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Contextual Continency and the Logic of Metatheoretical Homogeneity

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Abstract The philosophy of science encompasses three major approaches: the logical approach exemplified by Popper, the sociological approach exemplified by Bloor, and the historical approach exemplified by Laudan. While these approaches are valuable, they may overlook the unique and context-specific aspects of individual scientific changes, leading to Wittgenstein's fallacy of "seeing what is common". Our paper employs case studies as counterexamples to demonstrate how each approach fails to be descriptively exhaustive. We emphasize the significance of contextual contingencies and individual creativity in scientific innovation and contend descriptively that metatheoretical homogeneity is non-existent. By acknowledging the intricacies of scientific changes, we can enhance our comprehension of changes within science.

Keywords Scientific pluralism, Paradigm change, Methodology, Deductivism, Inductivism, Scientific monism

1. Introduction

To begin, I would like us to be clear about our language. Philosophers talk about theory choice, paradigm shifts, research programmes and so forth. What we want to talk about is a set of beliefs about the world that scientists hold (at least very roughly) to be accurate. Quine’s web of beliefs may apply here. What is important is my verbiage. I will use (a) “scientific discovery” for a noteworthy change in scientific understanding. Elliptical orbits were a scientific discovery, as was penicillin, the atom, and the like, for our purposes. Let use sweep ontological disagreements to the side for the time being, it is fine that these theories are classified in a broad, rough and ready way for my argument. The second issue (b) is *commonality*. I borrow from Wittgenstein the idea that “seeing what is common” in terms of a single continuous thread is problematic. He asks, famously using the example of games, in the Philosophical Investigations, “what is common to them all? — Don’t say: ‘There must be something common, or they would not be called ‘games’ — but look and see whether there is anything common to all. — For if you look at them you will not see something that is common to all, but similarities, relationships, and a whole series of them at that.” [1]

Seeing an anomaly in Venus’ orbit or viewing the results of the dual slit experiment are not similar events. Venus’ orbit might seem “anomalous” when viewed through a certain theoretical framework. The dual slit experiment suggests something altogether different—what our entire sensual apparatus suggests to us about the world and its stable nature is false. The differences between Johannes Kepler and Erwin Schrödinger may be as important as their similarities. The tendency of philosophers of science has been to see commonalities. Monists about scientific method tend to trample over differences between approaches, contextual contingencies, and internal dynamics. Cases of scientific discovery deviate from one another in significant ways. Far from shedding light, monist methodologies have a smearing effect. They brush over differences in order to, seemingly, argue for an antecedent thesis. Popper’s blunder is similar to Sir Francis Bacon’s—arguing for one de facto method of scientific discovery. I will break down the argument as follows:

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2. Logic

There is an assumption that all scientific discoveries have something in common.

There is disagreement about what that commonality is.

History shows diversity rather than commonality.

In predicate logic, the premises could be formalized as follows:

- p: $\forall x\ (S(x) \rightarrow C(x))$
- q: $\exists x,y\ (S(x) \land S(y) \land C(x) \land C(y) \land \neg(x=y))$
- r: $\forall x\ (S(x) \rightarrow \neg C(x))$ Where: • S(x): "x is a case of scientific discovery"
- C(x): "x shares a commonality with other scientific discoveries" And the conclusion formalized as:
- s: $\exists x,y\ (S(x) \land S(y) \land \neg C(x) \land \neg C(y) \land \neg(x=y))$ Where:
- $\neg C(x)$: "x does not suggest a commonality with other cases of scientific discovery" The conclusion can be

restated in English as thus:

The conclusion, therefore, is that there is a diversity among cases of scientific discovery. These premises acknowledge that the assumption about the commonality of cases of scientific discovery is antecedent, but also highlight the fact that there is disagreement about what that commonality is, and that history suggests a pluralistic nature of scientific discovery rather than a commonality. The conclusion then follows that cases of scientific discovery are heterogenous rather than homogenous. The rest of this paper will bear out that contention.

3. Alternative Pluralistic Accounts

Before we begin, we need to acknowledge alternative forms of pluralism which also recognize limitations of monistic outlooks. The pessimistic meta-induction (PMI) shows us more than just that the history of science has problems in terms of theoretical entities, it shows us where philosophers go wrong when they attempt to characterize science. Larry Laudan presented his "A Confutation of Convergent Realism" in 1981. [2] In this paper, Laudan acknowledged that there are often multiple empirically equivalent theories that can account for available data. Choice between these theories is often determined non-empirically. Dangerously courting a monistic outlook, though, Laudan argued later with Jarrett Leplin in 1991 that technological advancements can introduce new kinds of data and evidence that were previously unavailable, leading to a re-evaluation of existing theories and the rejection of some in favour of others. [3] They use examples of the microscope and telescope to illustrate how technological advancements break empirical ties by allowing scientists to observe new phenomena and gather new data that was previously inaccessible.

That all breakthroughs are reliant on some kind of technological advance is patently false, of course. But some forms of pluralism are subjectivist or relativist. The purpose of this paper is not to vouch for a particular view. The careful reader will note I remain quietist on many fronts. I wish to avoid similarities to relativist views, socially or otherwise, that may also be considered pluralist. [4] I am not arguing that scientific theories should be assessed regardless of truth values, or stressing that science is constructed in relation to cultural values, political interests and institutional structures. That e.g. the discovery of cosmic rays might involve something serendipitous does not have any bearing on whether or not those entities really exist, for example. What I intend to highlight is more benign and non-partisan, although relevant.

William Wimsatt, has argued that scientific explanation often involves multiple levels of analysis and that each level requires its own methods and criteria. He advocates for what he calls "redescription," where phenomena are described in different ways depending on the level of analysis. [5] Sandra Mitchell, on the other hand, emphasizes the importance of multiple explanatory frameworks, arguing that different frameworks may be needed for different aspects of a phenomenon. Her view is known as “integrative pluralism”. [6] John Dupré focuses on the importance of incorporating contextual factors in scientific inquiry, including social and historical factors, and advocates for a more pluralistic approach to science. [7] Overall, while there may be differences in emphasis and specific arguments, the views of these philosophers share a commitment to the importance of methodological pluralism in science. Kuhn has been characterized along with Feyerabend as pluralist. However, the anomalous view simply is not in the methodological sense. In contrast to pluralism are strictly “deductive and “inductivist” isolated approaches, both of which—we will see—are descriptively inaccurate.

4. Historical Cases of Methodological Discreteness

Not only is the shift from phlogiston to the oxygen theory the easiest to grasp and most popular in the history of science, but it is also quintessential to our type of analysis. It suggests the random, system-defying nature of how discoveries are codified. Oxygen is generally attributed to several scientists who worked independently of each other during the late 18th century. Joseph Priestley, a British chemist, is often credited with the discovery of oxygen. He first isolated the gas by heating mercuric oxide and collecting the gas that was given off. However, Carl Wilhelm Scheele, a Swedish chemist, also isolated oxygen around the same time as Priestley, but he did not publish his findings until after Priestley. [8] Another British chemist, Henry Cavendish, also discovered oxygen independently of Priestley and Scheele, but he did not publish his results until after them. Antoine Lavoisier, a French chemist, is credited with giving oxygen its name and for explaining its role in combustion and respiration through his experiments with oxygen and hydrogen. [9] The case of oxygen suggests against the “community view” of science which started with Thomas Kuhn.

Importantly, staggered and discrete discoveries foreshadow greater difficulties in pigeonholing cases as a general project in the philosophy of science. Even more antithetical to monistic views, some scientific discoveries were made purely by accident. The discovery of penicillin by Alexander Fleming in 1928 was one such case. He left a petri dish of Staphylococcus bacteria open and it became contaminated with mold, which killed the bacteria. Fleming noticed this and realized that the mold was producing a substance that could kill bacteria, leading to the development of penicillin as an antibiotic. [10] One would be hard-pressed to classify an accident as a social phenomenon, or the result of formal reasoning. However, this is just one case, there are many others.

Another such case involved Wilhelm Conrad Roentgen in 1895 who was experimenting with cathode rays when he noticed that a fluorescent screen in his lab was glowing even when it was not in the direct path of the rays, leading him to investigate the phenomenon further and discover X-rays. [11] Would this be a case of induction or deduction? What was the role of experimentation? Did the community of educated scientists in the field have an intersubjective bearing on these investigations? The monistic models of scientific change are not designed for contingencies which may be significant instrumentally and in terms of truth values.

Yet another challenge to the idea of metatheoretical homogeneity in science was the discovery of Cosmic Microwave Background Radiation (CMB) CMB is the thermal radiation left over from the Big Bang and is one of the most important pieces of evidence for the Big Bang theory. The discovery of CMB is often attributed to Arno Penzias and Robert Wilson, who detected it while working at Bell Laboratories in 1964. [12] However, the theoretical prediction of CMB was made years earlier by Robert Dicke and his colleagues at Princeton University. [13] Dicke and his team were searching for evidence to support the Big Bang theory, and predicted that there should be a faint radiation left over from the Big Bang that would permeate the entire universe. They even designed an experiment to detect this radiation, but were unable to detect it themselves. Meanwhile, Penzias and Wilson were using a large horn-shaped antenna to study microwave radiation coming from space.

They were surprised to find a persistent noise that seemed to come from all directions in the sky, and they couldn't get rid of it no matter what they did. They initially thought the noise was due to pigeon droppings that had accumulated in the antenna, but after cleaning it out, the noise persisted. [14] They eventually realized that they had discovered the very radiation that Dicke and his team had predicted years earlier. The discovery of CMB is a clear example of metatheoretical deviance in science. Dicke and his team had predicted the existence of CMB based on their theoretical understanding of the universe, but were unable to detect it. Meanwhile, Penzias and Wilson discovered it by accident while studying something completely different. [15] This provides a nice contrast against inductivists, abductivists and deductivists about science. While Dicke's team followed a particular pattern of inference, the actual success of the other team was accidental.

In the 1920s, physicist Victor Hess conducted a series of experiments in hot air balloons, which allowed him to measure the intensity of this "cosmic radiation" at different altitudes. He found that the radiation increased with altitude, indicating that it was coming from space rather than from the Earth. This discovery was initially met with skepticism and even ridicule, [16] as it challenged the prevailing view that the universe was composed only of matter that could be detected through electromagnetic radiation. At the time, scientists were aware of various types of radiation, such as alpha, beta, and gamma radiation. However, they noticed that even when they shielded their instruments from all known forms of radiation, they were still detecting some mysterious particles that seemed to be coming from outer space. However, over time, scientists came to accept the existence of cosmic rays and began studying their properties and origins. [16]

The discovery of cosmic rays is an interesting case study for methodological discreteness because it involved the identification of a new type of phenomenon that could not be explained by existing theories of radiation. It required the development of new experimental techniques and the formulation of new theories to explain the observed data. This may be a case where instrumentalist methodology interjects into experimentation, reminiscent of Laudan and Leplin's 1991 view, although we can see how this stands in contrast to the others.

The history of science is rife with examples that challenge in their unique ways purely monistic descriptions of scientific discovery. The discovery of oxygen, for instance, involved multiple scientists working independently and at different times, defying a strict historical characterization. The accidental discoveries of penicillin and X-rays show limitations of exclusively inductive, abductive, or deductive characterizations of science. The discovery of cosmic microwave background radiation and cosmic rays involved the identification of phenomena that could not be explained by existing theories of radiation, requiring the formulation of new theories and the development of new experimental techniques. If we contrast our case studies it appears as though there isn't a monolithic method, but rather a complex and often unpredictable process that involves the creative and innovative thinking of individual scientists. What that one descriptively accurate methodology would be has been the source of constant debate since the time of David Hume.

It may be worthy of note at the time of this writing that according to many philosophers of science, abduction is a key component of scientific discovery. However, it cannot be the sole account. Abduction alone cannot guarantee the reliability and objectivity of scientific knowledge, as it relies heavily on the concomitant antecedents from which the scientist works. Also, induction itself is too prevalent. Bayesian reasoning as descriptive faces the same fate as any other exclusively inductive method on our historically informed view. The history of science is replete with examples that have hitherto escaped strict codification by monistic philosophers of science. Case studies show us that science can is not always categorizable into a single logical framework. It looks to be as if inferential patterns may be mixed, and they may involve the human capacities for individual creativity and insight.

5. Conclusions

Philosophers of science have found stories like the following quite appealing: scientific models were often developed based on theological beliefs, rather than empirical evidence. For example, in the medieval period, the belief in a geocentric universe was supported by the idea of a hierarchical order in which God was at the center, followed by the angels, then humans, and finally the physical world. This led to the development of the Ptolemaic system, in which the planets moved in circular orbits around the Earth. However, as observational techniques improved and new data became available, this model was found to be inaccurate. Astronomers such as Copernicus and Galileo proposed a heliocentric model, in which the planets revolved around the sun. This model was eventually supported by empirical evidence, such as the phases of Venus observed by Galileo through his telescope. Over time, as new data and observations became available, scientific models have continued to evolve and change. This process is known as scientific progress, and is driven by the development of new theories, models, and methods that better explain and predict natural phenomena. This is supposed to be the exemplary model of how science progresses as presented in Kuhn's The Copernican Revolution (1957). Stories like these follow a reliable pattern—linear and predictable. Something under which—we have shown—philosophers have eschewed significant particularities.

To argue that all science moves like this involves an assumption that all of these cases must be in some way homogenous. History shows us the opposite of a single common thread underlying every scientific advancement humanity has made throughout history, however. To foist one upon the factual data when we look back to make an assessment has hitherto led to Wittgenstein problem of "seeing what is common." We argue for a pluralism somewhat like Larry Laudan, and hold that this homogeneity is a chimera. Historical cases differ not only over the issue of entity realism, but also on the homogenous nature of scientific discoveries.

Our case studies strongly suggest that a monistic view could not possibly be historically accurate, but also how a pluralistic outlook would be better equipped to accommodate for idiosyncrasies that populate the history of science. Monistic views are disingenuous in their desire to make every historical instance fit an antecedent pattern. It would be incorrect to use the term "scientific community" in many cases, and also, there may be accidents, despite how well planned an experiment may be (and these may indeed have bearing on further successful theories). Case studies suggest that scientific inquiry is a complex and multifaceted activity. It most definitely cannot be reduced to a single method or approach. Perhaps philosophers need to be more descriptively accurate in our assessments of scientific discoveries. The methodological brushes with which they have hitherto attempted to paint science have been unwieldy, clumsy, and inaccurate. The downside to this more historically accurate view is that the added precision may be less convenient.

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