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Linguistics, Science, and the Demarcation Problem

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Abstract

The article discusses the so-called “demarcation problem,” i.e. the difficulties of determining the defining criteria of legitimate science, in the context of linguistics. The demarcation problem preoccupied philosophers of science for a good part of the twentieth century and is now considered to be unsolvable – it is impossible to provide a cut-and-dried list of necessary and sufficient features of “scientificness.” Consequently, this means that it is impossible to establish by means of a rational argument whether linguistics is a science and if not, how to turn it into one. The article reviews several attempts at solving the demarcation problem, briefly discusses the reasons for their failure, and demonstrates their relevance for linguistics.

Linguistics, Science, and the Demarcation Problem[1]

1. Introduction

Some critiques of theoretical linguistics are buttressed by well-meant positivist worries about low scientific standards of this kind of research. Linguists may worry about the subjectivity of users’ intuitive judgments, the lack public of access to users’ mental realm, difficulties in applying quantitative methods, the lack of authenticity of expressions constructed specifically for the purpose of linguistic theorizing, and other practices of “armchair” linguistics amount to “bad science.” Some of these objections appear to be fueled by the misconception that science itself is a relatively clear cut-and-dry notion and can be used as a yardstick for evaluating the quality of various research practices. I tentatively term this misconception *the demarcation fallacy*, alluding to the recalcitrant quandary in philosophy of science known as the demarcation problem. This article attempts to rectify this frequent misconception not by demonstrating that “armchair” practices live up to the standards of science, but demonstrating there is no standard science to begin with. In Section 1, I give voice to linguists themselves to examine their ideas about how to make linguistics more scientific. In Section 2, I will summarize the philosophical discussion on the demarcation problem, which offers the context necessary to comprehend how various attempts at making linguistics more scientific are ultimately unsuccessful.

2. Linguistics – a science in the making

Some of the earliest records of philosophical and philological interest in language can be traced back to at least 5th century BCE. Textbooks of the history of linguistics mention Plato's *Cratylus* and Stoic studies in grammar in Europe and Pāṇini's Sanskrit grammar in India, but some sort of reflection on the nature of language, words, or signs are to be found wherever philosophical thought thrived, both in the West and in the East, from ancient times, through the Middle Ages and early modern period, into the 21st century. Even after the advent of modern linguistics, linguists did not monopolize the study of language; in his non-exhaustive list of "people [who] have professional need to know something about language as opposed to simply being able to use it," Charles F. Hockett mentions the "speech correctionist," the teacher of English composition, the foreign language instructor, the literary artist, the psychologist, the anthropologist, the historian, the philosopher, and the communications engineer (the quotation and the list from Hockett[1]).

The rise of linguistics as a distinct academic discipline occurred gradually throughout the 19th century and by the early 20th century linguists already recognized themselves as a distinct community of researchers. The young community realized that they asked questions and demanded answers different from the philosophers of the past and the people from Hockett's list, but they were still looking for their place in the larger landscape of science. For it was quite clear for them that linguistics should be a science, that linguists should employ scientific methods in their research, and that their theories should be as respectable and trustworthy as those of their colleagues from the faculties of physics, chemistry, and biology. Incidentally, around that time heated debates around the very definition of science and the demarcation problem erupted in philosophy of science. It is an irony of history that when linguists painstakingly strived to define their discipline as a science, philosophers were increasingly at a loss about the very definition of science.

Nonetheless, in the early 20th century linguists made valiant efforts to find a place for their discipline among either natural, or social sciences. To appreciate the weight of the problem, it is sometimes enough to take a look at the titles and tables of contents of classical books of the era. Ferdinand de Saussure's *Course in General Linguistics* has a separate, albeit short, chapter titled "Subject matter and scope of linguistics; its relations with other sciences," where he prescribes the task of self-definition and self-demarcation as one of the chief aims of linguistics:

The scope of linguistics should be:

a. to describe and trace the history of all observable languages (…);
b. to determine the forces that are permanently and universally at work in all languages, and to deduce the general laws to which all specific historical phenomena can be reduced; and
c. to delimit and define itself.[2] (my emphasis)

Immediately after the list, de Saussure attempts to sketch the connections between linguistics and other sciences:

> *Linguistics is very closely related to other sciences that sometimes borrow from its data, sometimes supply it with data. The lines of demarcation do not always show up clearly. For instance, linguistics must be carefully distinguished from ethnography and prehistory, where language is used merely to document. It must also be set*

> *apart from anthropology, which studies man solely from the viewpoint of his species, for language is a social fact. But must linguistics then be combined with sociology? What are the relationships between linguistics and social psychology? (…) The ties between linguistics and the physiology of sounds are less difficult to untangle. The relation is unilateral in the sense that the study of languages exacts clarifications from the science of the physiology of sounds but furnishes none in return. In any event, the two disciplines cannot be confused. (…) As for philology, we have already drawn the line: it is distinct from linguistics despite points of contact between the two sciences and mutual services that they render.*[2]

Later, the author undertakes an even more ambitious task of outlining a future science, semiology, which would incorporate linguistics.

> *A science that studies the life of signs within society is conceivable; it would be a part of social psychology and consequently of general psychology; I shall call it 'semiology' (…) Since the science does not yet exist, no one can say what it would be; but it has a right to existence, a place staked out in advance. Linguistics is only a part of the general science of semiology; the laws discovered by semiology will be applicable to linguistics, and the latter will circumscribe a well-defined area within the mass of anthropological facts.*

To determine the exact place of semiology is the task of the psychologist! (…) [Here] I wish merely to call attention to one thing: if I have succeeded in assigning linguistics a place among the sciences, it is because I have related it to semiology.[2]

Nowadays, few linguists and semioticians would agree that Saussure's vision has come true. It is linguistics rather than non-linguistic semiotics that seems to be better delineated and has greater ambitions (or pretense) to be considered a mature science. Nonetheless, the amount of time and effort devoted by de Saussure to disentangling the complexities of the demarcation problem in linguistics indicates that the matter was of great importance for the Swiss scholar. Taylor argues that de Saussure's worries from this part of *Course* are very much alive in the 21st century:[3]

> *Academics tend to be very territorial people, anxious to defend their disciplinary turf. Saussure was an academic through and through. One of his preoccupations was the question, how to justify the academic discipline of Linguistics? How can one mark out a territory in the academic curriculum that can legitimately be called 'Linguistics'? As a matter of fact, the question is probably just as relevant today as it was when Saussure was alive.*[3]

Another classical text under the telling title "The status of linguistics as a science" by Edward Sapir was written with the intention, as the author himself declared, "not to insist on what linguistics has already accomplished, but rather to point out some of the connections between linguistics and other scientific disciplines, and above all to raise the question in what sense linguistics can be called a 'science'"[4]. However, a philosopher of science interested in demarcation will be soon disappointed to learn that the article deals mostly with the way in which "[language] is becoming increasingly valuable as a guide to the scientific study of a given culture" and it "is a guide to 'social reality'" of a community (both quotations from[4]). Not unlike Saussure, Sapir recognizes the two-fold nature of language: the "natural" aspect falling within the domain of biology and the social aspect readily associated with anthropology and social sciences. Yet while Saussure projected linguistics as a part of greater complex of semiotic sciences, Sapir saw it as a bridge between two separate domains of knowledge: natural and social sciences. The relevant passage is worth quoting *in extenso*, since it suggests how natural and social sciences could be distinguished:

> *Where, finally, does linguistics stand as a science? Does it belong to the natural sciences, with biology, or to the social sciences? There seem to be two facts which are responsible for the persistent tendency to view linguistic data from a biological point of view (…) [The] regularity and typicality of linguistic processes leads to a quasi-romantic feeling of contrast with the apparently free and undetermined behavior of human beings studied from the standpoint of culture. But the regularity of sound change is only superficially analogous to a biological automatism. It is precisely because language is as strictly socialized a type of human behavior as anything else in culture and yet betrays in its outlines and tendencies such regularities as only the natural scientist is in the habit of formulating, that linguistics is of strategic importance for the methodology of social science. Behind the apparent lawlessness of social phenomena there is a regularity of configuration and tendency which is just as real as the regularity of physical processes in a mechanical world, though it is a regularity of infinitely less apparent rigidity and of another mode of apprehension on our part.*[4]

As evident from the above passage, Sapir associated natural sciences (biology is mentioned explicitly) as a realm of regularity, typicality, and lawfulness, which are much more difficult to obtain in social sciences. He appreciated natural sciences' power to discover regularities, make generalizations, and discover unity behind diversity – all of which are characteristics often associated with modern science – and believed that this power can pass osmotically to the social sciences via linguistics. It is unclear whether Sapir took this power of generalization and unification as a defining property of science, but apparently he took it as an important normative value: generalization and unification are something that natural sciences do well, they are desirable, and should be emulated by social sciences.

In *The Meaning of Meaning*[5] Charles K. Ogden and Ivor A. Richards criticized both de Saussure's early structuralism and Sapir-style American ethnolinguistics (Franz Boas is mentioned by name). Interestingly, the authors attacked the two schools of linguistics on the grounds they exemplified "bad science," which naturally raises the question of what is "good science" and places us in the larger context of the demarcation problem. The gist of Ogden and Richards argument was that both Saussure and American ethnolinguists failed to study language in its full complexity. The authors comment that "[de Saussure's] theory of signs, by neglecting entirely the things for which signs stand, was from the beginning cut off from any contact with scientific method of verification"[5] and "[by] leaving out essential elements in the language situation we easily raise problems and difficulties which vanish when the whole transaction is considered in greater detail"[5]. Thus, it appears that for the authors the mark of "good science" in linguistics was to see the global view of language. Given the complexity of language as a biological, social, cultural, and philosophical phenomenon, having the full view of language is no mean feat, but Ogden and Richards insisted mostly on introducing a triadic model of the linguistic sign and enriching contemporary linguistic theories with, what we would now call, the pragmatic aspect. Ogden and Richards appreciated natural sciences not only for their "scientific method of verification," but for the advantages of a radical theory change in scientific progress:

> *If such [direct relations between words and things] could be admitted then there would of course be no problem as to the nature of Meaning, and the vast majority of those who have been concerned with it would have been right in their refusal to discuss it. But too many interesting developments have been occurring in the sciences, through the rejection of everyday symbolizations and the endeavour to replace them by more accurate accounts, for any naive theory that 'meaning' is just 'meaning' to be popular at the moment. As a rule new facts in startling disagreement with accepted explanations of other facts are required before such critical analyses of what are generally regarded as simple satisfactory notions are undertaken. This has been the case with the recent revolutions in physics. But in addition great reluctance to postulate anything* ***sui generis*** *and of necessity undetectable was needed before the simple natural notion of simultaneity, for instance, as a two-termed relation came to be questioned. Yet to such questionings the theory of Relativity was due.*[5]

Thus, Ogden and Richards evoked the success of what we would now probably call "scientific revolutions" (Kuhn's[6] term) to justify the revolutionary changes in linguistics that they proposed. Perhaps this maneuver should not be taken as the authors' explicit endorsement of linguistics as a natural science, but it does suggest that they saw some normative similarities between linguistics and natural sciences ("What's allowed in physics is allowed in linguistics!").

Yet for a philosopher of science focusing on demarcation, Leonard Bloomfield is perhaps the most rewarding author. Bloomfield fostered anti-mentalist sentiments and took considerable effort to demonstrate that mentalism is inherently incompatible with science as he understood it. Bloomfield rejected a kind of mentalism which assumes "that the variability of human conduct is due to the interference of some non-physical factor, a *spirit* or *will* or *mind* (…) This spirit, according to the mentalistic view, is entirely different from material things and accordingly follows some other kind of causation or perhaps none at all"[7]. Katz is partly apologetic of Bloomfield for resisting what the former scorns as a "highly teologized conception of mentalism"[8], yet even though most scientists would balk at the idea of including the terms like *spirit* and *soul* into their theoretical vocabulary, *mind* has found its way into respectable science and philosophy. The problem of the interrelations between the mind and the body is nowadays a central research problem of cognitive sciences and philosophy of mind and it is far from obvious whether a reductive physicalist which Bloomfield seems to defend, i.e. the philosophical view that all mental phenomena are entirely reducible to underlying physical processes, is the only reasonable choice for "good science." Katz also points to "Bloomfield's endorsement of the empiricist viewpoint on scientific methodology"[8] summarized in the following passage:

> *[We] can distinguish science from other phases of human activity by agreeing that science that science shall deal only with events that are accessible in their time and place to any and all observers (strict* ***behaviorism****) or only*

> *with events that are placed in coordinates of time and space (**mechanism**), or that science shall employ only such initial statements and predictions as lead to definite handling operations (**operationalism**), or only terms such as are derivable by rigid definition (**physicalism**).*[9] *(original emphasis)*

And later:

> *These several formulations [– behaviorism, mechanism, operationalism, and physicalism –] independently reached by different scientists, all lead to the same delimitation, and this delimitation does not restrict the subject matter of science but rather characterizes its method. (Bloomfield 1939, 13)*

This disjunctive enumeration of properties appears to be a good summary of the folk understanding of natural sciences. However, a closer look at the demarcation debate in philosophy, attended to in the following sections, will show that by the 1950s it was already clear that the properties listed by Bloomfield failed to distinguish respectable science from fraudulent pseudo-science. For example, no combination of the above properties will allow us to classify astrology as non-science, since not only did the work of medieval astrologers display all of Bloomfield's characteristics, but it relied heavily on quantitative methods, which is another feature stereotypically associated with modern science.[2] Bloomfield should not be blamed for his failure to provide defining characteristics of science, since, as we will see in the section to follow, solving the demarcation problem by means of a list of necessary and sufficient features appears to be impossible in principle. On the contrary, despite the failure, Bloomfield should be given credit for his attempts to lay the ground for a truly scientific linguistic methodology inspired by the practice of natural scientists of his times.

Debates about the scientificness of linguistics, often disguised as debates about various aspects of methodology, have continued well into the post-World War II period. As Richard A. Harris elegantly writes, "just as the middle class is always rising, linguistics is always becoming a science"[10]. The picture that Harris paints of methodological disagreements in the post-World War II linguistics is not entirely unlike the disputes from the early 20th century, with the demarcation problem woven between the lines:

> *Not all linguists would agree that their science charts the sinuous relations of language to thought, thought to language, nor even that linguistics is a science, nor, if it is, about what sort of science it is. And these disagreements are crucial themes in much of what follows, as it is unavoidable conclusion that linguists are a contentious bunch (…) Katz and Postal, for instance, regard linguistics as something very much like mathematics, a pristine formal science without connection to anything as messy as thought. Lakoff and Chomsky both agree that linguistics is very much concerned with mind, and that it is an empirical science, but disagree severely on many specifics, including what it is to be an empirical science. Ross, McCawley, and Jackendoff are in the empirical science camp, but fall between Lakoff and Chomsky on various specifics, depending on the issue.*[10]

There is little reward in providing a more comprehensive summary at this stage. It suffices to note that even the linguists of today are more contentious about methodology and the place of their discipline in the landscape of sciences than their colleagues in physics, chemistry, and biology departments. Various criteria for distinguishing science from non-science have been offered during the period when the demarcation was the key research problem in philosophy of science and all of the criteria proved to be defective in one way or another.

3. Attempts at solving the demarcation problem

While for some philosophers the roots of the demarcation problem can be found antiquity (cf. Laudan 1983), the discussion unraveled in its full in the first decades of the 20th century. The philosophers of that time were appalled by the number of what they considered fraudulent pseudo-sciences and they tried to offer clear and strict criteria for distinguishing respectable science from non-scientific charlatanry. Throughout the history of the demarcation debate, several main strategies of attacking the problem are evident. Interestingly most of them steered clear of the ideas about the nature of scientific enterprise expressed by Sapir, Bloomfield, and other linguists.

3.1. Logical positivism

The early debate on demarcation revolved around the criterion of testability. Obviously, the conviction that scientific claims should be open to experimental testing was not novel at that time either in philosophy, or in science. For example, the physicist Ernst Mach held that "where neither confirmation nor refutation is possible, science is not concerned"[11]. Yet it was a group of philosophers known as the Vienna Circle that developed this intuitive conviction into a philosophical system known as logical positivism. As a philosophy of science, logical positivism is today an antiquarian program, but it helped to define the demarcation problem as a philosophical issue and set the tone for the subsequent debate.

William A. Gorton distinguishes seven key tenets of logical positivism:

1. **Primacy of sensory data:** data gained through the senses provides the foundation for our knowledge of the world.
2. **Verificationism:** the only statements or theories worthy of being called scientific are those that have been shown to correspond to empirical facts via observation and experiment.
3. **Antimetaphysics:** statements that cannot in principle be verified by empirical observation are, strictly speaking, meaningless.
4. **Antirealism:** unobservable entities, structures, and mechanisms invoked by scientists are at best useful fictions that help us organize phenomena, but they do not really exist. (…)
5. **Skepticism about causes:** necessary connections between events cannot be demonstrated empirically and lie outside of legitimate science. Thus positivists often interpret the claim that one event causes another as nothing more than the claim that the first event always precedes the second event.
6. **Support for deductive-nomological or "covering-law" explanation:** explanation of an event requires demonstrating that the event was logically necessary given certain initial conditions and the presence of one or more universal laws of nature. (…)
7. **Unity of scientific method:** the above six principles embody the one, true path to knowledge about the social as well

as the natural world.[12]

In the context of the demarcation problem, points (1) and (2) are crucial: the difference between science and non-science is that the scientific knowledge is based on observable data and scientific statements can be verified in experimentation or observation. As far as (1) is concerned, logical positivists embraced the legacy of John Locke's and David Hume's empiricism. As far as (2) is concerned, the members of the Vienna Circle viewed verifiability (or more generally: testability) as a property of logical propositions, usually expressible in affirmative sentences. Most simply put, a proposition is testable if it can be confirmed or disconfirmed in an experiment or observation. A testable proposition submitted to experimental testing is a hypothesis. To borrow one example from a logical positivist Carl G. Hempel[13],

- the hypothesis ***All ravens are black*** is testable, because it can be verified (upon finding a black raven) or falsified (upon finding a raven of any other color);
- the hypothesis ***Some ravens are black*** is not testable, because it can be verified (upon finding a black raven), but it cannot be falsified (finding a non-black raven does not falsify the proposition;
- the hypothesis about one specific raven ***Edgar Allan is black*** is testable, because it is verified if the raven named *Edgar Allan* is black and falsified when it is non-black.

For logical positivists, propositions like *Edgar Allan is black* are testable in principle, but they are of little use in science, because scientific statements aim to make generalizations and discover universal laws, rather than state the properties of singular objects. Therefore, only universal propositions, i.e. propositions about all members of a class of objects (e.g. *All ravens are black*) are genuinely scientific. The members of the Vienna Circle realized that this kind of the testing of such propositions is prone to David Hume's problem of induction[14], i.e. the logical impossibility of verifying a universal proposition on the basis of a limited number of observations. It is evident that even if a black ravens had been spotted any finite number of times, the observations would not confirm beyond any doubt that all ravens – past, future, and present – are indeed black. To solve this problem, Rudolf Carnap proposed that scientific verification did not show that the hypothesis under investigation was absolutely true, but merely that it was more probable[15][16].

One consequence of (1) and (2) is the anti-metaphysical stance of logical positivists in (3), where "metaphysics" becomes an umbrella term for all statements which cannot be verified or falsified in experiments or observations. On this view, "metaphysical" questions are the ones traditionally associated with metaphysics as branch of philosophy, i.e. the questions about being and its nature, but the label also applies to many other sorts of philosophical speculation. For example, for Karl Popper, who was ferocious critic of logical positivism, but used the term *metaphysics* in a similar way, Friedrich Nietzsche's philosophy and 20th-century existentialism falls squarely under this rubric[17]. Logical positivists believed that metaphysical questions are meaningless, because they cannot be confirmed or disproved (see tenet (2)) by means of direct observation (see tenet (1)). For this reason, metaphysical questions automatically fall outside the domain of science and therefore a relatively clear-cut line can be drawn between natural sciences and philosophy. The Vienna Circle realized that modern science makes many statements about "what exists" and "how things are," which appear to be metaphysical, but they believed that such statements are metaphysically non-committal. Thus, when a physicist claims that there are unobservable entities like electrons, they do not claim that such entities exist in physical reality, but they merely postulate theoretical entities which are a part of the theory describing some observable phenomena, such as glowing in a gas-discharge tube. More generally, logical positivists tended to believe that whenever a scientific theory talks about unobservable phenomena, it talks about "useful fictions" for describing, explaining, or predicting some observable phenomena (see tenet (3)). This qualifies logical positivism as a type of instrumentalism.

One of the key projects of logical positivism was to define accurate, clear, unambiguous language suitable for science[3]. Obviously, vernacular languages, while efficient in everyday communication, were believed to be too vague and "messy" for application in science. The only sentences admitted in science are the so-called protocol sentences and syntactic sentences. A protocol sentence is essentially a report of some observable phenomenon or event, e.g. *When burnt, substance X produces green flame*. Such sentences are synthetic, i.e. whether they are true depends on the state of affairs in the world and not merely on the linguistic and logical properties of words, and a posteriori, i.e. they can be verified only by experience. In order to determine whether substance X really burns with a green flame, conceptual analysis is not enough; one has to actually burn substance X. Syntactic sentences, in turn, express logical propositions, e.g. *Two plus two equals four*. They are analytic, because their truth value depends only on the meaning of the terms defined against the respective conceptual framework, and a priori, because they can be verified without any appeal to experience. To see whether 2 + 2 = 4 one only needs to understand how mathematics defines the concepts 2, 4, and the operation of addition.

The sentence *The Sun is a star (rather than a planet)* is, strictly speaking, neither a protocol sentence (one does not directly observe the property of "starhood", rather than "planethood," of the Sun), not a syntactic sentence (the truth of the sentence cannot be determined exclusively by conceptual analysis). However, the sentence *The Sun is a star* could be understood as a syntactic sentence if interpreted as a sentence about terms *the Sun* and *star* within the conceptual framework of astronomy; the orthographic form *"The Sun" is a "star"* would be more adequate in this context. It is important to realize that under such an interpretation, the sentence does not express some intrinsic metaphysical property of the Sun as a physical object, but it merely establishes a semantic compatibility of the terms *the Sun* and *star* (rather than *planet*) in modern astronomy. In other words, sentences like *The Sun is a star (rather than a planet)* are meaningful and scientific if they are viewed as statements about formal relations between scientific terms, and they are meaningless and "metaphysical" if they are viewed as statements about the state of affairs in the physical reality.

The logical positivist solution for the demarcation problem was the criterion of meaningfulness of a scientific sentence. The term "meaningful" in this context has a somewhat restricted and technical sense related to verifiability. As Waismann emphatically states, "[if] there is no possible way to determine whether a statement is true then that statement has no meaning whatsoever. For the meaning of a statement is the method of its verification"[18]. Thus, if sentence S cannot be conclusively proved true by means of observation yielding a protocol sentence and logical operations on protocol sentences, S is meaningless in the technical sense and does not belong to science.

Logical positivism was soon attacked by philosophers who pointed out the untenability of the philosophical program. One serious blow was inflicted by Willard Van Orman Quine[19], who famously questioned the clear-cut distinction between synthetic and analytic sentences: the heart of the logical positivist design of scientific language. Much later, David

Chalmers[20] pointed out that it is impossible to unequivocally determine the conceptual framework against which a sentence should be understood and consequently one sentence can be meaningful is several different ways in different frameworks. Other objections to the philosophical program will be discussed in more detail in the sections to follow. Today, it is widely believed that the positivist attempt at demarcating science from non-science on the grounds of logical structure of scientific language has failed to fulfill its promise.

3.2. Karl Popper's falsificationism

One of the earliest critics of the Vienna Circle's program was Karl Popper. Similarly to logical positivists, the philosopher was interested in the demarcation problem and believed that the solution lies partly in the logical structure of scientific statements, but Popper was a fierce critic of verification and, as a consequence, the criterion of meaningfulness sanctioned by the verifiability. The philosopher bluntly derided the logical positivist program in *The Logic of Scientific Discovery*: "Now in my view there is no such thing as induction. Thus inference to theories, from singular statements which are 'verified by experience' (whatever that may mean), is logically inadmissible. Theories are, therefore, *never* empirically verifiable"[21] (original emphasis). Popper's critique of verification runs essentially along the lines of Hume's problem of induction mentioned in Section 2.1: it is logically untenable to verify a universal statement (e.g. *All ravens are black*) from a limited number of singular statements (e.g. *This raven is black*). To vindicate induction, one would have to establish what Popper calls the principle of induction (cf.[21], 4), i.e. an additional statement or a rule validating inference from singular to universal statements. This principle, Popper continues, would have to be universal in order to validate all instances of inductive inference in science. Yet any attempt to verify such a universal principle encounters the original difficulty: how to verify a universal statement? If one attempt to verify the principle of induction inductively, one quickly ends up in infinite regress. To illustrate the problem with a more concrete example, to validate the inference from the protocol sentence *This raven is black* (abbreviated as P) to the scientific "law" *All ravens are black* (abbreviated as L), one needs to accept the auxiliary postulate like *All ravens are identical in terms of color* (AUX). But how does one know that AUX is true? The obvious answer is to check whether all ravens are identical with respect to color. Since it is impossible to observe all past, present, and future ravens, one could try to verify AUX by means of induction based on a limited number of observations. However, the principle of induction has not been established yet, so verifying AUX would require accepting an additional postulate *All ravens are identical in terms of being identical in terms of color.*. And so on. An easy way out of this infinite regress is to accept without evidence that AUX is true, or more generally, to accept without evidence the auxiliary postulate that all samples under inductive investigation are identical. This would make the extra postulate a kind of fundamental belief in the consistency and uniformity of nature, but such a postulate would have to be accepted without any evidence (which violates tenet (2) of logical positivism listed in Section 2.1) and it looks suspiciously metaphysical (which violates tenet (3)). Thus, it appears that inductive verification cannot be vindicated in the way envisioned by logical positivists.

The members of the Vienna Circle were aware of these difficulties and tried to bypass them by replacing strict verification with "probabilification": they believed that induction increases the probability of a hypothesis rather than proves the hypothesis absolutely true. Popper does not find this maneuver helpful.

> *People with inductivist leanings may tend to overlook the hypothetical character of these estimates: they may confuse a hypothetical estimate, i.e. a frequency-prediction based on statistical extrapolation, with one of its empirical "sources"—the classifying and actual counting of past occurrences and sequences of occurrences. The claim is often made that we "derive" estimates of probabilities—that is, predictions of frequencies—from past occurrences which have been classified and counted (…) But from a logical point of view there is no justification for this claim. We have made no logical derivation at all. What we may have done is to advance a non-verifiable hypothesis which nothing can ever justify logically: the conjecture that frequencies will remain constant, and so permit of extrapolation.*[21] *(Popper 2002 [1934], 158; original emphasis)*

And later:

> *Only an infinite sequence of events (…) could contradict a probability estimate. But this means (…) that probability hypotheses are unfalsifiable because their dimension is infinite. We should therefore really describe them as empirically uninformative, as void of empirical content.*[21] *(Popper 2002 [1934],182)*

As well as the problem of induction, verification faces other theoretical and practical challenges. One theoretical difficulty is Carl Hempel's paradox of confirmation, also known as the raven paradox.[13] The paradox exploits the fact that the proposition *All ravens are black* (A) can be transformed via the logical operation of contraposition into*Everything that is not black is not a raven* (B). Since logically A is equivalent to B, one can in principle verify A by verifying B, i.e. verify that all ravens are black by observing non-black non-ravens, for instance red apples or yellow pens. This conclusion is highly counterintuitive. Another problem with verification is that a scientist may formulate a research hypothesis as a tautology: a sentence which is always true by virtue of its logical structure. Tautological hypotheses are verified by all observation, but are also uninformative and quite useless from the point of view of science. The hypothesis *All ravens are black or non-black* is tautological and therefore verified by every observation, but it offers no interesting or useful insights into the nature of ravens.

For Popper, the problems plaguing verification cannot be overcome in principle and the philosopher famously proposes to replace it with falsification. The rationale behind this proposition is simple: while it is not possible to definitely verify the hypothesis *All ravens are black* on the basis on a limited number of observations, it is possible to definitely falsify this hypothesis when one non-back raven is spotted. In Popper's philosophy, the requirement that scientific hypothesis should be falsifiable, i.e. it should be possible to predict a result of an experiment that proves the hypothesis false, effectively replaces the logical positivist requirement of meaningfulness as the demarcation criterion.

> *I shall certainly admit a system as empirical or scientific only if it is capable of being tested by experience. These considerations suggest that not the verifiability but the falsifiability of a system is to be taken as a criterion of demarcation. In other words: I shall not require of a scientific system that it shall be capable of being singled out, once and for all, in a positive sense; but I shall require that its logical form shall be such that it can be singled out,*

> *by means of empirical tests, in a negative sense: it must be possible for an empirical scientific system to be refuted by experience.*[21] *(Popper 2002 [1934], 18)*

In *The Logic of Scientific Discovery*, the terms "falsifiable," "testable," "empirical," and "scientific" are almost synonymous. A hypothesis is testable only if it is falsifiable; if due to a faulty logical structure of a hypothesis or defective experimental procedure no experiment or observation can ever prove the hypothesis false, there is no point in testing the hypothesis in the first place. Only testable universal hypotheses are empirical and empirical hypotheses are the only type admitted in natural sciences. If we bear in mind that for Popper no scientific statement can be proven true beyond any doubt (absolute verification is logically impossible), the picture painted the author may seem slightly peculiar: science is a complex of hypothetical conjectures that has not been falsified (yet), but this does not warrant their veracity. Unlike logical positivists, Popper was an epistemological realist, i.e. he believed that even statements about unobservable entities can be true or false in principle,[4] but in practice scientists never have the certainty that their hypotheses are indeed true. The nature of scientific enterprise is to construct resilient hypotheses, which survive severe testing procedures.

> *The empirical basis of objective science has thus nothing 'absolute' about it. Science does not rest upon solid bedrock. The bold structure of its theories rises, as it were, above a swamp. It is like a building erected on piles. The piles are driven down from above into the swamp, but not down to any natural or 'given' base; and if we stop driving the piles deeper, it is not because we have reached firm ground. We simply stop when we are satisfied that the piles are firm enough to carry the structure, at least for the time being.*[21] *[1934], 93-94)*

According to Popper, logical positivism failed not only because its unrealistic requirements concerning meaningfulness of sentences and demarcation, but also because an unrealistic vision of actual research procedures. The philosopher does not subscribe to Gorton's tenet (1) of logical positivism, i.e. the "primacy of sensory data: data gained through the senses provides the foundation for our knowledge of the world"[12]. This view is a version of the myth of neutral observation that Popper disparaged as "the bucket theory of knowledge"[22]. Instead, the philosopher defended "the searchlight theory of knowledge," in which theoretical understanding guides observations and experiments from the stage of selecting research problems, through the stage of formulating hypotheses, designing and carrying out experiments, to the stage of interpreting results. It is largely due to Popper that modern philosophers widely acknowledge the essential theory-ladenness in every stage of scientific procedures.[5]

Popper's visions of science, although somewhat closer to the actual scientific practice, was criticized mainly on the grounds of underdetermination of scientific theories and confirmation holism, summarized neatly by the Duhem-Quine thesis. According to Willard Van Orman Quine[19] any item of knowledge, including a scientific hypothesis, functions is wider complex of theories, hypotheses, assumptions, implicit postulates, etc., which Quine and Ullian call "the web of belief"[23]. Thus, when a falsifying evidence surfaces, it indicates a problem in the web of belief, but it may not be possible to determine with absolute certainty which part of the web is affected. In the field of physics, the following situation is described by Pierre Duhem:

> *A physicist decides to demonstrate the inaccuracy of a proposition; in order to deduce from this proposition the prediction of a phenomenon and institute the experiment which is to show whether this phenomenon is or is not produced, in order to interpret the results of this experiment and establish that the predicted phenomenon is not produced, he does not confine himself to making use of the proposition in question; he makes use also of a whole group of theories accepted by him as beyond dispute. The prediction of the phenomenon, whose nonproduction is to cut off debate, does not derive from the proposition challenged if taken by itself, but from the proposition at issue joined to that whole group of theories; if the predicted phenomenon is not produced, the only thing the experiment teaches us is that among the propositions used to predict the phenomenon and to establish whether it would be produced, there is at least one error; but where this error lies is in just what it does not tell us.*[24]

By the same token, experimental and observational data do not lend full support to one particular theory, because any set of factual data can be accounted for by several mutually incompatible theories. In other words, "[at] the heart of the underdetermination of scientific theory by evidence is the simple idea that the evidence available to us at a given time may be insufficient to determine what beliefs we should hold in response to it"[25]. Therefore, it is hard to decide if a single hypothesis or a group of hypotheses is falsifiable outside the context of the web of belief, and the entire web of belief is hardly falsifiable *en bloc*, since it most likely involves untestable postulates (like a metaphysical untestable postulate about the existence of the external world). This suggests that if falsifiability is to be taken as the criterion for scientificness, no hypothesis can be ever recognized as scientific.

A more language-oriented example of a web of belief is Ferdinand de Saussure's famous principle of the arbitrariness of the linguistic sign: "The bond between the signifier and the signified is arbitrary" (cf. Saussure[2], 67). The fact that de Saussure discussed and refuted some possible counter-evidence to the principle suggests that the Swiss linguist treated it as a testable hypothesis rather than as a taken-for-granted postulate; after all, only statements believed to be inherently falsifiable require defense against falsification. The web of belief becomes evident when one realized that the principle of arbitrariness cannot be understood properly in isolation from the rest of de Saussure's theory of language. The terms *signifier* and *signified* have specific technical meanings and are defined against other elements of de Saussure's theory. The term *arbitrary* is, as I have argued elsewhere,[6] frustratingly vague, but it is defined by de Saussure as "lacking natural connection (between the signifier and the signified)" (Saussure[2], 69), so effectively the meaning of the term relies on the theoretical meaning of the term *natural*. Do motivated onomatopoeic words like *cuckoo* falsify the principle? De Saussure's defense of the principle (cf. pp. 69-70) makes it clear that in his understanding of the terms *arbitrary* and *natural* onomatopoeias do not falsify the principle. Yet if one disagrees and takes onomatopoeias as legitimate counter-evidence, a question arises: does *cuckoo* falsify the principle of arbitrariness as such or does it merely point to a problem in the theoretical framework in which the principle is formulated? Perhaps the term *natural*, in relation to which *arbitrary* is understood, is somehow defective? Or perhaps the whole model of the sign in which the signifier is linked with the signified by an arbitrary connection is faulty? Or perhaps the theory is wrong in an entirely different way? Even if one admits that onomatopoeic words like *cuckoo* are genuine falsifiers of de Saussure's theory, it is far from obvious which part of the theory is falsified.

Another problem with falsifiability as the demarcation criterion is that many theories widely recognized as pseudoscientific are, in fact, falsifiable. Admittedly, astrology and phrenology make testable predictions (which have been falsified), but this alone does not elevate them to the level of respectable empirical sciences. One could, in principle, add that falsified theories should not count as scientific, but even valid scientific theories produce predictions falsified in actual experiments. These cases of falsification can be always blamed on some other factors, located somewhere else in the web of belief, or on faulty research procedures.[7] Popper himself sustained that "[a] sentence (or a theory) is empirical-scientific if and only if it is falsifiable," and that falsifiability "only has to do with the logical structure of sentences and classes of sentences" (Popper[26], 82). This suggests that for Popper falsifiability was the necessary and sufficient feature, which makes it even harder to disqualify falsified theories as unscientific. One could, of course, bite the bullet and claim that astrology and phrenology were in fact scientific theories abandoned due to massive falsification. This, however, does not do justice the widely held conviction that these theories are not examples of falsified science, but genuine pseudoscience, as Popper himself admitted in *Conjectures and Refutations*[17].

Moreover, there is some tension between Popper's vision of science and recent developments in philosophy. Just like logical positivists, Popper leans towards the syntactic view, in which a theory is defined as system of statements formulated in a particular language. Unlike logical positivists, Popper did not aim at designing a special "language of science" and believed that the statements of a theory may be formulated in ordinary languages. However, the syntactic view does not appear to be a correct depiction of actual science. It is now widely believed that scientific theories are not merely collections of sentences, but collections of models expressible is various formalisms. The models are designed to represent phenomena rather than describe them. The task of experimental falsification is also complicated by the fact that many scientific theories are heavily idealized and their consequences cannot be observed directly in real-life circumstances. The problems are not necessarily fatal for Popper's criterion of demarcation, since idealized models can be used to approximate real-life phenomena, so that scientists may try to falsify the models indirectly. Nonetheless, the problems cast doubts on whether falsifiability is indeed as robust a criterion of demarcation as the philosopher hoped it to be.

Karl Popper should be credited for debunking the myth of "unbiased observation" as a key tool of science. All stages of actual scientific research are theory-laden and seeking objectivity through perfectly neutral protocol sentences, the crucial point in the logical positivist program, misses the point. Yet the criterion of demarcation proposed by the philosopher, the criterion of falsifiability, is besieged by its own difficulties engendered by underdetermination of theories, the Duhem-Quine thesis, and practical difficulties of discriminating between science and pseudoscience. By the 1960s the time was ripe to approach the demarcation problem from a different angle.

3.3. Thomas Kuhn's paradigms

Before turning to philosophy of science, Thomas Kuhn was a practicing physicist actively involved with the scientific community. Perhaps this explains why his seminal work, *The Structure of Scientific Revolutions*[6] (first edition published in 1962) emphasizes the sociological and historical aspects of science-making over the formal logical structure scientific theories. Kuhn appears to be somewhat less concerned with solving the demarcation problem than logical positivists and Popper, and the demarcation problem is not a central topic of his major works. Nonetheless, his ideas sometimes are believed to offer some clues for distinguishing science from other kinds of human activity.

Kuhn is known mostly for introducing two related notions: scientific paradigm and scientific revolution. Roughly speaking, according to the picture painted in *The Structure of Scientific Revolutions*[6] and to some extent in earlier *The Copernican Revolution*[27], science develops through cycles of the so-called normal science and revolutions. In the normal science phase, scientist typically work within a paradigm[8] defined broadly as “the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community”[6]. More specifically, a paradigm is defined by four key elements (cf.[6]) outlined below.

Symbolic generalizations. Defined as “expressions, deployed without question or dissent by group members, which can readily be cast in a logical form like (x)(y)(z) φ (x, y, z),” but some of them “are ordinarily expressed in words: ‘elements combine in constant proportion by weight,’ or ‘action equals reaction’”[6]. Kuhn suggested that symbolic generalizations describe the laws of nature, i.e. they specify the behavior of certain phenomena, but they also define the phenomena within relevant theoretical framework. For example, Kuhn observes that Newton’s second law of motion and its symbolic generalization “f = ma” (force equals mass multiplied by acceleration) both describes the quantitative relation between force, mass, and acceleration, and partly defines these notions within the framework of Newtonian physics. As a consequence, Kuhn blurred the distinction between laws of nature and theoretical notions, and therefore seems to argue for theory-ladenness of apparently “objective” laws, which can be found “out there” in physical reality independently of physical theories.

Metaphysical models. Defined as beliefs in what things are. In physics, Kuhn quotes the following sentences embodying metaphysical models: “heat is the kinetic energy of the constituent parts of bodies” and “all perceptible phenomena are due to the interaction of qualitatively neutral atoms in the void, or, alternatively, to matter and force, or to fields”[6]. This category also covers the so-called heuristic models, which are more metaphoric in nature and tend to specify what they are like, e.g. “the molecules of a gas behave like tiny elastic billiard balls in random motion”[6]. Thus, metaphysical models express metaphysical commitments in a community of scientists, while heuristic models defined acceptable analogies between phenomena. Generally, researchers are more committed to metaphysical than heuristic models, but these types of models form a continuum rather than clear-cut classes.

Values. According to Kuhn, values “are more widely shared among different communities than either symbolic generalizations or models, and they do much to provide a sense of community to natural scientists as a whole”[6]. Elsewhere[28], Kuhn, the author offered a non-exhaustive list of values, which he believed are inherent in natural scientific enterprises; these are: accuracy, consistency (internal, i.e. lack of contradictions within the theory itself, and external, i.e. coherence with other accepted scientific theories), broad scope (its consequences should extend beyond the original area of research), simplicity, and fruitfulness for further research. Unsurprisingly, the evaluation of these criteria is rather intuitive and in actual research there is frequently a tension between them. While scientists take the values as criteria for assessing two competing theories or two solutions to a problem, it is not always easy to ascertain whether (for example) a simpler and externally coherent theory is preferred to a more accurate and more broad-scoped one.

Exemplars. Defined as concrete solutions to the so-called puzzles, i.e. relatively narrow research problems, which scientists try to tackle during normal science stage. Kuhn emphasized the pedagogical role of exemplars: they are essential for education young scientists, because they show research problems and solutions accepted as valid within the community. The author believed that exemplars enjoy a special status within the paradigm, since the agreement on what constitutes a “good” solution of a puzzle largely determines the paradigm. Conversely, if two scientists do not agree on what the “right” puzzle and “right” solution are, it is very probable that they belong to different paradigms. Moreover, puzzles are the bread and butter of normal science; in Kuhn’s picture researchers typically work on small, limited, perhaps even unexciting or downright “boring” problems similar to exemplars accepted in their paradigm.

Stages of normal science, Kuhn argues, are followed by revolutions, which typically sweep away old paradigms and introduce new paradigms in their place. In the simplest scenario, a paradigm features a recalcitrant puzzle, which remains unsolved despite repeated attempts at providing a solution. Quite often the puzzle is ignored or set aside for further research, but given enough time it may engender the conviction within the community that the puzzle’s stubborn defiance to scientists’ efforts may indicate deeper problems within the paradigm itself. The puzzle becomes a serious anomaly within the scientific picture of the world presented by the paradigm and a crisis begins. A prolonged crisis motivates scientists to conceive of different theoretical approaches, which may develop into a new paradigm. The new paradigm may either successfully solve the anomaly in the previous paradigm or disqualify it as invalid in the new theoretical framework and consequently exclude it from the field of new normal science. Kuhn’s most famous example of this scenario is the Copernican revolution, discussed comprehensively in the book of the same title[27], but similar revolutions swept away alchemy in favor of chemistry, Aristotelian physics in favor of Galilean and Newtonian physics, the miasma theory of diseases in favor of the germ theory of diseases in medicine, and many more.

The new paradigm is usually incommensurable with the old one, i.e. scientists from different paradigms seriously disagree or talk past each other not only on what are the correct solutions of puzzles, but also what puzzles are permissible in the first place and what criteria should be adopted to evaluate them. Such great discrepancies may occur because a shift of paradigm replaces all or most of its elements: not only exemplar of accepted puzzles, but also metaphysical models, symbolic generalizations, and even values held dear within the community. As a consequence, scientists from different paradigms may subscribe to different views on such fundamental notions as science and rationality. Obviously, this makes communication across paradigms very difficult, since scientists not only disagree with each other, but, what is worse, talk past each other and have almost no common ground for rational discussion or collaboration. Instead, as Kuhn insisted, scientists from different paradigms “may hope to convert the other to his way of seeing science and its problems, neither may hope to prove his case. The competition between paradigms is not the sort of battle that can be resolved by proofs”[6].

Arguably, scientific revolutions happened in linguistics, too. It is widely believed that the advent of Noam Chomsky’s generative grammar was, in fact, a Kuhnian revolution which replaced the Bloomfieldian paradigm (indeed, Randy Allen Harris strongly suggests this in *The Linguistics Wars*; cf. Harris[10], 36). Perhaps, the emergence of cognitive linguistics, which from the very beginning defined itself in opposition to generative grammar, also counts as a Kuhnian revolution. Most probably, there was something like a Saussurean revolution in the early 20th century, when his *Course in General Linguistics* changed the way of thinking about the study of language. I will not attempt, however, to conclude whether these developments in linguistics constitute genuine revolutions in Kuhn's understanding. Yet regardless of whether linguistics developed in a revolutionary or gradual manner, it seems that modern linguistics hosts many incommensurable paradigms coexisting more or less peacefully. It is sterile to try to demarcate different paradigms in linguistics, even if there are any. Yet at least at first blush it appears that many groups of linguists feel and say that their work is fundamentally different from (and perhaps even better than?) the work of other groups. Thus, sociolinguists working "in the field" commonly disagree with theoretical "armchair" linguists on what is the "proper" subject matter of linguistics; corpus linguists may complain about "wishy-washy" methods of intuition-based linguistics on the grounds that science demands "hard data" processed by statistical tools; enthusiasts of Anna Wierzbicka's cultural linguistics may complain that Chomskyan and Langackerian grammarians pay too little attention to culture, while Chomskyites may protest against being put in the same bag as the enthusiasts of Langacker, and Generative and Cognitive Grammars are fundamentally different theories. Such differences are surely to be expected in a discipline composed of multiple Kuhnian paradigms.

Kuhn's vision of science offers a somewhat complex, but relatively straightforward demarcation criterion. An epistemic enterprise is a case of legitimate science if it forms a Kuhnian paradigm, complete with metaphysical models, symbolic generalizations, values, and, most importantly, its own puzzles to solve. Paul Feyerabend observes disapprovingly that Kuhn's vision of science is, in fact, often interpreted prescriptively and encourages misguided pretense for "scientificness" in fields of studies outside natural sciences.

> *I was quite unable to agree with the theory of science that [Kuhn] himself proposed; and I was even less prepared to accept the general ideology which I thought formed the background of his thinking. This ideology, so it seemed to me, could only give comfort to the most narrow-minded and the most conceited kind of specialism (…) More than one social scientist has pointed out to me that now at last he had learned how to turn his field into a "science" – by which of course he meant that he had learned how to improve it. The recipe, according to these people, is to restrict criticism, to reduce the number of comprehensive theories to one, and to create a normal science that has this one theory as its paradigm.*[29]

Feyerabend criticized attempts at using Kuhn's vision of science as a solution to the demarcation problem on the grounds that it fails to exclude many kinds of human activity that are certainly not science. In his well-known critique of Kuhn's idea for demarcation, Feyerabend provocatively points out another profession where paradigms can be found:

> *[If] the existence of a puzzle-solving tradition is so essential, if it is the occurrence of this property that unifies and characterizes a specific and well recognizable discipline; then I do not see how we shall be able to exclude, say, Oxford philosophy, or, to take an even more extreme example,* ***organized crime*** *from our considerations… Every statement which Kuhn makes about science remains true when we replace "normal science" by "organized crime"; and every statement he has written about the "individual scientist" applies with equal force to, say, a*

> *safebreaker.*[29] *(Feyerabend 1970, 199-200; original emphasis)*

If this critique is accepted, Kuhn's paradigms may perhaps successfully defined what is necessary for science, but they fail to capture what is sufficient. Kuhn himself entertained the idea that other fields of human activity can be described in terms of paradigms. In *The Essential Tension*[28] (Kuhn 1977, chap. 14) he analyzed the similarities and differences between science and art, while towards the end of *The Structure of Scientific Revolutions*[6] he noted that "[historians] of literature, of music, of the arts, of political development, and of many other human activities have long described their subjects in the same way [i.e. in terms of paradigms – HK]"[6] (Kuhn 1996 [1970], 208), but he believes that there are "features (…) none necessarily unique to science but in conjunction setting the activity apart"[6] (Kuhn 1996 [1970], 209). In other words, Kuhn proposes a more holistic approach to demarcation: science can be characterized by a set of properties, whose combination is necessary and sufficient for demarcating science. Nonetheless, this solution is hard to apply in practice, because of a certain vagueness around the concepts constituting the paradigm. For example, Hansson[30] (2015, sec. 4.3) notes that Kuhn and Popper could not agree on whether the research problems of astrology constitute legitimate puzzles and consequently could not agree on whether Kuhn's solution to demarcation problem successfully classified astrology as pseudoscience. Therefore, a linguist cannot rest assured that his discipline is legitimate science just because it fits the description of Kuhn's paradigm.

3.4. Imre Lakatos's research programs

Imre Lakatos is notable for his attempts to dovetail what he considered the strengths of Karl Popper's and Thomas Kuhn's ideas views on science, while steering clear of their shortcomings. Like Popper, Lakatos was a falsificationist: he believed that no scientific theory is ever conclusively verified and he denied the positivist clear-cut distinction between theoretical and observational statements, but he was aware that falsification is undercut by the Duhem-Quine thesis and under-determination of theory by data. Like Kuhn, Lakatos accepted that scientific theories are embedded in larger conceptual complexes and he appreciated the importance of a historical perspective on the development of science, but he did not believe that various "paradigms" are incommensurable; rather, he held that rational comparison and evaluation of paradigms is possible.

Lakatos observes that scientists tend to actively defend existing theories against falsifying evidence by evoking the*ceteris paribus* clause and construing *ad hoc* hypotheses. Popper was well aware of the fact that "it is always possible to find some way of evading falsification, for example by introducing ad hoc an auxiliary hypothesis, or by changing ad hoc a definition"[21] (Popper 2002 [1934], 19-20) and tried to elucidate the condition under which such practices should be forbidden by prescriptive methodological conventions, Nonetheless, as one may suppose, it is not always easy to judge which auxiliary hypothesis is valid simply because usually not all fact relevant for the evaluation are at hand. A proponent of a falsified theory may always argue that evidence is insufficient or irrelevant due to some sort of unknown factor, as Lakatos's imaginary case study illustrates:

> *The story is about an imaginary case of planetary misbehaviour. A physicist of the pre-Einsteinian era takes*

> *Newton's mechanics and his law of gravitation, N, the accepted initial conditions, I, and calculates, with their help, the path of a newly discovered small planet, p. But the planet deviates from the calculated path. Does our Newtonian physicist consider that the deviation was forbidden by Newton's theory and therefore that, once established, it refutes the theory N? No. He suggests that there must be a hitherto unknown planet p', which perturbs the path of p. He calculates the mass, orbit, etc., of this hypothetical planet and then asks an experimental astronomer to test his hypothesis. The planet p' is so small that even the biggest available telescopes cannot possibly observe it: the experimental astronomer applies for a research grant to build yet a bigger one. In three years' time, the new telescope is ready. Were the unknown planet p' to be discovered, it would be hailed as a new victory of Newtonian science. But it is not. Does our scientist abandon Newton's theory and his idea of the perturbing planet? No. He suggests that a cloud of cosmic dust hides the planet from us. He calculates the location and properties of this cloud and asks for a research grant to send up a satellite to test his calculations. Were the satellite's instruments (possibly new ones, based on a little-tested theory) to record the existence of the conjectural cloud, the result would be hailed as an outstanding victory for Newtonian science. But the cloud is not found. Does our scientist abandon Newton's theory, together with the idea of the perturbing planet and the idea of the cloud which hides it? No. He suggests that there is some magnetic field in that region of the universe which disturbed the instruments of the satellite. A new satellite is sent up. Were the magnetic field to be found, Newtonians would celebrate a sensational victory. But it is not. Is this regarded as a refutation of Newtonian science? No. Either yet another ingenious auxiliary hypothesis is proposed or… the whole story is buried in the dusty volumes of periodicals and the story is never mentioned again.*[31]

Lakatos's take-away message from this story is that "even the most respected scientific theory, like Newton's dynamic or theory of gravitation, may fail to forbid any observable state of affairs"[31], so it is far from obvious which observational or experimental fact constitutes a legitimate piece of falsifying evidence.[9]

To address this problem, Lakatos proposed to evaluate empirical hypotheses in conjunction with the whole theoretical context in which they are embedded, i.e. to evaluate the entire *research program* (Lakatos's 1970 term). While this proposition alone may not have been terribly controversial to a classical Popperian, Lakatos also held that a research program should be evaluated not only against the body of observational and experimental facts, but also against other – usually chronologically earlier – research programs. This move can be viewed as an attempt to tackle the problems raised by the Duhem-Quine thesis explicitly in the process of theory testing, but it also indicates the philosopher's sensitivity to the historical context of science-making. Lakatos believed that evaluation of a theory is not only a matter of judging how the theory withstands harsh attempts at falsification, but also a matter of whether it withstands the attempts better than competing research programs.

A research program is composed of the so-called "hard core," a group of theories considered fundamental and constitutive of the program. The "hard core" theories are not normally tested and are normally defended against falsification when potential falsifying evidence appears. The hard core is surrounded by a belt of auxiliary hypotheses considered less crucial for the program. The auxiliary hypotheses can be modified or sacrificed altogether in order to accommodate falsifying evidence and to leave the hard core intact. A research program is a notion meant to capture roughly same aspect of scientific enterprise as Kuhn's paradigm. Lakatos acknowledges the Kuhnian connection explicitly:

> *It is a succession of theories and not one given theory which is appraised as scientific or pseudo-scientific. But the members of such theories are usually connected by a remarkable continuity which welds them into research programmes. This continuity—reminiscent of Kuhnian "normal science"—plays a vital role in the history of science*[31]

As the above passage suggests, research programs are also central to the philosopher's solution to the demarcation problem. For Lakatos, it was vacuous to assess a theory as scientific in isolation from its conceptual framework; instead, it is the whole framework that can be assessed as scientific. The litmus paper is in this case the manner in which a research program handles problems like potential falsifiers or unsolved puzzles (as Kuhn might call them). If a program handles them by making adjustments in the form of novel predictions, which can be tested independently of the original context in which the predictions were made, the program is progressive and scientific. If a program fails to make new predictions and merely struggles to save its hard core by tampering with definitions of theoretical notion and *ad hoc* auxiliary hypotheses untestable outside the context of the original problem, the program is degenerative and unscientific. For a research program to be truly progressive, it needs to offer more epistemic content than its predecessors, i.e. it should explain as much as earlier research programs and add something more to the understanding of the subject matter. Under this interpretation, the difference between Renaissance astronomy and astrology was that the former made bold falsifiable prediction about celestial bodies and, viewed from an historical perspective, it gradually increased our understanding of nature. Astrology, in turn, offered no novel predictions and astrologers focused exclusively on "stitching up" their theories or, in the words of Thagard, made "little attempt to develop the theory towards solutions of the problems, [showed] no concern for attempts to evaluate the theory in relation to others, and [were] selective in considering confirmations and disconfirmations"[32].

One possible problem with Lakatos's solution to the demarcation problem is that it shifts the emphasis from the properties of the research program to the properties of scientific practice. What one typically, expects of a solution to the demarcation problem is some sort of normative framework which allows for evaluating inherent properties of a theory (research program, paradigm, hypothesis, etc.) rather than the behavior of a community. In other words, we typically want to be able to say that astrology is a pseudo-science because of what it claims or because of the logical structure of what it claims (e.g. the lack of falsifiability, the lack of legitimate puzzles) and not because of what astrologers do. Moreover, this solutions makes it difficult to evaluate current or new theories, since such evaluations require considerable historical context. This, however, is not a fatal objection to Lakatos's proposal, since perhaps various fields of science are indeed united by a set of shared practices rather than by a set of formal properties shared by all theories.

A more serious difficulty is that the criterion of progress based on novel predictions does not appear to account for all instances of legitimate science. In Renaissance, the transition from Ptolemy's geocentric model of the Solar System to Copernicus's heliocentric model is widely hailed as a magnificent case of scientific progress, but the two theories made the same predictions about the movement of celestial bodies. The first predictions that truly differentiated the two models (stellar parallax, predicted only by the Copernican model) remained unconfirmed until the 19th century, when the Copernican system came to be universally accepted as scientific. Instead, as Kuhn argues[27], the acceptance of the model depended on factors like the mathematical elegance of the Copernican model, the abandonment of Aristotelian physics in favor of Galilean and Newton physics, and general cultural and social changes in the early modern era. Novel predictions do not appear to have played a major role in this process.

What is more, the criterion of progress based on predictions may be biased against descriptive sciences. While it may be argued that hypothesis-driven research like physics and chemistry rely heavily on predictions tested in some sort of experimental setting, botany, zoology, and geography rely on observation, description, and classification. Such disciplines are not expected to predict novel facts, but it would be hard to argue that they do not progress[10] and are not sciences. Thus, the Lakatosian solution to the demarcation problem does not appear to respect the scientificness of some disciplines widely recognized as legitimate sciences. While in principle it would be possible to discard descriptive sciences as unscientific, a satisfying solution to the demarcation problem is expected to sanction our current understanding of science, rather than to demonstrate that our intuitive understanding of science is defective. Lakatos's proposal successfully captures many important aspects of scientific research, but ultimately fails to deliver a criterion for demarcation.

3.5. Science as a social and cultural construct

The difficulties in capturing the essential properties of science may create the temptation to approach the problem from a slightly different angle. Thomas Kuhn's and Imre Lakatos's ideas may suggest that the key to the solution of the demarcation problem lies in social and cultural factors. Perhaps science is merely a social construct and what counts as science is specified more or less arbitrarily by social conventions? Perhaps the only reason why astronomy is a "real science" while astrology is a "pseudo-science" is that in modern Western culture it is commonly agreed that the disciplines are thus classified? At first sight, this is attractive option, which quickly cuts the Gordian knot of demarcation and spares us from tedious, and possibly futile, attempts at disentangling the knot thread by thread. The solution is also convenient, because linguists could simply declare that their discipline is scientific without the need to adopt additional standards of scientificness. Nevertheless, the history of science shows that what constitutes "good science" is more than just a social construct within a community.

Of course, science is a social practice in a trivial sense: science is an enterprise undertaken by a community of experts and the community is a social group by definition. Science is also a cultural practice in a trivial sense, since the experts in the community were born and raised in a culture, which affects their thought and behavior. Moreover, one may plausibly claim that scientists participate in a certain professional culture, since they are committed to similar values welded into a professional ethos, like the ethos proposed by Robert K. Merton comprising universalism, communality, disinterestedness, and organized skepticism[33]. These claims are hard to deny, but professional ethos and embeddedness in a wider social, cultural, and historical context merely characterizes rather than constitutes science. While (good) scientists are committed to the professional ethos, it is not enough to be committed to the ethos to be a (good) scientist.

A stronger claim is that what is accepted as legitimate science is determined predominantly by social factors rather than the intrinsic properties of theories and research practices. Such views are particularly popular with sociologists of science (e.g.[34][35][36][37]). Even though few sociologists and philosophers of science would seriously hold that science is entirely immune to social and cultural influences, even fewer scholars would claim that scientificness can be established by means of a social consensus alone. Yet the extent to which various social factors establish the scientificness of a theory or a research practice is subject to heated debates. A cautious approach is to admit that while a scientific theory is a social (cultural, historical, etc.) construct, the theory is constrained by the phenomena that it is meant to account for. Even the relatively weak constraint of empirical adequacy imposed by modern empiricism (as opposed to truth imposed by epistemological realism) is largely independent of the social, cultural, and historical factors that shaped the theory. Ian Hacking aptly argues for such cultural and historical independence in a passage on the second law of thermodynamics[11]:

> *Yes, thermodynamics takes its name from the thermodynamic engine—the old name for the steam engine. Thermodynamics is vested in that ingenious piece of the industrial revolution and wage capitalism. But the content of the Second Law, what it now means, is independent of its history. The Second Law still uses the concept "work," which betrays its industrial origins, but that has no consequence for any present use of the law.*[38]

Hacking is therefore ready to admit that the second law of thermodynamics is a social construct in a fairly strong sense: the historical period in which it was formulated influences the very form and content of the law. What is more, the law was originally meant to describe the behavior of a restricted class of objects, i.e. steam engines, and only later was its applicability expanded to any thermodynamically isolated system. In this sense, the form and the use of the law is embedded in culture and history. Yet the law is successful not because of these cultural and historical factors, but because it is an empirical adequacy (or, as a realist would argue, because it is true), i.e. it correctly describes the behavior of an isolated system. One could speculate that the form of the law could have been different, for example if it were formulated in a different time in history or in a different place, but the alternative formulation would have to describe the behavior of an isolated system in an equivalent fashion in order to be accepted as a fully-fledged law of science.

One consequence of this approach is to accept the contingency of modern science. Our best scientific theories could have been different and they would still be good theories. This view is quite modest and prudent, especially in comparison to the alternative claim that all possible sciences inevitably converge towards one optimal shape and content. In this sense, social, cultural, and historical factors may exert a large influence on science, but that does not mean that science is sanctioned by a social consensus alone.

A good illustration of how extensive social and cultural support failed to sanction a theory as legitimate science is the case of Lysenkoist biology[39][40]. Beginning in the late 1920s, the Russian biologist Trofim Lysenko developed alternative theories in genetics, which conflicted with the Mendelian theory of inheritance and the Darwinian theory of evolution. Lysenko's ideas were in line with the official Marxist philosophy and ideology of the Soviet Union, while Mendel's and Darwin's theories were rejected as a part of "decadent" capitalist culture. From the 1930s to the 1950s Lysenkoism was the official doctrine in the Soviet Union and gained unprecedented social, political, ideological, financial, and institutional support. Opposition to new theories was actively suppressed: Mendelian geneticists were forced to "convert" to Lysenkoism, dismissed from their academic positions, and imprisoned. Yet despite vigorous social and political support and active persecution of the opponents, Lysenkoist biology proved not only wildly empirically inadequate, but almost entirely unsuitable for almost all practical purposes in agriculture and farming. In the late 1950s Soviet science started reverting to "Western" Mendelian genetics, which may have conflicted with the official Marxist ideology, but it nevertheless correctly accounted for the biological phenomena within its scope.

"The Lysenko affair," as Joravsky[39] calls it, illustrates that even massive and unquestioning social and political support is not enough to decree the scientificness of an empirically inadequate theory. The borderline between science and non-science cannot be constructed and reconstructed at will depending on social and political circumstances without taking into account how well a theory deals with the phenomena it is meant to describe or explain. Even moderate and balanced accounts of "the Lysenko affair," like Dominique Lecourt's *Proletarian Science?*[40], do not go as far as claiming that Lysenkoism was a case legitimate science for a couple of decades when it was socially constructed in the Soviet Union as legitimate science. On the contrary, Lecourt accepts that Lysenkoism was non-scientific even at its heyday in the 1940s. It is also worth noting that Lysenkoist biology was abandoned long before the disintegration of the supporting social and political forces, i.e. the collapse of the Soviet Union. The failure at establishing the Lysenkoist genetics as a legitimate science demonstrates that social and cultural factors are unable to secure the scientificness of a theory, and therefore they cannot be used to solve the demarcation problem.

3.6. Paul Feyerabend's epistemological anarchism

Paul Feyerabend was called by his opponents an "anti-science philosopher" and "the worst enemy of science"[41] (also[42]). He owes this reputation partly to the provocative tone of some of his writings and partly due to superficial or gratuitous interpretations by his opponents. In fact, Feyerabend was not actually an enemy of science, but he was highly critical of a particular way of looking at science. Moreover, his critique raises important points relevant for the demarcation problem.

Roughly speaking, the philosophers discussed so far tended to believe that there are some stable, if not universal, normative standards that secure the scientific character of knowledge. Logical positivists attempted to design a new language for science, Popper opted for falsifiability as the crucial criterion of scientificness, Lakatos emphasized the role of predictions and progress, and Kuhn focused on the role of puzzles within a paradigm. At the heart of Feyerabend's epistemological anarchism lies the conviction that there are no exclusive universal standards guiding scientists in all disciplines throughout the entire human history. Instead, he opted for "local" solutions to specific problems in specific circumstances. Feyerabend realized that his philosophy was prone to misinterpretation and crucial nuances may elude his readers (also because the philosopher's bold style obscured many subtle points). In *Against Method*, the author juxtaposed "naive anarchism" with his own, more sophisticated, version:

> *The limitation of all rules and standards is recognized by* ***naive anarchism****. A naive anarchist says (a) that both absolute rules and context-dependent rules have their limits and infers (b) that all rules and standards are worthless and should be given up. Most reviewers regard me as a naive anarchist in this sense, overlooking the many passages where I show how certain procedures* ***aided*** *scientists in their research (…) Thus while I agree with (a) I do not agree with (b). I argue that all rules have their limits and that there is no comprehensive 'rationality', I do not argue that we should proceed without rules and standards. I also argue for a contextual account but again the contextual rules are not to replace the absolute rules, they are to supplement them.*[43]

In later editions of *Against Method* the author retracted some of his radical claims and opted for a less relativistic approach to scientific rationality. A footnote in the third editions from 1993 reads:

> *Considering some tendencies in US education ("politically correct", academic menus, etc.), in philosophy (postmodernism) and in the world at large I think that reason should now be given greater weight not because it is and always was fundamental but because it seems to be needed, in circumstances that occur rather frequently today (but may disappear tomorrow), to create a more humane approach.*[43]

Yet even in his more radical formulations Feyerabend is not, contrary to popular opinions, an enemy of rationality. Instead, the philosopher claimed that the rationality of certain theories may not be immediately apparent and can be appreciated only from an historical perspective. One of Feyerabend's favorite example is the Copernican Revolution, i.e. the transition from the geocentric to the heliocentric model of the Solar System. The popular interpretation is that the revolution was a triumph of scientific rationality against naive and superstitious worldview dictated by the contemporary Church. Feyerabend turned the situation on its head and argued that in the days of Copernicus rejecting his theory was quite rational, i.e. the people had good reasons not to accept the model[43])[44]. Firstly, the idea that Earth is moving, while the Sun does not was against the evidence of the senses: Earth is not perceived as moving, but the Sun is. Secondly, the Copernican theory was incoherent with contemporary Aristotelian physics, requiring Earth to rest in the center of the universe, and the physics successfully explained many observable facts. Thirdly, the heliocentric system was incompatible with the popular view about the structure of the universe based on literal reading of the Bible. Feyerabend asks: "Were there any reasons for people strongly tied to a literal geocentric version of Christianity to drop this version and adopt Copernicus instead? (…) Can we imagine that hearing of Copernicus such people will stick to their views?"[44]. Feyerabend's answer is that the people of the day were justified in rejecting the heliocentric model not due to ignorance and superstition, but because the heliocentric system was 1) counter-intuitive, 2) incompatible with apparent facts, 3) incoherent with contemporary science, and 4) at odds with spiritually meaningful vision of the universe. The reason why the Copernican model appears more rational nowadays is that the factors supporting the medieval rationality have been removed: we are trained to think about the motion of the Sun as an illusion, modern physics does not require Earth to be immobile, and the Bible is not believed to present the literal vision of the universe. Thus, the rationality of Copernicus's model may not have been immediately apparent in the 16th century and its rejection was rational if evaluated in the historical context. This is true for any scientific theory.

Feyerabend does not believe that science is a unified and coherent epistemological project. In the chapter from *The Tyranny of Science* under the telling title "The Disunity of Science," he wrote that:

> *the people who say that it is science that determines the nature of reality assume that the sciences speak with a single voice. They think that there is this monster, SCIENCE, and when it speaks it utters and repeats and repeats and repeats again a single coherent message. Nothing could be further from the truth.*[44].

What is typically perceived as monolithic and unitary Science (with a capital "S") disintegrates after closer inspection into a collection of distinct and diverse epistemological projects, typically narrowly specialized, focused on different subject matters, employing different methodologies, and using different standards to evaluate their results. Hypothesis-driven sciences like physics may rely heavily on predictions of observable facts that can be used to test hypotheses, but descriptive sciences like botany do not. While the covering-law model may be desirable in inorganic chemistry, biologists do not typically believe that their subject matter can be adequately accounted for by means of deterministic, exceptionless principles and are much more open to probabilistic regularities discovered by means of statistical analysis. Remarkable differences can be found even within one scientific discipline, which Feyerabend illustrated with various attitudes towards the theory of elasticity in physics:

> *Which worldview does the theory of elasticity suggest? This is difficult to figure out. For some people elasticity is a peripheral subject which, naturally, is a special case of elementary particle physics, only nobody has yet shown that and nobody (among the people I am talking about) really cares. Others say that elasticity is a separate subject which has as little to do with elementary particle physics as it has to do with the Bible. There are scientists who eschew speculation and regard it as a piece of metaphysics. Many scientists of that creed avoided the general theory of relativity.*[44]

Like logical positivists, Popper, and Lakatos, philosophers may have fantasized about scientists subscribing to a common standards of rationality and methodological norms which licensed the scientific character of their projects; the only problem was that the standards were implicit and the task of philosophy of science was to bring them to light. Yet the picture of natural sciences that Feyerabend painted is not unlike the stereotypical picture of social sciences: scientists are a garrulous crowd of experts in their own narrow fields, enclosed in their own paradigms and their own communities, and focused on their own agendas. Perhaps scientists are united by their professional ethos, but certainly not by a set of prescriptive methodological norms. Feyerabend's science is not merely heterogeneous and pluralistic, but utterly chaotic and anarchist.

Consequently, Feyerabend's anarchism discourages attempts to find a cut-and-dry solution to the demarcation problem. Science is – and should be – heterogeneous, so the attempts to capture the essence of science by means of a list of necessary and sufficient features is not only sterile, but, what is worse, distorts the very nature of the enterprise. At best, science is a cluster concept similar to Wittgenstein's 'game' established by "a complicated network of similarities overlapping and criss-crossing: sometimes overall similarities, sometimes similarities of detail"[45] and not by a stable core of shared properties.

4. Other criteria

It could be argued that even though it is hard to compile a list of necessary *and* sufficient properties defining science, we could at least propose properties necessary *or* sufficient for a scientific enterprise. Having only one of the lists would not count as a fully-fledged solution to the demarcation problem, but a list of sufficient properties would allow us to make sure that our enterprises are scientific and a list of necessary properties would allow us to classify enterprises lacking these properties as non-science. Yet by now it should be evident how popular candidates for necessary or sufficient properties of science fail to live up to this expectation. Extensive use of mathematics is not a mark of science, since non-scientific astrology is heavily mathematized, while Darwin's classic theory of evolution is considered to be one of the most spectacular successes of science despite extremely limited mathematical and quantitative component. It is not the case that science deals only with observable phenomena that can be examined in the real world, since many esteemed scientific theories are so idealized that they fail to describe any real-world phenomena and virtually all theories feature notions corresponding to unobservables. Even the elements testable in principle are entangled in vast webs of beliefs and not all elements of the web can be tested at all. In practice, as Lakatos argued, vigorous defense of the "hard core" elements of theories against falsification is more typical in scientific communities than the vigorous testing prescribed by Popper. While it is true that rationality should be a guide in all scientific enterprises, it is often difficult to see the rationality of present decisions and their rationality (or lack thereof) can be properly evaluated only in a historical perspective.

Another property considered crucial for science is objectivity. The discussion on this property is notoriously hindered by the polysemy of the term; in philosophy the term has several logically independent senses, which tend to be conflated in less rigorous discussions on methodology of linguistics. One of the senses pertains to impartiality, which is certainly an important virtue of a scientist, but it is hard to see how impartiality alone could secure the scientific character of research. After all, there are domains of human activity, like legal proceedings, which should be impartial, but are not science. Objectivity is also associated with the notion of neutral observation uncontaminated by theoretical preconceptions. Neutral observers supposedly report "what they really see," because their reports are not affected by any personal agendas that biased observers may have. It is now commonly accepted in philosophy of science that all observation is inevitably theory-laden and even if neutral observation were possible, it would be ineffective for all practical purposes, because it would fail to have any useful conceptual connection to scientific theory. Popper's[22] and Nagel's[46] conceptions of objectivity point to important properties of scientific knowledge, but they can hardly be considered sufficient and it is debatable to what extent they are necessary for science. Moreover, the conceptions are almost never mentioned in the methodological and metatheoretical debates about linguistics.

Similar terminological problems surround the notion of empiricness. Many linguists would subscribe to the claim that a truly scientific linguistics should be empirical, but it is not always clear what the term *empirical* refers to. The two senses discussed so far, i.e. the connection to observation and Popper's testability, are important properties of scientific theories, but it should be clear by now that they are much less straightforward than usually believed. Not all hypotheses via observation are scientific (e.g. statements about singular objects), not all scientific hypotheses are testable through observation (e.g. due to heavy use of idealizations), and not all scientific claims are testable in principle (e.g. Kuhn's metaphysical models) or in practice (Lakatos's hard core theories). Linguists and social scientists also tend to associate the term *empirical* with quantitative methods (cf. Kowalewski[47]), but, as astrology and classical Darwinism, mentioned at the beginning of the section, demonstrate, the use of mathematics and quantitative methods fail miserably as necessary and sufficient properties of science.

5. Conclusion

Since the late 1970s the demarcation problem was being gradually abandoned as a hopeless puzzle in favor of other topics. Not all philosophers of science explicitly subscribe to Feyerabend's epistemological anarchism, but it is hard not to have the impression that his vision of heterogeneous science lacking shared core properties has been more influential than the solutions of the demarcation problem proposed by logical positivists, Popper, and Lakatos. The lack of a commonly accepted solution gives little hope for progress in the debate about the scientificness of linguistics. Since we do not know exactly what makes disciplines like physics, chemistry, and biology respectable sciences, we also do not know what would make linguistics a respectable science.

Fortunately, many issues concerning the methodology and metatheory of linguistics can be fruitfully discussed even when we cannot determine whether linguistics is a natural science on par with physics, chemistry, and biology. For example, one may argue that the function of idealizations in linguistics is different than the function of idealizations in physics and analyze the differences in the context of research practices in the two fields of study without fore-judging whether linguistics is a "real" science like physics. In fact, it is hard to see how establishing the scientificness of linguistics could help us to understand the role of idealizations in the first place. The two issues are largely independent and one can be discussed in isolation from the other.

Footnotes

1 This article was originally an excursus in an early draft of my book*The Puzzle of Vehicle Selection in Conceptual Metonymies*. Since the final version of the book is focused more closely on metonymies rather than philosophy of linguistics, this article was not included in *The Puzzle…*

2 See Thagard[32] for a more detailed discussion. For a comprehensive analysis of the shift from non-scientific astrology to scientific astronomy, see Thomas Kuhn's *The Copernican Revolution*[27].

3 This paragraph is based on the views outlined by Rudolf Carnap[16].

4 This is particularly evident in the philosopher's later works including*Objective Knowledge: An Evolutionary Approach*[22].

[5] Other classical critiques of the myth of neutral observation are offered by Kuhn[6] and Sellars[48].

[6] See Kowalewski[49][47].

[7] Retaining a seemingly falsified theory is not always a case of “bad science,” as the history behind the discovery of the planet Neptune shows. In the 19th century, anomalies in the predicted orbit of Uranus seemed to falsify a good portion of Newtonian physics. Nonetheless, the discrepancies between the predicted and the actual orbits of Uranus were not taken as falsifying evidence against contemporary physics, but as an indication of some hidden factor, a massive object whose gravitational field disturbed Uranus’s orbit. The hypothetical orbit of the new planet was calculated independently by Urbain Le Verrier and John C. Adams from 1845 to 1846 and the existence of Neptune was confirmed by observation shortly after[50].

[8] There is some terminological confusion between various editions of The Structure of Scientific Revolutions, and the way the terminology is employed by other authors. In later editions, Kuhn defined the broad sense of paradigm as “the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community,” which he later preferred to call a disciplinary matrix. The narrow sense of paradigm is “one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science,”[6] which was Kuhn’s preferred sense of paradigm, but this is now often referred to as exemplar. Throughout this article I will use the terminology more entrenched especially outside philosophy of science and use paradigm for the broad sense (“constellation of beliefs”) and exemplar in the narrow sense (“concrete puzzle-solutions”).

[9] And as the discovery of Neptune shows, not all resistance to instant falsification is a case of “bad science” and may lead to valuable discoveries; see footnote 6.

[10] For a comprehensive discussion of the historical progress in biological taxonomy, see LaPorte[51].

[11] The second law of thermodynamics states that the entropy of an isolated system, i.e. a system that does not exchange energy (in the form of heat and work) with its environment, is either constant or increases, but never decreases.

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