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Steffen Ducheyne*

Some Worries for Norton's Material Theory of Induction

Abstract

In this essay, I take the role as friendly commentator and call attention to three potential worries for John D. Norton's material theory of induction (Norton, 2003). I attempt to show (1) that his "principle argument" is based on a false dichotomy, (2) that the idea that facts *ultimately* derive their license from matters of fact is debatable, and (3) that one of the core implications of his theory is untenable for historical and fundamental reasons.

* The author is Postdoctoral Research Fellow of the Research Foundation (Flanders) and is associated with the Centre for Logic and Philosophy of Science and the Centre for History of Science, both at Ghent University (Belgium). The author can be contacted by e-mail at Steffen.Ducheyne@UGent.be or by regular mail at Centre for Logic and Philosophy of Science, Ghent University, Blandijnberg 2, room 2.26, B-9000 Ghent, Belgium <URL: http://logica.ugent.be/steffen>.

Introduction

In philosophy of science numerous competing theories that seek to provide an adequate systematization of inductive inference have been developed (e.g. Bayesianism, IBE, hypothetical induction, demonstrative induction, bootstrapping, etc.). At present, scholars have not reached consensus on what such systematization might look like. How do we account for this proliferation of competing accounts?

In part to explain this proliferation, Norton defended and spelled out a "material theory of induction" (Norton, 2003), according to which "*[a]ll inductions ultimately derive their licenses from facts pertinent to the matter of the induction*" (p. 650 [italics in original], cf. p. 668). According to J.D. Norton, the quest for a universal systematization of inductive reasoning is futile, since all inductive inferences are "grounded in matters of fact that hold only in particular domains, so that *all inductive inference is local*" (Norton, 2003, p. 647 [emphasis in original]).[1] That we have failed to provide a universal framework is "not because of a lack of effort or imagination, but because we seek a goal that in

[1] Strictly speaking, Norton provides a material theory of ampliative reasoning in general.

principle cannot be found" (p. 648). I agree with Norton on the futility of the quest for a universal account of inductive reasoning: inductive inference is a inductive reasoning is such a complex phenomena – for instance, it includes not only standard enumerative reasoning, but also hypothetical reasoning, eliminative reasoning, etc. – that an overarching account seems rather impossible or, if such account turned out to be possible after all, it would be so vague that it becomes futile in specific domains. Different accounts of inductive inference should therefore rather be seen as complementing each other: different accounts have their use in different domains, and taken together they form a repertoire for handling inductive reasoning in various domains. Developing a rich and domain-sensitive repertoire (or *instrumentarium*) is definitely the way to go and seems more promising that the quest for an *inductionis logica universalis*.

According to Norton's material theory of induction, the admissibility of an induction is traced back to matters of facts and not to "universal schemas", which derive from a formal theory (p. 648). Inductive inferences derive their license from facts; facts are therefore the "material" of inductive inferences. For instance, Norton considers the case in which the melting points of bismuth and wax are observed. In the case of bismuth our observation of several samples of heated bismuth, showing that the samples have a melting point at 271°C, correctly underwrites our generalization that all samples of bismuth have a melting point at 271°C. In the case of wax, however, our observation of several samples of wax, showing that the samples of heated wax have a melting point at 91°C, does not correctly underwrite our generalization that all samples of wax have a melting point at 91°C, since contrary to bismuth wax is a generic name for a family of substances. Norton then comments as follows:

In the material theory, the admissibility of an induction is ultimately traced back to a matter of fact, not a universal schema. We are licensed to infer from the melting point of some samples of an element to the melting point of all samples by a fact about elements, we have a license to infer that other samples will most likely have the same properties. The license does not come from the form of the inference, that we proceed from a "some…" to an "all…." It comes from the fact relevant to the material of the induction. (p. 650)

By "formal theories" Norton means something very broad – which I shall henceforth refer to as (**FT**):

They are certainly not limited to accounts of induction within some formalized language or logic. The *defining characteristic* is just that the admissibility of an inductive inference is ultimately grounded in some universal template. (p. 649 [emphasis added], cf. p. 669)

The material theory, by contrast, is based "on the supposition that the material postulates obtain in specific domains; that is, facts that obtain "locally"" (p. 652). They are admissible "in the right context" (p. 669). For instance, a universal schema for inductive inference based on simplicity is absent and "our decisions as to what is simple or simpler depend essentially upon the *facts or laws* that we believe to prevail" (p. 656 [emphasis added]). *Idem* for Inference to the Best Explanation (p. 658). Then Norton states his Principle Argument - which I shall henceforth refer to as "**PA**":

(PA)

My principal argument for a local material theory of induction is that no inductive inference schema can be both universal and function successfully. (p. 652)

Somewhat further in the paper, Norton spells out an important implication of his account – which I shall henceforth refer to as "**Imp**":

(Imp)

At the same time, exactly because we learn more from the new evidence, we also augment our inductive schemas. For according to the material theory, all these schemas obtain only locally and are ultimately anchored in the facts of the domain. Crudely, the more we know, the better we can infer inductively. The result is that scientists do not need to pay so much attention explicitly to inductive inference. As we saw in the examples of section 4 [Norton is referring to Lavoisier's chemistry and Leverrier's discovery of Neptune], with each major advance in science has come a major advance in our inductive powers. *The mere fact of learning more will augment their inductive powers automatically*. (p. 664 [emphasis added])

In cases like these, "the added inferential power that comes from knowing more does not come from the delivery of some new schema" (p. 663) and thus Norton's account "does not separate facts from inductive inference schema[s]" (p. 669).

Discussion

Now I shall assess the tenability of Norton's account. I shall argue that Norton's account, as it stands, is untenable for tree fundamental reasons.

Worry 1: Either Material or Universal: A False Dilemma

My first point is that the fact that no universal inductive schemas are at hand does not establish the claim that all inductive inferences are licensed by mere matters of fact. Norton's acceptance of the material theory of induction is based on his rejection of what he calls formal theories (FT), which are by definition, i.e. by Norton's definition, universal. The observation that, up until the present no universal and successful inductive schema has been established, motivates Norton to opt for his material account of induction – his argument is basically a disjunctive syllogism. However, Norton neglects at least one alternative: that non-"material" schemas are local. By "non-material schemas" I refer to those schemas which license inductive generalisations without being completely data-driven (also see *infra*). Although I agree with Norton that all inductive inferences have their origin in empirical knowledge (stated more clearly: that empirical knowledge is a necessary condition for induction), I disagree that inductive schemas are *exclusively* licensed by matters of fact. Inductive inferences are, by definition, data driven, but not completely determined by the data. As Daniel Steel has pointed out, they also depend upon "judgements concerning the *desiderata* of inductive reasoning" (Steel, 2005, p. 189), i.e. on *normative* components. Indeed, local schemas contain *interpretative* and *pragmatic* components which do not, in a straightforward way, relate to empirical knowledge. Inductive schemas cannot be directly "read off" the phenomena under consideration. Consider, for instance, that logically characterizing an inductive consequence relation is not a straightforward empirical thing: it involves fixing structural relations between sets of premises and their consequence sets.[2]

Norton does not make the distinction between formulating an empirical theory of induction and a normative theory of induction. He limits a generic theory of induction to the empirical components of a theory of induction and, correspondingly, bypasses the important normative features of inductive reasoning. I am inclined that this justifies the claim that Norton's theory of induction is *not* a complete account of induction, as it stands. A theory of induction should address the normative features of induction, i.e. it should spell out – given a certain context – what the criteria of valid inductive inference are in cases where other than strictly empirical considerations license inductive generalizations. What Norton has successfully proven, however, is that inductive schemas are contextual.

[2] Adaptive logics have been used to characterize several inductive consequence relations (see Batens & Haesaert, 2001, Batens, 2005, and Batens, 2006).

Worry 2: Can matters of fact "ultimately" license inductive generalizations?

Moreover, the idea that empirical generalisations *ultimately* derive their license from the relevant facts of a domain is somewhat debatable. Suppose that *scientist*$_1$ (S_1) and *scientist*$_2$ (S_2) reason from two mutually inconsistent theories (T_1 and T_2), that the choice between them cannot be obviously decided on strictly empirical grounds, and that both scientists have different theoretical views and background assumptions: BG_1 and BG_2 respectively (I use *BG* to refer to both the theoretical views and background assumptions). While *scientist*$_1$ might try to isolate the consistent parts of T_1, *scientist*$_2$ might try to isolate the consistent parts of T_2 – for reasons relative to their theoretical views and background assumptions (see figure 1). Obviously, such *BG*'s might refer to epistemic values (in the sense of McMullin, 1983).

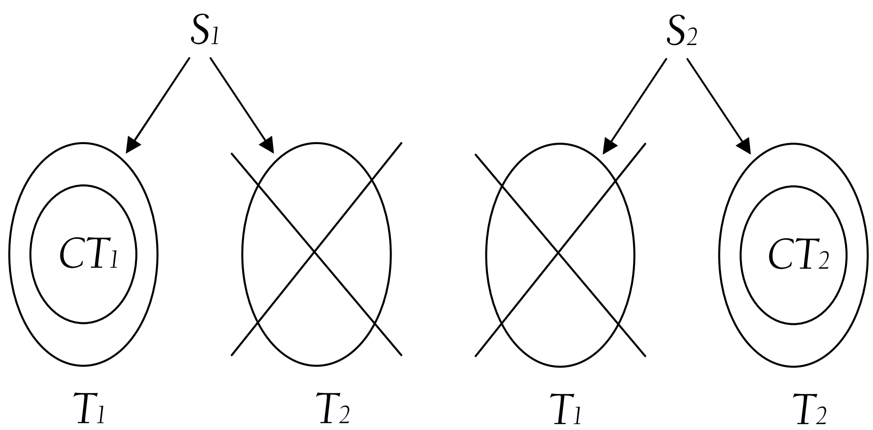

Figure 1.

Note that the inductive generalisations in this example *are* licensed by their respective *BG*'s (for scientist_1 the inductive generalization is licensed by BG_1; for scientist_2 the inductive generalization is licensed by BG_2). Thus: at a certain moment in the development of science, in which the choice between two theories was empirically undecidable, specific inductive strategies, which are based on *BG*'s, licensed specific inductive generalizations. One might respond to this that in the long run the consistent theory will be detected, but this seems rather untenable. There is no guarantee that inductive generalization in general will "ultimately" become empirically decidable. It is therefore reasonable to suggest that epistemic values and other relevant *BG* will continue to play an important *rôle* in future science and inductive reasoning.

*Worry 3: Why **Imp** Is Untenable: Fundamentally and Historically*

According to **Imp**, the mere fact of having more and more empirical knowledge *automatically* improves our inductive powers. So according to Norton, both are closely aligned and he essentially argues that the *quantity of the empirical data at our disposal* (**Q(ED)**) is directly proportional to the *improvement of our inductive powers* (**I(IP)**). This conclusion is, however, untenable for two reasons:

> $\mathbf{O_1}$: It is possible that a cornucopia of empirical data is at hand, but that no significant inductions are made based upon these data (i.e. there might be ***Q(EM)*** *without* ***I(IP)***).
>
> $\mathbf{O_2}$: It is possible that our inductive inferences turn out to be false, empirically speaking, but shed light on normatively sound inductive criteria – valid within a domain of application (i.e. there might be ***I(IP)*** *without* (***Q(EM)***).

As an example of O_1, we might consider the fact that although Tycho Brahe and Johannes Kepler had the same data at their disposal, embodied in the *Rudolphine Tables* (finally published by Kepler in 1627), only Kepler succeeded in deriving his so-called laws (which were, in fact, considered as rules at the time). The tables were generally considered trustworthy and accurate to two or three minutes of arc (Wilson, 1972, p. 2; Stephenson, 1987). That extending our data automatically improves our inductive inferences is therefore a *non sequitur*.

Joan Baptiste Van Helmont's tree experiment provides an ample illustration of O_2. Van Helmont reported on the following experiment (see Ducheyne, 2005 for further details):

> But I have learned by this handicraft-operation, that all Vegetables do immediately, and materially proceed out of the Element of Water only. For I took an Earthen Vessel [*vas*], in which I put 200 pounds of Earth that had been dried in a Furnace, weighing five pounds; and at length, five years being finished, the Tree sprung from thence, did weigh 169 pounds, and about three ounces: But I moystened the Earthen Vessel with Rain-water, or distilled water (always when there was need) and it was large, and implanted into the Earth and least the dust that flew about should be co-mingled with the Earth, I covered the lip of the mouth of the Vessel, with an Iron-plate with Tin, and easily passable with many wholes. At length, I again dried the Earth of the Vessel, and there were found the same 200 pounds, wanting about two ounces.

Therefore 164 pounds of Wood, Barks, and Roots, arose out of water onely.[3] (Van Helmont, 1664, p. 109)

The *explanandum* here is the weight and growth of plants. First of all, the weight of the earth is measured. That the earth has been dried on a fire and is isolated from the external world by means of a plate is significant here, since these conditions guarantee, according to van Helmont, that no other elements than earth could reside in the pot. That the water is distilled (or is rainwater) equally guarantees that no other elements than water reside in the pot. (This assumption was later challenged by James Woodward (1700).) In contemporary parlance, we

[3] Translation of: "Omnia verro vegetabilia immediatè, & materialiter, ex solo aquae elemento prodire hac mechanica didici. Caepi enim vas terreum in quo posui terrae in clibano arefactae ℔ 200, quam madefeci aqua pluvia, illique implantavi truncum salicis, ponderantem ℔ 5. ac tandem exacto quinquennio, arbor inde prognata pendebat ℔ 169, & circiter unas tres. Vas autem terreum, sola aqua pluvial, vel distillata, semper (ubi opus erat) maduit, eratque amplum, & terrae implantatum, & ne pulvis obvolitans terrae commisceretur, lamina ferrae, stanno obducta, multoque foramina pervia, labrum vas tegebat. Non computavi pondus soliorum quaterno autumno deciduorum. Tandem iterum siccavi terram vasis, & repertae sunt eaedem librae 200 duabus circiter uniciis minus. Librae ergo 164 ligni, corticum, & radicum, ex sola aqua surrexerant." (Van Helmont, 1648, pp. 108-109).

would say that Van Helmont attempted to control these variables (the amount of earth and water). Then, the gained weight of the tree is measured (*ca.* 164 pounds). Note however that after five years Van Helmont weighed the "Wood, Barks, and Roots". Apparently, Van Helmont did not include the weight of the leaves for a reason unknown to me. Notice further that Van Helmont is not worried at all by difference of two ounces. Given that there did not reside any other elements than earth and water in the pot, and that the earth did not diminish significantly, Van Helmont (wrongly) concluded that *only* the water *produces* the growth of the tree. According to Van Helmont, only the addition of the water can explain the growth of the plant.

We now know that Van Helmont was completely wrong, but his experiment sought (quite unsuccessfully) to provide a controlled experiment, where certain variables are kept fixed and others varied. Although he was wrong, he had some profound insights how inductive inferences should be based on controlled experiments. This is what exactly what van Helmont's attempted with his tree-experiment: the earth is kept constant and the water is purified. In many of Van Helmont's experiments, procedures of keeping variables fixed – as well as reference to relatively closed physical systems, in which all external variables are screened off – frequently occur (again see Ducheyne, 2005).

Van Helmont had a particular and profound insight in the idea that knowledge of nature is produced by isolating certain natural processes or creating – or at least, trying to create as good as possible – relatively closed physical systems. In line with the above interpretation, Woodward's refutation of Van Helmont's willow experiment included both more exactness and more variables being fixed. Woodward weighed plants and the composition of water in more detail. He put different plants of the same kind near the same window (hence: species, warmth, and, amount of air and light are kept fixed) (Woodward, 1700, p. 199). He further compared water of different origin (rain water, Thames water, etc.) and constructed an artefact which guaranteed that the water can only be exhaled by the plants (ibid., pp. 201-202). Van Helmont's intuitions on controlled experimentation were later put to practice more successfully by Woodward in 1700.

How the Previous Problem can be Resolved

The previous problem can be avoided by simply distinguishing between descriptive and normative features of induction. $\mathbf{O_1}$ is then explained by pointing to the fact that our empirical knowledge is well-developed but our corresponding inductive criteria are not ($\mathbf{O_2}$ is then explained conversely).

Conclusion

Future research should highlight the locality of inductive criteria in scientific praxis. I contend that studying specific branches of science and by further developing logical systems for inductive reasoning will offer promising paths to do so.

Despite Norton's appealing inductive anti-mono-criterial attitude and his equally attractive pluralism, his account faces three serious drawbacks. I have briefly outlined how they can be avoided by modifying Norton's proposal but with sticking to its pluralistic core. If correct, this analysis shows that **PA** and **Imp** are untenable.

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[4] This translation is at times quite incorrect and should be read with necessary precaution.