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Inductive reasoning in the context of discovery: Analogy as an experimental stratagem in the history and philosophy of science

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A B S T R A C T

Building on Norton's "material theory of induction," this paper shows through careful historical analysis that analogy can act as a methodological principle or stratagem, providing experimentalists with a useful framework to assess data and devise novel experiments. Although this particular case study focuses on late eighteenth and early nineteenth-century experiments on the properties and composition of acids, the results of this investigation may be extended and applied to other research programs. A stage in-between what Steinle calls "exploratory experimentation" and robust theory, I argue that analogy encouraged research to substantiate why the likenesses should outweigh the differences (or vice versa) when evaluating results and designing experiments.

1. Introduction

> Among the many methods which he [the philosopher] may use – always depending, of course, on the problem at hand – one method seems to me worth mentioning. It is a variant of the (at present unfashionable) historical method. It consists, simply, in trying to find out what other people have thought and said about the problem in hand: why they had to face it: how they formulated it: how they tried to solve it.
>
> Karl Popper (1959, p. 16)

In "A Material Theory of Induction," Norton (2003, p. 647) addresses the philosophical 'elephant' in the room: the problem of induction and the triumph of the sciences that use inductive reasoning. He notes that, "[a]fter two millennia of efforts, we have been unable to agree on the correct systematization of induction. […] The problem is deepened by the extraordinary success of science at learning about our world through inductive inquiry. How is this success to be reconciled with our continued failure to agree on an explicit systematization of inductive inference?" Norton (p. 648) suggests that this failure occurred because "we seek a goal that in principle cannot be found." Why? Because "all induction is local."

Norton argues for a material theory of inductive inquiry in which "all inductions ultimately derive their licenses from facts pertinent to the matter of induction" (p. 650). For example, he writes (2010, p. 766), "Here are two formally identical inductive inferences:

1. This sample of bismuth melts at 271 °C.
2. Therefore, all samples of bismuth do.
1. The temperature of the first day of the new millennium was 8 °C at noon in Pittsburgh.
2. Therefore, all first days of new millennia in Pittsburgh will be so.

Whether the inferences are good depends on what the propositions say—their matter or material." In other words, he argues that each inference must be evaluated on its own merit and in reference to the "facts" to which the inference pertains. Although the two inductive inferences cited above have the same form, they do not carry the same weight—the first is reasonable in light of experimental results whereas the second is misguided because of a deficiency in the quality and quantity of meteorological observations. By focusing on locally contingent factors which support (or undermine) specific inferences, Norton (2010) concludes that there is no solution to the problem of induction because "there are no universal rules of induction; " each inference must be evaluated on a case by case basis.

Re-examining the work of William Whewell, John Stuart Mill, and Isaac Newton, Achinstein (2010), however, brings to the fore a

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central tension between Norton's position and Mill's and Newton's own descriptions of how inductive reasoning is used in science. In light of our inability to accurately describe inductive inquiry, Norton's argument seems like a practical step forward—perhaps it is better to concede defeat than to continue to search for an elusive formal structure that captures the character of all inductive inferences—but, if we accept that all inductive inferences are locally contingent, how do we reconcile this position with Mill's and Newton's assertions about the significance and *generality* of inductive reasoning to science? In other words, how can something that depends on local contingencies also have purported universal characteristics?

Achinstein (2010, p. 735) asks:

> When Mill characterizes induction as 'the process by which we conclude that what is true of certain individuals of a class is true of the whole class,' what is he doing? For one thing … he [Mill] is giving a definition of 'induction' or 'inductive generalization.' And as he himself emphasizes, the definition permits both good and bad inductions. […] But he is doing something else as well. For Mill, one of the main aims of science is to 'discover and prove' causal laws that enable one to explain and predict phenomena. Such discovery and proof requires making inductions to causal generalizations, which is what laws are for him.

Similarly, in the *Principia*, Newton (1729, p. 205) states the "Rules of Reasoning in Philosophy," as part of his explanation for how he derived the law of universal gravitation. He asserts that "[i]n experimental philosophy we are to look upon propositions collected by general induction from phænomena as accurately or very nearly true, notwithstanding any contrary hypotheses that may imagined, till such time as other phænomena occur, by which they may either be made more accurate, or liable to exceptions." As Achinstein (2010, p. 735) notes, Newton's methodological considerations do not necessarily preclude Norton's argument, but "inductive reasoning of the sort expressed in [Newton's] rules … are crucial in establishing propositions in empirical science," in this case that all objects in the universe are subject to an attractive force proportional to the inverse-square of the distance between them.

Achinstein (p. 739) argues that Newton's and Mill's respective approaches to inductive inquiry can "peaceful[ly] coexist … with [the] physically indeterministic systems of the sort John Norton describes," suggesting that some forms of inductive reasoning are more robust than others and may be described differently. Achinstein implies that Newton's use of inductive reasoning to establish the law of universal gravitation belongs to a different category of inductive inference than the proposition "the temperature of the first day of the new millennium was 8 °C at noon in Pittsburgh; therefore, all first days of new millennia in Pittsburgh will be so." But, this strikes me as an unsatisfying restatement of the status quo, and it is precisely the philosophical conundrum that Norton is trying to address. I am not, however, unsympathetic to Achinstein's argument. In fact, I think his main point is well taken, namely that "inductivists can put up a better fight than their opponents [Whewell, Popper, Norton, etc.] might imagine" (p. 729), but no one, especially Norton, wants to deny that inductive reasoning is a powerful tool in science.

So, where does this leave us? Can both Norton and Achinstein (and by extension Mill and Newton) be right?

I argue that the key to resolving the tension between Norton's and Achinstein's positions lies in Mill's and Newton's own respective emphases on *discovery* and *experimental philosophy*. For almost eighty years, the bulk of our philosophical analyses have been anchored in the context of justification, so much so that we tend to ignore the fact that prior to the 1930s no such strict distinction existed (and we tend to ignore this fact often to our own intellectual peril). Given this historical reality, I ask: what would happen if we shifted our analysis of inductive reasoning from the context of justification to the context of discovery? I discuss the advantages and disadvantages of changing our perspective on the problem of induction. By analyzing inductive reasoning in the context of scientific practice rather than formalism, I show that the problem of induction becomes more tractable.

More specifically, in this paper I focus on the use of analogy in science as one specific case study of inductive reasoning. I argue that analogy plays a substantive role in the context of discovery. Rather than using analogical reasoning *ex post facto* to justify or demonstrate the reasonableness (or unreasonableness) of a scientific claim, I show that scientists use analogy (and by extension inductive reasoning) as a methodological principle or stratagem. In particular, analogy offers experimentalists a useful, first-order framework with which to assess data and devise novel experiments. A stage in-between what Steinle (2016) calls "exploratory experiment" and robust theory, analogy encourages research to substantiate why the likenesses should outweigh the differences (or vice versa) when evaluating results and designing experiments. By considering the use of analogical reasoning in the context of discovery, I show how Norton's perspective may be reconciled with Achinstein's (and Mill's and Newton's) position(s) on induction.

2. Inductive reasoning in the context of discovery

Before turning to my case study, is there a precedent for shifting the problem of induction from the context of justification to the context of discovery? The answer is yes.

In *Experience and Prediction* (1938), Reichenbach codified the distinction between the context of discovery and the context of justification. He created these categories to separate the social, cultural, and psychological factors affecting scientific breakthroughs from the evidence marshaled in favor of and the reasoning used to support scientific claims. In doing so, he helped to demarcate the goals and aims of philosophy of science from other disciplinary endeavors, such as history (of science) and psychology. He also offered his own "pragmatic justification" for what he called the practice of induction.

Reichenbach outlined Hume's objections to inductive reasoning as follows (p. 342):

1. We have no logical demonstration for the validity of inductive inference.
2. There is no demonstration *a posteriori* for the inductive inference; any demonstration would presuppose the very principle which it is to demonstrate.

He noted (p. 348) that Hume had "started with the assumption that a justification of inductive inference is only given if we can show that inductive inference must lead to success. In other words, Hume believed that any justified application of the inductive inference presupposes a demonstration that the conclusion is true." Reichenbach argued that Hume's criticisms of induction "are valid only in so far as the Humean assumption is valid." He questioned: "[i]s it necessary, for the justification of inductive inference, to show that its conclusion is true?"

Reichenbach argued that it was possible to change the criteria by which inductive reasoning was judged to circumvent Hume's key assumption by reconsidering the goal(s) of induction in science. The "practice of induction" in this context took on a distinctly teleological meaning as Reichenbach asked: what is the purpose of induction? Instead of focusing on demonstrating the truthfulness of inductive inference, he wrote (p. 350) that "*the aim of induction is to*

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