Kuhn and Duhem on Scientific Inquiry

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Thomas Kuhn and Pierre Duhem present radically different interpretations of how science is practiced. Though both of them can be labeled as anti-realists, they both use contrasting historical accounts to support their particular interpretations. On the one hand Duhem argues that there are legitimate ways of practicing science, and that scientific theories are the most valid pieces of information to understanding scientific practice. That is, when we put together a historical account of scientific practice, it is the theories that are most important. Kuhn, on the other hand argues that scientific practice is not always logical, because a scientist's idiosyncrasies and biases can often play a role in scientific practice. I argue that Kuhn presents a clearer and more accurate picture of scientific inquiry, using Albert Einstein's uneasy relationship with quantum mechanics as a prime example.

To fully understand the distinction between Kuhn and Duhem's interpretations, we first need to understand the difference between a context of justification and a context of discovery. Individuals that favor a context of justification approach prefer to deal with the actual theories that have developed throughout time. That is, they are not interested in the scientists themselves. In fact, they prefer to remove scientists from the picture and concentrate on how theories supersede one another and how theories are developed over the course of time. In other words, they would not be interested in the metaphysics behind a particular theory or the idiosyncrasies of any particular scientist. In contrast, those who prefer a context of discovery approach to scientific inquiry look at the issue from an almost opposite angle. They believe that we cannot have a complete picture of scientific practice unless we consider the scientists themselves. Another way of putting it is that scientific theories are important, yet the scientists behind the theories

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play a crucial role in our understanding of science. The metaphysics of a theory are important in its development, as are the actions that occur in the laboratory. For example, context of discovery supporters would be interested in Newton's religious beliefs in relation to his theory of gravity. Kuhn makes this distinction between the context of discovery and context of justification apparent in *The Structure of Scientific Revolutions.*

As an instrumentalist, Duhem prioritizes the context of justification. That is, science aims to "save the appearances". Theories become more refined and provide better explanatory power throughout history. Yet, Duhem has a particular interpretation of how science ought to be practiced and this approach needs to be understood if we are to gain a clearer picture of Duhem's historical interpretation. Much of this is explained in his paper, "Physical Theory and Experiment" (1954). Duhem makes it clear that he is against the concept of falsifying a hypothesis. Suppose we have the following conditional statement: if theory 1 is correct, then x follows. Suppose that it is not the case that x. Therefore, it follows that theory 1 is not correct. In this case, we're applying the rule of *Modus Tollens*. Now, as just a purely logical argument, *Modus Tollens* holds, but as Duhem argues, this is not a form of reasoning that can adequately capture scientific reasoning. That is, a theory cannot be refuted by simply showing that something that is supposed to follow from it does not hold.

Perhaps an example can clarify things. Take, for instance, the famous debate between the wave and particle theories of light. The wave theorists argued that their account was supported by phenomena such as the dark bands that formed when light is diffracted. Bands would not occur if light was composed of particles because there would not be instances of constructive or destructive interference as caused by waves. Particle theorists, on the other hand argued that experiments involving mirrors supported their account of light because the experiments allowed them to explain phenomena such as reflection and refraction. Since the particle theory grew out of Newtonian mechanics, the following was proposed: if light is composed of particles, then light will travel faster in water rather than air. This is due to the increased density of water. There are more molecules present, hence there will be a greater gravitational attraction that will cause the particles to travel faster through water.

In order to prove one theory correct and the other false, Foucault proposed an experiment to measure the speed of light in water. In other words, if the particle theory were correct, then light would travel faster in water than in air. If the wave theory were correct, then the opposite would hold. As it turns out, the experiment revealed that light travels faster in air than in water. According to the logical argument outlined above, the experimental results refuted the particle theory of light because if the particle theory were correct, then light should travel faster in water than in air. It is not the case that light travels faster in water. Therefore, the particle theory is incorrect, yet, this is the exact reasoning that Duhem argues cannot apply to scientific research. Why?

As Duhem argues, scientists are never just testing an isolated theory, but the entire system in which the hypothesis is rooted. Another way to put it, when the above experiment "proved" that light was not composed of particles, exactly which part of the system did it disprove? As Duhem asks, "Is it the fundamental hypothesis that light consists in projectiles thrown out…by luminous bodies? Is it in some other assumption concerning the actions experienced by light corpuscles due to the media through which they move?" (Klee 1999, p. 62) An experiment can tell a scientist that at least one hypothesis is wrong, but the experiment doesn't designate a particular one as false. A rather helpful analogy Duhem makes can further reiterate this point. A scientist is like a doctor in that if an organism is experiencing discomfort in a specific organ, the doctor will have to relate its effects through the entire system in which it plays a role, and cannot just isolate it from the entire system. S/he has to pinpoint the cause of the ailment by inspecting problems that affect the entire body. (Klee 1999, p. 62)

Let's tie things together to fully understand Duhem's argument. A "crucial experiment" is used to confirm one theory and disconfirm a competing one. This is very similar to a proof by contradiction that is commonly found in mathematics. Duhem argues that this form of reasoning doesn't work in science, because scientists do not know whether they have exhausted all of the possible explanatory theories. That is, there may be other theories that may explain the same phenomena that a refuted theory did but in an improved manner. Just because a theory was contradicted does not in any way imply that the competing theory is the correct one. For instance, we may assume from Foucault's experiment that light is *not* made of particles but we do *not* know that light is therefore made of waves. Essentially, Duhem is trying to point out that logical schematics don't completely map onto scientific reasoning.

For the most part, Kuhn appears to agree with Duhem regarding the idea of falsifying a hypothesis. However, he does not look at it in the same logical way that Duhem does. He is less concerned with the technical details of how such a process works and more concerned with the way scientists react in these instances and the actions that occur thereafter. In other words, rather than approaching the idea from a logical standpoint, Kuhn is more concerned with human commitment when such problems arise.

To better understand Kuhn's account, we discuss the concept of anomalies. Anomalies are instances that a given theory cannot explain. Let us build on the example that has been running throughout this essay. Suppose that Newtonian mechanics is the current paradigm that we are in. In other words, by "paradigm" Kuhn means that Newtonian mechanics is the system through which we view the world; it directs the research during a given period of Normal Science. That is, it is the system by which the majority of science operates; it directs the puzzle-solving process. Now suppose further that the above experiment has taken place and that it has been shown that light does not behave the way that our theory predicts it should. For a scientist who accepts Newtonian mechanics, this is certainly a problem, for an anomaly has arisen that cannot be fully explained by the current paradigm. However, as Kuhn argues, it must be kept in mind that anomalies are not actually seen as counter-instances to the established paradigm. Instead, they are viewed as puzzles. Since scientists, in Kuhn's view, try to extend their paradigm to explain phenomena, they are unlikely to initially see anomalies as threats to their system. Yet suppose they do arise, what does a scientist do? As Kuhn points out, in Ch. VII of *Structure,* there are some options.

First, let's assume that the above counter-instance isn't very serious for particle theorists in the sense that it threatens their theory (even though, from our standpoint, it is). Just one counter-instance would not be enough for a scientist to overhaul the entire theory of Newtonian mechanics. At this point, s/he may just ignore it, because s/he does not interpret the anomaly as endangering the paradigm in any serious way. However, what if the counter-instance was more serious? Would the scientist be more warranted to abandon the paradigm? Kuhn argues that s/he would not. Rather, a scientist will try to fix the problem within the theory. In other words, the scientist is committed to the current system and will work within it to try and accommodate the anomaly. This can be done in a couple of ways. It may involve abandoning a certain hypothesis within the theory. For our current example, light was shown not to be composed of particles, but the particle theory of light is closely connected with Newton's theory of gravity. Therefore, the scientist may try to either save the particle theory by formulating it in another way, or just abandon it, if possible; it is really a matter of the degree of commitment to the theory and the paradigm. Notice that, in contrast to Duhem's argument, scientists in Kuhn's account do not always consider the merits of a whole general theory when a particular hypothesis is shown to be flawed. They are more likely to take the hypothesis as an isolated entity and attempt to fix it, while maintaining the general structure of the current paradigm. It is only when the number of anomalies increase that doubts about the accepted paradigm start to grow.

This also brings up the issue of vitalness. Since gravity and light are closely tied together, a scientist will have to find a way that the system can be kept intact, while adjusting or abandoning a part of it. For instance, since Newton's theory of gravity plays a more vital role in Newtonian mechanics than a particle theory of light, s/he may be more inclined to keep it (or not adjust it) than s/he would the particle theory. Only when the counter-instances become too overwhelming do scientists eventually abandon a paradigm. As Kuhn puts it, "…resistance guarantees that scientists will not be lightly distracted and that the anomalies that lead to paradigm change will penetrate existing knowledge to the core." (Kuhn 1996, p. 65) In other words, for a paradigm to be overthrown, the anomalies have to overwhelm a scientist's resistance to paradigm change and put a paradigm's very foundation into doubt.

Notice that each philosopher's theory about the way science is and ought to be practiced ties in rather neatly with their respective historical approach. For Duhem, because theories are the most important aspect of understanding scientific practice, his interpretation of scientific inquiry almost demands a logical and more methodical routine from scientists as to cut out the metaphysics and idiosyncrasies. Scientists can't let biases play a role in scientific inquiry, because it may not allow proper theory progression That is, when scientists attempt to falsify a hypothesis, there isn't room for metaphysics in a theory. If scientists begin to bring in things such as bias into a theory, it interferes with the theory. For Kuhn, on the other hand because he believes that scientists do not practice science in such a logical and rigorous manner, any approach to understanding scientific inquiry has to include an acknowledgement of scientists' viewpoints and metaphysics because scientists do influence the progression of science – at least more so than Duhem would care to admit.

Based on the arguments presented, Duhem appears to be giving a normative account of science, while Kuhn is giving a normative account that is far more descriptive than Duhem's. Namely, Duhem puts forth a view of the way science ought to be practiced. It should follow a more methodical method. At the same time, Kuhn also feels that there should be a methodical approach to science, but argues that this is not what actually takes place. Any opinion presented as to whom has a more convincing argument has to account for this particular distinction, otherwise a comparison becomes difficult.

Though Duhem presents a rather compelling argument for the understanding of scientific inquiry, I have to side with Kuhn in this case because Kuhn presents a more accurate account of scientific inquiry. Science isn't as clear-cut as Duhem may like it to be. Scientists are committed to their particular paradigm, and anomalies are not always viewed as threats. Even when they are, scientists will stick with the paradigm because of their deep level of commitment, and try to fix apparent flaws rather than abandoning the system. They often do not treat collapses of a single hypothesis as threats to an entire system; rather, they often try to deal with the particular flaw by adjusting the hypothesis within the given paradigm. Only after multiple anomalies or counter-instances have seriously undermined a system are scientists willing to abandon a paradigm, and even then many don't.

Albert Einstein, one of the premier physicists of the 20th century, helped bring about the end of the paradigm known as Newtonian mechanics with his theories of relativity. Yet he, and most other scientists, still kept a component of Newtonian mechanics in the new paradigm, namely, they maintained that the universe was deterministic, such that actions of particles and other objects could be predicted and calculated mathematically. However, around this time, quantum mechanics (hereby QM) was being developed and threatened this belief in a deterministic universe. Despite ideas such as the uncertainty principle and demonstrations such as the two-slit experiment, Einstein refused to believe that the universe was probabilistic at the atomic level. Even his own findings about the photoelectric effect contributed towards the development of QM. It showed that light was composed of particles, yet had wave-like properties; hence, light has a wave-particle duality. This would set the stage for the modern two-slit experiment from which the measurement problem would arise. The experiment shows that light's properties depend on the property we wish to measure; we can't measure both at the same time. When light isn't being measured, it behaves like a wave, but if it is measured, suddenly it exhibits particle-like characteristics. Yet Einstein's commitment to the current paradigm was strong enough that he continued throughout his lifetime to counter many of the claims put forth by QM. It wasn't even the case that he figured that the entire system of QM that contained the flawed concept was wrong, just that the hypotheses concerning determinism may have to have been adjusted. He realized that QM gave experimentally powerful results, so it had merit, just that there were issues with indeterminacy that needed to be resolved.

In order to make sense of the experimental results that pointed to a probabilistic universe, Einstein proposed that

QM was not a complete theory. As he put it, "…quantum theory is solely to be ascribed to the fact that this [theory] operates with an incomplete description of physical systems." (Schilpp 1970, p. 666) At the macroscopic level, relativity gave a causal account of physical behavior. Einstein expected the same at the microscopic level as well. Notice that Einstein was not attempting to put an entire theory on the line, as Duhem may claim; rather, he was attempting to neutralize the specific counter-instances that attacked specific parts of the paradigm. In this case, QM attempted to adjust the relativistic interpretation at the microscopic level by arguing that there was a degree of probability involved. However, not all of relativity was on the line (as we see in modern theories, at the macroscopic level relativity survives, while QM dominates at the microscopic level).

This of course does not give the full account of Einstein's feelings towards QM. Einstein had the following to say about accepting a quantum mechanical interpretation, "To believe [what QM is telling us about the microscopic world] is logically possible without contradiction; but it is so very contrary to my scientific instinct that I cannot forego the search for a more complete conception." (Schilpp 1970, p. 235) In other words, despite what QM was indicating and regardless of how logical the theory was and the effectiveness by which it superseded the previous theory, Einstein's intuitions were so firmly rooted in the previous paradigm (a deterministic microscopic world), that counter-instances were still not enough to convince him to adopt the changes. That is to say, he couldn't let go of the idea that the universe, from the macroscopic to the microscopic, is fully deterministic.

Let's take a look at a particular series of events involving Einstein and examine how both Duhem and Kuhn would interpret it historically. Einstein was hard-pressed to show that QM was incorrect because it had such impressive predictive and explanatory power. Instead, his approach was to try and show that QM was an incomplete theory, as mentioned earlier. Recall the uncertainty principle. Basically, it states that we cannot know both the position and speed of a particle. We can measure one or the other, but once we measure one aspect, we lose the ability to measure the other.

Einstein along with Podolsky and Rosen (hereby EPR) concluded that QM was an incomplete theory. They argued, using a thought experiment composed of two particles, that particles have definite positions and speeds. If one believes, as most physicists do, that a theory of nature should describe all qualities of nature, than QM fails because it misses some qualities and hence is incomplete. (Greene 2004, pg. 102)

For many years, the EPR thesis was a debatable topic. It was a question of metaphysics more than physics. However, in 1964 John Bell gave a wonderful insight into this problem. If EPR were correct, then statistically, two particles at a distance with seemingly no connection, when passed through separate detectors, would have to agree at least 50 percent of the time. However, experimentally this was shown not to be the case. This implied a nonlocality was present at the quantum level. In other words, two particles regardless of the distance they were apart had a connection. When data of one was measured, data of the other could be known instantaneously. Hence, this contradicted the idea that for objects to affect one another, they had to be near one another, or have the quality of locality. That is, without meaning to, Einstein had helped further build support for QM. Instead of showing that particles had definite preexisting properties, their paper had helped show that this isn’t the case. Qualities are not determined until they are measured. (Greene 2004, p. 113)

Duhem would probably interpret the aforementioned series of events as follows. QM superseded relativity at the microscopic level because it had better predictive and explanatory capabilities than Newtonian-dependent relativistic mechanics. The uncertainty principle was one such aspect of QM. Nonlocality explained the changes to particles better than the local account that Einstein had been pushing for. Notice the clear movement through the theories without a mention of the metaphysics that helped spurn the theories.

Kuhn, however, would present a much different picture. QM did solve more puzzles than relativity and fixed many of the anomalies that came up under relativity. But now this is where the real difference occurs between the two interpretations. EPR were motivated, in part, because they were unwilling to accept the new paradigm change at the microscopic level. The attack upon QM, and the uncertainty principle in particular, was more of a disagreement over metaphysics than physics because determinism wasn't something that was scientifically verified. Rather, it is an assumption that is built into the theory. Bell's insight came about due to EPR, and actually strengthened QM, because it helped to show that determinism isn't a characteristic of particles, but that a definite value is only established by measurement and the same can be said for a distantly connected particle as well.

Both theories explain the events well, but Kuhn's account is far more accurate. Note that metaphysics do play a role in these events. Duhem's account is incomplete because it fails to takes that into account. The EPR paper would not have come about if Einstein was not so insistent that the metaphysics of determinism were correct. Hence, Bell's insight would not have come about to strengthen QM. Without taking the idiosyncrasies and metaphysics into account, Duhem misses vital aspects of quantum mechanical development.

Of course, Duhem can raise an objection. He can make the distinction that was mentioned earlier, namely, that he does not wish to give a descriptive account of scientific methodology. Instead, he is more concerned with a normative account, or the way science *ought* to be. His issue is with logical schematics that do not fully map the way science has been practiced. Scientists, as he argues, are using logic when doing scientific research, yet they have to understand that it is not as straightforwardly applicable as, say, it is in mathematics. Thus, Duhem is not worried about Kuhn's anomaly/counter-instance problem. However, the idea of falsifying a theory is key for him, and if scientists are to do science correctly, they must realize that they are not just testing an isolated hypothesis, but the entire system within which that hypothesis is imbedded.

Taking Duhem a bit further, certain historical interpretations take idiosyncrasies of scientists into account, and these do not necessarily contribute to our understanding of what is universal of scientific knowledge. It isn't the case that scientists aren't important for Duhem—they are. They, after all are the ones doing the science. It's just that all of their thought processes, philosophical attitudes, and other idiosyncrasies do not help us in understanding the nature of scientific inquiry. What the theories tell us as being right has nothing to do with how the ideas came to the scientists or the metaphysics involved in the theory. Hence, any historical account must come from a context of justification standpoint and leave out unnecessary details that do not further our understanding of theories improving upon one another.

Does Kuhn have an answer to such an objection? Certainly. More than likely, Kuhn would answer back that Duhem cannot give a normative account of scientific methodology that scientists are not likely to follow. Using his particular historical approach, Kuhn can argue that if scientists within a particular paradigm do not follow the line of reasoning Duhem proposes when dealing with a counter-instance, then we certainly can't expect that they ought to do as Duhem recommends when such situations arise. Science is a humanly created endeavor; anything we learn from it is tied to us. That is, we can't separate the scientists from the science and simply look at science as just being an accumulation of theories as Duhem may wish. Because there are cases like Einstein's, where scientists don't follow a "logical" method when dealing with anomalies, they are enough to question instrumentalist viewpoints such as Duhem's. As much as we may like to believe science is a logical endeavor, it isn't always the case, and we shouldn't expect scientists to be held to such standards. What can we expect of them? Exactly what they have been doing in the past: staying committed to the paradigm and combating potential counter-instances.

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