problem is that we need an independent foundation for knowledge and science and that, I think, is wrong.

Many people believe a general justification of induction is required because they assume it is the business of epistemology to provide a foundation for the sciences. Science is that kind of human activity that should fulfil the highest standards of rationality, which 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.

However, 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. However, I cannot see how such a priori knowledge is possible; no good reason can be given for the assumption that we humans have a non-sensoric faculty by which we can obtain knowledge about nature. Hence no purely a priori principle with empirical content is in our reach. Epistemological naturalists like myself thus reject the presupposition that epistemology is, or could be, a non-empirical inquiry into the foundations of knowledge and science, a first philosophy.

Should we then stop doing epistemology? 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 a priori knowledge with empirical content is needed.

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, 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.[1]

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 ellipsis, the tacit presupposition being '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

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1 I endorse the deflationary view on truth; saying that $\ulcorner p \urcorner$ is true, is the same as asserting $\ulcorner p \urcorner$ and by that one is only forced to say that the singular term(s) of $\ulcorner p \urcorner$ refer to object(s): it does not follow from the truth of a sentence that it refers to a *fact*, nor that its general term refers to a universal. Hence no further commitments about the truth-predicate is called for.

them. In addition, 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.

2. Justification in the Naturalistic Perspective

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

As already pointed out, epistemological naturalists do not believe that there are any *a priori* grounds for empirical knowledge; no set of basic and self-evident statements with empirical content can be found. Hence, any statement can be doubted; even the simplest observation or the most obvious logical principle can be 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'. However, 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 as '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 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.[2] ([2], p. 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. Furthermore, doubts about a particular belief are based on other beliefs not in doubt.

However, 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. In addition, this fact is the reason I prefer to talk about statements/sentences instead of beliefs.

Rationalistists follow Descartes' search for ultimate justification in terms of subjective certainty. That is a big mistake in my view; as basis for knowledge we need *intersubjective agreement* about statements, both in science and in our everyday interactions with the environment, including other persons.

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[2] Davidson's premise can be formalised in quantified modal logic as 'For all x, if x is a bill in my pocket, it is possible that x has the highest serial number.' and the conclusion 'It is possible that for all x, if x is a bill in my pocket, x has the highest serial number.' Hence, it has the same logical form as the sceptical argument.

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.

The agreement is about the utterance, not about what it means; two persons may agree to assent to an utterance while interpreting it differently. In other worlds, they may have different beliefs. What they agree on is that the predicate in the uttered sentence is true of the object referred to by the singular term in the utterance (or the ordered set of objects, if the predicate is many-placed); but beliefs about the meaning of the predicate may differ.

Agreement is not a guarantee for the truth of the sentences agreed upon. However, 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. However, this in turn depends on agreement about the truth of other observation sentences.

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 infants learn language. For example, we learn a little child words for colours by pointing; we point to several 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 agrees with competent language users about which things to classify as blue and which not. In other words, we have learnt it the (approximate) extension of this predicate. As with all learning, it is an inductive process. No one will ask for reasons.

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 Quine [3].

This is the way we begin to learn predicates in our mother tongue. Wittgenstein argued this point at least at two places in his oeuvre. The first is in §§143–202 of *Philosophical Investigations*, [4] 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: 'In addition, 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.[3]

The second place is remark 150 in [5]:

[3] 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. In addition, we empiricists agree.

> 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 do not 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, such as the observation report 'This is my right hand.', function as criteria for use of the predicates occurring in such statements, i.e., as partial implicit definitions of those predicates. Asking for justification of such sentences is to misunderstand their function. The same is true of some theoretical sentences, the fundamental laws; the predicates occurring in such laws are implicitly defined by us accepting those laws as true, as will be exemplified in a moment.

Every chain of justification ends in 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' [6]. 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.
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, explicitly accepting modus ponens as a valid inference is the same as accepting it as part of an implicit definition of the sentence operator 'if...... then.......'.[4] Similarly, many basic beliefs, when expressed as sentences held true, function as implicit definitions of predicates occurring in these sentences.

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[4] This is explicit in natural deduction, where the logical constants, for example 'if....then...', each are defined by two rules, on for its introduction into discourse, one for its elimination.

In science we introduce many new predicates, i.e., scientific terms, in this way. One early example is the introduction of the predicate 'mass'. In *Principia* [7] 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.' However, 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 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 some 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 their velocities in constant proportions, i.e., $\Delta v_1/\Delta v_2 = constant$, which can be written as $k_1\Delta v_1 = -k_2\Delta v_2$. (Their velocity changes have opposite directions.) One only needs 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. One may say that mass is that quantitative attribute m being such that when two bodies collide, the equation $m_1\Delta v_1 = -m_2\Delta v_2$ is true.

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 sceptic's doubts 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.' ([8], p. 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 sceptical concerning the existence of external objects and other mundane things is not serious; he professes scepticism, but that is just empty talk. Hume's stance is the first exposition of epistemological naturalism.[5]

The most explicit proponent of naturalism is Quine [1] (p. 82). 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 belong to our nature. For my own part I would say that naturalism is the natural development of empiricism.

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5 There are other passages in Hume's writings that not so easily fit into a naturalist stance. However, I, and other naturalists, do not claim to give a coherent interpretation of all Hume's writings; we only point out that he is the first suggesting a naturalist position.

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.)

It is obvious that there must be endpoints of justification, but foundationalists' mistake is to conceive these endpoints as certain *knowledge*, i.e., a true *justified* beliefs. It seems to me wrong to say that a sentence we accept as true without asking for further reasons is justified, and still worse, to say that it is self-justifying. (This is perhaps a reason to doubt the correctness of the classical definition of propositional knowledge, but I leave this topic for another occasion.)

Hume did not argue that our of bodies were evident or justified; rather, he pointed out that we accept the existence of external objects without justification. Using modern semantics, we may describe his position as: when we accept an observation sentence as true, it follows that the referent of a singular term in that sentence refers to an existing object and this object satisfies the predicate in the statement made.

There is a class of sentences we legitimately accept as true without asking for justification, viz., explicit, stipulative definitions. An observation sentence is of course not an explicit definition, but it may reasonably be viewed as an *implicit* definition, more precisely, a partial and implicit definition of the predicate occurring in that sentence. In addition, explicit and implicit definitions alike are held true without justification.

In our vernacular it should be rather obvious that no predicates have predetermined and strict criteria of application; language use is an ongoing negotiation. When we accept a sentence, which lacks any kind of justification, as true, we in fact treat it as a partial implicit definition of the predicate in that sentence.[6] This is the core point of remark 150 in *On Certainty*.

This is true also in scientific language. However, we should not assume that these endpoints of justification forever will be conceived as such. It is possible to change what we treat as definitions and what we treat as empirical statements. This was, I think, one of the points Kuhn wanted to make in his [10].

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. What we may discuss is whether intersubjectively available things, viz., *utterances*, are true or not; how people interpret utterances, what they believe, is not public. However, epistemology is fundamentally a social endeavour. The 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, or an entire theory, as true, we 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. Such observation reports are neither self-evident, nor justified by other sentences. However, they are agreed upon by several observers.

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6 The same is true of axioms in mathematics. A well rehearsed example is Euclid's axioms, which nowadays are understood as implicitly defining 'point', 'line', 'circle' etc. Euclid had 'defined' a point as an object having no parts, but this 'definition' is irrelevant in Euclid's geometry viewed as pure mathematics. Points were in fact implicitly defined by those axioms talking about points. This view was first clearly defended by Hilbert, see e.g., [9] (pp. 64–66).

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. Induction in the Naturalistic Perspective

In the naturalistic view the problem of induction is thus *not* that of justifying induction in general. We do inductive reasoning all the time, it is a natural habit. However, 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 [11]. More precisely, he asked what kind of predicates are 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 and Goodman suggested that the notion of entrenchment could be used in order to distinguish between projectible and non-projectible predicates; in [12] (ch. 4), he suggested that a predicate is entrenched when we have used it in successful predictions in the past. On this account I completely agree, it is a naturalistic stance.

Goodman analysis is a step forward, but 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.[7] 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 the 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 concerns 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 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 physical laws, that it is

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[7] In the appendix 'Emeroses by Other Names' to his paper 'Mental Events', Davidson brought up this point, see [13] (p. 225).

green, independent of time.[8] Hence if something satisfies the predicate 'emerald', it satisfies also the predicate 'green'.

I rely here on the predicate 'physical law' and on the fact that laws are supported by empirical findings, by being generalisations of observations. (I'll discuss the distinction between laws and accidental generalisations in the next section.) Hence the argument depends on previous inductions. This is no vicious circle, as already pointed out.

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 in fact no counter instances ever will be found. 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 strict regularity, which we are inclined to call a natural law.

Suppose we have found a strict regularity, thus calling it 'a natural law', by inductive reasoning. Isn't the existence of such regularities 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 merely 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, such as 'mass', 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*.) However, his insights were forgotten and for a long time these mammals were classified as fishes. However, fishes have gills, while cetaceans have no gills, so how to resolve this conflict? It was John Ray (1627–1705) who in his [14] finally recognised that dolphins, porpoises and whales are not

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8 A physical body looks green when it reflects electromagnetic radiation of wavelengths around 400–500 nm, and absorbs radiation of longer wavelengths in the visible part of the electromagnetic spectrum. This is determined by the available excitation levels in the molecules at the surface of the body, which in turn is determined by quantum mechanical laws.

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. However, 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 or for that in the consequent. New counter examples might trigger new adjustments and 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 example is possible; we have arrived at a *epistemically fundamental* law.

One example of such a law is momentum conservation, as shown in Section 2. Another example is provided by Maxwell's equations+ Lorentz' law, which are the fundamental laws of electromagnetism. These jointly define the quantities *charge, current, electric field* and *magnetic field*. (I have argued this in detail in 'An empiricist view on laws, quantities and physical necessity' submitted to *Theoria*.) Since the predicates occurring in such laws are the result of successive adjustments, these laws function as (partial) implicit definitions of the predicates in the law sentences. The further question why we say of laws that they are necessary is discussed in the next section.

In an axiomatic exposition of a theory we label 'fundamental' those laws that are the starting points from which we derive other laws. However, it is well known that we always have a choice as to what laws in a certain body of theory to take as starting points in derivations. In Newton's exposition of classical mechanics it is his three laws+ the gravitation law that are taken as fundamental, whereas Hamilton's equations are the fundamental laws in Hamilton's version of classical mechanics. (In addition, there are more alternatives.) So it clear that a law is in this sense fundamental only relative to particular *theory formulation*.

I call a law 'epistemically fundamental' when it further satisfies the condition that it belongs to the set of laws being most closely connected to empirical observations within a particular theory. In classical mechanics it is neither Newton's laws, nor Hamilton's equation that satisfy this requirement, but the law of momentum conservation, as described in Section 2. (The law of gravitation also satisfies this condition.) From momentum conservation applied to a collision of two bodies:

$$d/dt(m_1v_1 + m_2v_2) = 0 \tag{1}$$

we immediately get the equation

$$m_1a_1 = -m_2v_2 \tag{2}$$

where 'a' is short for acceleration. By introducing the quantity *force* ('f') as short for *mass · acceleration*, i.e., introducing Newton's second law, we get

$$f_1 = -f_2 \tag{3}$$

which is Newton's third law. Thus, Newton's third law is derived from momentum conservation and, obviously, Newton's second law is an *explicit* definition of force. So it is clear that momentum conservation is, from an epistemological perspective, a fundamental law of classical mechanics, whereas Newton's three laws are not. (Neither are Hamilton's equations; in order to apply these equations to real physical events one must equate momentum with mass times velocity, thus relying on mass, which is defined by the law of momentum conservation.) Hence there is reason to distinguish between the concepts *epistemologically fundamental* and *logically fundamental*. A law is fundamental in the logical sense only relative to a particular axiomatisation.

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 two predicates in a universally generalised true conditional are in fact conceptually dependent on each other, or can be so made, either by deriving the regularity from fundamental laws or else the criteria of application for the predicate in the consequent are a subset of those for predicate in the antecedent. This argument applies not to ordinary language, only to a well structured scientific theory with well defined predicates. So called 'laws' expressed in ordinary language are most often not strict regularities.

5. Laws and Accidental Generalisations

Suppose we have observed a regularity, 'All A:s are B' and have not hit upon any counterinstance. Is this a law or an accidental generalisation?

A well-rehearsed contrast is that between 'All spheres of gold have a diameter of less than 1 km' and 'All spheres of U-235 have a diameter of less than 1 km'. The first generality we label 'accidental generalisation' whereas the second is is believed to be a law. What is the reason for making this distinction between two true general statements with roughly equal amount of support from empirical evidence?

The reason is, I think, that we can infer the generality about U-235 from general principles (which also are called 'laws') of nuclear physics, while no such inference to the accidental generalisation is available, and we use the principle that if a true general sentence, not being logically true, can be inferred from a set of laws, it is itself a law. So accidental generalisations are true general sentences which cannot be integrated into a scientific theory built upon a set of well defined theoretical predicates.

Outside physics, chemistry and related sub-disciplines there may be many regularities called 'laws' without being derivable from fundamental laws. I my view we should be careful in calling these regularities 'laws', waiting until they can be integrated into a theory. The extension of the predicate 'natural law' in our vernacular is not very clear.

We saw above that Newton's second law in fact is an explicit definition of 'force'. It is not uncommon to label explicit definitions 'laws', Ohm's law is one further example. (For a long time it was the definition of resistance in terms of current and voltage, but nowadays it defines voltage in terms or resistance and current.)

From an epistemological point of view, implicit and explicit definitions differ. As already argued, those fundamental laws that are implicit definitions of theoretical predicates have empirical content, since such laws are inductive generalisations from limited sets of observations, and such a generalisation might prove wrong one day. An explicit definition, on the other hand, introduces a new general term as short for a longer expression, hence any such term may be replaced by its definiens without change of any testable consequences; thus it has no empirical content.

Why, then, attribute necessity to laws? For those laws that are derivable from fundamental ones, the necessity is 'inherited': if we say about P that it is necessarily true and can derive Q from P, (and perhaps using other sentences being necessarily true) we likewise say that Q is necessarily true. So the question comes down to the necessity of fundamental laws.

Epistemically fundamental laws are implicit definitions, which function as rules for introduction of new predicates into discourse. Hence, those laws are *necessary conditions* for the use of the defined predicates in an ensuing inquiry. This is, I think, the reason we say that fundamental laws are *necessarily true*. For example, the introduction of the predicate *mass* by means of accepting momentum conservation as true, is a necessary condition for the construction of the dynamical part of classical mechanics, and in fact for physics in its entirety. Thus, I interpret 'necessary' not as a modal operator, as is common, but as short for 'necessarily true', i.e., as a semantic predicate, just as 'true'. Thus I only need to enter into what Quine labelled 'the first grade of modal involvement' in his [15], when explaining the necessity of laws; no modal logic is needed.

An anonymous referee to an earlier version of this paper got the impression that my view on laws was close to Michael Friedman's account of fundamental laws as being *relativised a priori* conditions for empirical knowledge. This is not so. Friedman argued that some core principles, both mathematical and physical ones, are *preconditions* for empirical research. Thus he wrote:

> The idea is that advanced theories in mathematical physics, such as Newtonian mechanics and Einsteinian relativity theory, should be viewed as consisting of two asymmetrically functioning parts: a properly empirical part containing laws such as universal gravitation, Maxwell's equations of electromagnetism, or Einstein's equations for the gravitational field; and a constitutively a priori part containing both the relevant mathematical principles used in formulating the theory (Euclidean geometry, the geometry of Minkowski space-time, the Riemannian theory of manifolds) and certain particularly fundamental physical principles (the Newtonian laws of motion, the light principle, the equivalence principle). ([16], p. 71)

That mathematics is known a priori is uncontroversial. Furthermore, no one will oppose saying that *explicit* definitions of new predicates as shorthand for longer expressions are known a priori. However, Friedman's notion *relativised a priori* encompasses more things, viz., 'particularly fundamental principles' such as Newton's laws and the light principle. Now, Newton's second law is an explicit definition of force, thus it may be said to be known a priori[9], but the third law ('To every force there is an equal but oppositely directed force') is an immediate consequence of the law of momentum conservation and Newton's second law, as we saw above, and since momentum conservation surely is not known a priori, we may conclude that neither is the third law. In addition, the light principle, i.e., Einstein's postulate that the velocity of light is the same for all observers, is certainly not known a priori. It is in fact derivable from Maxwell's equations, which Friedman says belong to the 'properly empirical part' of physics; hence 'the light principle' belongs to the empirical part.

One might accept Friedman's general idea of dividing physical theories into two parts, an empirical part and a constitutive part, while holding that some laws are incorrectly classified. However, I do not really see the point with the notion of *relativised* a priori. Friedman holds, for example, that Newton's second law is a precondition for mechanics, and that we know that law a priori, so long as we accept classical mechanics. (In addition, there is a relativistic version of Newton's second law in relativity theory.) However, as already pointed out, there are empirically equivalent versions of classical mechanics (e.g., Hamilton's and Lagrange's) that do not need the predicate 'force'. So Newton's second law is not a necessary condition (nor a pre-condition) for doing classical mechanics, only for a particular formulation of this theory. In addition, since 'force = mass · acceleration' is a stipulative definition of 'force' one can systematically replace any occurrence of 'force' in the theory by its definiens; in fact, that is what we do when calculating observable consequences from initial conditions and theory. So it is not, in a logical sense, any precondition for doing mechanics, as Mach observed in [17].

From an empiricist point of view the trouble with Friedman's distinction between a proper empirical part of a theory and some relativised a priori principles is the same as the trouble with Kant's

[9] The vernacular use of the term 'force' is quite another thing. Newton's second law may be said to introduce another sense of this common word.

distinction between an empirical level and a transcendental level of discourse. The transcendental analysis was by Kant conceived as a non-empirical inquiry into the functioning of our mind, which, Kant hoped, would explain how synthetic a priori knowledge were possible. However, we empiricists resolutely reject the idea that we can have a priori knowledge about nature, relativised or not.

6. Induction as a Heuristic Device

The picture emerging from all this is that induction should not be seen as a particular form of reasoning for which one needs independent and non-empirical justification, but as a heuristic device in theory construction. We observe in several 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. When formulating this regularity we need new predicates, or refinements of old ones, and the sentences by which we express such regularities at the same time function as implicit, and sometimes partial, definitions of these predicates. Thus we arrive at epistemically fundamental laws.

What I have just said resembles to some extent what Aristotle claims in *Posterior Analytics*. According to Hankinson [18] (p. 168), Aristotle's word '$\epsilon\pi\alpha\gamma o\gamma\epsilon$' (epagoge) which usually is translated as '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 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 good 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.[10]

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. However, we cannot

[10] Aristotle furthermore held that universals, such as 'man' exist as instantiated in individual objects. This does not follow from his account of induction, and I do not follow Aristotle on this point. Like other empiricists I see no need for assuming the existence of universals. Thus I reject the realist conception that laws somehow 'mirror' nature.

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. In addition, it is the metaphysician who has the burden of proof.

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 the task of natural science.

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