Growth of Science

How the Growth of Science Ended Theory Change

Ludwig Fahrbach

Heinrich-Heine-Universität

Universitätsstr. 1

40225 Düsseldorf

Ludwig.fahrbach@googlemail.com

Abstract

This paper outlines a defense of scientific realism against the pessimistic meta-induction which appeals to the phenomenon of the exponential growth of science. Here, scientific realism is defined as the view that our current successful scientific theories are mostly approximately true, and pessimistic meta-induction is the argument that projects the occurrence of past refutations of successful theories to the present concluding that many or most current successful scientific theories are false. The defense starts with the observation that at least 80% of all scientific work ever done has been done since 1950, proceeds with the claim that practically all of our most successful theories were entirely stable during that period of time, and concludes that the projection of refutations of successful theories to the present is unsound. In addition to this defense, the paper offers a framework through which scientific realism can be compared with two types of anti-realism. The framework is also of help to examine the relationships between these three positions and the three main arguments offered respectively in their support (No-miracle argument, pessimistic meta-induction, underdetermination).

How the Growth of Science Ended Theory Change [1]

The paper consists of two parts. In the first part, I introduce three positions and the three main arguments of the scientific realism debate: I define the version of scientific realism that I want to defend and present the No-miracle argument; I present pessimistic meta-induction (PMI), and define projective anti-realism which is based on PMI; I define empiricism (a simplification of van Fraassen's constructive empiricism 1980), which is usually taken to be based on the argument of underdetermination. I then analyse the relationships between the three positions and the three arguments. In the second part, I describe the exponential growth of science, and use it to outline a refutation of PMI. The envisaged refutation will in part be fairly programmatic, as a detailed elaboration of the full argumentation and a discussion of all possible objections cannot be done here. Finally, I examine the consequences of the refutation of PMI for the three positions.

1. Scientific Realism

In this paper, I will discuss three positions, scientific realism, and two forms of anti-realism.[2] I understand the position of scientific realism to consist in the claim that our current empirically successful scientific theories are probably approximately true; in other words, for our current empirically successful scientific theories the inference from their success to their approximate truth is a valid inductive inference. Examples of such theories are the atomic theory of matter, the theory of evolution and claims about the role of viruses and bacteria in infectious diseases. Realists support their position with the no-miracles argument: "Given that a theory enjoys empirical success wouldn't it be a miracle if it nevertheless were false? Wouldn't it be miracle, if, for example, infectious diseases behaved all the time, as if they are caused by viruses and bacteria, but they are not caused by viruses and bacteria?"

In what follows, I often omit "probably" and "approximately" in "probably approximately true" and simply use "true".[3] Furthermore, I use the term "theory" in a rather generous sense so that it also denotes laws of nature, theoretical statements, sets of theoretical statements and even classification systems such as the Periodic Table of Elements. The reason for this use is that realists usually want to defend the truth of the statements involved in these things as well.

In the realism debate the notion of empirical success is usually left rather vague. I want to make it at least a bit more precise. Thus, a theory is defined as being empirically successful (or simply successful) at some point in time, just in case its known observational consequences fit with the data gathered until that time, i.e., the theory describes correctly, as far as scientists are aware, all observations and experimental results gathered until that time, and there are sufficiently many such cases of fit. By contrast, if a consequence of a theory conflicts with some observations and scientists cannot find any other source of error, e.g. cannot blame an auxiliary statement, this application of the theory becomes an anomaly for the theory. If the anomaly is significant or the anomalies accumulate, the theory is refuted and does not count as successful (compare Paul Hoyningen-Huene 1993, 7.1-7.3). In that case a theory change may take place. Needless to say these definitions of the notions of empirical success of a theory and an anomaly of a theory rest on a fairly simple view of theory testing, but a more realistic view would make our discussion much more complicated and has to be left for another occasion.

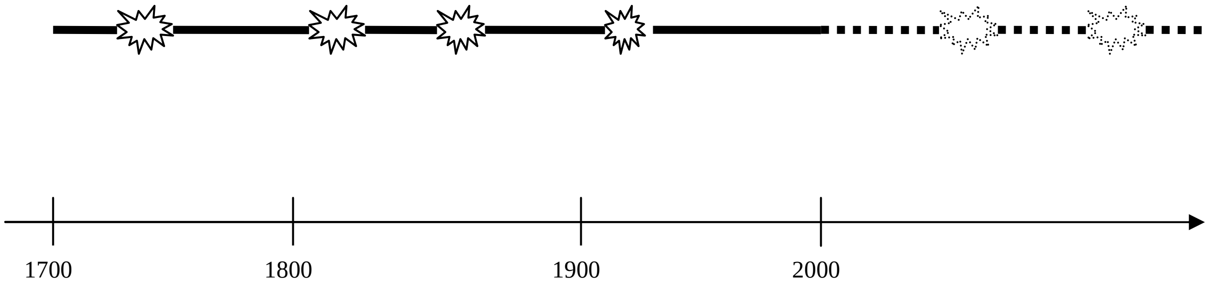
2. PMI and projective Anti-realism

An important argument against scientific realism is PMI. The version of PMI that I will use in this paper starts from the premise that the history of science is full of theories that were accepted for some time, but were later refuted and replaced by other theories, where these theory changes occurred even though the refuted theories were empirically successful while they were accepted. There are different ways to make the premise more precise, especially how to understand the term "full of" which will be dealt with later (see also Fahrbach 2009).

The premise of PMI requires evidence. Thus, antirealists present long lists of examples of such theories. Larry Laudan (1981) famously presents the following list of theories, all of which were once successful, and all of which are now considered to have been refuted[4]:

- The crystalline spheres of ancient and medieval astronomy
- The humoral theory of medicine
- The effluvial theory of static electricity
- ’Catastrophist’ geology (including Noah’s deluge)
- The phlogiston theory of chemistry
- The caloric theory of heat
- The vibratory theory of heat
- The vital force theories of physiology
- Electromagnetic ether
- Optical ether
- The theory of circular inertia
- Theories of spontaneous generation.

An example discussed especially often in the literature is the sequence of theories of light in the 18th, 19th and early 20th century. Here is a highly compressed history of these theories: Newton and others suggested that light consists of particles. This hypothesis had some success and was therefore accepted. Later came wave theories of light, which described light as waves in the ether, an all-pervading substance. These theories explained a lot of known phenomena and even predicted new kinds of phenomena. Nevertheless, the concept of ether was subsequently rejected and light waves were accepted as something not in need of a carrier. Then, Einstein reintroduced particles; and finally the “probability waves” of Quantum Mechanics came up. See Figure 1. Anti-realists like to point out that this sequence of theories cannot be viewed as gradually getting closer to the truth, because it involves deep ontological changes, e.g. from particles to waves or from accepting to abandoning the ether.



[figure 1: The sequence of theory changes in the case of theories of light and the projection of that sequence into the future]

Many philosophers have considered these examples to be impressive evidence for the premise of PMI (that the past of science is full of successful but refuted theories). PMI invites us to infer from this premise that many of our current successful theories will be refuted some time in the future. If this reasoning is correct, scientific realism (which holds that our current successful theories are probably true) is proven false. (I assume here that PMI trumps the NMA.) PMI forms the basis of the first kind of anti-realism that I want to consider in this paper, which I will call "projective anti-realism". Projective anti-realism accepts the conclusion of PMI, expecting future refutation of many of our current successful theories, and holds that because we don't know which ones will be refuted, we should not believe any of them, i.e., we should either disbelieve or be agnostic about them. A variant of this position is Kyle Stanford (2006, Ch 8). Further variants of both PMI and projective anti-realism are presented and discussed in Fahrbach (2009).

3. Empiricism

The second anti-realist position I want to discuss is a simplified version of Bas van Fraassen's position (1980) which I will call "empiricism". According to empiricism we may only believe what successful theories say about observables (entities or phenomena), but should always be agnostic about what they say about unobservables: Even if we knew that a theory is compatible with all observable evidence, we should not believe what it says about unobservables. Here, an entity or phenomenon counts as unobservable if humans cannot observe it with unaided senses, but only possibly with the help of instruments such as microscopes or Geiger counters. To take an example which is particularly implausible from a realist point of view, human sperm cells measuring 50μm including tails are unobservable individually, while human egg cells are observable having the size of the dot at the end of this sentence. Hence, we may believe in the existence of human egg cells, but should be agnostic about the existence of human sperm cells.

Empiricism differs from projective anti-realism, for the reason that some successful theories about observables have been subject to theory change. To see this note that there exist many phenomena that are observable in van Fraassen's sense, but have not been observed by humans directly so far. For example, while important parts of the theory of evolution are about observable phenomena (since the main mechanisms of evolution, namely random variation of phenotypic properties of organisms and selective retention operating on phenotypic properties, can be understood as observable correlations between observable properties), the direct observation of the observable long-term consequences of these mechanisms such as the development of new organs, new species or new higher taxa has mostly not been possible for us humans up to now, because humans have not existed for long enough. Instead, biologists have mostly had to rely on inferences from other kinds of observations (fossils, the current distribution of phenotypic similarities and differences, etc.). Similarly, the moons of other planets count as observable for van Fraassen, because humans are in principle able to observe them from nearby, but so far and for some time in the future, humans have not been able and will not be able to perform these kinds of observations. Furthermore, there are processes of the past which humans could have observed had they been around at the time. For example, the large-scale movements of tectonic plates and large parts of the development of the "tree of life" on earth are observable, but they could not be observed by us, because they happened before human beings existed; instead scientists have had to rely on inferences from the more limited stock of observations and measurements that are actually available to them.[5]

Scientists have often devised and accepted theories about all these kinds of observable phenomena and processes. They have done so, because they thought they possessed indirect evidence that allowed them to infer the truth of the theories. Nonetheless, some of these theories were refuted later on. For example, the theory of evolution, plate tectonics and theories about the existence of other planets' moons had predecessors that were arguably successful and accepted for some time, but later refuted. Because of these theory changes, projective anti-realism expects future changes for many of these theories and recommends not believing them, while empiricism offers no such recommendation. This shows that the two positions are distinct. That the successful, but refuted theories were not specifically about unobservables matches nicely with the claim argued for in Ludwig Fahrbach and Claus Beisbart (2009) that PMI offers very little support for the claim that is distinctive of empiricism that even if we knew a theory to be compatible with all observable evidence, we should not believe what it says about unobservables. Accordingly, the main argument usually thought to support empiricism is the argument from underdetermination which states that even if scientists possessed all possible empirical evidence any theory compatible with that evidence would still have indefinitely many incompatible rival theories also compatible with that evidence.[6]

4. <u>A Framework for Realism</u>

Let us further compare the three positions (scientific realism, projective anti-realism, and empiricism) and the three arguments (NMA, PMI, and underdetermination). Scientific realists may describe and defend their position as follows. The inference from success to truth for our current successful theories can be decomposed into three inferential steps: from past success to past-plus-future success, from past-plus-future success to empirical adequacy, and from empirical adequacy to truth. Here past-plus-future success of a theory means that scientists have neither refuted it in the past nor will ever refute it in the future. Instead of "past success" and "past-plus-future success" I will also use the terms "past stability" and "past-plus-future stability". From the realist's perspective all three inferential steps are reliable inductive inferences.

By using probabilities the three steps can be represented as follows. Consider the equation

$$\Pr(\text{T true}|\text{T successful so far}) = \Pr(\text{T true }|\text{T empirically adequate}) \cdot \Pr(\text{T empirically adequate }|\text{T successful forever}) \cdot \Pr(\text{T successful forever }|\text{T successful so far}),$$

where the equation holds because of both the probability theorem $\Pr(ABC|D) = \Pr(A|BCD) \cdot \Pr(B|CD) \cdot \Pr(C|D)$, and the logical implications:

truth → empirical adequacy → past-plus-future success → past success.

The three inferential steps correspond to the three terms on the right hand side of the equation (in reverse order), such that accepting an inferential step from X to Y means believing that $\Pr(Y|X)$ is close to one. Realists accordingly believe that for any of our current successful theories T all quantities on the right hand side of the equation are very close to one and consequently the posterior Pr(T true|T successful so far) is close to one as well.

Realists may support each of the three steps with the help of the NMA, by braking it up into three parts. For the first step the NMA states that it would be a miracle, if a theory that has been stable despite numerous empirical tests and challenges in the past were empirically refuted in the future. (Note that the corresponding inference to the best explanation cannot be broken up in such a way, because for realists empirical adequacy of a theory is not the best explanation of its past success.) In a similar vein, the second step is plausible, because if a theory is compatible with all past and future observations actually made by humans, then it is very probably compatible with all observations humans could possibly make.[7] Hence, theories which will remain stable in the future are very probably empirically adequate. Some theories, e.g., the multiple occurrences of ice ages on earth or important parts of the theory of evolution, are exclusively about observables. For these theories, empirical adequacy coincides with truth, and we have reached truth already after the second step. Finally, the third inferential step from empirical adequacy to truth is supported by the NMA as well. Needless to say proponents of both kinds of anti-realism reject the NMA, whether as a whole or in any of the three steps although they may accept some of the three steps for other reasons.

Scientific realism is threatened by PMI and underdetermination. Consider first PMI. In the version I presented it above, it has the conclusion that many of our current successful theories will be refuted in the future. Therefore, it is an attack on the first step from past success to past-plus-future success. If PMI is cogent, then in the equation above the term Pr(T successful forever |T successful so far) cannot be near one. So it is this part of realism (the first step) that is threatened by PMI. Also it is this part of realism that proponents of projective anti-realism reject: Because they expect future refutations of many current successful theories, they hold that many of our current successful theories are false in many of their statements about observables. By contrast, empiricism as defined above is silent on the first step, (although some proponents of empiricism, e.g., van Fraassen (2007), seem to accept PMI and therefore would also have to reject the first step). If the first step cannot be taken, the two other steps cannot be taken either, at least in this framework. Accordingly (and because many abandoned theories of the past were about unobservables as well as about observables), proponents of projective anti-realism also don't believe the "theoretical superstructure" of our current successful theories.

Actually, the first step may be divided into two sub-steps. The first sub-step is the inference from past success of a current successful theory to its past-plus-future success where the future success is restricted to the non-novel predictions of the theory. This inference can be thought of as an instance of enumerative induction, and is more plausible and less under threat from PMI than the second sub-step. (It is under threat from the problem of induction, of course, but no position discussed here accepts inductive scepticism.) The second sub-step is the step from past-plus-future success restricted to non-novel predictions to full past-plus-future success also involving the novel future predictions of the theory in question. This step is not an instance of enumerative induction. For example, the theory's future novel predictions may involve new concepts which don't occur in the theory's consequences that constitute its past success. Kyle Stanford (forthcoming) then can be understood as claiming that the history of science shows that theories adduced for the premise of PMI predominantly failed in their novel predictions; therefore he can be understood as by and large accepting the first sub-step, but rejecting the second sub-step.

The argument from underdetermination of theory by all possible evidence attacks the third step from empirical adequacy to truth. It implies that for those of our current successful theories T which say something about unobservables the term Pr(T true |T empirically adequate) cannot be near one (and timelessly so, because the value of the term does not change over time). Thus, underdetermination and PMI target different parts of the realist position: While underdetermination leads one to expect our current successful theories to be wrong in their unobservable parts only, PMI as presented here leads one to expect many of those theories to be wrong in their observable parts already. The argument from underdetermination is accepted by empiricism, which therefore rejects the third step. In contrast, projective antirealism as defined above is silent about underdetermination, and Stanford, for one, argues that underdetermination is not a cogent argument (2006, Ch. 6).

Realists are, of course, not impressed by the argument from underdetermination. They normally reject it, typically because they endorse (implicit in the NMA) superempirical virtues, IBE, and so on which they think can serve to overcome most cases of underdetermination. They may argue, for example, that normally only one of the empirically equivalent rival theories possesses superempirical virtues to a sufficient degree, and is therefore the only possible candidate for the true theory.

5. The Exponential Growth of Science

Let us now turn to developing the argument against PMI. Consider the amount of scientific work done by scientists in different periods of time, and how that amount increased over time. Here, "scientific work" means such things as making observations, performing experiments, constructing and testing theories, etc. Let us examine two ways of how the amount of scientific work done by scientists in some period of time can be measured: the number of journal articles published in that period and the number of scientists working in that period. Because we are only interested in very rough estimates of the amount of scientific work done in different periods of time, we can accept either quantity as a plausible way to measure overall scientific work during those times. As will turn out in a moment, both ways of measurement lead to roughly the same results. [8]

First, consider the number of journal articles published by scientists every year. Over the last few centuries, this number has grown in an exponential manner. When a quantity grows exponentially, an interesting characteristic of that growth is its doubling rate, i.e., the length of time in which the magnitude doubles. The doubling rate of the number of journal articles published every year has been 15 – 20 years over the last 300 years (see figure 2; further references and calculations can be found in Fahrbach 2009).

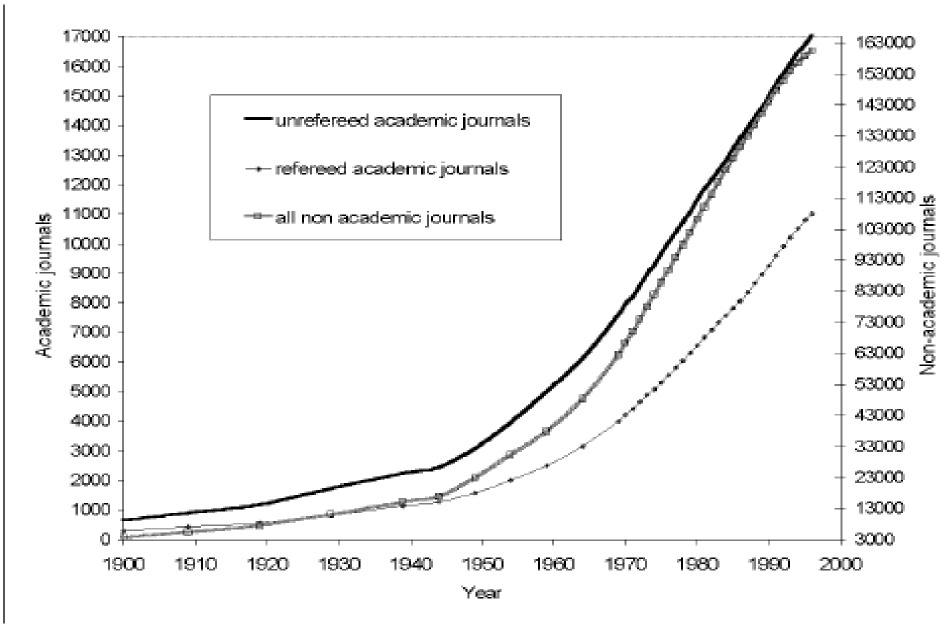
Where do these numbers come from? They come from Bibliometrics, the “quantitative study of documents and document-related behaviour” (Jonathan Furner 2003, p. 6). Among other things, Bibliometricians attempt to describe the quantitative development of scientific publishing as a whole and of different scientific disciplines and sub-disciplines over various periods of time. (Not surprisingly, it turns out that for many scientific disciplines and sub-disciplines an exponential function does not provide the best fit with the data.[9])

The other measure of scientific work I want to consider is the number of scientists. In the 1960s, Derek de Solla Price claimed that in the last 300 years, the number of scientists doubled every 15 years (De Solla Price, 1963, pp. 7, 10). This estimate of the doubling rate seems to be too low (Jack Meadows 1974, Ch. 1). Unfortunately, reliable numbers of the number of scientists for times earlier than 1960 are surprisingly difficult to find (Mabe/Amin, 2001, p. 159). Nevertheless, we only need crude estimates. Thus, for the last 300 years until the 1960s a reasonably conservative estimate is a doubling rate of 20 years (Meadows 1974, Ch. 1).

De Solla Price famously stated:

> During a meeting at which a number of great physicists were to give first-hand accounts of their epoch-making discoveries, the chairman opened the proceedings with the remark: 'Today we are privileged to sit side-by-side with the giants on whose shoulders we stand.' This, in a nutshell, exemplifies the peculiar immediacy of science, the recognition that so large a proportion of everything scientific that has ever occurred is happening now, within living memory. To put it another way, using any reasonable definition of a scientist, we can say that 80 to 90 percent of all the scientists that have ever lived are alive now. Alternatively, any young scientists, starting now and looking back at the end of his career upon a normal life span, will find that

> 80 to 90 percent of all scientific work achieved by the end of the period will have taken place before his very eyes, and that only 10 to 20 percent will antedate his experience. (de Solla Price, 1963, p. 1)



[Figure 2. Cumulative academic and non-academic journal growth (from Michael Mabe and Mayur Amin 2001, p. 154).]

De Solla Price's dictum that "80 to 90 percent of all the scientists that have ever lived are alive now" dates from 1963. Is it still true today? What is certainly true is that in the last three decades, since the 1970s, the growth of the number of scientists in Europe and America has slowed down considerably. Still, between the early 1960s and 2000 the number of research doctorate recipients in the U.S. doubled twice, which also gives a doubling rate of roughly 20 years (Allen Sanderson et al 2000, pp. 11-14, 49-50). In addition, the growth rate in Asian countries such as India and China has been very strong in the last two decades, counteracting any tendencies of slowdown in the West.[10] Thus, De Solla Price's dictum is very probably still true today.

It is interesting to compare De Solla Price's dictum with the corresponding statement about the number of humans of the species Homo sapiens that have ever lived on earth until today. Estimates of the latter are also surprisingly difficult to muster, but a reasonable estimate seems to be that around

100 billion people have so far lived on earth.[11] Assuming this estimate, only around 7 percent of all people who have ever lived on earth are still alive today.

Thus, both the number of journal articles and the number of scientists have grown with a doubling rate of 15-20 years.[12] This is a very strong growth. It means that half of all scientific work ever done was done in the last 15-20 years, while the other half was done in all the time before; and three quarters of all scientific work ever done was done in the last 30-40 years, while in all the time before that, only one quarter was done. As is shown in Fahrbach (2009), these doubling rates imply the results we need for the next section, namely that at least 95% of all scientific work ever done has been done since 1915, and at least 80% of all scientific work ever done has been done since 1950.

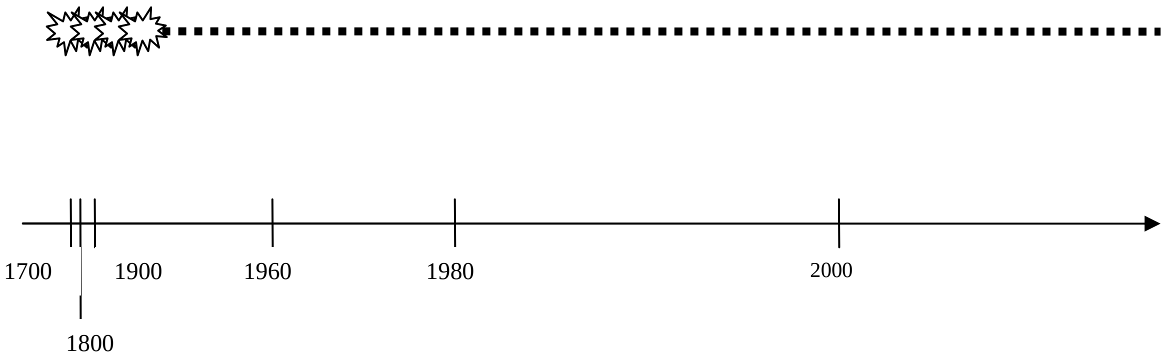
6. Refutation of PMI

Let us now examine how the exponential growth of science affects PMI. I will just offer an outline of an argument against PMI, because for reasons of space I cannot fully develop all parts of the argument at this place. In addition, I cannot discuss all objections and follow them through, as this is also beyond the bounds of a single paper. My aim here is merely to offer a rough outline of how such an argument may go, hoping to show its promise for refuting PMI, while having to leave many details for future work.

PMI has the premise that the history of science is full of theories that were once successful and accepted, but were later refuted. Let’s first consider this premise in its intuitive sense. As we saw earlier anti-realists support it by offering numerous examples of such theories. But now, given the exponential growth of science, we have to recheck whether these examples are really evidence for the premise of PMI. If we do so, we get a very different idea of the matter. Inspecting Laudan’s list, we see that all entries on that list are theories that were abandoned more than 100 years ago. This means that all corresponding theory changes occurred during the time of the first 5% of all scientific work ever done by scientists. As regards the example of theories of light, all changes of those theories occurred before the 1930s, whereas 80% of all scientific work ever done has been done since 1950. The same holds for practically all examples of theory changes offered in the philosophical literature. Thus, it seems that the set of examples offered by anti-realists is not representative and cannot be used to support the premise of PMI. If this is right, the premise lacks support and PMI does not work.

Examining the premise of PMI ("the history of science is full of successful, but empirically refuted theories") more closely, we see that it can be understood in two ways.[13] It can be understood as making a claim solely about the number of successful, but false theories in the history of science. Alternatively, the premise can be understood as making a claim about the distribution of theory changes over the history of science. I will show that the second interpretation is more appropriate.

Consider figure 1. In that figure, the x-axis is weighted in a linear fashion such that equal lengths of time are represented by intervals of equal length on the x-axis. With the exponential growth of science in mind, a second weighting suggests itself: The x-axis could be weighted in such a way that the length of any interval on the x-axis is proportional to the amount of scientific work done in that interval, see figure 3. I will call these two ways of weighting the x-axis 'the linear weighting' and 'the exponential weighting'. If we want to project the past development of science into the future the exponential weighting is more plausible, for the following reasons. If we want to determine how stable or instable successful scientific theories have been in the past, we should look at the amount of scientific work done by scientists, because the amount of scientific work can be expected to be very roughly proportional to the amount and quality of empirical evidence compiled by scientists to confirm or disconfirm their theories. More concretely, but still on a very general level, more scientific work results in the discovery of more phenomena and observations, which, in turn, can be used for more varied and better empirical tests of theories. More varied and better empirical tests of theories, if passed, amount to more empirical success of theories. Although it is certainly not plausible that all scientific fields profited from the increase in scientific work in this way, it is even less plausible that no scientific fields and no scientific theories profited from the increase in scientific work in this way, and it is obviously the latter – the fields and theories that did profit – that realists want to focus on. In other words, the realist should want to focus on the best, i.e., most successful scientific theories. This consideration is a good reason to adopt the exponential weighting of the x-axis in the premise of PMI.



[Figure 3: Exponential weighting of the x-axis and the sequence of theories of light.]

Another reason to adopt the exponential weighting is provided by a second consideration. This consideration concerns only certain theories, namely those that are highly unifying (see Michael Friedman 1981, p. 8). For some of these theories it is especially plausible that their degrees of success have roughly been growing proportionally to the amount of scientific work done in their respective fields. To see this we need the notion of an anomaly. As pointed out when defining the notion of success at the beginning of the paper, when an application of a theory goes wrong and scientists cannot find an error in an auxiliary statement, the application of the theory becomes an anomaly for the theory. If the anomaly is significant or the anomalies accumulate, the theory is refuted. Therefore, if it is possible for an application of a theory to become an anomaly for the theory, then it constitutes a (possibly weak) test of the theory. If the anomaly does not arise in the application, then the test was passed, and a (possibly only modest) measure of success is imparted on the theory. Now if the theory is highly unifying, then it is relevant in a large number of applications in its respective field. For example, for all chemical reactions in chemistry the Periodic Table of elements is relevant, as it implies (together with mass conservation) that in chemical reactions no chemical elements are created and disappear. So, if such a highly unifying theory was stable for at least a substantial part of the last few decades despite the very strong rise in amount of scientific work, then that means that even though there were a huge number of occasions where it was applied and could have suffered from significant anomalies or could have accumulated anomalies it has not done so. [14] This shows that such a theory would enjoy a very high degree of success (before, maybe, succumbing to some significant anomaly after all). Therefore, at least for highly unifying theories the exponential weighting of the x-axis should be the basis for any projection of stability or instability into the future. By comparison, the bare amount of time has nothing to do with the degree of success of theories, so the linear weighting is implausible.

Although both considerations offer a prima facie strong case for the exponential weighting, they clearly are in need of further elaboration. However, in order to develop them more fully, we would need a better worked-out notion of the degree of success of scientific theories. Such a notion has to be developed in future work. The main tasks will be, first, to establish a sufficiently strong connection between the amount of scientific work (as measured by the number of journal articles) and the degree of success of the best scientific theories, and second, to show that such a connection can be exploited for a more fully developed argument against PMI. From now on, I will proceed using the assumption that such a connection can be established.

The assumption implies that the exponential weighting of the x-axis is the correct weighing. Time matters, because different time periods differ very strongly in the amount of scientific work done in them, and the amount of scientific work is linked to the degree of success of the best theories (or so I assumed). Let us proceed by examining what the exponential weighting implies for PMI. Our observations from the beginning of this section still hold: All examples of theory changes discussed in the philosophical literature are rather old which shows that this set of examples is not representative and therefore cannot support the premise of PMI. On the basis of this set of examples nothing can be inferred about the future change or future stability of scientific theories. This is illustrated in Figure 3.

But suppose for a moment that we ignore this conclusion and try to infer the rate of future theory changes from the rate of past theory changes on the basis of this sample set. Then the assumption seems plausible that the typical rate of theory change in science is something like that of the theories of light until the beginning of the 20th Century. From 1600 until 1915, theories of light changed at least four times. In the same period, all scientists in all of science published around 3 million journal articles. This amounts to more than one theory change per one million journal articles. Today, more than 6 million journal articles are published every year. Hence, we should expect more than 6 theory changes every year today, i.e. one “revolution” every other month. That is certainly not what we observe.

Because the joint sample set of all examples of refuted theories offered by anti-realists is not representative, we need to come up with a more representative sample set. We should examine the last 50 to 80 years. Only then can we decide whether the premise of PMI is plausible, and more generally whether change or stability should be projected into the future, or whether we can project anything at all into the future. So, let us look at this time period. Moreover, as we just observed, we should focus on the best (i.e., most successful) scientific theories. (From now on I will understand the three positions of the realism debate to be concerned only with our current best theories.) If we do so, it quickly becomes clear that virtually all of our best scientific theories have been entirely stable in that time period. Despite the very strong rise in amount of scientific work, refutations among them have basically not occurred. Here are some examples of such theories (remember that the realist endorses the approximate truth of those theories):[15]

- The theory of evolution[16] (practically all organisms on earth are related by common ancestors, and natural selection is an important force for change)
- The Periodic Table of elements[17]
- The conservation of mass-energy
- Infectious diseases are caused by bacteria or viruses
- $E = mc^2$
- The brain is a net of neurons
- There are billions of galaxies in the universe.
- Sound consists of air waves
- In the past of the Earth, there were many ice ages
- And so on[18]

The anti-realist will have a hard time finding even one or two convincing examples of similarly successful theories that were accepted in the last 50 to 80 years for some time, but later abandoned, (and one or two counterexamples could be tolerated, because we are dealing with inductive inference here, after all). This does not mean to say that there were no theory changes in the last 50 to 80 years. Sure, there were: The large amount of scientific work of the recent past has also brought a lot of refutations, of course. It only means to say that there were practically no theory changes among our best (i.e., most successful) theories.

At this point, antirealists might object that the notion of "most successful theory" is intolerably vague; neither can it be used to delineate a set of theories, nor can it be used for the statement that the most successful theories have been stable. A satisfactory reply to this objection would have to rely on a better elaboration of the notion of success, a task that, as noted earlier, has to await another occasion. In this paper, I have to appeal to our intuitive understanding of it and have to trust that it is not too vague to serve my purposes. However, as a preliminary reply to this objection, I want to offer the following argument (which differs from the argument offered in Fahrbach 2009).

The argument concerns those of our current best theories that are highly unifying in their respective fields, some of which occur on my list above, e.g. the theory of evolution in biology or the Periodic Table of elements in chemistry. As pointed out earlier, if an application of a theory goes wrong and no auxiliary statement can be blamed, the application of the theory is an anomaly for the theory. If in the application the anomaly could arise, but doesn't, the theory enjoys some measure of success. What the observation from the absence of theory changes in the face of the very strong rise in amount of scientific work then means is that despite a huge number of occasions where our best unified theories have been applied and could have suffered from significant anomalies or could have accumulated anomalies, they have not done so. All the time samples of earthly matter could have turned up that are not decomposable into the 92 chemical elements, substances could have been discovered in which heat was a fluid, fossils could have been found that entirely refute the general outline of the tree of life as we know it, one of the numerous cranks could have managed to construct a functioning perpetuum mobile, etc, etc. Given the huge number (as well as the diversity and ever increasing precision) of applications of highly unifying theories, they could have run into difficulties many times over, but they have very rarely done so in any serious manner. If this is true, if anomalies could have turned up on numerous occasions for such theories, but didn't, then this means that they have been rechecked numerous times (even if mostly only implicitly) accumulating a large amount of success. Compare this with the examples of refuted theories offered by anti-realists from the more distant past of science. Measured by the amount of scientific work employed those theories often encountered problems rather quickly. Only comparatively low numbers of applications or tests had to be made, before those theories encountered anomalies that led to their abandonment. Hence, our current best highly unifying theories enjoy far higher degrees of success than any of the refuted theories of the history of science. This shows that at least for highly unifying theories the notion of "most successful theory" is not intolerably vague and can be used to delineate a set of theories. In addition, this argument supports that some of the highly unifying theories belong to our current best theories and were entirely stable in the last few decades.

What follows from all this for PMI? I have shown that (at least for our best theories) the premise of PMI is not supported by the history of science. Therefore, PMI is proven false; its conclusion that many of our current best scientific theories will fail empirically in the future cannot be drawn. What is more, the fact that our current best theories have not been empirically refuted, but have been entirely stable for most of the history of science (weighted exponentially) invites an optimistic meta-induction to the effect that they will remain stable in the future, i.e., all their empirical consequences which scientists will ever have occasion to compare with results from observation at any time in the future are true. The refutation of PMI and the correctness of optimistic meta-induction have consequences for the three positions of the realism debate discussed in this paper. I will confine myself to some short remarks concerning how proponents of the three positions should – from their perspective – assess the effects of the refutation of PMI, and of optimistic meta-induction on their respective positions.

Recall that scientific realism, the claim that our current best theories are true, can be divided into three inferential steps: from the past success of our best theories to their future stability, from their future stability to their empirical adequacy, and from their empirical adequacy to their truth. PMI threatens the first step. But because it is not cogent, this threat no longer exists, and scientific realism is saved from being undermined by PMI. What is more, the first step is obviously vindicated by optimistic meta-induction. Therefore, scientific realists will welcome optimistic meta-induction as additional support for the first step (in addition to NMA) and hence for their whole position.

Second, proponents of projective anti-realism accept PMI, and therefore expect future refutations of many of our best theories and recommend not believing those theories. PMI being refuted, they can no longer claim support from it. What is more, in relying on PMI, they rely on empirical considerations from the history of science and endorse projections from the history of science to the future of science. Because optimistic meta-induction does likewise, they should be receptive to it. But, of course, optimistic meta-induction undermines projective anti-realism, as it has the conclusion that no refutations of our current best theories are to be expected in the future. In sum, in so far as proponents of that position thought that their position is supported by PMI, they should now think that their position is undermined by optimistic meta-induction.

Third, empiricism, which rejects the third inferential step from empirical adequacy to truth, receives little or no support from PMI, and therefore does not suffer from its refutation. Furthermore, empiricists can argue that because optimistic meta-induction concerns only the first step, and does nothing to support the view that empirically adequate theories are true about unobservables, it does not undercut the argument from underdetermination, and does not threaten empiricism. A proponent of empiricism can accept the conclusion of optimistic meta-induction that our current best theories will remain stable in the future and can even accept that those theories are empirically adequate, and still maintain that we should not believe what they say about unobservables. From the perspective of empiricism, optimistic meta-induction neither threatens the argument from underdetermination nor weakens empiricism in any way.

7. Conclusion

PMI asserts that many of our current successful scientific theories will be refuted, because in the history of science there were many successful scientific theories that were later refuted. To argue against PMI, I started from the observation that the amount of scientific work done by scientists has grown exponentially over the last 300 years, doubling every 15-20 years, which implies that almost all scientific work ever done has been done in the last few decades (the last 50 to 80 years). I then outlined an argument according to which, if for the most successful theories we want to determine whether to project theory changes or theory stability into the present and future, we should weight periods of time according to the amount of scientific work done in those periods. If we do so, it turns out that the set of examples of abandoned theories presented by anti-realists is not representative at all, as almost all of those examples are older than 80 years. By contrast, when we examine the last few decades, we discover that barely any of our most successful scientific theories of that time period have been abandoned. In other words, during the time in which most of the scientific work that has been done, our most successful scientific theories have been entirely stable. This refutes PMI.

8. References

Bird, A. (2007). "What is Scientific Progress?" Noûs 41 (1), 64–89.

Butler, Declan (2008). "The Great Contender", Nature, Vol 454, 24 July 2008, p. 382-383.

Curtin, C. (2007), "Fact or Fiction?: Living People Outnumber the Dead", Scientific American, 2007, March 1.

Devitt, M. (2008). "Realism/Anti-Realism". The Routledge Compan*ion to the Philosophy of Science.* edited by Stathis Psillos and Martin Curd. London: Routledge and Kegan Paul

Egghe, L. and I.K. Raviachandra Rao, (1992). "Classification of Growth Models Based on Growth Rates and its Applications", Scientometics, Vol. 25. No. 1, 5-46.

Fahrbach, L. (2009), "The Pessimistic Meta-Induction and the Exponential Growth of Science", to appear in: Alexander Hieke and Hannes Leitgeb (eds). *Reduction and Elimination in Philosophy and the Sciences.* Proceedings of the 31th Internation*al* Wittgenstein Symposium.

Fahrbach, Ludwig and Claus Beisbart (2009). „The Pessimistic Meta-Induction and Constructive Empiricism", ms.

Friedman, Michael (1981). "Theoretical Explanation", in Healey, R. (ed.) Reduction, Time and Reality. Cambridge University Press.

Furner, J. (2003). "Little Book, Big Book: Before and After Little Science, Big Science: A Review Article, Part II", Journal of Librarianship and Information Science, Vol. 35, No. 3, 189-201.

Haub, C. (2002), "How Many People Have Ever Lived on Earth?" http://www.prb.org/Articles/2002/HowManyPeopleHaveEverLivedonEarth.aspx

Hoyningen-Huene, P. (1993). *Reconstructing Scientific Revolutions: Thomas S. Kuhn's Philosophy of Science*, Chicago: University of Chicago Press.

Kukla, A. (1998). Studies in Sc*ientific Realism*. Oxford: Oxford University Press.

Ladyman, James, (2002). Understanding Philosophy of Science. London: Routledge

Ladyman, James and D. Ross, (2007). *Everything Must Go*, Oxford University Press.

Lange, M. (2002). "Baseball, Pessimistic Inductions, and the Turnover Fallacy" Analysis 62, 281-5.

Laudan, L. (1981) "A Refutation of Convergent Realism", Philosophy of Science, 48 March, 19-49.

Leplin, J., (1997). A N*ovel Defence of Scientific Realism*, Oxford: Oxford University Press.

Lewis, P. (2001). “Why The Pessimistic Induction Is A Fallacy”, Synthese 129: 371-380.

Lyons, T. (2007). ““Scientific Realism and the Stratagema de Divide et Impera,” *British Journal for the Philosophy of Science*, 57(3):537-560.

Mabe, M. and M. Amin, (2001). “Growth Dynamics of Scholarly and Scientific Journals”, Scientometrics. Vol. 51, No. 1, 147-162.

Meadows, J. (1974). Communication in Science. Butterworths, London.

Okasha, S. (2002). Philosophy of Science. A Very Short Introduction, Oxford University Press.

Psillos, P. (1999). Scientific Realism: How Science Tracks Truth. New York and London: Routledge.

Psillos, P. (2000). “The Present State of the Scientific Realism Debate”, British *Journal for the Philosophy of Science*, 51 (Special Supplement), pp.705-728.

Reynolds, G. (2005). http://instapundit.com/archives/025289.php

Sanderson, Allen R. and Bernard L. Dugoni, Thomas B. Hoffer Sharon L. Myers, (1999). *Doctorate Recipients from United States Universities: Summary Report 1999*. http://www.norc.uchicago.edu/studies/sed/sed1999.htm.

Stanford, P. Kyle (2006). Exceeding Our Grasp: Science, History, and the Problem of Unconceived Alternatives. Oxford University Press.

Stanford, P. Kyle (forthcoming). “Reading Nature: The Interpretation of Scientific Theories”, in Larry Sklar, ed., The Oxford Handbook of the Philosophy of Science.

Tenopir, C. and D. W. King, (2004). “Communication Patterns of Engineers“, IEEE Press, Wiley-Interscience.

van Dyck. 2007. “Constructive Empiricism and the Argument from Underdetermination”, in Bradley Monton (ed), Images of Empiricism, p. 11-31.

van Fraassen, B., 1980. The Scientific Image. Oxford: Oxford University Press.

van Fraassen. 2007. “From a View of Science to a New Empiricism”, in Bradley Monton (ed), Images of Empiricism, p. 337-384.

Vickery, B.C. (1990). “The Growth of Scientific Literature, 1660-1970”, in: The Information Environment: A World View, Studies in Honour of Professor A. I. Mikhailov, Elsevier Science Publishers B.V. North-Holland.

Vickery, B.C. (2000). Scientific *Communication in History*, The Scarecrow Press, Lanham, Maryland.

Williamson, T. (2006). “Must Do Better.” In: Greenough, P. and M. Lynch Truth and Realism, Oxford University Press, pp. 177-181.

Wolfe, R. M. (2007). "Research and Development in Industry: 2003", http://www.nsf.gov/statistics/nsf07314/.

---

[1] I would like to thank Berna Kilinc, Holger Leerhoff, Claus Beisbart, Ionannis Votsis and Gerhard Schurz for very helpful discussions.

[2] For book-length treatments of the scientific realism debate see Stathis Psillos (1999), André Kukla (1998), Jarrett Leplin (1997), Ladyman and Ross (2007), and Kyle Stanford (2006). An overview of the debate is presented in Psillos (2000).

[3] Realists usually admit that a *general* explication of the notion of approximate truth has not yet been devised and may even be impossible to devise, but they think that our intuitive grasp of that notion and scientists' successful application of it in many specific situations suffice to justify its use for defining realism. A major obstacle for devising a general explication of the notion of approximate truth is identified in Alexander Bird (2007).

[4] Note that these examples stem from many different scientific disciplines such as physics, chemistry, astronomy, geology, etc. This disproves the main claim of Marc Lange (2002) (also endorsed by Michael Devitt 2008), namely that PMI is a fallacy because it ignores the possibility that most theory changes occurred in just a few scientific fields, while all others were spared of any.

[5] Samir Okasha notes that "many things that are observable never actually get observed. For example, the vast majority of organisms on the planet never get observed by humans… Or think of an event such as a large meteorite hitting the earth. No-one has ever witnessed such an event, but it is clearly observable. … Only a small fraction of what is observable ever gets observed." (2002, p. 74)

[6] For discussions of the argument from underdetermination see, for example, Kukla (1998, Ch. 5 -7), Psillos (1999, Ch. 8,) and Ladyman (2002, Ch. 6). Surprisingly van Fraassen himself remarks that he has never relied on that argument (2007, p. 346-7). See also Maarten Van Dyck (2007) who argues that van Fraassen nowhere uses it for backing his position.

[7] This inferential step may be false in some unusual cases. For example, sets of theories of fundamental physics may very well exist whose members have incompatible empirical consequences and will enjoy enormous success one day, but to be able to decide between them experiments are necessary which are technologically too demanding to be ever performed by humans such as experiments

requiring colliders as big as galaxies. Such theories are compatible with all empirical evidence humans will factually ever possess, while the inference to the empirical adequacy of any of them would not be justified, because of the existence of the rival theories. In such a situation only one of the theories can be empirically adequate, but we don't know which one. However, such cases are rare exceptions. My example is merely hypothetical, moreover such examples seem to be possible in fundamental physics only, if at all, and fundamental physics is a special case which needs special treatment anyway.

[8] Other ways of measuring scientific work include government and industry expenditures on research, the number of scientific journals, the number of universities and the number of doctorates. Where data is available it shows that all these ways of measuring yield essentially the same results.

[9] For the interested: The best fit for the growth of philosophy articles from 1968-1987 in the *Philosopher's Index* is provided by the equation $\#\text{articles}(1968 + t) = 4509 + 2355\, t^{1.28}$, where t is measured in years. This amounts to a doubling in 4 years from 1972 to 1976 and in 8 years from 1977 to 1985. This is less telling than one would wish, because the *Philosopher's Index* does not cover all of philosophy, especially in its early years. Even more beautiful is the cumulative growth function of publications in economics over the same time period: $\#\text{articles}(\text{until } t) = 285\,914 \cdot \exp_{0.015} 0.903^{t}$ (Leo Egghe, et al 1992, pp. 29, 33, 34).

[10] See the graphics at the end of Reynolds (2005). In the past 10 years China has increased its spending on colleges and universities almost tenfold (Newsweek, January 9, 2006, p. 9). "The number of students taking science or engineering degrees in China each year climbed from 115,000 in 1995 to more than 672,000 in 2004, putting the country ahead of the United States and Japan." (Declan Butler 2008 in *Nature*) See also Wolfe 2007, tables 1 and 31, for the development of U.S. industrial research and development expenditures from 1953 until 1999; the growth of expenditure shows a doubling rate of roughly 20 years.

[11] See Carl Haub (2002) and Ciara Curtin (2007). Haub offers an entertaining discussion of the difficulties involved in estimating the number of humans who have ever lived on earth. Haub states that "any such exercise can be only a highly speculative enterprise, to be undertaken with far less seriousness than most demographic inquiries. Nonetheless, it is a somewhat intriguing idea that can be approached on at least a semi-scientific basis."

[12] The data, where available, shows that the number of scientists and the number of scientific journal articles have been growing at roughly the same rate. It follows that we cannot observe that the pressure on scientists to publish has lead to a significant increase in output per scientist per time. Compare Mabe and Amin (2001, pp. 159, 160) and Carol Tenopir and Donald King (2004, p. 5).

[13] Thanks to Claus Beisbart for help with this point.

[14] According to Hoyningen-Huene, Kuhn thought that scientists "trained in normal science … are … extraordinarily suited to diagnosing" anomalies of theories (1993, p. 227)

[15] A similar list of stable theories is offered by Bird (2007). My list is almost entirely disjoint from his. Note that my list does not contain any theories from fundamental physics. Such theories have special problems which need special treatment, which they will receive somewhere else. Note in addition that my list contains some less general, even rather specific statements, because I think a realist should be interested in their truth and stability as well.

[16] The theory of evolution has been the dominant theory about the development of life on earth at least since 1870. According to a conservative estimate, over the history of science the number of biologists doubled every 20 years. This doubling rate implies that for 99% of all biologists from the whole history of biology the theory of evolution was the "reigning paradigm" (see Fahrbach 2009).

[17] As remarked at the beginning of the paper, I use the term "theory" in a very broad sense.

[18] What about philosophy? An interesting qualitative assessment of the progress in philosophy is offered by Timothy Williamson: "In many areas of philosophy, we know much more in 2004 than was known in 1964; much more was known in 1964 than in 1924; much more was known in 1924 than was known in 1884. … Although fundamental disagreement is conspicuous in most areas of philosophy, the best theories in a given area are in most cases far better developed in 2004 than the best theories in that area were in 1964, and so on." (2006, p. 178)