Robustness and The Rejection of Wegener's Continental Drift in the Thirties

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1. Introduction

Just a little more than half a century ago, *continental drift* (hereafter CD) – the idea, first put forward in a coherent and logically argued model by German meteorologist Alfred Wegener in 1912, that the Earth's continents move laterally with respect to each other over geological time, and arguably the most important phenomenon pertaining to the structure of the Earth's outermost shell (lithosphere) – was still and barely a mere working hypothesis considered worthy of attention by only a few scientists. Today, instead, *plate tectonics*, the uncontroversial theory of modern geology – quite different from Wegener's original theory, but still deeply rooted in the unceasing wandering of lands – is simply the basic pillar of the Earth sciences. Plate tectonics, in fact, which now belongs to current 'normal' (in a Kuhnian sense) science, has over the last half a century had an all-pervasive influence and a unifying role in Earth sciences, and has given geology a stability that it has never before had in its history[1].

For almost five decades, however, starting from the beginning of the 1910s, when Wegener exposed his new approach, most geologists preferred

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1. The seminal idea of plate tectonics was proposed in 1965 by the Canadian geophysicist Tuzo Wilson. According to modern plate tectonics, the Earth's crust is similar to a mosaic of very large blocks – akin, on giant-scales, to paving stones or ice floes – which move as separate rigid units, and experiencing deformations at their borders due to the collisions with other plates. These plates, however, do not coincide with the continents or individual ocean floors, but can include both or parts of them. Therefore, strictly speaking, Wegener's CD is not correct, the main difference being that his theory of individual continents in motion has been replaced by a theory which relegates such motions to just the visible part of the more fundamental motions of plates. Furthermore, plate tectonics is supported by a theoretical structure very different from that of CD, in which the diversities in research methodologies and in the comparisons with the experience are substantial.

the idea of a static Earth in which continents and oceans never appreciably changed their positions relative to each other, except perhaps very early in the Earth's history. The famous Swiss geologist Émile Argand, one of the first converts to CD after reading the first edition of Wegener's book, *Die Entstehung der Kontinente und Ozeane*, introduced the term *fixism* to designate the latter view, whereas he used the word *mobilism* for the view sustaining that such relative displacements occurred. In reality, fixists did not always agree among themselves, apart from the common general idea about the continents that remained fixed in the same place: some claimed that the axis of rotation moved relative to the Earth as a whole so that continents changed their positions relative to the geographical poles; others disagreed about the formation of the continents (whether they increased in size, how their mountain belts formed, why their geological disjunctions and intercontinental biotic arose, etc.). On the contrary, most mobilists agreed on the horizontal displacement of the continents, on their change of position – both in relation to one another and in relation to the geographic poles – and on the change of their latitude and longitude over time (actually, just a very few of them did not maintain the latter hypothesis because they believed in the Earth's expansion, therefore in a change of the relative distances but not in a change of the latitudes and longitudes). In any case, the scientific controversy between fixists and mobilists, obviously centered around the acceptance of CD, also went beyond those aspects more strictly related to this issue (see Segala, 1990).

From the 1910s to 1950s, those few adherents to Wegener's theory were often dismissed as cranks, in particular in North America. The geological, and especially geophysical, establishments, considering mobilism mechanically implausible and inadequately supported by evidence, were not seriously influenced in their mainstream interests by Wegener's ideas. Notwithstanding, as Segala (1990) affirms, the latter remained much less silent and corrosive than has often been judged by most historical reconstructions, and actually slowly continued their doubt-spreading activity among the fixist mainstream.

In the late 1950s and early 1960s, things quickly changed in favor of mobilism, especially as soon as seafloor spreading, developed as a geophysical model and verified principally thanks to technical improvements, was accepted by almost the entire scientific community as the crowning of plate tectonics and consequently as the last nail in the fixists' coffin[2].

2. Seafloor spreading is the process of gradual moving away from the mid-ocean ridges of that new oceanic crust which is formed through volcanic activity. In practice, basaltic magma rises up through the fractures generated by the tensional stress of the oceanic plates divergence, and, by cooling on the ocean floor, forms new sea floor, which,

As Henry R. Frankel wrote in the back cover of his four-volume monumental work, *The Continental Drift Controversy* (2011), plate tectonics – substantially the resolution of the sixty-year debate over CD – can be considered alongside the theories of evolution in the life sciences and of quantum mechanics in physics in terms of its fundamental importance to our scientific understanding of the world[3]. It goes without saying that CD and the controversy surrounding it, or more generally the *mobilism* controversy, was not only the most important and longest debate of the past century in Earth sciences, and one of the most important in all of sciences, but still today constitutes an inexhaustible epistemological mine rich in insights about how science evolves[4]. Indeed, this controversy on Wegener's model led, among other things, a lot of scientists to take completely wrong ways or even to continue along blind alleys in their struggles. And yet the most important kinds of evidence were already among them: they just needed to open their eyes and pick them out.

The aim of our paper, therefore, is to evaluate the effective weight of this sort of blindness to the facts that led, particularly in the North American context in the thirties, to the rejection of CD. But we will try to do this analysis in the light of the recent notion of robustness in the philosophy of science. We will find that such a rejection was not at all rational, in the sense that even if scientists obviously had reasons prompting their rejection (otherwise history in general would risk to be an unjustified sequence of facts), they could also have opted, *on rational bases provided by the available evidence*, for its acceptance.

The plan of the paper is the following. In section 2 we introduce the notion of robustness and Bayes' formula necessary to evaluate the degree of belief in CD by taking into account all the evidence available in the thirties in support of CD. Section 3 is devoted to the investigation of the evidence favoring and disfavoring CD, with the evaluation of their respective conditional probabilities, while in section 4 we enter into the epistemological peculiarities of geologists in the North American context

by moving away from the ridge, carries the continents with it. In such a way, this process – initiated by the convection currents responsible for plates divergence, occurring in the upper mantle (just below the lithosphere) of the Earth – easily explains CD.

3. On the other hand, similar comparisons were used even in the early years after the proposal of Wegener's theory. For instance, as Segala (1990, p. 23) points out, Reginald Daly, in his 1926 book *Our Mobile Earth*, likened the Wegenerian conceptual change to the Copernican revolution. On the revolutionary character of CD see also Cohen (1985, section 29).

4. The fascinating story of how the rejection of mobilism was converted into consensus in the 1960s has been told many times, not always with the same overtones. See, for instance, Frankel (1989; 2011), Hallam (1973; 1989, chap. 6), Marvin (1973), Oreskes (1999; 2003), Segala (1990), Sullivan (1974).

of those years. Finally, section 5 and 6 offer, respectively, our Bayesian evaluation of CD rejection and our conclusions.

2. Robustness

Lena Soler *et al.* (2012) published an important book on the notion of robustness. Here we start from one of its contributions, the excellent paper by J. Stegenga on robustness and acceptance of hypotheses. In it, we find the following definition:

> A hypothesis is *robust* if and only if it is supported by concordant multimodal evidence. (Stegenga, 2012, p. 210)

By *mode of evidence* Stegenga means a particular way of finding out about the world (it is a type of evidence, a technique or a study design); *multimodal evidence*, therefore, is the total set of evidence that is relevant to a hypothesis of interest and which is generated by multiple modes (Stegenga, 2012, p. 208). Moreover, the concordance, for a given hypothesis, of multimodal evidence makes the latter useful and epistemically valuable, as also maintained by many philosophers of science.

According to Stegenga, the best argument favoring the at least partial truth of a robust hypothesis for a moderate scientific realist is a no-miracles one: it would simply be a miracle if the hypothesis, supported by concordant multimodal evidence, were not true; on the other hand, since miracles cannot be accepted as scientific explanations, we have strong reasons to believe in the truth of a hypothesis when it is supported by concordant multimodal evidence.

Multimodal evidence is an exceptionally important notion, as Stegenga affirms, but it has to tackle a fundamental conceptual difficulty he calls the "individuation problem", i.e., how we can distinguish and individuate the different modes of evidence, what criteria should be adopted to determine independence among modes. On this topic, he proposes a subtle and intriguing epistemological solution, based on the idea that different modes are individuated by different background hypotheses and that the robustness of a hypothesis increases when the background hypotheses are epistemologically complementary. But in the case of CD acceptance, in order to discriminate modes of evidence, it seems sufficient to take the probabilistic definition suggested by Howson & Urbach (1989, p. 114): two evidences e_1 and e_2 with respect to the hypothesis h belong to different modes if $\mathrm{p}(e_1/e_2\&{\sim}h)$»$\mathrm{p}(e_1)$ and $\mathrm{p}(e_2/e_1\&{\sim}h)$»$\mathrm{p}(e_2)$ (where the symbol "»" indicates "almost equal"). In the conditioned part of our equations we have also inserted the negation of the hypothesis we are investigating, because, otherwise, by assuming h, it would be probable that the two evidences become dependent, or rather: there is no guarantee that the endorsement of h would not spoil the independence of the evidence in question. Note the use of the symbol "»", to emphasize that the different evidences we will evaluate are not completely independent.

A second problem of robustness is *discordant* evidence modes, that is different modes of which some support a hypothesis and others sustain either negation or else other hypotheses. And, in the case of acceptance of CD in the thirties, we have to face discordant modes of evidence. On this point Stegenga concludes that "discordant evidence diminishes the value of robustness" (2012, p. 216). Or, equivalently,

> when multimodal evidence for a hypothesis is concordant, that hypothesis is more likely to be true, or explanatory, or phenomena-saving, or whatever predicate of epistemic success fits most comfortably with one's philosophical inclinations. (Stegenga, 2012, p. 222)

Moreover, in the appendix he presents the Bayesian approach to the question, which is able to amalgamate all kinds of evidence modes, both concordant and discordant. Since this approach is simple, received and powerful, we will conform to it. In this perspective, let e_i be a set of evidence of the same mode i relevant for the hypothesis h. We order all modes from 1 to n. Then we apply Bayes' theorem iteratively. Remembering the total probability formula:

$$p(e_i) = p(e_i/h)p(h) + p(e_i/{\sim} h)p({\sim} h),$$

where $p(e_i/h)$ and $p(e_i/{\sim} h)$ are the so-called likelihoods, the result is:

$$p(h/e_i, \ldots e_n) = \frac{p(h)\Pi p(e_i/h)}{p(h)\Pi p(e_i/h) + p({\sim} h)\,\Pi p(e_i/{\sim} h)} \qquad (1)$$

(where Π indicates a repeated multiplication). Since multiplication is commutative, the order of application of Bayes' theorem is irrelevant with respect to the final probability. Remember that the e_i must be independent evidence modes.

The problem in applying this method is that we have to establish a lot of a priori relevant probabilities. In the following, we will investigate one by one all evidences, for and against CD, available at the beginning of the thirties, in order to assign reasonable values to the right-hand terms of the equation (1), and so eventually find the degree of belief in the hypothesis h (which here is obviously CD) having taken the evidence e_i into account.

3. Evaluating evidence

There are many books which attempt an historical reconstruction of the American rejection of CD, but the best is surely Oreskes (1999) and its résumé Oreskes (2003). The latter is very clear, and we will base much of our rational reconstruction on it, although criticizing some of her points of view and especially disagreeing on the moral she draws from the CD controversy.

3.1. *Orogenic evidence*

The origin and formation of mountains, the processes that squeezed and folded the rocks, in a word *orogeny*, was one of the most challenging of all geological problems of the 19th century. In the 19th century, most theories favored the so-called *contractionism*, namely the idea that the Earth, formed in a distant past as a hot body, was cooling and thus contracting insofar as most materials contract as they cool. In such a way, the deformations of the Earth's surface due to the contraction would have produced mountains. In Europe, at the turn of the 20th century, Austrian geologist Eduard Suess proposed his version of contractionism centered around the past existence of a giant supercontinent (called Gondwana) that once covered all (or much) of the Earth's surface. The continuity of such initial crust was then broken apart by the cooling and shrinking of the Earth's interior, leading to the formation of continents by the elevated portions of the crust, and of the ocean basins by the collapsed portions. But the further cooling also lead to a sort of interchangeability of continents and oceans, in the sense that the initial continents – becoming unstable and collapsing, and forming other generations of ocean floors, while the original oceans becoming dry lands – led to a continuous rearrangement and interchange of seas and lands.

In North America, geologist James D. Dana developed in the 19th century another version of the contraction theory, which actually came to be known as *permanence* theory[5]. According to this theory, the formation of the Earth's continents was due to the solidification of those minerals with relatively low-fusion temperature, whereas the ocean basins were formed in a second phase when the Earth, continuing to cool and contract, allowed the high-temperature minerals to solidify. When the Earth was solid, a further contraction induced the deformation of its

5. Contractionism refers more to the dynamics of the Earth, whereas permanentism – which, as already mentioned at the beginning of our paper, is also called fixism – relates more to its surface history, so that permanentism is the direct alternative to mobilism.

surface. The mountains began to form especially along the continental margins insofar as the greatest pressures were concentrated at the boundaries between continents and oceans. Even if further contraction caused a continuous deformation, both continents and oceans persisted in the same relative positions, so that the Earth has globally preserved permanent characteristics.

In the early 20th century, however, contraction views were challenged and discredited by three independent lines of evidence. Very briefly (but see Oreskes, 2003, pp. 6-7), terrestrial contraction could not explain the following facts: 1) the field mappings of some folded sequences of rocks (of particular mountain belts) – these folds proved so extensive that, in the hypothetical case of an unfolding, the rock layers would extend for hundreds of miles; 2) the discrepancies in the geodesic measurements of some stations' distances, which instead complied with *isostasy* (see section 4.2), i.e., the new theory that assumed the fluidity (highly viscous) of the substratum underlying the Earth's crust; 3) the discovery of radiogenic heat, which contradicted the basic fact that, according to contractionists, the Earth was steadily cooling.

Wegener's theory[6] instead maintained that the continents, for several hundred million years, were united forming a huge supercontinent (Pangea). It then rifted and its fragmented components moved apart forming continents and oceans, whose persisting motions continuously changed their respective positions. So, the global configuration of the Earth's crust constantly changed, and actually still changes. The formation of mountains, and the origin of earthquakes and volcanoes as well, is easily explained by appealing to this mechanism involving the continuous interactions of drifting and rifting land masses: the moving continents were compressed, fractured, and folded by the resistance of the ocean floor. That this mechanism was conceivable in the thirties, and actually conceived by those few supporters of CD, derives from the fact that isostasy was then well established. And, as it implied vertical movements of the continental masses through the substratum, there was no reason – as Hallam (1989, p. 144) underlines – why continents should not also be able to move horizontally, provided that there were sufficient forces acting for a sufficiently long time to do this. And the existence of such forces was evident from the horizontal compression of strata in mountain ranges such as the Alps, Himalayas, and Andes.

Hence it seems that CD explains better the distribution of mountains than fixism. In spite of this we will not consider this argument for our

6. Even Wegener (1924, chap. 4) provides arguments against contractionism, based on the distribution of the heights on the Earth.

evaluation of the CD hypothesis, since on one side the explanatory advantages of CD are not completely clear, on the other, if we reach a good confirmation of CD without orogeny, our hypothesis on the irrationality of Earth scientists in the thirties becomes more robust.

3.2. *Paleoclimatic evidence*

Wegener was a meteorologist and in the first he understood (1924, p. 5) that certain paleoclimate anomalies – such as the finding, in contemporary temperate zones, of fossils, distinctive rock types and other deposits typical of cold places, and vice versa – could be easily explained by CD. Indeed, these paleoclimate changes would have been both the direct result of continents which moved through the various climate zones, and the indirect consequence of the different distribution of lands and oceans on oceanic circulation and climate patterns.

So, let e_1 be this kind of paleoclimatic evidence. It is reasonable that p(e_1/h)»0.9 and p(e_1/~h)»0.1. The first claim is supported by the fact that CD provides a good explanation of this mode of evidence, whereas a reason for the second claim is that no alternative explanation was readily available.

3.3. *Paleontological evidence*

Partially similar to paleoclimate evidence was paleontological evidence, usually held to be one of the strongest. In the 19th century, paleontologists discovered that some fossil plants and animals were extraordinarily similar in southern continents today isolated by oceans. Basic biological principles demand some sort of connection between those lands to account for these fossils distributions. The presumed existence of Pangea, hence of the past closeness, or unity, of the continents, explained these similarities very plainly, without the need of the ad hoc hypothesis of long isthmuses. This hypothesis, unfortunately widely accepted, was proposed in 1933 by geologists Schuchert & Willis; the latter introduced long transoceanic land bridges (isthmuses), intermittently elevating from the Earth's crust and connecting the continents and now sunk into the ocean floor. This conjecture had to be refused as overtly ad hoc insofar as there was no evidence of such isthmuses other than the paleontological data they were designated to explain (Oreskes, 2003, p. 12).

Furthermore, the latter hypothesis was shown to be untenable, on geophysical grounds, by Wegener himself: this hypothesis would have violated isostasy insofar as the land bridges would have been composed of granitic crust too light to sink into the denser rocks of the ocean floors. On the contrary isostasy supported CD. Nonetheless, considering our definition of what are different modes of evidence, it is clear that, though both paleoclimate and paleontological characteristics are “paleo-evidence”, they have a different nature. So we can introduce e_2 in order to indicate paleontological evidence, and, in analogy with the preceding case, we can say that p(e_2/h)»0.9 and p($e_2/\sim h$)»0.1.

3.4. *Geological evidence*

A third series of evidence, analogous but different from the two previous ones, were the geological similarities of rocks placed on the two sides of the Atlantic Ocean. In particular, the matching of their orogenic fold belts (and also of the terminal moraines of the North American and European ice sheets) suggested, according to Wegener, a former continuity. These similarities become reasonable through the Pangea hypothesis, which is founded on CD. Again, if we indicate this geological evidence with e_3, we can evaluate that p(e_3/h)»0.9 and p($e_3/\sim h$)»0.1. In this case the isthmuses hypothesis is not a possible alternative explanation. There was also an explanation based on the contractionist view, i.e., as already said, the supposition that the present terrestrial crust is the result of a contraction, due to the steadily cooling of the Earth, of a once warmer Earth; but, as previously explained in section 3.1, this view in the thirties was already discredited for good reasons.

3.5. *Morphological evidence*

A fourth series of evidence came from the jigsaw-puzzle form of the continents first noted by the great 16th century cartographer Abraham Orthelius in 1596 (Romm 1994) ~~(Romm, 1992)?~~. Šešelja & Weber (2012, p. 149) suggest that the jigsaw-puzzle argument requires arbitrary changes of continents shapes. This is partially true, but not sufficient to eliminate the positive relevance of this mode of evidence. Therefore, by indicating this morphological evidence with e_4, we can say that p(e_4/h)»0.9 and p($e_4/\sim h$)»0.4. The latter evaluation is due to the fact that e_4 did not unquestionably necessitate an explanation insofar as such kind of evidence could be judged, at the time, simply the result of chance.

3.6. *Geophysical explanatory evidence*

As emphasized by Oreskes (1999; 2003), the influent English geologist Arthur Holmes already in 1929 formulated the hypothesis of the convective cells in the mantle, which we now know (Turcotte & Schubert, 2002, p. 3) is part of the correct explanation of the at that time mysterious continent moving force. Therefore we cannot consider, as many standard geophysical handbooks still do, the lack of a reasonable causal mechanism, able to explain the movement of the crust, as a definitive motive for rejecting CD. However, it is important to take into account that a very authoritative geophysics book of 1924 by Harold Jeffreys – the famous probability scholar – showed that seismic data suggested that the mantle was too stiff to allow crust movements (on this see also Marvin, 2001), in this way demolishing the physical bases of CD. It should be noted that Jeffreys' attack, mainly directed towards Wegener's hypothetical explanations of CD, easily resolved into his victory insofar as these explanations were in reality particularly poor and plainly destroyable on geophysical bases[7].

More important is that Wegener did not have direct evidence of the effective movements of continents[8]. Therefore, by indicating this absence with e_5, we can say that p(e_5/h)»0.1 and p($e_5/\sim h$)»0.9.

4. American geologists' approach

Oreskes identifies three main factors able to explain the American animosity towards CD. These factors allow us to further refine our analysis.

4.1. *The method of multiple working hypotheses*

American geologists of that period were strongly empirically committed. They were constrained by the so called "method of multiple

7. According to Wegener, the mechanism that could explain CD was the continents' floating on the oceanic crust, while, as real causes of the continents' wandering, he proposed those forces which continuously act on the Earth's surface: tidal forces, forces acting away from the Poles and forces produced by the Earth's rotation. This was the Achilles' heel of the Wegenerian system, as Segala (1990, p. 171) points out: it was a child's play, for Jeffreys and colleagues, to show through few calculations the exaggerated insufficiency of those forces to move the continents.

8. It is well known that Wegener died in Greenland during a fruitless attempt to verify the actual displacement of this continent; see Oreskes (1999, *passim*).

working hypotheses" (Chamberlin, 1890). Oreskes (1999) considers this methodological principle as fundamental in the justification of the American rejection of CD. Indeed, following the American founding fathers Franklin and Jefferson, many thought that good science, like good politics, had to be based on pluralism, scant speculation and empiricism[9]. In this sense Wegener's theory was even *bad science*. We cannot consider this *erroneous* methodological principle (indeed accepted by Oreskes, with dramatic skeptical consequences) as a good reason to reject CD. Already Huygens, Kant and Whewell, well before Einstein and Popper, knew that science is not a mere Baconian empirical enterprise. {Citation}

But it is clear that, as CD is highly speculative, we have to assign a very low a priori probability to it. Therefore p(*h*)»0.1.

4.2. Isostasy

In the middle of the 19th century, Welsh geographer Georges Everest found a discrepancy in the measurement of Earth distances in northern India through astronomical and geodetical methods (see Watts, 2001 chap. I). An English physicist, George Biddell Airy, and a British mathematician, John Henry Pratt, found a quantitative explanation of this discrepancy. Both were persuaded that it was due to the gravitational force caused by the Himalayas. Both calculated that the discrepancy was *less* than one should expect from this hypothesis. Both explained this further incongruity through the hypothesis that beneath the mountains there was a lighter layer of rocks; but Pratt assumed that the heavier crust was homogeneously distributed like "tables" on what would be called the "mantle", whereas

9. It is also interesting, for the sake of completeness, to consider that Oreskes' opinion is debatable, curiously taking as a paradigmatic case the behavior of Chamberlin's son, Rollin T. Chamberlin, who "provides a striking example of how little effect the methodological pronouncements of scientists may have on the actual practice of science" (Giere, 1988, p. 291). One of the most vehement objectors to Wegener's model, Rollin Chamberlin, in 1926 listed eighteen objections to such model. In these pages, his attitude – according to Giere – is just the reverse of the one glorified by his father: instead of including in his elaboration widely divergent hypotheses, as widely divergent as mobilism and stabilism, "he was far more concerned to defend his father's theory [i.e., the planetesimal hypothesis, according to which the Earth formed by gravitational attraction from small chunks of matter] than his father's methodological pronouncements" (Giere, 1988, p. 292). In brief, Rollin Chamberlin was guided much more by his professional interests (he was professor of geology at Chicago) in his reaction to Wegener's model than by a priori methodologies, such as those claimed by his father. And such an attitude inclined to favoring professional interests, as Giere explains (1988, p. 239) quoting other analyses, was pretty common in those years. For a different perspective with respect to Oreskes' description of American geologists' approaches (see also Segala, 1990).

Airy proposed a sort of "iceberg model" of big pieces of crust with deep roots in the mantle. Both models reached good results. In general, this consideration led to the affirmation of the so-called "isostasy", i.e., the hypothesis that crust floats, with vertical movements, on a partially fluid mantle in order to reach a state of gravitational equilibrium.

The point, as Segala explains, was that

> while paleogeography, tectonics and geodesy had raised, during the second half of the 19th century, questions that opened the way for mobilist solutions, geophysics had rigorously remained permanentist: neither the theory of isostasy, nor the indications on the crust structure given by seismology had suggested something different from the accepted vertical movements of portions of the Earth's surface. (Segala, 1990, p. 150 our translation)

So isostasy was an accepted conception in such a generally permanentist background, but its sense was eminently static (i.e., it consisted of the reinstating of equilibrium), whereas Wegener made isostasy the guarantee, even though not the cause, of horizontal movements and dynamical processes. Therefore, even if isostasy did not directly favor CD, it contributed in making it possible and strengthening other modes of evidence.

Oreskes (2003, p. 11) explains very well that many American geologists endorsed Pratt's model of isostasy, since it was mathematically simpler. Moreover, they produced plenty of useful geological data on the basis of this approach. But if CD were true the crusts would be compressed and therefore more similar to floating icebergs (Airy's model) in spite of the skimming tables. Nonetheless, rejecting CD on the basis of this argument was overtly a sort of consequent fallacy. Indeed, if Pratt's model explained the American data on crust, this should not necessarily mean that Airy's model was incompatible with them! On the other hand, we can say that these data confirm Pratt's model, so that they are in a certain sense against CD. By indicating these data with e_6, we may presume that p(e_6/h)»0.4 and p(e_6/~h)»0.9. In other words, ~h explains these evidences very well, but h is not strongly incompatible with them.

4.3. *Uniformitarianism*

To exclude any use of the Bible as a font of geological data, and thus consolidate geology as a science, British geologist Charles Lyell enunciated, in his seminal *Principles of Geology* (1830-33), a fundamental principle of modern geology, which was called, in 1932 by William Whewell, *uniformitarianism*. This principle could be synthesized in a slogan: the past must be explained on the basis of the present[10]. More explicitly: in order to understand the Earth's past (for instance, geological records such as fossils), we must refer to currently observable processes (present organisms in similar habitats).

According to Oreskes (2003, pp. 11-12), the central point is that American geologists of the thirties, who accepted uniformitarianism, were inclined to reject CD because the latter raised substantial doubts about the possibility that the present really is the key to the past. Indeed, at that time geologists usually used fossil assemblages to make inferences about climate zones. CD, instead, did not necessarily imply that the continents had tropical faunas in tropical latitudes, insofar as the different disposition in the past of oceans and continents could have completely changed faunas. Therefore, according to CD a given geological epoch in the past could have been just a moment in Earth history substantially detached from the present, so that CD limited, if not completely prevented, any inference to the past.

However, as explained by a young Stephen J. Gould (1965), one can distinguish between a *substantive* and a *methodological* uniformitarianism. The latter, which states that fundamental laws do not change with time, is a fundamental principle of science. The former, on the contrary, affirming that the Earth's present situation is similar to that of the past, is now, after having established the validity of plate tectonics, proved false. The problem is that American geologists rejected CD on the basis of a misinterpreted mixture of these two senses, practically by using the correct methodological meaning to erroneously infer the validity of the false substantive meaning. This is one reason why we think that uniformitarianism was not a good justification for rejecting CD.

Moreover, we believe that another more general and insidious factor, a psychological one, surely played a not secondary role in curbing the acceptance of CD: human irrational bias favoring static situations. Think, for instance, of Einstein's difficulty in the twenties to accept Hubble's expanding universe. Again in the fifties a strong hostility arose with respect to the big bang theory, and indeed, many authoritative cosmologists were still inclined towards a stationary state universe. The same troubles have been faced by evolution in biology.

To sum up, in order to be completely fair with respect to anti-Wegenerian geologists, substantive uniformitarianism could at best be a valid motive for giving a low a priori probability to CD. But we have already accepted this point on the basis of the method of multiple working

10. Actually, Lyell was mostly a popularizer of such principle, which was originally proposed in the late 18th century by Scottish geologist James Hutton, and later refined by Scottish scientist John Playfair.

hypotheses, that is we have already given a low probability rate to the CD hypothesis. Anyway, to cautiously not discard completely the negative influences of uniformitarianism and to include the aforementioned "static bias", we further decrease a priori probability so that p(*h*) is no longer 0.1 (as established in section 4.1), but now we have p(*h*)=0.05.

5. Bayesian evaluation

We now have all the probabilities necessary to realize our simple Bayesian evaluation. In our situation, to simplify the calculation we can consider evidence as certain, so we do not have to introduce further probabilities about the fairness of our empirical results. That is, to update our probabilities, we can use equation (1), i.e., a simple conditionalization (see Jeffrey 1965, chap. 11). Introducing into equation (1) all the values found so far in our analysis, we reach the conditional probability

$$p(h/e_1, \dots e_n) = 0.81,$$

i.e., the result is that the updated probability of CD is significantly greater than 0.5. Moreover, when one modifies the values of a priori probabilities a little the result does not change a lot. This means that in the thirties it was not rational to reject CD. At least, it was rational to pursue this hypothesis if not to accept it altogether. To our knowledge, the only scholar who endorses a point of view similar to ours, though on the basis of quite different reasons, is Eldredge (1999 chap. 4).

6. Conclusions

In our opinion the epistemological meaning of this story is not a substantial abandonment of the pursuit of truth in science as maintained by Oreskes (1999), but a strong restatement of fallibilism. In other words, Oreskes attempts to implement into the logic of scientific justification the erroneous argument proposed by American geologists. Following this perspective, it is no longer possible to maintain that one of the principal aims of science is looking for truth. In our opinion, on the contrary, it could happen that science follows a completely wrong route for many years, but this does not compel us to renounce the notion of truth. Since it is in general possible that a whole generation of very good scientists not only maintains an erroneous point of view, but endorses it even in presence of a good justification to change their minds, it is necessary that science policy be oriented to sustain – though, obviously, in a minimal way – research based on heretical points of view. Such a conclusion is also supported by the theoretical physicist Lee Smolin, who, in his book of 2006, emphasizes the unjustified hegemony of string theory in contemporary physics, underlining the importance of promoting divergent ideas in science (Smolin, 2006). In other words, the awareness that current science could remarkably fail – as in the case of CD – suggests to us a strongly open-minded attitude towards our hypotheses.

Acknowledgments

We thank Giorgio Spada and Claudio Calosi for some very helpful comments on an earlier draft of this paper.

References

Chamberlin, T.C. (1890). The method of multiple working hypotheses. *Science*, *7 February*.

Cohen, B. (1985). *Revolution in Science*. Cambridge: Harvard University Press.

Eldredge, N. (1999). *The Pattern of Evolution*. New York: W.H. Freeman and Co.

Frankel, H.R. (1989). The Continental Drift Debate. In H.T. Engelhardt & A.L. Caplan (eds.), *Scientific Controversies: Case Studies in the Resolution and Closure of Disputes in Science and Technology* (pp. 203-248). Cambridge: Cambridge University Press.

Frankel, H.R. (2011). *The Continental Drift Controversy. Vol. I: Wegener and the Early Debate*. Cambridge: Cambridge University Press.

Giere, R.N. (1988). *Explaining Science. A Cognitive Approach*. Chicago; London: University of Chicago Press.

Gould, S.J. (1965). Is uniformitarianism necessary. *American Journal of Science*, *263*, 223-228.

Hallam, A. (1973). *A Revolution in the Earth Sciences*. Oxford: Oxford University Press.

Hallam, A. (1989). *Great Geological Controversies* (2nd edn.). Oxford: Oxford University Press.

Holmes, A. (1929). Radioactivity and Earth movements. *Transactions of the Geological Society of Glasgow*, *18*, 559-606.

Howson, C., & Urbach, P. (1989). *Scientific Reasoning. The Bayesian Approach*. La Salle: Open Court.

Jeffrey, R.C. (1965). *The Logic of Decision*. Chicago: University of Chicago Press.

Jeffreys, H. (1924). *The Earth, Its Origin, History and Physical Constitution* (5th, 6th edn.). Cambridge: Cambridge University Press.

Marvin, U.B. (1973). *Continental Drift: The Evolution of a Concept*. Washington: Smithsonian Institute Press.

Marvin, U.B. (2001). Reflections on the American rejection of continental drift. *Metascience*, *10*, 208-217.

Oreskes, N. (1999). *The Rejection of Continental Drift*. New York: Oxford University Press.

Oreskes, N. (2003). From continental drift to plate tectonics. In N. Oreskes & H.E. LeGrand (eds.), *Plate Tectonics* (pp. 3-27). Boulder: Westview Press.

Romm, J. (~~1992~~). A new forerunner for continental drift. *Nature*, *367*, 407-408.

Segala, M. (1990). *La favola della terra mobile. La controversia sulla teoria della deriva dei continenti*. Bologna: Il Mulino.

Šešelja, D., & Weber, E. (2012). Rationality and irrationality in the history of continental drift: was the hypothesis of continental drift worthy of pursuit? *Studies in History and Philosophy of Science*, *43*, 147-159.

Smolin, L. (2006). *The Trouble with Physics*. Boston: Houghton Mifflin.

Soler, L., Trizio, T., Nickles, T., & Wimsatt, W. (2012). *Characterizing the Robustness of Science*. Berlin: Springer.

Stegenga, J. (2012). Rerum concordia discors: Robustness and discordant multimodal evidence. In L. Soler, T. Trizio, T. Nickles, & W. Wimsatt (eds.), *Characterizing the Robustness of Science* (pp. 207-226). Berlin: Springer.

Sullivan, W. (1974). *Continents in Motion: The New Earth Debate*. New York: McGraw-Hill.

Turcotte, D.L., & Schubert, G. (2002). *Geodynamics*. Cambridge: Cambridge University Press.

Watts, A.B. (2001). *Isostasy and Flexures of the Lithosphere*. Cambridge University Press.

Wegener, A. (1924). *The Origins of Continents and Oceans*. London: Meuthen & Co.