Rationality and Irrationality in the History of Continental Drift
Was the Hypothesis of Continental Drift Worthy of Pursuit?

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Abstract

The revolution in geology, initiated with Alfred Wegener's theory of continental drift, has been the subject of many philosophical discussions aiming at resolving the problem of rationality underlying this historical episode. Even though the debate included analyses in terms of scientific methodology, applications of concrete accounts of epistemic justification to this case study have been rare. In particular, the question as to whether Wegener's theory was epistemically worthy of pursuit in the first half of the twentieth century, that is, in its early development, remained open or inadequately addressed. The aim of this paper is to offer an answer to this question. The evaluation of Drift will be done by means of an account of theory evaluation suitable for the context of pursuit, developed in Šešelja & Straßer (201x). We will argue that pursuing the theory of continental drift was rational, i.e., that it was irrational to reject its pursuit as unworthy.

Key words: Alfred Wegener, continental drift, rationality, context of pursuit, pursuit worthiness

1. Introduction

Ever since the revolution in the earth sciences culminated in the overall acceptance of the theory of plate tectonics, philosophers and historians of science have been analyzing this shift in geology. What made the development in geology very interesting is its specific dynamics. Even though the hypothesis of continental drift was proposed by Alfred Wegener already around the 1920s, it was firmly rejected by many geologists not only as unacceptable but even as unworthy of further pursuit. Almost half a century had to pass in order for this hypothesis to become finally accepted and elaborated into the theory of plate tectonics. Such a development inspired discussions among philosophers and historians of science, which especially focused around two issues: first, the nature

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of the revolution in geology and the applicability of different methodological frameworks to it, and second, the rationality or irrationality of the stances of scientists throughout the revolution.

With regard to the first question, some philosophers and historians of science argued that this episode can be described in terms of Kuhn's notion of scientific revolution (e.g. Stewart (1990)). However, the majority of them agreed that Laudan's account of progress of science is more suitable for this case-study (see e.g. Le Grand (1988), Frankel (1979), Laudan (1987)). The aptness of Laudan's framework stems from two important notions in his account. On the one hand, his notion of a research tradition as a broader theoretical framework, constituted by specific scientific theories, has been shown useful for capturing the rivaling camps in geology, neither of which could be reduced to one generally accepted theory.[1] On the other hand, his distinction between the context of pursuit and the context of acceptance (Laudan, 1977, p. 108-110) proved to be important for analyzing questions of rationality regarding this case study. According to Laudan, "acceptance, rejection, pursuit and non-pursuit constitute the major cognitive stances which scientists can legitimately take towards research traditions (and their constituent theories)" (Laudan, 1977, p. 119). As the names suggest, the context of acceptance deals with the question as to whether a certain theory is to be accepted as the standard in the given field, while the context of pursuit deals with the question as to whether a (possibly young, undeveloped) theory is at all worthy of further pursuit. Such a distinction allows for a twofold analysis of the rationality of judgments made by geologists during this time. This brings us to the second important topic that attracted interests of philosophers and historians of science, as we have mentioned above.

With respect to the context of acceptance, there has been a general agreement that it was rational to reject Wegener's theory of drift when it first appeared in the 1912-1915, and to accept Drift[2] in the early 1960s, after it had developed into the theory of plate tectonics. With respect to the context of pursuit, most of the authors simply described the views of geologists at the time. Nevertheless, the question: *Was pursuing the theory of continental drift in the first half of the twentieth century at all rational (in the sense of being epistemically justified), or was it rational to reject its pursuit?* – so far has not been properly addressed.

The aim of this paper is to answer this question in terms of a concrete account of epistemic justification. To this end, we will use the account developed

[1]In contrast to Lakatos' notion of a research program, Laudan's notion of research tradition allows for an evolving hardcore, which, according to some authors, makes it especially suitable for describing the geological sciences in the first half of the twentieth century (see e.g. (Frankel, 1979, p. 53)). According to Laudan, what makes theories belong to the same research tradition is its hardcore, which, even though sacrosanct for its proponents, can still evolve. In other words, theories belong to the same tradition not because some of their crucial assumptions are identical, but rather because these assumptions overlap (see Laudan (1977), p. 99).

[2]As it is usual in the literature on this case study, we will call this research tradition and its constituting theories (running from Wegener's theory of continental drift to the theory of plate tectonics) – Drift.

in Šešelja & Straßer (201x), which offers the criteria of epistemic justification suitable for the context of pursuit.

The paper is structured as follows. We begin in Section 2 by offering a brief historical overview of the revolution in geology. InSection 3 we will explicate our research question in some more detail, and in Section 4 show that so far this question has not been properly addressed in the literature on this historical episode. In Section 5 we will present a summary of the account of epistemic justification which we shall employ in this paper. Sections 6 to 9 bring an evaluation of pursuit worthiness of Drift in the first half of the twentieth century. In Section 10 we will discuss the epistemic stances of geologists in this time period in view of our evaluation. Moreover, we will point out the importance of the evaluation of epistemic pursuit worthiness and some undesirable implications for scientific debates once this type of assessment has been neglected. Section 11 concludes the paper.

2. Historical Overview of the Revolution in Geology

2.1. Rivaling Theories

Alfred Wegener launched the idea of continental drift in a short article in Wegener (1912). A more elaborate version appeared as a book in 1915 Wegener (1915). His central claims were that all the continents had once been united, had broken apart and had drifted through the ocean floor to their current locations (Le Grand, 1988, p. 1). Following Le Grand we call adherents of the of large scale lateral movements of continents drifters. Wegener probably was not the first drifter, but he was the first to develop a full-fledged argumentation for it. He tried to show that Drift is superior to the two theories that already existed, viz. permanentism and contractionism. The summary of views and arguments presented below is based on Le Grand (1988) (especially p. 19-28, 40-46 and 55-57).[3]

According to permanentists "... the continents were formed in remote geological times as the earth had gradually cooled down and contracted. Since then, they had been permanent features of the earths surface." (Ibid., p. 20-21). Continents do not move laterally (this is how permanentism differs from Drift) and do not disappear (this is how it differs from contractionism, see below). James D. Dana, professor at Yale University from 1850 to 1892 and one of the main adherents of this theory, used the slogan "Once a continent, always a continent; once an ocean, always an ocean" (quoted from: Ibid., p. 21). According to permanentists there have been small elevations (producing mountains) and small subsidences (producing e.g. shallow inland seas).

The most important representative of contractionism was the German geologist Eduard Suess. The central claim of his theory was the following: "As the earth lost its heat, a rigid crust formed. As the earth continued to cool and

[3]Even though Le Grand offers a good overview of this debate, some details that are relevant for our discussion are better worked out in Oreskes (1999) (see Sections 6-9).

shrink, this crust wrinkled, folded and subsided" (Ibid., p. 25). This process explains how mountains come into existence (lateral movement of parts of crust). The collapses occur sporadically, creating new oceans and new continents: when the crust collapses in a certain region, the water flows to the new lowest point; continents thus can become oceans and oceans land. Because the collapses occur sporadically, contractionists see the history of the earth as divided into periods of rapid change and periods of stability.

According to Wegener the earth consists of a series of concentric shells with different compositions and densities (highest density in the near to the core). The temperature also is higher closer to the core. The continents constitute the outermost shell and consist of blocks of *sial* (silica + alumina) which (like icebergs in the sea) partially float on and extend into blocks of *sima* (silica + magnesia). The oceans are situated between the blocks of sial, and ocean floor is made of sima. The continents once were united in the super-continent Pangaea, which broke apart in the Cretaceous (Wegener did not give e reason for this break). Since then, the continents are propelled by one or more forces through the ocean floor. Sometimes they move apart (Africa and South America). Sometimes they collide, resulting in mountains (e.g. the collision of India and Asia creating the Himalayas).

In order to understand Wegener's theory and the arguments discussed below well, it is important to elaborate the analogy with the icebergs. Icebergs are solid, while the water in which they float is fluid. Analogously, the continents are solid, and the sima in which they float is (relatively) fluid. The density of the icebergs is lower than that of the surrounding water, so they float; analogously, the density of the continents (sial) is lower than that of the surrounding ocean floor (sima), so the continents float. Wegener invoked two forces: pole-flight (Polflucht, a force due to the rotation of the earth and directed form the poles to the equator) and a tidal force (from east to west) as a result of gravitational attraction of the sun and the moon.

2.2. Arguments in the Debate

Here is an overview of the arguments that were exchanged:

1. Permanentism cannot explain the distribution of fossils (palaeobiogeography) and living species. If oceans and continents are permanent, similarities between species separated by oceans cannot be explained. Drift explains the similarities, because the continents were once united. contractionists assumed that there are sunken continents and/or sunken landbridges that connected the continents we have now. These sunken continents and landbridges explain the similarities. Suess postulated the existence of the palaeocontinent Gondwana, of which the central part sunk in the Indian Ocean. What remained is now: Australia, India and Africa. For the connection between Africa and South America he left the two options open. In the terminology of Laudan (1977), the distribution of fossils and living species is an anomaly for permanentism.

2. Permanentism cannot explain geological similarities between continents. Mountain chains and coal-basins seem to continue on both sides of the Atlantic Ocean (e.g. coal fields in Pennsylvania on one side, French-Belgian coal basin on the other side). Drifters can easily explain these similarities. Contractionists have to assume that the sunken parts have the same geological structure as the parts to be explained (so they formed a geological connection, not just a route for plants and animals). Permanentists have to claim that the similarities are accidental. This is another anomaly in Laudan's terms.

3. Contractionism is incompatible with isostasy (see above: sima and sial) that was well supported at the beginning of the 20th century by all kinds of measurement. If the landbridges consist of sial, they cannot sink through the denser sima of the ocean floors. The same for the sunken connecting continents. In Laudan's terminology, this is an external conceptual problem for contractionism.

4. Contractionism is incompatible with the presence of radioactive materials in the earth's crust. Physicists discovered that radioactive material was widely distributed in the earth's crust, and that they produce heat when decaying. In 1909 the Irish physicist John Joly argued that for this reason it is very problematic to maintain that the earth cools down through loss of internal heat. Rather, the most plausible state is that the temperature of the earth remains constant or increases a little bit. This is incompatible with the extreme cooling down that contractionists have to assume (e.g. 1200°C to explain the formation of the Alps, much more for higher mountains). This is another external conceptual problem for contractionists.

5. As mentioned above, Wegener invoked two forces (pole-flight and tidal force) that propelled the continents. The problem is that these forces are too weak:

   > The earth did behave like a fluid in some respects, but no one was proposing that the ocean floors were in fact liquid: they were composed of dense, basaltic rocks. How could the continents move laterally through such floors without crumbling to bits? What enormous force not only moved the continents but had crumpled them up to form the Alps, Rockies, Andes and Himalayas? The forces which Wegener invoked did exist but they were far too weak. A force nearly 1 000 000 times stronger was needed, and if it did exist it would surely have been noticed by physicists. (Le Grand, 1988, p. 55-56)

   In Laudan's terminology, this is an external conceptual problem. The problem is of a less strong type than the conceptual problems facing contractionism: physics makes Drift implausible, but there is no logical incompatibility.

6. The complementary shapes of coast lines (e.g. of Africa and South America) can be explained by Wegener if he assumes arbitrary changes of shape. His rivals judged that he could not explain the jig-saw fit, and that this was a problem for all three theories. In Laudan's terminology, this is an unsolved problem for all three theories.

7. Wegener tried to develop geodetic evidence. He participated in several expeditions in Greenland (and died during one of them in 1930). He tried to measure whether Greenland was moving. His results were well within the margin of error of his apparatus and method (it was not sure that the measurements were made at exactly the same spot). In Laudan's terminology this is a failed attempt of Wegener to create an extra anomaly for his rivals.

2.3. Seafloor Spreading

Given these arguments for and against the three theories, it is not surprising that none of them became dominant. Each of them was confronted with a number of problems. The situation changed in the 1960s, after findings in paleomagnetic studies and investigations of the seafloor gave additional evidence for Drift. Moreover, Harry Hess' idea of seafloor spreading offered a mechanism of drift that both solved the problem of mechanism and was supported by a sufficient amount of evidence:

> The nub of his theory was that new seafloor was generated at ridges by the upwelling of mantle material. The old seafloor gradually moved from the ridges and was eventually dragged down at the trenches and reconverted into mantle. The cycle was driven by convection currents rising under ridges and descending under trenches. . . .
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> The continents were passive passengers seated on dynamic ocean floors. Hess's model provided a solution to the conceptual problem with which Drifters have wrestled for fifty years: how could the continents drift through the ocean floor? Hess's answer was that they moved with the crust, not through it. (Le Grand, 1988, p. 197)

In order to see why the conceptual problem disappears, it is important to notice that the new theory requires different forces (at other places, and less strong).[4] After Hess introduced this idea, Drift became the dominant theory very rapidly. Adherents of the other theories changed side.[5]

[4]As we shall see, Arthur Holmes proposed a similar mechanism of the drift already few decades earlier. For a detailed discussion of Hess' proposal and its development see Frankel (1980).

[5]Some geologists accepted Drift only after the evidence from some of its novel predictions was confirmed (see (Laudan & Laudan, 1989, p. 221)).

3. What Does it Mean to be Worthy of Pursuit?

As we have stated in the Introduction, the aim of this paper is to evaluate whether Drift was epistemically worthy of pursuit in the first half of the twentieth century. We will say that a cognitive system is (epistemically) worthy of pursuit in case it has a promising potential for growing into an epistemically justified theory.[6] Therefore, our guiding question will not be whether Drift at this point of its development was epistemically justified in the sense of being an acceptable theory, but if it was a promising candidate for later inclusion into the scientific knowledge base of accepted theories. We are thus interested in the question as to whether it was rational, in the sense of being epistemically justified, to spend resources such as time, money and intelligence for its further investigation.

There are two aspects of the evaluation in the context of pursuit which are worth mentioning at this point. First, such an assessment needs to be understood as necessarily involving some level of uncertainty. We can never say with full certainty that a theory will eventually develop into an acceptable one. Nevertheless, that does not mean that there are no good reasons for pursuing it in spite of the unavoidable risk. What we are after is an evaluation on the basis of which it should be possible to say whether taking a risk of pursuing Drift in the first half of the twentieth century was epistemically justified, or not.

Second, it is important to notice that concluding that a theory is worthy of pursuit does not mean that the entire scientific community is supposed to actually pursue it.[7] It may be rational for the given scientific community to ascribe the pursuit of a new, developing theory only to a small group of scientists, while the rest of the scientists are to investigate other theoretical rivals (for example, a much more developed, dominant theory in the field). The division of labor in a given scientific discipline will depend on the epistemic status of all available theoretical candidates, as well as on some non-epistemic factors, such as the number of scientists working in the field, the financial resources, etc. The evaluation of Drift which will be presented in this paper is not meant to give a proposal for such a division of labor, nor to answer the question as to whether any individual geologist should have engaged in the actual pursuit of Drift. Rather, our aim is to answer the question, whether Drift was worthy of pursuit for the geological community at the time, or in other words, whether

[6] Epistemic justification is usually considered as providing a set of standards for the acceptability of certain beliefs in the knowledge base or cognitive system of an intelligent agent. These standards or criteria are considered to be conductive of either the cognitive goal of truth (e.g. see Bonjour (1985), p. 9) or similar cognitive goals or virtues. In the context of scientific cognitive systems epistemic justification can be conceived of as providing the criteria for the inclusion and acceptance of scientific theories, hypotheses, etc. in the grand corpus of our scientific knowledge base. It concerns the question as to whether we have good reasons to consider a certain theory as being reliable, (approximately) truthful, empirically adequate, etc.

[7] To pursue a theory means to engage in its further investigation, aiming at its development or a development of its variants that fit in the same research tradition.

it was rational for the geological community at the time to at least partially engage in the research of Drift.

The significance of our research question is two-fold. On the one hand, an epistemic evaluation in the context of pursuit will allow for a clarification of certain aspects of rationality of the geological community at the time. For example, answering this question will help us to understand not only if pursuit conducted by drifters was rational, but also, whether rejecting Drift as unworthy of pursuit was irrational.

On the other hand, by distinguishing theory evaluation in the context of acceptance from the one in the context of pursuit, we can clarify certain confusions in the debates among geologists in the 1920s. Since rejecting a theory in the context of acceptance and accepting it in the context of pursuit are two compatible stances, distinguishing these two types of evaluation is important not only for philosophers of science, but also for scientists who may sometimes engage in disputes on compatible ideas.

Before presenting our evaluation of Drift, let us take a look at other similar discussions in the literature on this episode in the history of geology and see whether our question has so far been addressed.

4. Others on Drift in the Context of Pursuit

In this section we will discuss the work of other scholars that have analyzed the development of Drift in the first half of the twentieth century. Our aim is to show that the question as to whether Drift was worthy of pursuit has been either neglected or inadequately addressed.

On the one hand, most of the authors who analyzed this revolution focused primarily on the context of acceptance. For example, Paul Thagard (in his Thagard (1992)) offered an analysis of this revolution in terms of his account of explanatory coherence. His primary aim was to answer the question: *"Why was Wegener's theory of continental drift largely rejected in the 1920s, and why, in contrast, were the new ideas about seafloor spreading and plate tectonics largely accepted in the 1960s?"* (p. 171, italics in original[8]). Both research questions address only the context of acceptance. Similarly, Ronald Giere in his Giere (1988) discussed why there was no revolution in the 1920s in contrast to the 1960s (p. 227-277).

On the other hand, those authors who did discuss the rationality in the context of pursuit, did not take into consideration what we are primarily interested in. They were concerned with the question as to why some geologists pursued Drift and why some others did not pursue it, but not whether pursuing Drift in general was rational or not.[9] For example, Miriam Solomon in her

[8]Henceforth, italics appearing in a quoted text are present in the original as well, unless otherwise indicated.

[9]Most of these approaches make use of Laudan's criterion for when pursuing a theory is rational. We will take a closer look at Laudan's criterion in Section 5.

Solomon (2001) explicates which epistemic and non-epistemic factors influenced a dissent among geologists in the first half of the twentieth century, and which factors later on led to the consensus over the theory of plate tectonics. Her approach aims at explicating why some geologists decided to pursue Drift, and why some others decided not to purse it. But this does not answer the question: Was Drift around the 1920s-1930s epistemically worthy of pursuit or not?

Another example is Henry Frankel who agued in his Frankel (1979) that if Laudan's methodological framework is applicable to the development and reception of Drift, then this tradition "should not be accepted by the relevant scientific community if it does not have greater problem-solving effectiveness than competing traditions, and *should be pursued only by those scientists who believe it has promise of its future ability to solve problems*" (p. 75; italics added). In addition, he suggested that for certain geologists (such as Arthur Holmes and Alexander du Toit) the theory of drift was sufficiently promising since it could solve a number of empirical problems (in Laudan's sense of the term) without necessarily creating more conceptual or empirical problems. Without going into discussion on the notion of rationality underlying such a "subjective" assessment of pursuit worthiness, let us just notice that Frankel's approach to the rationality in the context of pursuit does not answer the question we are interested in. What we wish to investigate is not whether Drift was worthy of pursuit *for some*, but whether it was worthy of pursuit in general, for the geological community at the time.

In a similar vein, Homer Le Grand (Le Grand, 1988, p. 95) as well as Rachel Laudan (Laudan, 1987, p. 205-213) both explicate why some geologists engaged in the actual pursuit of Drift, and why other geologists did not pursue it. As mentioned above, such an approach does not answer our research question.

Let us then present the account of epistemic justification in the context of pursuit, which we shall use to evaluate Drift.

5. The Notion of Pursuit Worthiness and the Main Claims of the Paper

According to Larry Laudan, "*it is always rational to pursue any research tradition which has a higher rate of progress than its rivals (even if the former has a lower problem-solving effectiveness)*" (Laudan, 1977, p. 111). Even though Laudan's criterion may represent a sufficient condition for a theory to be worthy of pursuit, there are two problems with it. First, as a sufficient but not a necessary condition, this criterion can be applied only to research traditions that exhibit a higher rate of progress than their rivals (and only when they do so), but it does not tell us anything about the pursuit worthiness of theories that do not satisfy this standard. Since only one tradition at the time can have a higher rate of progress than its rivals, Laudan's criterion can evaluate pursuit worthiness of only one tradition in cases in which different rivaling traditions are simultaneously worthy of pursuit. Indeed, if a tradition is worthy of pursuit, that does not imply rejecting its rivals as unworthy of pursuit. There are situations in which it may be rational for a given scientific community to pursue two or more research traditions at the same time. One of these traditions may exhibit a higher rate of progress than its rivals at one point, but soon it may turn out to be the other way around. The revolution in geology is the case in point, since it could very well be the case that all three research traditions were worthy of pursuit at the same time. Only if it turns out that Drift exhibited a higher rate of progress than the other two theories (and only for the time period when it did so), Laudan's criterion would affirm its pursuit worthiness. But this brings us to the second problem with Laudan's approach: his criterion is too vague, since it does not explicitly state how to asses the relative rate of progress.

In Šešelja & Straßer (201x) a detailed account of what it means that an emerging scientific theory (representing a new research tradition) is epistemically worthy of pursuit has been offered. The criteria of theory evaluation are presented there in terms of a coherence theory of epistemic justification, where a theory is epistemically worthy of pursuit if it has a sufficiently high *potential coherence*. The notion of potential coherence is built on the basis of one of the most influential coherentist accounts – Laurence Bonjour's coherence theory of epistemic justification (see Bonjour (1985), Bonjour (1989)). By satisfying a certain set of criteria, theories can have a higher or lower potential coherence and thus be more or less worthy of pursuit.

In this paper though we will show that Drift was *worthy of pursuit in a strong sense* (henceforth: WPSS),[10] since it fully satisfied all the criteria of pursuit worthiness. More precisely, the main claims of our paper are, first, *that Drift was initially WPSS*, and second, *that it remained WPSS* throughout the first half of the twentieth century. We make a distinction between these two claims since the fact that a research tradition is initially promising does not imply that it will remain promising throughout its development. We say that a research tradition is initially WPSS if it satisfies the following criteria:

1. Presence of significant explanations
2. Inferential Density
3. Programmatic character

We say that the research tradition remains WPSS if in addition to these criteria it also satisfies:

4. Theoretical Growth and the growth of the programmatic character.

We will explicate each of these criteria in sections to follow, where we will apply them to the case of Drift. In contrast to the above mentioned account of potential coherence, which allows for a gradual evaluation of pursuit worthiness of theories (or research traditions), the criteria of WPSS will be presented in a discrete manner and (to make things simpler) without introducing the concept of coherence. In other words, the criteria of WPSS are formulated in such a way that if a theory or a research tradition satisfies them, we can say that it is (or in case of the past theories: was) certainly WPSS.

[10]We will use the same shortcut also for "pursuit worthiness in a strong sense".

It is important to notice that satisfying the criteria of WPSS is a sufficient condition for a research tradition to be worthy of pursuit, even though it may not be a necessary one.[11] There are various examples of scientific theories that were worthy of pursuit even though they did not fully satisfy all of these criteria.[12] In such cases, a theory has to score very high in some respects in order to compensate for a low score in others. However, by showing that Drift was WPSS we can avoid a discussion involving such a weighting of its properties. Moreover, showing that Drift was WPSS makes it easier to argue that the view according to which Drift was not at all worthy of pursuit can be considered epistemically unjustified.

When evaluating pursuit worthiness of research traditions, we are in fact evaluating the pursuit worthiness of its constituting theories. Therefore, examining the initial WPSS of Drift will refer to Wegener's model. However, it is important to mention that even though Wegener's theory appeared around 1915, most North American geologists became familiar with his work only in 1924, when English translation of Wegener's book was published (Marvin, 2001, p. 21). This complicates rooting the initial WPSS of Drift in a specific year, and somewhat blurs the demarcation line between the initial WPSS and the property of remaining WPSS. Our approach will be the following: we will answer the first issue in view of the discussion that occurred in between 1912 and the mid-1920s, including Wegener's model of Drift as well as the first criticisms of it. The latter issue will be discussed in view of the arguments and alternative models of Drift that were offered by Wegener's followers (even though some of them were proposed already in the early 1920s).

The historical presentation will be mainly based on Oreskes (1999), which is one of the most recent studies of this episode, as well as on Le Grand (1988).

6. Presence of Significant Explanations

Our first condition for pursuit worthiness in a strong sense is formulated as follows:

A theory has to offer explanations that are significant at that point of the scientific development.

By *significant explanations* in the context of pursuit we mean those that address the phenomena for which the dominant rival[13] has either no explanation,

[11] That means that if a theory satisfies our criteria then it is (or was) WPSS; however, if it does not satisfy them, we cannot make any claims about its WPSS.

[12] This is especially the case with theories that are initially internally inconsistent. As we shall see, our account of WPSS requires that all the major anomalies, including inconsistencies, are addressed by the programmatic character of the given theory (see Section 8). Nevertheless, a theory with a strong explanatory power, whose programmatic character might not address some inconsistencies in it, could still be worthy of pursuit (see (Lakatos, 1978, p. 55)).

[13] Even though our criteria evaluate a pursued theory in view of its dominant rival, in case there is no dominant theory in the given scientific field, the pursued theory is to be evaluated against the background of other pursued theories that are its rivals. This is precisely the case with Drift, which had two main rivals, neither of which was fully accepted at the time among

or has an explanation that is very weak (a weak explanation would be, for example, an explanation that introduces new conceptual problems).

As we have mentioned in Section 2, when Wegener first proposed his theory in 1912 its main explanatory rival – Suess's theory of contraction – had been under a serious attack (also see (Oreskes, 1999, p. 21-55)). If the principle of isostasy was correct (and by the time of Wegener's proposal there was a growing conviction that it was), continents and ocean floors had to be different either in structure or in composition. This conflicted with Suessian idea of interchangeability of oceans and continents, and thus with the hypothesis of land bridges, used for explaining the similarities between the coastal regions on the opposite sides of oceans. Even though some geologists found a solution for the conflict between contractionism and isostasy in the permanentist perspective, permanentism had even more trouble explaining such similarities. Therefore, both rivals of Drift – contractionism and permanentism – can in this case be regarded as offering either a weak explanation for the similarities between the continents or no explanation at all. Two particularly significant types of similarities that Wegener's theory could account for were the following.

First, the paleontological similarities. The evidence for them was at this point already well established by paleontologists independently of Wegener's hypothesis (Oreskes, 1999, p. 56). Not only was there an overall resemblance of fossil forms indicating that continents must have been somehow connected in the past, but there was also an evidence of a distribution of certain organisms, such as earthworms, which aren't capable of swimming or flying or having resilient seeds or a dormant life cycle or free-floating larval stage, which could allow for their passive distribution (Ibid., p. 57). A similar case are certain species of snails that were, just like the earthworms, unlikely to have crossed all the way across the land bridges (Le Grand, 1988, p. 43).

Second, there was an evidence of nearly identical stratigraphic sequences and structural patterns on the coastlines of the matching continents. Moreover, Caledonian fold belts in North America matched with the Appalachian ones in Europe, while the Gondwana beds in India were nearly identical to the Karroo sequence of southern Africa (Oreskes, 1999, p. 57).

Another significant explanation that Drift offered regarded a paleoclimatic evidence. By the twentieth century there was a consensus that the earth's climate had undergone repeated fluctuations. For example, glacial deposits in South Africa and Australia indicated that the climate had been much colder in Permian. However, the cause of these climatic fluctuations, which came to be known as the problem of Permo-Carboniferous glaciation, was unknown (Ibid., p. 58). The main problem for permanentist and contractionist accounts was that different climatic conditions occurred at about the same time at different

the geologists, but each of which was pursued by some of them. Thus, an explanation offered by Drift was significant if both of its two rivals could offer only a weak explanation or no explanation of the given geological phenomenon. For the sake of simplicity we will formulate all our criteria by mentioning one dominant rival theory, though the reader can easily see how each of them is to be adapted to the case of multiple pursued rivals.

parts of the globe, which prohibited explanations stating that the earth as a whole was once hotter or cooler than now (Le Grand, 1988, p. 44). In contrast, Wegener offered an explanation in terms of a shift of geophysical poles (so-called polar wandering), as well a shift of continents relative to the poles.

Without going further into details of the explananda which the Drift was able to account for, it is important to notice that the presence of significant explanations does not mean that Drift had no explanatory anomalies. For example, the evidence of Carboniferous glaciation was also found near Boston, which, according to Wegener's account, must have been in the tropical climate at the time (Le Grand, 1988, p. 56). Nevertheless, such explanatory anomalies are a usual component of young scientific theories, and should, thus, not be the reason for rejecting their pursuit. One of the main tasks of further developments of the theory is exactly to remove such problems. And that a research towards fulfilling this task can proceed is guaranteed by the programmatic character of the theory, which is our next criterion of evaluation.

We can thus conclude that Wegener's theory had significant explanations in the above defined sense of the term.

7. Inferential Density

Our next standard consists of the following two sub-criteria:

1. *Internal inferential density: A theory should not have a less unified core of hypotheses than its dominant rival.*
2. *External inferential density: A theory should be at least as inferentially connected with established theories from other scientific domains as its dominant rival or it should be able to address the lack of such connections by means of its programmatic character.*

When it comes to internal inferential density, it is easy to see that none of the three rivaling theories was especially unified. Drift could not provide a precise mechanism of drifting, but it was able to account for many geological phenomena with the same hypothesis (namely, the hypothesis of the continental drift). In contrast, both contractionism and permanentism had to introduce an additional hypothesis, such as the idea of land bridges and isthmian links which in the past connected the continents, in order to account for the vast evidence of similarities between the coastal regions on the opposite sides of oceans (or to leave the phenomena unexplained). Thus, Wegener's theory at least did not have a less unified core of hypotheses than its rivals.

As for the external inferential density, we will show in Section 8 that Drift successfully addressed potential problems with physics and seismology. Moreover, we will show that it had inferential links with the theory of isostasy, in contrast to contractionism that was not well connected with it. In view of these insights it will be clear that Wegener's theory satisfied the criterion of external inferential density as well.

8. Programmatic Character

This criterion of pursuit worthiness is defined as follows:

A theory has to have a programmatic character that addresses all the major problems of the theory (such as explanatory anomalies, inconsistencies, etc.).

A theory has a programmatic character if it is embeded in a theoretical and methodological framework that allows for a further development of the theory to proceed in spite of the encountered problems, and towards their systematic resolution. For example, an inconsistency is addressed by the programmatic character if the scientists can show that they have certain methodological means which can help in resolving the given inconsistency in the further development of the theory.[14] Another example would be the above mentioned evidence of glaciation in the North America. With respect to this explanatory anomaly Wegener pointed out that the vast majority of other evidence indicated that the area was in a tropical climate which made the glacial origin of these deposits suspicious. He thus suggested that the deposits could have originated in some other way (Brooks, 1949, p. 232). For instance, the Appalachian orogenic belt, in which some of the deposits were found, could have been a high mountain range at the time (see also (Holmes, 1944, p. 502). This hypothesis also indicated in which direction could the further examination of this problem proceed.

In this section we will focus on two major problems Drift was confronted with: first, the problem of the mechanism of Drift, and second, the conflict between Drift and seismology. We will show that with respect to both of them Wegener's theory had a programmatic character.[15]

8.1. The Mechanism of Drift

As we have already mentioned, the main problem of Drift was the question of the mechanism governing the continental drifting. It has become common in

[14]In the case of pursuit worthiness in a strong sense we have conjoined the criterion of programmatic character and the criterion of potential consistency which are in the original account of potential coherence two separate standards. When speaking here of inconsistencies, we mean those within the theory or those between the theory and other already established or very promising theories from other scientific domains, with which the dominant rival (or another pursued rival) is consistent. The reader will notice that our evaluation does not take into account inconsistencies between the given theory and its theoretical rivals, even though they are usually a reason for a suspicion towards the former one. However, when evaluating the pursuit worthiness of a theory, we need to leave open the possibility that this could be an emerging research tradition that could replace its dominant rival, in which case such inconsistencies would simply become obsolete. Hence, the fact that a theory is inconsistent with its dominant rival is no reason for decreasing its WPSS.

[15]We have chosen these two problems as the major attacks on Drift on the basis of Oreskes' study. According to Henry Frankel though, the problem of explaining the Southern Glaciation was another heated topic in debates over the continental drift Frankel (1987). Frankel presents a number of objections raised against Wegener's explanation of Permo-Carboniferous Ice Cap, but also replies by drifters, where particularly important were the arguments given by Alexander du Toit, Arthur Holmes and George C. Simpson (Ibid., p. 212-216). These replies were the basis of the programmatic character of Drift with respect to this issue, though for the details of this discussion, we are referring the interested reader to Frankel's article.

the literature on Drift to argue that in the first half of the twentieth century it was not only unclear which forces could be responsible for continental drifting, but that any conceivable mechanism of the drift seemed to conflict with the physical theory (e.g. (Le Grand, 1988, p. 129), or (Laudan, 1981, p. 230); also see Section 2.2 in this paper).

In contrast to such a view, Oreskes argues that Wegener's proposal was well rooted in the theory of isostasy. She starts off by pointing out that for Wegener the principle of isostasy was "nothing more than hydrostatic equilibrium according to Archimedes' principle, whereby the weight of the immersed body is equal to that of the fluid displaced." (quoted from (Oreskes, 1999, p. 65)). Thus, continents could be seen as floating in hydrostatic equilibrium, which means that the substrate in which they are embedded has to behave, *over geological time*, in a fluid manner.

But such an idea of a mobile substrate was not a novelty of Wegener's theory. The basic idea of isostasy refers to a condition to which the crust and the mantle tend, in the absence of disturbing forces. The first conceptions of isostasy from the second half of the nineteenth century conceived of crust as floating on the denser underlying mantle (Watts, 2001, p. 1). Only by introducing the idea of a fluid or plastic substrate could they account for the oscillations of the earth's crust. Airy's model of isostasy, which was well accepted throughout Europe, hypothesized that a thin layer of crust overlays a fluid layer of greater density just like timber blocks float on water (Ibid., p. 12, 21). English geologist Reverend O. Fisher in his book *Physics of the Earth's Crust* from 1881 suggested that the crust is analogous to the broken-up area of ice, floating upon water, obeying Archimedes' principle (Ibid., p. 15). Similarly, North American geologist Clarence E. Dutton spoke of the flotation of the crust upon a liquid or highly plastic substratum ((Ibid., p. 16-17); (Oreskes, 1999, p. 67)). By the time Wegener's work was translated into English in 1924, there was a rough consensus among European and North American geologists that there was a mobile layer beneath the earth's crust (Oreskes, 1999, p. 66-80). Moreover:

> The idea of moving continents was perhaps not as great a conceptual leap as might otherwise appear. Chamberlin, Dutton, Hayford, and others had written explicitly of 'lateral creep' and continental 'spreading'; the unknown issue was the *scale* of these effects, and whether they operated in a cyclical manner, as Chamberlin seemed to suggest, or whether they could actually produce a net lateral motion, as Hayford might be interpreted to imply. (Ibid., p. 72-73)

Oreskes adds that the prominence of the advocates of isostasy makes it unlikely that other geologists did not know about their research. And even if they were ignorant of isostasy, the idea of a mobile substrate was inherent in the theory of geosynclines which was well known among the North American geologists (Ibid., p. 73-74). Finally, the Fennoscandian uplift represented a phenomenological evidence for the mobile substrate. The uplift was a result of the removal of glacial ice, and according to Fisher, a direct consequence of isostasy. For

Wegener, the Fennoscandian uplift demonstrated that the substrate had to be sufficiently mobile to flow out of the way of the depressing continent, and upon the removal of the glacial load to flow back under the continent (Ibid., p. 76).

Thus, the main novelty of Wegener's theory was not the possibility of the horizontal movements, but their scale and extent. But what about the rigid ocean floor through which, according to his model, continents had to plow? Oreskes writes:

> Wegener's argument hinged on the belief that the ocean floor was more like the crustal substrate than the continental blocks – or, to use the terminology of the day, it was simatic (rich in silicon and magnesium) rather than sialic (rich in silicon and aluminium). This was not a particularly controversial view: it had long been suggested by evidence from ocean dredging and the basaltic composition of most ocean islands. ... And if ocean floor *was* primarily composed of basalt, then ... the continents had deep roots *and* the ocean basins were composed of denser material than the continents. If so, then the continents plowed mostly through plastic substrate and needed only to dislodge a thin veneer of crust at the top. (Ibid., p. 77)

Using the results of gravity work obtained by others, Wegener estimated that the oceanic crust was no more than 5 km thick in contrast to continental blocks, the average thickness of which was taken to be around 100 km. Consequently, in Wegener's model continents moved mainly through the fluid substrate, and had only a thin semi-rigid oceanic crust in their way. In reply to the objection that such a plow would result in deformations of the ocean floor, Wegener called upon isostasy which precluded the formation of significant elevations in the seefloor (Ibid., p. 78-79).

Thus, as Oreskes explicates, even though Wegener's model may seem completely wrong from the nowadays geological perspective, in the 1920's it was consistent with the available understanding of terrestrial kinematic properties. And even though the model was based on hypotheses which could not be proved, it showed that the problem of the mechanism could in principle be resolved. Moreover, it pointed to the problem-field that required further investigation: isostasy and the nature of the substrate, which seemed to be directly related to the question of the mechanism of drifting. Thus, it gave a programmatic character to Drift with respect to this issue.

As for the origin of forces governing the drift, Wegener hypothesized two possible causes: pole-flight force and tidal retardation. Even though they turned out to be too weak to account for drift, at the time when Wegener proposed them, they contributed to the programmatic character of Drift, by indicating in which direction a further investigation of this question could go (namely, examining if these forces are strong enough to move the continents).

8.2. The Conflict with Seismology

The second major objection to Drift concerns the inconsistency (and hence also the lack of inferential connections) between Drift and seismology.

Harold Jeffreys was a strong opponent of Drift coming from the contractionist side. One of his main arguments against the drifters was that their theory was inconsistent with seismology (in contrast to contractionism which was compatible with it). According to him, the propagation of seismic waves at depth of the earth's interior implied a solid and rigid earth. This conflicted with the idea of a fluid substrate upheld by the drifters (Oreskes, 1999, p. 83).[16]

However, Jeffreys' arguments were rebutted, on the one hand, by the theory of isostasy which required a fluid substrate, and on the other hand, by the drifters themselves. As for Wegener's reply to this objection, he argued that earth materials may behave in a rigid manner in response to short-duration disturbances, such as seismic waves. But the same materials may exhibit plasticity in response to a small, steady and slow pressure over geological times (Ibid., p. 79). Such a reply showed that further examination of the substrate was needed to confirm the relation between Drift and seismology, and that the conflict between the two was not at all inevitable.

We can thus conclude that Wegener's theory had a programmatic character, which at least temporarily addressed the major problems it was confronted with.

So far we have shown that Drift satisfied all the required standards for being initially WPSS. In the following section we will examine whether it also remained WPSS.

9. Theoretical growth and the Growth of the Programmatic Character

The criterion of theoretical growth is formulated in the following way:

A theory has to exhibit a theoretical growth that occurs as a development in the previously mentioned standards of WPSS, that is:

1. by increasing the number of significant explanations or by improving already existing explanations. The number of significant explanations is increased by introducing a new evidence for the theory, that is, by explaining the phenomena for which the dominant rival has either only a weak explanation of no explanation at all;

2. by increasing the internal or the external inferential density;

[16]It is interesting to notice that even though Jeffreys' arguments are often considered to have played a significant role in the rejection of Wegener's hypothesis, Oreskes suggests that they "proved quite insufficient to move most geologists" (Ibid., p. 89). She mentions both British and North American geologists who were either not inclined to fully accept Jeffereys' views or who did not have too high opinion of him as a scientist. Also see (Oreskes, 2001, p. 218).

3. by improving the programmatic character, so that all the major problems remain to be addressed by it.

In this section we will show that Drift exhibited a theoretical growth throughout the 1920s and the 1930s.

9.1. Growth that Drift Exhibited in the 1920s
Increase in the number and quality of significant explanations. Up to the 1920s the main geological evidence, first for Suess' hypothesis of Gondwana and then for Wegener's Drift were the similarities between Karroo formations in South Africa and age-equivallent rocks elsewhere in the world (Oreskes, 1999, p. 157). However, a direct comparative study of the so-called Gondwana beds was missing, and both Suess and Wegener built their ideas by combining results obtained by other geologists, rather than themselves conducting a field investigation. North American geologist Frederick Wright realized that the similarities between these regions could thus be taken as a prediction of Drift. He proposed that the examination of the evidence be conducted by an expert in this field, namely Alexander Logie du Toit, a leading specialist in the geology of South Africa who thus had a sufficient expertise for comparing South African coast with the South American one (Ibid., p. 158). The proposal was accepted and in 1923 du Toit embarked on a journey to South America, sponsored by the Carnegie Institution of Washington, to study the geology of the eastern coast of the continent. The results of his study were, according to du Toit himself, strikingly in favor of the Drift hypothesis. Litho-stratigraphic characteristics of the South American east coast were so similar to those of the South African coast that du Toit concluded the two continents must have been at one time no more than 400-800 kilometers apart (Oreskes, 1999, p. 161). For example, the facies patterns on both sides of the Atlantic exhibited less change when compared to each other than to much closer facies within their respective continents (Ibid., p. 166). According to du Toit, this evidence required direct physical proximity of the continents, and could thus be explained only by Drift. The study thus greatly contributed to the increase of significant explanations offered by Drift. The results were presented in Du Toit's monograph *A Geological Comparison of South America with South Africa* published in Washington, D.C. in 1927.

In addition to du Toit's work, Drift showed a growth in the work of some other geologists as well. Swiss geologist Emile Argand improved Wegener's solutions of orogenic problems by offering a more detailed account of the formation of mountain ranges and island festoons ((Frankel, 1981, p. 202); (Oreskes, 1999, p. 115)). Arthur Wade, a geologist who first lived in England and then in Australia, approached Drift from the perspective of his oil exploration work. He found Drift to be fruitful for accounting for the structure and history of New Guinea, whose crustal deformation he attributed to its mashing against Australia. Calling upon Drift even had practical consequences in this case, since it could help in locating sites for future oilfields (Le Grand, 1988, p. 86).[17] The

[17] Wade made further contributions in the 1930's after emigrating to Australia, where he

explanatory power of Drift was also improved by the work of some Australian biologists. For example, zoologist Launcelot Harrison considered the problems of Australian bio-geography and suggested that if the land connections between the southern continents had to be rejected on geophysical grounds, Drift was the only remaining hypothesis offering an explanation of species distribution in the South (Ibid., p. 87).

Improved Programmatic Character – The Mechanism of Drift. As we have seen, Wegener addressed the problem of the mechanism of the drift by calling upon the theory of isostasy, and in addition, by hypothesizing two forces that could be responsible for the continental movements. Nevertheless, neither of these forces turned out to be sufficiently strong to account for the drift. Wegener eventually had to distance himself from this question and to admit that "The Newton of drift theory has not yet appeared." (Wegener, 1966, p. 167). The opponents argued that finding such a force was improbable, which introduced an external probabilistic inconsistency between Drift and the physical theory. Nevertheless, throughout the 1920s Wegener's followers (Reginald Daly, John Joly and Arthur Holmes) offered possible solutions for this problem. We will take a closer look at the most important of these accounts – Arthur Holmes' model of Drift.

Among all the models of Drift from the first half of the twentieth century, the account offered by British geologist Arthur Holmes in the late 1920s most successfully addressed both the question as to whether Drift was conceivable in light of the physical theory, as well as the question as to which forces could be responsible for such a movement of continents (see (Frankel, 1978, p. 131)). Coming from the field of radiology, Holmes argued that due to the thermal processes resulting from the radioactive materials in the inside of the earth, there was an accumulation and a discharge of the heat. Thus, on the one hand, he disqualified the basic idea of the contractionist tradition – the hypothesis of the cooling earth. On the other hand, he proposed the idea of the convection currents in the substratum, resulting from the differential heating by radioactivity. His model has been sometimes labeled as "seafloor thinning": it supposes that the continents drift apart by being carried along the backs of the convection currents, which arise beneath continents, diverge and move towards the continental edge; as a result, the currents produce a "stretched region" of crustal material, which eventually becomes a new ocean floor (Ibid., p. 131-143). In addition to being compatible with the results of the research in radiology, his hypothesis had a strong explanatory power: it could account not only for the features of the continental drift, but also for the phenomena such as mountain building, oceanic deeps, geocynclines, rift valleys, the distribution of earthquakes and volcanos, etc. (Holmes, 1931, p. 600).[18] Holmes thus managed to address both, the

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worked on the geology of Western Australia. His research showed that there was a fitting of the southern continents to Antarctica as well as the matching-up of some of their geological features (Ibid., p. 86).

[18]Note that these features of Holmes' model (its compatibility with radiology and a strong

alleged conflict between Drift and physics (by showing that the continental drift is possible even without assuming that continents plow their way through the seafloor), as well as the question of the forces governing the drift (by proposing the forces of the drift, namely, the convection currents in the earth's mantle).

However, Holmes was careful enough to see his hypothesis as a "preliminary survey":

> So far the treatment has been almost entirely qualitative and therefore it inevitably stands in need of criticism and quantitative revision. The hydrodynamics of the substratum and its behaviour as a heat engine need to be attacked on sound physical lines. The capacity of substratum currents to promote magmatic corrosion, transport and crystallisation, and to produce migrating sub-crustal wave forms, calls for detail treatment. The full bearings of the hypothesis on petrogenesis have yet to be investigated. Meanwhile its general geological success seems to justify its tentative adoption as a working hypothesis of unusual promise. (Ibid., p. 600)

We see here a number of tasks that Holmes points out as relevant for further examination of his hypothesis. Hence, the idea of the convective currents in the substratum can be taken as a prediction of his model of Drift, which could be investigated in different ways in future field work.

To sum up, Holmes' model obviously gave a programmatic character to the theory with respect to the problem of the mechanism of Drift. Even Harold Jeffreys, one of the biggest opponents of Drift, had to admit that Holmes' proposal rendered the idea of Drift "physically possible" (though he still found it to be very implausible) (Frankel, 1978, p. 147).

Improved Programmatic Character – Seismology. In addition to Wegener's replies to Jeffrey's objections (see Section 8.2), other proponents of Drift also discussed this issue. Already in the early 1920s Reginald Daly argued that this objection to Drift conflated rigidity with solidity (Oreskes, 1999, p. 93). The results of seismic studies supported the idea that the substrate was rigid, but this did not imply that it was solid (similar to the properties of glass, which is solid at room temperature, but which under pressure and over time actually flows). Daly further explicated that when it comes to properties of the substrate which are relevant for Drift, what mattered was not the distinction between liquidity and solidity, but the one between crystalline and non-crystalline materials. And if the substrate were non-crystalline (like glass), it could appear as rigid in response to seismic waves, but plastic in response to long-term effects (Ibid., p. 93-94). John Joly had a different response to this objection: according to his model, the substrate beneath the continents was periodically and locally (rather than continuously and uniformly) molten (Ibid., p. 108).

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explanatory power) are not directly relevant for the programmatic character of Drift, though they will turn out to be important for the criterion of external inferential density.

Therefore, the proponents of Drift offered further possible solutions for the conflict between their theory and seismology, the validity of which depended on further investigations of the properties of the substrate.

Increase in the internal and external inferential density. As we have seen, Holmes' model of Drift provided a more unified core of hypotheses than Wegener's one in view of the growing criticism of the latter for the lack of plausible forces of drifting.[19] Furthermore, through Holmes' model (and previously through Joly's model as well) Drift obtained inferential connections with the theory of radioactivity, which became problematic for both of its rivals. The incompatibility of radioactivity with either contractionism or permanentism was the primary reason for Holmes to become a proponent of Drift, since it was the only theory that could account for the accumulation and discharge of heat, necessitated by the presence of radioactive materials.

Drift showed an additional growth by improving its inferential connections with the theory of isostasy. In the second half of the 1920s William Bowie, a proponent of permanentism, organized an international collaboration aimed at investigating isostasy, but indirectly relevant for the hypothesis of drift as well (Oreskes, 1999, p. 236-261). The aim of the investigation was to test the theory of isostasy by obtaining gravity data from the ocean floor. Up to that point isostasy had been confirmed only on the continents due to the fact that there were no precise instruments for measuring gravity at sea. In the meantime, Dutch geologist Felix Vening Meinesz developed an improved gravimeter, suitable for the sea measurements as well. The results of the investigation conflicted with the assumptions of Pratt's model of isostasy, used in the permanentist conception of geology. Indirectly, it indicated that Airy's model of isostasy, compatible with Drift, might be correct after all. Thus, the inferential connections between Drift and isostasy were in this way improved.

9.2. Growth that Drift exhibited in the 1930s

Increase in the external inferential density. Expeditions organized in the 1930's corroborated the above mentioned results of the research on isostasy, which were in accordance with Airy's model, and thus with Drift as well (Oreskes, 1999, p. 245). By the mid-1930s seismic evidence refuted Pratt's model of isostasy (Ibid., p. 258). In view of these results, American geologist Richard M. Field pointed out in 1937 that Wegener's hypothesis played a great role in stimulating geological and geophysical investigations (Ibid., p. 259). Moreover, Bowie himself acknowledged in 1936 the link between these results and Wegener's ideas:

> The Wegener hypothesis has received a great deal of attention in recent years and deservedly so. It is based upon the idea [of] isostasy .... Many students of the Earth's crust feel that the Wegener hypothesis does violence to certain mechanical principles, but, in any

[19]For a detailed discussion of the unifying aspect of Holmes model see Lewis (2002).

event, it is something that should be looked into. (quoted from Ibid., p. 261).

Further Improvement of the Programmatic Character - The Mechanism of Drift. Vening Meinesz in a volume from 1934, which discussed results of his investigations of gravity, addressed some of Jeffreys' objections to the hypothesis of convection currents as the force governing the drift. According to Jeffreys, any significant thermal differentials in the earth must have been eradicated throughout its cooling history. However, Meinesz pointed out that the heterogeneous nature of the earth's inner structure, with an uneven distribution of radioactive constituents and thermal properties meant that "in the actual earth there can be no doubt that convection currents must develop" (quoted from Ibid., p. 245-248). Meinesz's work offered support to the hypothesis of Holmes' model, which, as we have seen, gave a programmatic character to Drift with respect to the problem of the mechanism of drifting.

Increase in the quality of significant explanations. Some explanatory anomalies pointed out by the opponents of Drift were addressed by Alexander du Toit's capital work *Our Wondering Continents*, which came out in 1937 du Toit (1937). For example, in order to account for more explananda, Du Toit proposed in place of Wegener's one super-continent, two original super-continents – Laurentia in the north and Gondwanaland in the south.

9.3. State of affairs in the 1940s

In spite of exhibiting a theoretical growth and a growth of its programmatic character throughout the 1920s and the 1930s, Drift was rarely discussed in the '40s ((Le Grand, 1988, p. 117); (Oreskes, 1999, p. 226)). This was partially due to the effects of World War II, during which many North American geologists were employed in the war effort, while the immediate post-war years were not very fruitful of theoretical developments.

The development of Drift seemed to have reversed by the articles published by North American paleontologist George Gaylord Simpson. However, Simpson's objections were rebutted by du Toit ((Oreskes, 1999, p. 295-296); (Frankel, 1987, p. 217-219)).[20]

To sum up, we have shown that Drift had a theoretical growth throughout the 1920s and 1930s. In spite of this growth, it received hardly any attention in the 1940s. It is thus not surprising that its theoretical growth was missing in this decade.[21]

[20]Moreover, in the mid-40s du Toit began a work on a manuscript entitled "On the mathematical probability of continental drift" (Ibid., p. 297) in which he planned to offer a quantitative account of Drift based on the degree of similarity among species and the distances among them. The work was never finished due to du Toit's death in 1948. Thus, this work cannot be considered as a contribution to the explanatory growth of Drift, but it does represent a contribution to its programmatic character in the time when Drift was mainly abandoned.

[21]Note that this conclusion differs from R. Laudan's view according to which "far from

10. The Consequences of Our Account for the Epistemic Stances of Geologists

Our analysis has shown that Drift scored well in each of the criteria of our evaluation. Thus, we can conclude that it was both initially WPSS and that it remained WPSS in the first half of the twentieth century. That means that it was rational to consider Drift as worthy of pursuit, and that it was irrational to reject its pursuit as unworthy. It also means that characterizing Drift as worthy of pursuit was not conflicting with rejecting its full acceptance.

In this section we are going to show that most of those who had a positive opinion of Drift actually found it to be worthy of pursuit. Furthermore, we will also show that there were geologists whose opinions of Drift can be considered irrational in the above explicated sense.

10.1. The Supporters of Drift

Even though Wegener himself had a strong epistemic stance towards his own theory, most of those who argued in favor of Drift, maintained that it was a theory requiring and worthy of more investigation. Let us mention some of them.

In Europe, German paleontologist Karl Andree found Wegener's theory to be a stimulus to research even though it could not be accepted in all of its details (Le Grand, 1988, p. 58). Austrian paleontologist Bruno Kubart maintained that a combination of ideas taken from older theories together with those from Wegener's Drift could form a suitable basis for further research (Ibid., p. 60). Dutch geologist Gustaff A. F. Molengraff argued that eastward drift was a possibility (Stewart, 1990, p. 37). For a British geologist Charles Seymour Wright, Drift offered a promise since it could explain certain fossil deposits on Antarctica which indicated that previous to glaciation there was a period of warmth in this area (Le Grand, 1988, p. 89-90). Irish geologist John Joly suggested that Drift was a logical possibility within his theory of periodic convection currents (Stewart, 1990, p. 37). North American geologist Chester Longwell pointed out that "if the doctrine of continental displacement is accepted as a working hypothesis, to be tested and tried fairly along with others, it may be productive of valuable results" (quoted from (Stewart, 1990, p. 38)). Joseph T. Singewald suggested that, in spite of the obvious failures of Wegener's presentation, the hypothesis should be tested on the basis of its worth for guiding research (Le Grand, 1988, p. 71). Even Arthur Holmes found Drift to be a possible working hypothesis rather than a theory sufficiently developed to be accepted (Stewart, 1990, p. 41).[22] Leo Arthur Cotton, Australian geologist, found Drift (though

showing a greater rate of progress than rival theories, drift stood still, or even regressed between 1930 and 1955" (Laudan, 1987, p. 214). In view of our analysis, Laudan's estimation is too rough since it does not apply to the 1930s.

[22] Stewart remarks that the fact Drift was for Holmes only a possible working hypothesis shows that Holmes's stance towards it was very weak and can hardly be seen as the one of a strong supporter of the theory, who would encourage his colleagues and students to advocate

not Wegener's version) to be worthy of pursuit or entertainment with respect to the problems that concerned him (Le Grand, 1988, p. 85). Arthur Wade, who was educated in England, and afterward emigrated to Australia and who was engaged in oil exploration around the world, characterized Drift as a working hypothesis and pointed out its application to economic geology of those regions in which he had conducted his research (Ibid., 86). Similar was the opinion of Australian zoologist Launcelot Harrison who found Drift explanatory of the southern species distribution and thus to be a useful working hypothesis (Ibid., p. 88).

As we have mentioned, the positive stance towards pursuit of Drift is not necessarily conflicting with rejecting Drift in the context of acceptance. It is easy then to see that debates among the proponents of Drift advocating its pursuit and the opponents rejecting its full acceptance sometimes consisted of not necessarily conflicting arguments. That means that the awareness of the distinction between theory evaluation in the context of pursuit and theory evaluation in the context of acceptance may sometimes help scientists to avoid unnecessary debates. In other words, the question of pursuit worthiness is not only of significance for philosophical discussions regarding issues of rationality, but it is also of significance for scientific practice and epistemic stances of scientists.

Among the opponents of Drift there were also those who rejected not only its acceptance, but also its pursuit worthiness. Let us take a closer look at their points of view.

10.2. Opponents who Rejected Pursuit Worthiness of Drift

That Drift was not always acknowledged as worthy of pursuit is exemplified in the opinions of geologists who explicitly ridiculed it. For example, Bailey Willis' 1944 article was titled "Continental Drift, Ein Märchen" – a fairytale. As Le Grand puts it: "His hostility to Drift, even as a permissible working hypothesis for other geologists, was unabated in 1944" (Le Grand, 1988, p. 118). Similarly, Charles Schuchert still spoke of "the Wegener sliding circus" in 1931 (Oreskes, 1999, p. 212), while Max Semper explicitly rejected the idea of pursuing this "absurd theory" (Le Grand, 1988, p. 59). Even in the 1950s advocates of Drift were still publicly ridiculed (Oreskes, 1999, p. 218). These opponents not only found Drift to be unworthy of pursuit, but disregarded it even as a serious scientific theory.[23] A similar attitude towards Drift can be found also in later discussions. For example, geophysicist Seiya Uyeda suggested that Drift could scarcely be regarded as scientific since it could not explain what had orig-

a novel and widely opposed theory (Ibid., p. 42). However, Stewart's conclusion shows that he does not recognize that judging a theory as worthy of pursuit represents a valuable contribution to its further development, even though such a stance may very well be the most rational form of supporting a newly developing theory.

[23]As Frankel remarks: "I do not find it surprising that they would not accept the drift hypothesis, but I do find it surprising that they would not treat it as a serious research program." (Frankel, 1976, p. 319).

inally caused the continental movements (Oreskes, 1999, p. 63, fn. 28).[24] All of these views strongly diverge from the result of our analysis.

But we have to pause here and take into consideration a possible objection that geology in the first half of the twentieth century had different methodological standards, and that thus our criteria of pursuit worthiness are not applicable to the notion of rationality governing scientific research at the time. More precisely, North American geology in the first half of the twentieth century was rooted in a methodological framework which was deeply embedded in inductivist ideals. Many authors who discussed this historical episode suggested that these geologists primarily focused on field research and practically valuable results, placing less significance on global geological theories and their explanatory power. And if this is correct, then their criteria for what counts as epistemically worthy of pursuit might have been different as well.

However, Naomi Oreskes shows that the view according to which North American geology was deeply inductivist and anti-theoretically driven is in fact a historiographic cliche, and that describing these geologists as naive empiricists or narrow utilitarians doesn't do justice to their research. Not only were they not opposed to theoretical activities as such, but some of the major theoretical contributions to earth sciences came from the United States (for example, James Dana's work on the origin of continents and oceans, James Hall's geosyncline theory, or Clarence Dutton's work on isostasy) (Ibid., p. 129). Furthermore, Thomas C. Chamberlin, one of the most important American geologists from this time period, promoted the unity of theory and practice (Ibid., p. 130-133).

Where American geologists differed from European ones was in their suspicion of theory-driven science and in requiring a thorough empirical research as a necessary step preceding any theoretical claims (Ibid., p. 134-136). A direct observational statement of geological phenomena was to come before any theoretical conclusions. Moreover, the research was to be done as much as possible in terms of G. K. Gilbert's and T. C. Chamberlin's method of multiple working hypotheses. As the name suggests, the underlying idea of the method was to view observational facts in light of competing explanatory frameworks, rather than in view of an already established theory. The goal of the method of multiple working hypotheses was to navigate between the risks of dogmatic deductivism and the infertility of naive inductivism (Ibid., p. 140).

In view of such methodological standards, Oreskes argues that the key reason why North American geologists reacted so negatively to Wegener's theory is the fact that Wegener violated these standards in several respects. First, his program aimed at proposing a grand geological theory. Second, he regarded the supporting evidence as "proofs" necessitating Drift, rather than as observations or geological facts which were best explained by his theory. Finally, he presented

[24]Note that Uyeda's epistemological standard, requiring for a deepening of explanations offered by a given theory as the condition for it to be regarded as scientific, differs from our standards which allow for problems of this kind to be tackled by the programmatic character of the pursued theory. For further discussion on the validity of Uyeda's standard see: Ibid,, p. 63-64.

the idea of Drift not as a working hypothesis, but as a "fundamentally correct" theory, in contrast to contractionism and permanentism which he saw as based on erroneous premises (Ibid., p. 153-154). As a result, some American geologists not only rejected Drift, but found Wegener's approach to be unscientific.[25]

It is not difficult to understand then why Wegener's approach was not appealing for North American geologists. The fact that his theory violated the standards of how science is to be done in their view explains why his theory could not be accepted at the time. But was Drift, in view of these standards, also unworthy of pursuit? All that the above mentioned objections show is that Wegener might have been incautious and that he might have had an unjustified epistemic stance towards his own theory. But they do not attack the fact that Wegener's theory exhibited an explanatory power for a certain set of phenomena. The closest Wegener's opponents came in criticizing the fact that Wegener's theory offered some explanations was to argue that he "generalized too easily from other generalizations" (Stewart, 1990, p. 37). Nevertheless, they did not mind that their own "generalizations" depended on ad hoc hypotheses – for example, on the idea of land bridges for which they had no mechanism which would explain their disappearance (Ibid.). In addition, the explananda addressed by Drift were not merely posited by Wegener, without any empirical back-up. For example, paleontological similarities between coastal regions on the opposite sides of the oceans were researched by others as well, and even aknowledged by Wegener's opponents.[26] Finally, the research conducted by du Toit, Holmes and others introduced much more substantiated evidence and thus improved Wegener's theory in view of American methodological requirements. Therefore, there was no methodological reason why Drift would not be taken seriously as any other working hypotheses. Yet, as we have seen, some geologists found Drift to be unworthy of pursuit even in the 1930s – long after Wegener's original proposal had been significantly improved. The root of their epistemic stance can thus be found primarily in their biasness towards the fixist frameworks, rather than in a fair application of a specific set of methodological standards.[27] As a matter of fact, our criteria of pursuit worthiness do not con-

[25]For example, American geologist Rollin. T. Chamberlin questioned the scientific status of the entire field of geology in view of the fact that it allowed for theories like Drift "to run wild" (Le Grand, 1988, p. 64).

[26]For instance, Charles Schuchert, very critical of Wegener's theory, acknowledged at the 1926 American Association of Petroleum Geologists symposium on continental drift "that Wegener's hypothesis has its greatest support in the well known geologic similarities on the two sides of the Atlantic, as shown in strikes and times of mountain-making, in formational and faunal sequences, and in petrography." (quoted from (Oreskes, 1999, p. 180)). Ironically, Wegener was actually attacked for using results of the research conducted by others, instead of doing all the field work on his own since, as Schuchert remarked, "it is wrong for a stranger to the facts he handles to generalize from them to other generalizations" (quoted from (Oreskes, 1999, p. 156)).

[27]A biased approach of North American geologists is also reflected in the fact that their judgment was made in view of locally relevant sets of explananda, disregarding geological phenomena belonging to other regions around the world, for which Drift was highly explanatory. Le Grand calls such an approach "localism" (Le Grand, 1988, p. 95-97), while Oreskes

flict in any significant way with these methodological standards. In contrary, they are compatible with the underlying idea of the method of multiple working hypotheses: they allow for a simultaneous pursuit of different hypotheses, since more than one theory (or a research tradition) can be, according to our framework, WPSS.

We can thus conclude that, in spite of the methodological differences among geologists, the opinion that Drift was worthy of pursuit in the first half of the twentieth century, and especially in the 1920s and the 1930s, can be characterized as rational (in the sense of being epistemically justified), and the rejection of its pursuit worthiness as irrational.

11. Conclusion

In this paper we have presented en epistemic evaluation of the pursuit worthiness of Drift in its early development. For this purpose we have used the framework of epistemic justification suitable for theory evaluation in the context of pursuit, which we have adapted for the evaluation of pursuit worthiness in a strong sense. We have shown that Drift had a number of significant explanations, that it did not have a lower internal or external inferential density than its rivals, and that it had a programmatic character with respect to its major problems. Moreover, we have shown that throughout the 1920s-'30s Drift exhibited a theoretical growth and a growth of its programmatic character, and thus remained worthy of pursuit in a strong sense throughout this time period. On the one hand, this means that it was not only rational to pursue Drift, but that characterizing Drift as worthy of pursuit was not conflicting with rejecting its full acceptance. Hence, we have emphasized that the distinction between theory evaluation in the context of acceptance and the one in the context of pursuit may help scientists to avoid some unnecessary debates. On the other hand, we have shown that it was epistemically unjustified to reject Drift as unworthy of pursuit, and that consequently, opinions of some geologists in the first half of the twentieth century can be regarded as irrational.

It is important to clarify that our analysis did not take into account the question of pursuit worthiness of other rivaling theories at the time. However, it may very well be the case that a closer look at contractionism and permanentism would reveal that they were also worthy of pursuit. A detailed evaluation of each of them remains a task for the future research.

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characterizes it as "epistemological chauvinism" or "epistemological affinity"(Oreskes, 1999, p. 52-53), pointing out that placing a higher preference on certain subsets of the available data was often motivated not only by a specific geographical context, but also by a national or disciplinary one.

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