Divination by Science

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Introduction and Thesis

For over a hundred years now, we have been heatedly arguing over how to distinguish science from pseudoscience (or rather, from any non-scientific activity). This problem of demarcation is still around because we have difficulty in pinning down just exactly what we mean when we say “science” or when we call something “scientific”. “Scientific” is a term that *should* be reserved for our most respected knowledge - knowledge that we can be confident of due to the rigorous nature in which we ascertain it.

To pin down what we mean when we say “science” and hence, to settle this problem of demarcation, we will need to find out what is/are the defining characteristics of science. In this paper, I will attempt to show that the **essential** criterion for what makes a theory scientific is its predictive power. I will first introduce the problem by providing Kuhn and Lakatos’ views, which I believe are great descriptions of science. Then I will propose that what we care most about science is its technological applications and that scientific theories must make predictions in order to have any application. Finally, I will provide possible problems for using prediction as the quintessential criterion for science and also discuss some other criteria (proposed by Kuhn) to help settle the problem of demarcation.

Kuhn & Lakatos on Science

To discuss the demarcation problem, I think I should first briefly introduce Thomas Kuhn, who came up with an extremely persuasive description of how science works, and Imre Lakatos, who refined the model to something that was more appealing to Kuhn's critics.

Kuhn believed that science could only start when people came to an agreement on the **methodology of assessing the correctness** of ideas. He used the term *paradigm* to describe a pattern or set of rules that people would follow to carry out their assessments. Now, once people have a paradigm to settle disputes, they could make progress by developing more theories about how the world works and figuring out if their ideas were right or wrong. Kuhn called this *normal science*, a period of science (science was cyclical and had different periods in Kuhn's view) where people could engage in *puzzle* solving through "fact collection and theory articulation"[1]. The period succeeding normal science however, is what Kuhn called *crisis*. Sometimes, the paradigm that people are working under does not allow them to resolve issues or account for anomalous data that they get from their observations. When a critical amount of these anomalies build up, their paradigms become open to criticism as people begin to look for new paradigms that might satisfy their observations. Eventually, a *scientific revolution* occurs with a new paradigm replacing the old one and normal science ensues under the new paradigm. The cycle then continues, possibly ad infinitum. Kuhn also went on to say that competing paradigms were incommensurable and could only be assessed by their own standards. Many people took issue with this due to the implication that if Kuhn was right, scientific theories are chosen for personal (subjective) and hence "irrational" reasons. One of the persons with such an issue, was Imre Lakatos.

Lakatos had a view that was for the most part the same as Kuhn's but with different, clearer terminology. Much like paradigms, Lakatos' *research programmes* had a stubbornly defended *hard core* that was surrounded by a *protective belt* of auxiliary hypotheses that were amenable to change in order to protect the hard core. Apart from this terminology change, the critical difference in Lakatos' view is that he believes research programmes can run in parallel (whilst Kuhn didn't) and that we do have a "rational"[†] way choose between these paradigms/research programmes. Lakatos refers to research programmes as either *progressive* (good, scientific, fruitful) or *degenerate* (bad, pseudoscientific, unproductive). The way to differentiate between progressive and degenerate programmes is to look at their predictive power. Progressive research programmes lead to the discovery of novel facts (finding observations predicted by theory) whilst degenerate ones lag behind and play catch-up to the facts. Lakatos' solution however, still did not say when a degenerate programme should be terminated - there's always the possibility of a degenerate programme turning into a progressive one. Because of the inability of Lakatos' model to determine when a degenerate programme crosses the point of no return, it cannot be used as a solution to the demarcation problem.

Regarding this issue, Lakatos himself believed that a hard line couldn't be drawn to separate science from pseudoscience. For Karl Popper, that line could be drawn and was called *falsifiability*.[2] For Kuhn, the line seemed to be whether or not a field had *puzzles* to solve.[3] Lakatos on the other hand, thought there was no line and the scientificness of a theory had to rest on a continuum with our best science at the top end of the spectrum and pseudoscience at the tail end. I am very much with Lakatos in this respect, along with using his concept of research programmes to describe how science works… but what I wish to do

† I put "rational" and "irrational" in scare quotes because Kuhn thinks that even though scientists choose paradigms based on a mixture of "subjective and objective, or of shared and individual criteria", science **does** operate rationally (with intersubjectivity), which I'm inclined to agree.

today is to, in Kuhn's terminology, "articulate" the importance of the criterion of **prediction** and to show that it is quintessential to science.

Prediction is what we really care about

In philosophy, I believe we are mostly arguing about how to describe concepts which we are very familiar with, concepts that we seem to intuitively understand and are capable of providing examples for… but these concepts are difficult to define concretely. "Science" is just such a concept and I think that since we have a pretty good intuition of what science is, we should appeal to that intuition to figure out what we really mean when we talk about "science".

To expose our intuition of what science really is, I will begin with a hypothetical quotation:

> ***"Scientifically proven!"***
>
> - Every advertisement ever

This phrase has been used so frivolously that people are now unreceptive to it but you will still frequently find the same **tone** used in many advertisements today. Why? Because *most* people trust science and consider scientific knowledge to be one of the most robust kinds of knowledge we can have. To have something be scientifically proven is to have it be "really" proven.

It's "scientifically proven"! Though I think "designed or tested with some methodology that kind of resembles science" is more accurate.

People understand that a lot of the tools they use are contingent on technology that is developed using science. Many people would also agree that technology improves our efficiency, safety, and by extension quality of life. For this reason, I say that what we (people in general, not just the intelligentsia) treasure most about science is its powerful **applications** through engineering and technology. It can be argued that there is great value in the "explanatory power" of scientific theories, or that going on the never-ending quest for "truth" is, in and of itself, a great virtue. However, people are often happily satisfied by nonsense explanations (I guess I can only call them nonsense with the benefit of hindsight) and also enjoy casually considering "truth" to simply be "based on perspective". Nonetheless, everyone has ultimately been convinced about the efficacy of science **due** to its technological applications.

Just to be thorough, it should be stated that technology does not **require** science, as can be seen in more primitive technologies such as *obsidian spear points* and *wheels*.

However, much of our technology today relies on information revealed to us by science. An oft cited example is the radio, which (I believe it cannot be overstated) has dramatically changed our lives for ever. Our understanding of electromagnetic waves and its manipulation has allowed for experimentation and refinement of radio technology to the point where most people, almost anywhere in the world, can communicate with one another. Although experimentation with wireless (not just radio) communication began before James Clerk Maxwell predicted the existence of electromagnetic waves, the prediction his theory made stimulated the field greatly and, along with Hertz's experimental verification, led Marconi to develop the first device capable of long range radio communication. From this short example, it can further be restated that science reveals to us what is possible and shows us where to focus our efforts for technological advancement.

So how does all this connect to the predictive capability of scientific theories? Scientific models of the world that provide any **useful** information to us at all have to make predictions. A theory that makes no predictions has, in a sense, no effect on the world. Another way to look at it is in terms of falsifiability. If a theory cannot be falsified by some observed effect in the world, it has no **effect** or **usefulness** in terms of technological applications for us.

Take for example the hypothesis that "God is love". What does this predict? The words "God" and "love" by themselves are so ill-defined, people will happily attach their own meanings to those terms. For the hypothesis to be useful to us, we will need to agree on a definition, and the resultant hypothesis would need to somehow entail that if "God is love", then some specific observable thing will happen. For example, let's say that in our theory "God" actually means "dog" and "love" actually means "hungry". If our model also says we expect something to happen when “dog is hungry” - something like “dog will look for food to eat” - then we have made a prediction and our theory is now falsifiable (it will be falsified if our prediction does not come true). Without falsifiability, the hypothesis supposes no observable **effect** for us and hence is not useful to us and can be considered to be a **meaningless** hypothesis/sentence.

Possible problems with prediction

One counterexample that might serve to derail this line of thinking is *string theory*. String theory is commonly considered to fall under the purview of science but in is current state is effectively unfalsifiable… how do we deal with this? I say the issue is fairly straight-forward - string theory is an example of a degenerate research programme and is becoming **less and less of a science**. According to Lee Smolin, string theory originally precluded the possibility of dark energy and when the universe was discovered to be accelerating (which implied dark energy), string theory modified itself to fit the observation.[4] String theory today, has many forms and is referred to as superstring theory and even M-theory, which is basically a combination of all the internally consistent string theories. I must admit that I know very little about string theory and that Lee Smolin’s argument is also controversial (to the small group of people qualified to talk about string theory) but string theory itself has been a constant target in the scientific community for being very unfruitful throughout its lifespan. This is not to say that we should suspend research on string theory, certainly not. Though Lakatos might say that it is not our business to decide on such policies, I am willing to go further to say that we should continue such research **indefinitely**, with the resources spent being **appropriate** to how fruitful people anticipate the field to be. I’m probably going off on a tangent here but I think we should **never** consider a research programme to be fully killed or un-revivable. Much in the same way that the famous (or infamous) atheist polemicist

Christopher Hitchens, when asked if we should "in fact wish to see a world without faith", to the surprise of his peers, replied "I think I would have to say that I don't."[5], I think we should **not** wish to see thoroughly degenerate research programmes abandoned forever. Hitchens goes on to weakly (at least I think it's weak) justify that we require an opposing view in order apply a sort of dialectic method, so that we can understand why we believe in what we believe in (in his context, religion or God, in our context, degenerating research programmes or theories). He does not however go on to explicitly say that there is a chance we might be wrong, but I think that since we can all agree that there is always such a miniscule chance, that is reason enough that we should apply just as miniscule an effort to continue research in degenerating fields (e.g. some pitiful person somewhere might still be unsuccessfully trying to rectify the Ptolemaic model, and that person should not be stopped on "scientific" grounds… perhaps that pity might turn to praise one day, though we are all right to highly doubt that). Before I end this section with string theory, I should probably note that there is this notion floating around that theories with mathematical elegance, a kind of simplicity, has historically often turned out to be true… the implication of this is that we might want to include theories (like string theory) with mathematical elegance as falling under the purview of science even though they might as of yet make no predictions. I think this makes a case for the criterion of simplicity being important in science but I don't think that makes it **necessary**. Perhaps this is more a question of mathematics than science but in any case, for something like string theory, the fact that defenders of it frequently push back on the notion that it does not have any predictive power is also good indication that these same people who argue for elegance do believe that predictive power is of crucial importance in science.

Going back to talking about the criterion of predictive power, I believe that the discourse above is persuasive in saying that predictive power is necessary for considering whether or not something is scientific… but is it sufficient? Well, I don't claim that it is but to help explore this question, we can now, instead of looking at string theory - a field we generally think of as scientific but difficult to test - look at a field that is actually quite easy to test but that we think of as unscientific: astrology.

Astrology is now considered by most scientists, universities and careful thinkers in general to be a pseudoscience but it used to be treated much more seriously and was a very widely practiced system (there are still plenty of people and even large organisations that believe in or practice astrology, but this is rare in academic and governmental institutions). Just like the science of today, astrology actually also made testable predictions. The difference is that these predictions were never reliable. Even in its current form, the claims of astrology are testable. Shawn Carlson published a paper in Nature in 1985, where he described two double-blind tests performed under conditions which both advisors of the scientific community and of the astrological community agreed upon.[6] The results, unsurprisingly, showed that predictions made by natal astrology proved no better than chance. There have been more tests with unfavourable results for astrology but despite its constant failure, astrology remains popular (at least we can agree though that it has been relegated to the status of pseudoscience – even believers of astrology themselves generally accept that the system is unscientific.) In this sense, we can consider astrology to be a **former** science that has since been disproven but for reasons unknown (probably psychological and historical), continues to be practiced as a **pseudoscience**… the Lakatosian way to put it would be that astrology is now a degenerate research programme. The factors leading to astrology's transition from science to pseudoscience can be viewed as due to the constant **failure of astrology's predictions** as well as due to its **lack of fruitful research opportunities** (such as to develop the theory to make new predictions). These two issues mentioned above in bold can be thought of as more criteria for what makes a scientific theory good and serves as my segue into talking about criteria other than predictive power.

So what about criteria other than predictive power?

The two issues of astrology just mentioned (above in bold) can be seen as *accuracy* and *fruitfulness*... and constitutes two of the five criteria that Kuhn proposed is his paper "Objectivity, Value Judgment, and Theory Choice".[7] The criteria are all listed below and for the sake of brevity, are tagged with a short definition that I think fairly describes what Kuhn meant when he listed them.

1. **Accuracy**

   Theories' predictions should match observations.

2. **Consistency**

   Theories should be both internally and externally consistent.

3. **Scope**

   Theories should be widely applicable; it should predict more than we already observe.

4. **Simplicity**

   Theories should make previously confusing sets of phenomena more orderly and hence easier to understand.

5. **Fruitfulness**

   Theories should lead to showing us new, previously unknown phenomena or relationships.

I actually want to say that these criteria can sort of be seen as measures of predictive power but before I do that, I should point out some problems for Kuhn's five criteria (which he also pointed out himself). These criteria make sense in the abstract but looking at them carefully and attempting to apply them is very difficult.

First off, the individual criteria are difficult to quantify and are kind of open to interpretation depending on the context/theory they are being applied to. For example, one theory might be more accurate in one matter whilst the competing theory might be more accurate in another (let's say theory A of the solar system is more accurate at predicting the location of the moon but theory B of the solar system is more accurate at predicting the location of mercury… all else being equal, how do you decide?)

Secondly, since we have multiple criteria, apart from "theory choice" we now also have the issue of "criteria choice". For example, what do we do when one theory has better *scope* and another has better *fruitfulness*? Which should take precedence? That is again, unclear.

Because of problems like these, Kuhn proposed that these criteria were intersubjective (I'm using this term, again, for the sake of brevity) and argued that although it is not does follow hard and fast rules or "algorithms", it is not using these criteria for theory choice is not irrational because they are agreed upon standards. Kuhn also felt that the fact that there were no hard and fast rules was actually a strength and not a weakness of science, since there was room for creativity (instead of strictly following some algorithm that always lead to the same conclusion), science could grow.

Apart from what Kuhn thinks, I think (with my admitted tunnel vision for supporting predictive power) the criteria are all actually very closely related to predictive power. *Accuracy* obviously refers to how well predictions and observations match, *consistency* is really about having predictions from theories that don't conflict with one another, *scope* is about how a scientific theory should not just account for current data but also be able to account for future data (make predictions!) and *fruitfulness* talks about how likely we think a theory can be developed to give us more predictions. *Simplicity* however, is a different beast that probably deserves its own paper and is beyond the scope of this one but it suffices to say that you could also rate predictions by their simplicity or complexity (though this is not what Kuhn meant, the definition Kuhn gave is more akin to saying that theories that are simple are able to **explain** phenomena).

With all that in mind, I think that though you can come up with other criteria, it still all boils down to prediction.

Summary

Scientific theories can be grouped into degenerate or progressive research programmes but what criteria should we use to differentiate degenerate programmes from progressive ones? Predictive power is a prime candidate and here is why:

1. We should define science by what people consider to be most important about science
2. People consider the technological advancements of science to be the most important thing about it
3. For a scientific model to be able to contribute to technological advancements, it must have predictive power

Therefore, predictive power is the defining characteristic of what makes a model or theory scientific. There are still issues with trying to use predictive power as a hard and fast rule for determining whether or not something is scientific (it might, for example, be that the domain is not dichotomous but continuous) but the point I'm making in this paper is not that using predictive power as the criterion for theory choice solves demarcation, it is that predictive power is at the heart of all that we think is science and so must play a role in defining what science is (if we ever somehow come to an agreement on that).

Our reverence and awe for science can be seen as analogous to the awe and fear tribesman had for their soothsayers. Throughout human history, we find all methods of divination spread throughout the world: geomancy, oneiromancy, hydromancy, hepatomancy, cartomancy, cleromancy, palmistry… the list goes on and on. But today, one method of divination stands out above the rest. Today, with science, we can have that power of divination, but this time, we can have it for real.

References

[1] Kuhn, T. (1962) The Structure of Scientific Revolutions: 50th Anniversary Edition, page 59

[2] Popper, K. (1963) Conjectures and Refutations, page 39

[3] Kuhn, T. (1970) Logic of Discovery or Psychology of Research, page 9

[4] Smolin, L. (2006) The Trouble with Physics: The Rise of String Theory, the Fall of a Science, and What Comes Next, page 150

[5] Hitchens, C. (2007) https://www.youtube.com/watch?v=TaeJf-Yia3A#t=9m40s (9m 40s)

[6] Carlson, S. (1985) A double blind test of astrology (in Nature Vol 318) http://muller.lbl.gov/papers/Astrology-Carlson.pdf

[7] Kuhn, T. (1977) Objectivity, Value Judgment, and Theory Choice

Link to the weight-loss pill advertisement picture: http://www.amazon.com/SimplySupplements-Slimmex-Capsules-Scientifically-Targeted/dp/B00KX8CINQ