

&HPS as a Branch of Natural Philosophy, Beginning With Induction, Supposition, Meaning, Reference, and the 'Discovery' of Hypothetical Entities

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At bottom, if not self-consciously so, such works as Kuhn's *The Structure of Scientific Revolutions* or van Fraassen's *The Scientific Image* amount to sketches of research programs for a global theory of the aim(s) and development of science. Treatments of particular episodes from the history of science serve only as thumbnail illustrations of how to understand various components of the program or as plausibility arguments to the effect that detailed scrutiny of any case should more or less bear out certain overarching claims. Moreover, disciples and opponents of such proposed programs have tended to confine their discussions to defenses or attacks of the general viability of the program. Rare, indeed, is the appearance of anything that might be called a result, which establishes, independently of global commitments and to the satisfaction of (at least, almost) all parties, some property or feature of scientific activity that is both robust and elucidating. That is to say, the systematic study of science has produced little that corresponds to what is one of the hallmarks of natural philosophy (as empirical science), in contrast to philosophy as a speculative, scholastic activity, viz., a reasonably secure, systematic body of low-level inductive conclusions that, on the one hand, can serve as premises for further inductive inferences and, on the other, serve as conditions of adequacy for any proposed high-level theoretical or all-encompassing scheme or framework.

My hope, here, is to get a handle (or two) on how to get this up and running. For sure, we have heard calls to "naturalize" the study of science before. However, these other calls have typically proposed or attempted to appropriate theories or methods from some established body of science (e.g., cognitive science, sociology, anthropology) for application to the history and current practice of science. Rather than such dubious strategies of colonization, what I have in mind is instead an autonomous development in cooperation with various subareas of academic philosophy, most notably, epistemology, philosophy of language, philosophy of mathematics, and even to some extent, the philosophy of mind, remain in a state similar to that of historical philosophy of science. Although historical philosophy of science borrows doctrines freely from them, these areas have yet to acquire empirical legs largely because their evidential bases have not grown beyond the mere commonplace, except for widespread appeal to myths about the history of science. What is required is the cultivation of authentic history in a form suitable for generalization.

I begin with a preamble about the prospects for a material typology of the multifarious uses of induction in scientific reasoning (I), but quickly move on so as to be able to render, in sufficiently prominent relief, certain fundamental semantic variegations in scientific discourse (II). If we can get an adequate start here, we may be poised to begin harnessing the history of science in the service of natural philosophy.

I. Norton has recently argued in his material "theory" of induction, that there is no inductive logic in the sense of universal formal canons of reasoning, as we have for deductive logic.

Rather inductive inferences are licensed ultimately by contingent facts peculiar to the domain of the inference in question. If the material “theory” of induction is on target (as I think it is), then what we would like something on the order of an atlas charting the hierarchies of the inferential chains over the course of history leading up to our current theories. The comparison of historical charts with their synchronic counterparts current day counterparts is presumably where the rubber meets the road in the materiality thesis. The unavailability of global charts would reflect the “located” character knowledge while the constructibility of temporally (and spatially) overlapping charts would elucidate its later interpretability. Nonetheless should anticipate systematically different types inductive relations transcending specific material domains. For example, the inferential moves that motivate the introduction of hypothetical entities are various: e.g., missing causal links, preservation of conservation laws, explanation of whole data tables for experimental parameters, theoretical unification (think, ‘squarks’ from ‘super-symmetry’). But such inferences are clearly of a stature different the type of inductive support required, for example, for the empirical “discovery” of a hypothetical entity.

II. The realist-antirealist debate is widely seen to be sustained by the tension between, on the one hand, the (otherwise surprising) instrumental reliability of theory, and, on the other, the pessimistic meta-induction on the non-monotonicity of historically accepted theory. This tension, I argue, relaxes if we pay detailed attention to the nuances sufficiently extended segments of scientific discourse and also factor in certain forms of language use prevalent in other social enterprises, but which, in the philosophy of language, are typically set aside as anomalous outliers. The overlooked phenomenon is the pervasive linguistic practice of entertaining suppositions (under the suspension of belief or disbelief). The storyteller’s yarn generates a supposition (what’s true according to the story) typically along with objects of supposition (fictive characters), just as the logician’s *reductio ad absurdum* supposes the negation of what is to be shown, which may involve reasoning with non-existent, even, impossible entities.

To even get off the ground, realist-antirealist debate presupposes a dubious observable-unobservable distinction that is not only completely at odds with usage in actual scientific practice, but entirely void of interest to anyone not engaged in the debate. I urge those who sense there is really something at stake in the debate to reformulate the issues, to the extent they can, against the backdrop of the far less contentious distinction: referring-non referring. Failed hypothetical entities clearly conform to the linguistic patterns of talk about objects of suppositions. This invites the question whether currently hypothetical entities should also be assimilated to this category. This would entail that cases in the history of science customarily described as “the discovery of E” (where E is the name of an entity that was hypothetical at the time) should turn out under scrutiny as more aptly characterized as an appropriation of the name of the hypothetical for the baptizing a nascent, experimentally identifiable entity. Impressions to the contrary, I argue, derive primarily from hagiography and a willing forgetfulness that the initially adopted signature of the experimental entity is often but loosely connected with the governing supposition (or Ramsey sentence) for the hypothetical, and that subsequently the Ramsey sentence frequently pans out to describe, even approximately, nothing at all. To be clear, now, the thesis at hand is NOT to insist on a strict duality between

the hypothetical and the empirical. Discovery can be a protracted process involving shifting inductive standards and even social negotiation. Moreover, it is defeasible. I back up all this up with an extensive survey of actual usage in the history of physics.

Philosophers have typically regarded inferential role semantics (meaning holism) as a strict all-or-none competitor with the causal acquisition theory of meaning. If meaning is determined by use, there is no a priori they need be. And to the extent that meaning is something that guides reference, inferential role semantics governs use terms for objects of supposition co-referentially, while the causal theory explains the preservation of reference to empirical entities under refinement or augmentation of signatures. (Concepts associated with empirical entities are far more idiosyncratic: 'Dirac-electron' and 'Dehmelt-electron' involve suppositions that piggyback on the empirical electron; the man-in-the-street concept is but a stereotype.)