PREDICTIVE NETWORKS AND THE PLATE TECTONICS REVOLUTION

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Abstract

Alfred Wegener's *The Origin of Continents and Oceans* was published in 1915. Therein, Wegener deviated from prevailing *fixist* expectations and argued for the relative displacement of continents across geological time. This hypothesis of continental *mobilism* languished for decades but rapidly became authoritative toward the end of the 1960s due to remarkable predictive successes of seafloor spreading and plate tectonics. In this work, I develop an account of the rapid ascendence of mobilism that is receptive to both the historical contingency and epistemic authority of scientific knowledge. I do this by developing an analytic framework for the assessment of knowledge claims, wherein predictive relationships within a set of commitments can provide epistemic insight into those commitments. I identify fundamental models of prediction testing. In the simplest cases, these models consist of pairs of commitments that either discord or concord with one another. Discordance falsifies a set of commitments and requires problem solving. Alternatively, concordance may provide epistemic support to commitments therein. Scientific knowledge may form predictive networks which consist of sets of partially overlapping concordances. These predictive networks facilitate the isolation of falsification and constrain problem solving. Additionally, the formation of certain kinds of network structures may provide epistemic support to commitments therein, when predictive successes are made particularly remarkable by their networked context. These *snapping together* events can unite previously independent lines of research and may result in the sudden recognition that a growing network is on the right track. I argue that alternative problem solving efforts undertaken by fixists and mobilists contributed to the formation of alternative predictive networks. By the 1960s, the accumulation of constraints during problem solving increasingly required grand modifications to fixist networks. Alternatively, a series of snapping together events – incorporating previously independent research in paleomagnetism, marine geology, and geochronology - supported mobilism in the second half of the 1960s. This resulted in the rapid ascendance of mobilism.

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Chapter 1: Introduction

In this work, I aim to develop a simple, flexible, and broadly applicable view of scientific knowledge as networked. I identify simple generalized models of prediction testing between pairs (or sets) of commitments, based on the capacity for deductive falsification. Partially overlapping predictive relationships form predictive networks. Predictive networks may change or grow due to the identification of discordance within the network and/or the addition of new commitments to the network. I argue that predictive networks have epistemic bearing upon the commitments therein.

I intend for this notion of predictive networks to be sufficiently simple and flexible as to be capable of accommodating highly diverse views on the nature of scientific beliefs and epistemic strength therein. Accordingly, the epistemic bearing of predictive networks that I identify in this work may have broad relevance across languages or more highly prescribed systematic approaches to epistemology.

I develop upon the idea of predictive networks to reach two novel inter-related epistemic insights. First, I develop an enriched programme for the analysis of problem solving and the isolation of falsification. I argue that the Duhem-Quine thesis is a special case of a more general logic of falsification and problem solving. Falsification can be unambiguously isolated in suitable circumstances, and theory choice can be strongly guided by eliminative induction. Consideration of predictive networks facilitates the identification of such circumstances. I argue that as predictive networks grow and as problem solving therein takes place, identifiable logical constraint can accumulate, thereby directing subsequent problem solving efforts and facilitating consensus formation. This accumulation of constraint may eventually demand large scale modifications within a predictive network. Second, I argue that the formation of certain network structures can have positive epistemic significance. The formation of these structures may be impossible or implausible if some commitment(s) therein were false. I call the formation of these structures *snapping together* events. When network structures snap together, this may result in sudden appreciation of profound epistemic strength. Some predictive networks may grow and confront problems that accumulate constraint until large changes to the network are required. Other predictive networks may grow and snap together, thereby raising the epistemic strength of commitments therein.

I apply this view of scientific knowledge to make sense of the plate tectonics revolution and the rapid uptake of continental mobilism toward the end of the 1960s. I examine historical research contexts in paleomagnetism, marine geology, and geochronology leading into the 1960s. I argue that these three strands of research snapped together toward the end of the 1960s, thereby facilitating the formation of plate tectonics and the uptake of continental mobilism. Appreciation of the epistemic significance of a snapping together event often requires awareness of the range of ambiguities and uncertainties involved in the growth of some predictive network(s). Richer knowledge of such ambiguities facilitates stronger judgements of the implausibility of snapping together, were commitments therein false. Accordingly, the historical case study that I provide is quite detailed in order to offer a thorough account of alternative research contexts and the ranges of ambiguities and debates therein.[1]

The plate tectonics revolution is frequently discussed in relation to an extended debate on continental mobilism which began in the early 20th century. My historical approach is somewhat different. Instead of focusing on a single debate spanning multiple decades, I emphasize the disunity of scientific research contexts. Much of the research that eventually contributed to the formation of plate tectonics and the uptake of continental mobilism toward the end of the 1960s initially developed in distinct contexts that were not particularly concerned with the mobilism debate. In the 1960s, these independent research contexts came together in remarkable and unexpected ways, and this proved to be highly relevant to the mobilism debate.

The Mobilism Debate

Alfred Wegener's *The Origin of Continents and Oceans* was published in January 1915.[2] Therein, Wegener proposed a theory of continental drift. His central thesis was that continents undergo large scale horizontal motion, and the relative position of continents has changed across geological time. This general contention may be called *mobilism*.[3] Wegener was not the first scientist to endorse mobilism, but his work, more than that of predecessors, sparked a broad debate.[4]

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[1] This detail also provides a broad range of historical examples of scientific inquiry and debates at varying levels wherein predictive networks may have epistemic relevance. Some of these examples are explicitly isolated and examined for illustrative purposes, but many others are not. Still, the effect is to demonstrate the breadth of potential applications for the analytic framework developed in this work.
[2] Wegener, 1915
[3] I borrow this categorization of *mobilism* and *fixism* as well as *permanentism* and *contractionism* from Frankel (2012). These terms were developed and utilized retrospectively by scientists and historians writing about the debate on continental drift and its subsequent development.
[4] In 1858, Antonio Snider-Pellegrini claimed that centrifugal forces caused the breakup of a supercontinent (Snider-Pellegrini, 1858). Numerous other speculations upon drift were also made in the 19th century and earlier with

At the beginning of the 20th century, the great majority of geologists and those in adjacent fields could be described as *fixists*. Whereas mobilism is the contention that the relative position of continents has changed over geological time, fixism is the contention that continents have not undergone large scale horizontal motion. Into the first decade of the 20th century, fixism was the default implicit position within varied contexts of geological research.[5]

When Wegener wrote *The Origin of Continents and Oceans,* there were two fixist theoretical frameworks within geology with widespread appeal.[6] These frameworks may be called *contractionism* and *permanentism*.

Contractionists maintained that the Earth began in a molten state and cooled and contracted over time. This contraction, and remanent internal heat, drive tectonic processes. Contraction compresses the crust, resulting in horizontal and vertical displacements. Horizontal displacement can result in folding and faulting which may manifest as mountain ranges, island arcs, and other large-scale formations. Contractionists maintained that oceans may form or change position over geological time due to relative vertical displacement of the crust. Apparent global patterns of geological formations, structures, rock types, paleontology, and biogeography, including apparent disjuncts wherein geographically distant regions seemed more similar to one another than to directly adjacent areas could be accounted by such continental subsidence.[7] Austrian geologist Eduard Suess was the leading proponent of contractionism toward the end of the 19th century.[8] He claimed that North America, Greenland, and Europe were previously connected. He also claimed that Africa, Madagascar, India,

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respect to the coastlines of South America and Africa. Osmond Fisher (1882) claimed that the Pacific Ocean was a depression formed when the Moon split from the Earth, and that the Atlantic had rifted as continents shifted (for other accounts of crustal motions associated with the formation of the Moon, see W.H. Pickering (1907), H. B. Baker (1912, 1913, 1914)). Frank Taylor argued for mobilism, as related to mountain formation, in 1910 (Taylor 1910). He proposed that sheets of crust slide across a solid substratum, driven by tidal forces. For more detailed accounts of historical developments of mobilism see: Du Toit, 1937; Hallam, 1973; Marvin, 1973.

[5] Fixism is defined in contrast to mobilism. Thus, fixism was not explicitly defended prior to the rise in popularity of mobilism following Wegener's work. Instead, fixism was implicit in certain global interpretative frameworks.

[6] Most geological research at the time was not global in scope. Accordingly, these global frameworks were often not needed to carry on with research. Each framework was typically recruited to account for large scale geological formations such as oceans or mountain ranges.

[7] Similar disjuncts across contemporary continental margins separated by oceans were most significant.

[8] Suess published substantial works on contractionism between 1883 and 1909 (see Suess 1904-1924 for English translations). A host of other prominent researchers – including Elie De Beaumont, Henry De La Beche, Pierre-Simon Laplace, Georges-Louis Leclerc Comte de Buffon, and Immanuel Kant – had also contributed to the notion of secular cooling and contraction as well.

South America, and Australia were previously connected in a southern continent. According to Suess, the subsidence of continental crust resulted in the formation of the Atlantic and Indian Oceans.

Permanentism was an alternative global theoretical framework, most commonly endorsed in the USA. Permanentists believed that oceans and continents are archaic. Sediment deposition along the coastlines of continents result in the formation of geosynclines, wherein isostatic adjustments due to the weight of accumulated sediment facilitates still more sediment accumulation. The accumulated sediment is then compressed and uplifted by some tectonic process, resulting in the formation of coastal mountains. Continents thereby undergo extended processes of denudation, coastal deposition and accumulation, and subsequent uplift. North America was considered to exemplify these processes, with the Appalachian Mountains along the East Coast demonstrating the archaic presence of the Atlantic Ocean. Permanentists sometimes supposed that the Earth had cooled and contracted, but they did not support the idea of subsumed continents. The most prominent permanentist of the 19th century was American Geologist James Dwight Dana who died in 1895.[9] In the early 20th century, American geologist Bailey Willis championed permanentism.[10]

In the early 20th century, contractionism confronted several notable challenges. It was apparent, especially among American geologists, that central features of contractionism conflicted with isostasy. The accumulation of gravimetric measurements in the 19th and early 20th centuries made it increasingly apparent that the Earth's crust is in (or moves toward) isostatic equilibrium: Denser crust rests atop a substratum at a lower elevation than lighter crust, and changes in density result in compensatory isostatic adjustments toward equilibrium.[11] For Suess, and other contractionists, oceanic crust was deemed to be subsided continental crust. Such subsidence, however, would violate isostatic equilibrium. Additionally, the discovery of radiogenic heat in the early 20th century challenged the fundamental assumption that the Earth was cooling. It was also apparent that mountains were not evenly distributed around the globe, that orogenic activity in Earth's history varied over time, and that the amount of folding that would be required to account for the formation of the Alps would be far greater than contractionism allowed.[12] Permanentism avoided most of these challenges, but confronted others. Most

[9] Dana, 1846, 1881
[10] Willis, 1910
[11] Fisher, 1881
[12] For overviews of permanentism and contractionism see Oreskes, 1999; Frankel, 2012; and especially Le Grande, 1988.

notably, the geological and biotic disjuncts that motivated the contractionist postulation of sunken continents were left unaccounted within permanentism.

Wegener positioned his theory of continental drift as a superior alternative to both contractionism and permanentism.[13] With the permanentists, he endorsed isostasy and the permanence of continents, and he rejected the notion of sunken continents. Against the permanentists, he claimed that oceans were not archaic, but can open as continental drift takes place. The breakup of continents also allowed Wegener to account for biotic and geological disjuncts while avoiding the difficulties that confronted the contractionist approach.

Wegener accumulated evidence for continental drift across several works published between 1912 and 1929.[14] He claimed that continental margins fit together like puzzle pieces, and the biotic and geological disjuncts across continental margins also fit together in a complementary way. This was most apparent in the outlines of South America and Africa. Wegener also argued that paleoclimate inferences from the late Paleozoic indicated extensive glaciation in South America, Africa, India and Australia, while the Northern continents were not only free of glaciation at this time but also contained coal deposits indicative of tropical conditions. For Wegener, such global climate conditions – with glaciers covering most of the Southern Hemisphere while the Northern hemisphere remained temperate and tropical - would not be possible if the continents were in their present positions. Alternatively, less extensive glaciation would be required if the southern continents were collected near the South Pole and covered by a continental glacier. Wegener also attempted to directly measure continental drift geodetically, typically with respect to relative displacement of Greenland, where he undertook several scientific expeditions.

In addition to offering evidence for relative continental motion, Wegener also developed a description of the physical processes of continental drift. He proposed that continental crust is different in composition from oceanic crust. Oceanic crust is solidified substratum, while continental crust is less

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[13] At the time, large theoretical frameworks in physics were in the process of upheaval. Special and general relativity challenged the worldview of classical mechanics. Early work in quantum mechanics (which blossomed especially in the 1920s) was challenging assumptions about the nature of energy and matter. Of course, World War I had also thrown most of Europe into social and political upheaval.

[14] *The Origin of Continents and Oceans* went through four editions (1915, 1920, 1922, 1929). The 3rd edition was translated into English in 1924 (Wegener, 1924). Wegener first sketched his argument for mobilism in 1912, and he published a monograph on paleoclimates with Wladimir Köppen in 1924 in an effort to infer paleolatitudes from paleoclimate traces including glacial tills, tropical coals, salt deposits and gypsum, and desert sandstones (Köppen and Wegener, 1924). Wegener used his work on paleoclimatology in the fourth edition of his book in an effort to reconstruct Pangaea.

dense. Though continental crust is less dense than oceanic crust, oceanic crust is not as strong as continental crust. Over extended periods under stress, oceanic crust behaves like a viscous fluid. Continents drift by plowing through the oceanic crust. At the leading edge of a continent, the continental crust and oceanic crust push into one another. Coastal mountains are a manifestation of the crumpling of continental crust as it pushes against oceanic crust. Wegener offered several possible forces that might account for continental drift, including tidal action, magnetic effects, and currents beneath the crust. The mechanism that he developed most clearly was that rotation of the Earth forces continental mass toward the equator, thereby resulting in continental drift away from the poles of rotation.

Initial reception to Wegener's work was mixed. Notable objections pertain to Wegener's description of the physical processes of drift.[15] Oceanic crust was often considered to be denser and stronger than continental crust, so it seemed highly implausible that continents could plow through oceanic crust.[16] Even if this were possible, crumpling at the leading edge of continents should be far greater than observed, and the lagging edge of continents did not exhibit the tectonic or volcanic activity that might be expected from Wegener's account. Wegener's claim that the continents were forced toward the equator due to rotation of the Earth was also problematic. This force was widely considered to be insufficient, especially given Wegener's estimated drift rate. Additionally, this force would not account for the relative motions that Wegener endorsed.[17]

Several other factors may have blunted the impact of Wegener's work. Wegener was trained as a meteorologist, yet his argument was primarily directed toward topics in geology.[18] The evidence that Wegener amassed in support of continental drift was almost entirely selected from other researchers whose work was outside of Wegener's expertise and who did not endorse drift. Accordingly, Wegener

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[15] Wegener's geodetic measurements changed across the four editions of his work and were often deemed to be unreliable. For an account of Wegener's geodetic measurements, and objections, see Longwell, 1944.
[16] In *The Rejection of Continental Drift*, Naomi Oreskes claims that adherence to Pratt isostasy resulted in the expectation in America that the Earth's crust is not sufficiently strong as to withstand forces that would be required for extensive lateral motion (Oreskes, 1999). Alternatively, Wegener endorsed Airy Isostasy wherein continents have large roots, created and sustained by lateral pressure. Oreskes thereby argues that alternative views on isostasy inhibited the uptake of continental drift among American researchers.
[17] British geophysicist Harold Jeffreys was likely the most prominent scientist to extensively criticize Wegener's mechanisms (Jeffreys, 1924; also see Lambert 1921, 1923; Longwell 1928). Jeffreys remained an opponent of mobilism until his death in 1989.
[18] Mott Greene characterizes Wegener as a highly competent physicist and claims that Wegener was trying to situate geophysics as the foremost authority on the study of Earth (Greene, 2015). This, Greene claims, had very limited influence upon geologists.

may not have proved himself to be sufficiently competent to understand the nuances involved in the interpretation of the geological and paleontological evidence that he propounded. Similarly, the charge could be laid that Wegener selected only the evidence that supported his theory, while ignoring all evidence to the contrary, which could be viewed as a violation of methodological and professional standards. In *The Rejection of Continental Drift*, Naomi Oreskes argues that American geologists endorsed a methodology of multiple working hypotheses.[19] Championed by highly influential American geologist Thomas Chrowder Chamberlin, the method of multiple working hypotheses demanded Baconian induction, theoretical pluralism, and weighing of observation against alternative explanatory theories. Oreskes claims that American geologists viewed Wegener's work as violating these methodological standards, thereby resulting in the impression that his work was unscientific.[20]

The two most significant defenders of mobilism, following Wegener, were South African geologist Alexander Du Toit[21] who expanded geological and paleontological support, and British geologist Arthur Holmes[22] who developed theory pertaining to the physical processes of mobilism. Other notable proponents of mobilism included Russian climatologist Wladimir Köppen who worked closely with Wegener on paleoclimatology,[23] British climatologist George Clarke Simpson,[24] Swiss geologist Émile Argand who developed a mobilist account of orogeny,[25] Dutch geologist William A. J. Van der Gracht,[26] British botanist and paleontologist Albert Charles Seward,[27] and Canadian geologist Reginald

[19] Oreskes, 1999
[20] Homer Le Grande likewise argues that Wegener's work was viewed as a violation of inductivism (Le Grande, 1988). Oreskes also claims that continental drift conflicted with American acceptance of uniformitarianism, as illustrated in the work of Charles Schuchert (Oreskes, 1999). Oreskes is forced to define a peculiar sort of uniformitarianism to make this argument, especially because Wegener's geodetic work was clearly aligned with uniformitarianism. As previously noted, she also highlights that American adherence to Pratt isostasy conflicted with Wegener's account.
[21] Du Toit began defending mobilism in the early 1920s and continued publishing on the subject until his death in 1948. In 1927 he published a comparison of the geology of South America and Africa, based on his own field work, and argued that the geological patterns between the two continents strongly supported mobilism (du Toit, 1927). In 1937 he published *Our Wandering Continents,* wherein he argued for mobilism by elaborating upon Wegener's lines of evidence, especially with respect to geological and biotic disjuncts across continental margins (du Toit, 1937).
[22] Arthur Holmes will be introduced Chapter 4. He endorsed mobilism by the end of the 1920s (see Holmes, 1929).
[23] Wegener married Köppen's daughter in 1913 (Greene, 2015).
[24] Simpson 1929, 1930
[25] Argand, 1924
[26] Van der Gracht, 1931
[27] Seward and Conway, 1934

Daly, the most notable early proponent of mobilism in North America.[28] Each of these researchers made notable contributions to the mobilism debate between the 1920s and 1940s.

Wegener's supporters offered alternative accounts of the physical processes of drift. In *Our Mobile Earth,* Daly proposed that oceanic and continental crust are both rigid, but the substratum beneath continental crust is glassy malleable basalt of slightly less density than oceanic crust.[29] Continental crust may become domed, which facilitates the formation of geosynclines. The growth of geosynclines may then result in increasing tension upon continental crust, which may fracture and then "slide" toward the geosyncline.[30] Alternatively, Van der Gracht recruited a theory of thermal cycles, proposed by John Joly, to account for mobilism. Joly proposed that oceanic and continental crust floats atop a malleable basaltic substratum.[31] Tidal forces encourage westward motion of continents, relative to the Eastward motion of the substratum. Crust will be differentially affected by these forces, based on the depth with which the crust sits in the substratum. Geosynclines on the west side of continents become compressed because of this. Van de Gracht expanded on Joly's work by proposing that the oceanic crust may be sufficiently weak to facilitate relative continental motion.[32] During the 1930s, Arthur Holmes developed a sophisticated account of the physical processes responsible for mobilism.[33] He proposed that the substratum was a viscous fluid, heated by radioactive decay. Differential heating results in convection currents that rise beneath continents and then diverge outward. These convection currents carry continental crust along with them, resulting in tension, fissuring of continental crust, and eventually the formation of ocean basins.[34] The continents do not plow through oceanic crust, but overthrust it. Oceanic crust flexes downward as continents advance, resulting in ocean trenches. The overridden oceanic crust metamorphically increases in density, and magma lubricates the continent's passage. Where opposing convection currents meet, they descend, resulting in subsidence of the crust. This may result in geosyncline formation, which may then become compressed at the leading edge of the continent.

[28] Daly, 1923, 1926
[29] Daly, 1926, 1929a
[30] Daly's support for mobilism declined by the 1930s.
[31] Joly 1923, 1925, 1930
[32] Van der Gracht, 1928
[33] Holmes, 1928, 1929, 1931, 1933
[34] In 1945, Holmes proposed that upwelling basalt could fill the tensional fissures that would be produced in the stretched continental crust (Holmes, 1945).

Du Toit incorporated Holmes' convection currents into his book, *Our Wandering Continents.*[35] Harold Jeffreys, who strongly objected to other proposed mobilist processes, disagreed with Holmes, but considered his work to offer the best physical defense of mobilism. [36][37]

Early followers of Wegener often considered evidence from paleontology and paleoclimatology to offer the best case for mobilism. During the 1930s and 1940s, especially, fixists were able to integrate much of this relevant data. As previously noted, contractionists could account for paleontological data by postulating the subsidence of continents or land bridges. However, this violated isostasy, and attempts to identify the location and duration of such land connections raised a host of additional problems.[38] Alternatively, permanentists could not readily account for biotic disjuncts across continents. In 1932, American geologist Bailey Willis and paleontologist Charles Schuchert argued that slender isthmuses of oceanic rock might rise from the ocean floor to briefly link continents and then subside.[39] These links could account for paleontological and biogeographical patterns, without violating isostasy. In 1940, prominent American paleontologist George Gaylord Simpson convincingly argued that only a small number of such isthmian links would be sufficient to account for all mammalian paleontology.[40]

Meanwhile, Wegener's paleoclimate interpretations were challenged in several ways. If the southern continents were collected around the South Pole, as Wegener proposed, then where did the moisture come from to feed such massive glaciers?[41] The identification and interpretation of glacial tillites in the Southern Hemisphere were also debated. Whereas Wegener claimed that North America did not show traces of Permo-Carboniferous glaciation, others argued that glacial tillites were apparent.[42] Additionally, apparent Permo-Carboniferous glaciation in the Southern Hemisphere could be accounted within fixist paleogeographic reconstructions that were developed in the 1930 and 1940s. Differential subsidence or uplift could adjust landmass distributions and ocean currents in such a way as

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[35] Du Toit, 1937
[36] Jeffreys, 1935
[37] The notion that convection currents could drag portions of the crust along with them was subsequently taken up by Felix Vening-Meinesz and others, especially in marine geology. Vening-Meinesz did not endorse mobilism. This will be elaborated in Chapter 3.
[38] This will be examined in Chapter 7. Common difficulties included the following: Subsidence would influence sea levels, connections across continents would influence global and local climate, postulated connections would not be of suitable climate to account for biotic interchange, biotic interchange was not apparent where it should be or was apparent when it shouldn't be.
[39] Willis, 1932; Schuchert, 1932
[40] Simpson, 1940
[41] Coleman, 1933
[42] Brooks, 1926

to result in glaciation at lower latitudes in the Southern Hemisphere along with abnormal warmth in the Northern Hemisphere.[43]

By the beginning of the 1950s, support for mobilism was likely at its lowest point.[44] Many of the strongest proponents of mobilism had died,[45] including Wegener (1880-1930) and Du Toit (1878-1948). Arthur Holmes was the most notable surviving proponent of drift, but his confidence in mobilism had waned. The paleoclimate and biotic evidence for drift that seemed most convincing earlier in the century, now seemed far more equivocal.[46] Meanwhile, permanentists could incorporate biotic patterns into their framework by postulating isthmian links, and updated theories of contractionism offered by Jeffries and others could incorporate isostasy and radioactivity.[47]

During the 1950s, two important developments took place. First, a group of researchers in the specialized field of paleomagnetism began to use mobilism to interpret their measurements. As molten rock solidifies, magnetic minerals become oriented parallel to the ambient magnetic field. If the Earth's magnetic field is predominantly dipolar and typically oriented toward the axis of Earth's rotation and magnetic minerals remain frozen in place following solidification, then such paleomagnetic properties of rocks may provide insight into their geographic position at the time of rock formation. In the second half of the 1950s, a group of mainly British paleomagnetists endorsed mobilism in order to account for patterns of paleomagnetic measurements within and between continents. Some researchers in the field objected to mobilist interpretations, and mobilist interpretations among paleomagnetists had little influence outside of their speciality.[48]

The second important development was that research in marine geology resulted in growing appreciation that oceans and continents are geologically distinct, and that ocean basins seemed to be significantly younger than continents. Not only is oceanic crust denser, thinner, and comprised of

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[43] Brooks 1926, 1949; Shuchert 1932

[44] In *The Continental Drift Controversy*, Henry Frankel offers a detailed account of developments in the mobilism debate from the early work of Wegener to the 1950s (Frankel, 2012). He examines the initial reception of drift, the promising case for mobilism in the 1920s, and waning support thereafter despite theoretical progress pertaining to the physical processes of drift. Frankel also emphasizes that Holmes' work had limited influence. Subsequent work of Harry Hess and Robert Dietz on seafloor spreading was apparently developed independently from Holmes', despite notable similarity. This may go to show some of the influence of regionalism on the mobilism debate: Arthur Holmes was British while Hess and Dietz were American.

[45] Argand (1879-1940), Van der Gracht (1873-1943), Seward (1863-1941), Köppen (1846-1940)

[46] Holmes, 1953

[47] Jeffreys, 1929

[48] During the 1960s, paleomagnetic measurements and interpretations were increasingly recruited outside of the speciality itself, in order to support mobilist interpretations elsewhere.

different rock than continental crust, but there are also geological formations like trenches, ridges, fracture zones, basins, and guyots that are typical to the seafloor but not continents. Additionally, accumulated sediment on the seafloor was far thinner than would be expected if ocean basins were permanent features of crust, and all fossils obtained from these sediments were no older than the Cretaceous. The distinct features of the seafloor, and its apparent youth, deviated from expectations of both permanentists and contractionists and resulted in growing efforts, toward the end of the 1950s, to develop accounts of the physical processes responsible for the unique features of marine geology. Some of these accounts incorporated mobilism.

Near the end of the 1950s, mobilism was again a lively topic of debate, but outside of paleomagnetism, very few researchers strongly endorsed mobilism. Australian geologist Samuel Carey endorsed mobilism in the 1950s and developed a related theory of Earth expansionism.[49] Most other defenses of mobilism (outside of paleomagnetism) in the early 1960s were tentative. American marine geologist Bruce Heezen defended expansionism with varying degrees of conviction.[50] Marine geologists Harry Hess, Robert Dietz, and Henry Menard endorsed theories with mobilist connotations, but these theories were proposed within research contexts that participants recognized to be speculative. Hess famously called his theory of seafloor spreading “geopoetry” and Menard’s interest in mobilism vacillated in the early 1960s.

In the second half of the 1960s, a remarkable transformation took place in the mobilism debate. Support for mobilism rapidly expanded. Tentative defenders became strongly convinced of mobilism. Opponents of mobilism changed their convictions, sometimes very rapidly, to endorse mobilism. In marine geology, many prominent long-time permanentists rejected their previous research framework and endorsed mobilism. In paleomagnetism, those who had previously dissented against mobilist interpretations endorsed mobilism. Young geophysicists began to endorse mobilism as well. By the early 1970s, the mobilist theoretical framework of plate tectonics resulted in widespread reconsiderations

[49] Carey, 1958
[50] Heezen, 1957, 1959a

and reorganization across the Earth sciences. [51][52] This is frequently described as a recent scientific revolution.[53]

Argument and Outline

To what can the profound change in the reception of mobilism in the second half of the 1960s be attributed?

The answer that I provide to this question is interesting in two ways. First, I grant mobilism privileged epistemic status. I argue that in the second half of the 1960s, mobilism became indisputable for a rapidly expanding group of researchers. Contractionism and permanentism were both highly successful and widely employed theories, but neither obtained the apparent epistemic standing obtained by mobilism. Second, to account for this privileged epistemic status of mobilism, I develop a general description of the growth of scientific knowledge, building upon a broad collection of ideas relevant to the appraisal of epistemic strength via prediction testing. Within this description, scientific knowledge may be highly contingent, ambiguous, and diverse, yet this contingency, ambiguity, and diversity may facilitate the formation of epistemically strong conclusions.

I argue that in the second half of the 1960s, research in paleomagnetism, marine geology, and geochronology came together in support of mobilism. These alternative specialities, with their own sets of methods, instruments, objects of study, aims, theories, researchers, ambiguities, and debates yielded a series of predictive successes that would have been incomprehensible, were mobilism false. It is this predictive success to which I attribute the rapid uptake of mobilism in the second half of the 1960s.

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[51] Plate tectonics is a kinematic theory. Therein, the Earth's crust is deemed to be comprised of several rigid plates atop a viscous fluid substrate. Oceanic crust is thinner and denser than continental crust, however, continental plates may be comprised of either, or both types of crust. The plates are in relative motion with one another. Continents are embedded within a plate, so as the plates move, the continents do as well. There are different types of plate boundaries. Where plates diverge, new crust is created. Where plates converge, oceanic crust may be subducted, while collision of continental mass may result in compression, folding, and mountain formation. Applying this notion of crustal plates to spherical geometry facilitates assorted predictions pertaining to relative plate motions.

[52] Mobilism resulted in a theoretical reorganization and unification of many fields across the Earth sciences. Le Grande argues that this reorganization was, in part, methodological. Inductivism gave way to hypothetico-deductivism (Le Grande, 1988). Oreskes, however, would argue that this transition was already well underway, and that methodological changes facilitated the theoretical changes (Oreskes, 1999).

[53] John Stewart favors a Kuhnian account of plate tectonics but offers a modified Kuhnian framework involving overlapping paradigms within a field, which became unified under plate tectonics (Stewart, 1990).

This work is divided into two parts. Part 1 provides a detailed historical case study on the origins and development of research that was most influential to the surging acceptance of mobilism in the second half of the 1960s.

In Chapter 2, I provide a brief history of the study of paleomagnetism leading into the 1960s. Paleomagnetism is the study of rock magnetism. The orientation of magnetic minerals during rock formation may be influenced by ambient magnetic fields. Accordingly, the study of rock magnetism can provide insight into the history of the geomagnetic field. In the 1950s, paleomagnetism became a sophisticated speciality, centered around research institutions at Cambridge and Imperial College London in the UK, the Carnegie Institution, UC Berkeley and the USGS Menlo Park in the USA, and ANU in Australia.[54] Paleomagnetic measurements upon a sampled rock could be used to infer the relative position of the geomagnetic pole at the time of rock formation. Such measurements indicated that geomagnetic paleolatitudes (the latitude of a physical location at some point in the past) could drastically change over geological time, and that rock of similar age within a continent tended to show clustering of paleopole positions (the position of the north magnetic pole at some point in the past, typically relative to contemporary latitude and longitude) that deviated significantly from measurements between continents. Several prominent researchers, especially in the UK and Australia, endorsed mobilism to account for these measurements. Several prominent American researchers objected to mobilist interpretations. Alternative interpretations included the possibility that the geomagnetic field was not always dipolar, that the geomagnetic poles wander over time, or that paleomagnetic measurements were not generally reliable. It was also apparent from paleomagnetic measurements that rock magnetism within a column seemed to reverse periodically. Some researchers endorsed the notion that the Earth's magnetic field reversed over geological time, while others proposed that reversals of rock magnetism were due to petrological properties.

Chapter 3 examines the history of marine geology leading into the 1960s. The growth of marine geology as a field of study is closely tied to the development of instruments and techniques that facilitated bathymetric, gravimetric, seismic, heat flow, and magnetic measurements. Many of these instruments became widely produced and used during and after World War II. The Cold War also contributed to increased funding and opportunity for strategically useful kinds of marine and seismic research. Consequently, centers for the study of marine geology proliferated. Notable research centers were located at Columbia and UC San Diego in the USA, and Cambridge and the National Physical

[54] Notable research was also undertaken elsewhere, especially in Japan, France, and the Soviet Union.

Laboratory the UK. Notable researchers also worked out of Princeton, the US Navy Electronics Laboratory, the University of Toronto, the California Institute of Technology, and Woods Hole Oceanographic Institution in Massachusetts.[55] Through the 1950s, researchers primarily focused on data acquisition and pattern identification. Marine geology was recognized to be distinct from continental geology. The ocean crust was thinner and denser than continental crust, and the seafloor seemed to have several unique geological features not found on continents. Marine magnetic anomalies were also identified, and typically considered to reflect variations in seafloor petrology. It was also increasingly apparent that the seafloor was cover in relatively young sediment. Toward the end of the 1950s, researchers began to theorize about large scale physical processes that could account for the features of marine geology and their relation to continental geology. Seafloor spreading was one such proposal. The seafloor spreading hypothesis postulates that new oceanic crust is created at mid-ocean ridges and spreads outward in either direction, creating ocean basins. At trenches, oceanic crust may plunge back into the substratum.

In Chapter 4, I provide a history of geochronology into the 1960s, with emphasis upon the development of radiometric dating methods. Toward the end of the first decade of the 20th century, radioactivity was often deemed to be the result of spontaneous atomic decay, resulting in the transmutation of a parent element into a daughter element (later refined to isotopes). The daughter element could itself be radioactive as well. It also seemed that the rate of decay was unique and constant for each radioactive element. Researchers quickly recognized that the ratio of radioactive parent to radiogenic daughter elements within a closed system could provide insight into the age of that system, assuming that decay rates are constant. Much of the subsequent development of radiometric dating into the 1960s pertains to the refinement of these assumptions and measurements and the identification of conditions wherein such assumptions and measurements are optimized for rock dating. A general method used to establish the reliability of radiometric dating methods was by corroboration against other dating methods such as relative geological age. Accordingly, establishing the reliability of a radiometric dating method typically required rock samples of known geological age and of suitable isotopic constitution. Since different radioactive isotopes decay at different rates, and ratio measurements can be of varying precision depending on the isotopes involved, many different radiometric dating methods were pursued. This could expand the range of rocks that could be dated and thereby encompass a greater extent of the geological timescale. In the 1930s and 1940s, mass

[55] Cal Tech was also the leading center for seismological research.

spectrometry greatly improved the precision of isotopic ratio measurements. Aided by the development of a static-mode spectrometer that could measure trace quantities of argon isotopes, potassium-argon dating developed during the 1950s and 1960s. With an intermediate decay rate, and capacity for highly precise ratio measurements, potassium-argon dating of ancient samples could be corroborated by lead dating methods while younger samples could be corroborated by radiocarbon dating. By the 1950s, centers for potassium-argon dating research included UC Berkeley, the University of Chicago, the Carnegie Institution in Washington D.C., and later, USGS Menlo Park and ANU in Australia.

In each of these three chapters, I aim to account for the historical development of the field and the general state of research leading into the 1960s. Accordingly, I identify the aims, questions, and problems in the field, and how these developed over time in concert with interpretative structures, practices, instruments, and objects of study. I pay particular attention to functionality and assumptions associated with measurement instruments and methods and the theoretical frameworks that organize such measurements. With this emphasis, I hope to highlight the diversity and plurality of views within each field, the broad range of problems and debates therein, as well as the ambiguities and uncertainties involved in measurement, interpretation, and problem solving. With this historical approach, it becomes apparent that the process of knowledge formation in each speciality is highly contingent, that problems are often worked through rather than definitively resolved, and that ambiguities and debates within any one speciality were largely distinct from the ambiguities and debates elsewhere.

In Chapter 5, my historical aims are somewhat different. Rather than thoroughly examining the debates and ambiguities across multiple fields, I highlight certain interdisciplinary research that combined research in paleomagnetism, marine geology, and geochronology, and contributed to the surging support for mobilism in the second half of the 1960s. In particular, researchers at USGS Menlo Park and ANU combined potassium-argon dating methods with paleomagnetic measurements in an effort to construct a *geomagnetic reversal timescale*, and thereby test the hypothesis of geomagnetic field reversals. The first of such efforts were published in 1963 and refined thereafter. Meanwhile, researchers at Cambridge combined the geomagnetic reversal hypothesis with seafloor spreading in an effort to account for the origin of marine magnetic anomalies. The *Vine-Matthews hypothesis* was first published in 1963 and developed thereafter. In 1966, the geomagnetic reversal timescale was used to model marine magnetic anomalies, and this modeling was found to correspond remarkably well with measurement. This convinced several prominent researchers– including long-standing opponents of mobilism in multiple fields - of seafloor spreading. The pattern of magnetic anomalies could then be used to infer seafloor spreading rates. Meanwhile, apparent interconnections between marine geological formations, along with improved understanding of seismic properties of the Earth's crust, contributed to the hypothesis that the Earth's crust was comprised of large rigid plates with certain types of boundaries defined by relative motion. This notion of rigid plates, combined with seafloor spreading, predicted that the relative motion at ridge offsets was opposite to general expectation. This new class of fault was dubbed a *transform fault* and was first described in 1965. When the hypothesis of crustal plates is applied to the geometry of a spherical surface, measured seafloor spreading rates at plate boundaries can be used to predict geological features and relative plate motions elsewhere. The first such work in plate tectonics was published in 1967. The predictive successes of plate tectonics convinced many researchers across the earth sciences of mobilism. In the second half of the 1960s, support for mobilism grew rapidly, long standing opponents of mobilism rejected their previous interpretations, there was a flurry of predictive successes with wide ranging consequences, and researchers expressed shock and amazement at ongoing developments in the field. Mobilism was still known to confront difficulties, but the apparent quality of these difficulties changed. Difficulties that had previously seemed highly significant, or insurmountable, were reconceived as difficulties that would eventually be solved within a mobilist framework. My contention is that the surging support for mobilism in the second half of the 1960s was due to the manner in which paleomagnetism, marine geology, and geochronology came together in support of mobilism. In Part 2, I develop an analytic framework to make sense of this.

Unlike most other histories of the mobilism debate which trace an overarching narrative from Wegener through plate tectonics, I emphasize that much of the research that contributed to the eventual uptake of mobilism developed in independent research contexts, often without consideration of mobilist implications.[56] Paleomagnetists only began to debate and endorse mobilism toward the middle of the 1950s, and research on polarity reversals did not, at first, seem to have any bearing on mobilism at all. Indeed, several researchers who spearheaded the development of a geomagnetic reversal timescale – which eventually was instrumental in the confirmation of seafloor spreading and the development of plate tectonics – were openly opposed to mobilism and did not consider their research to have relevance to mobilism until well into the 1960s. Mobilism was not a topic of sustained

[56] William Glen's *The Road to Jaramillo* also emphasizes how potassium-argon dating and the geomagnetic reversal timescale contributed to accelerating support for mobilism in the 1960s yet developed in research contexts that were mostly divorced from the mobilism debate (Glen, 1982).

debate in marine geology until the end of the 1950s. Paleomagnetic mobilist interpretations had virtually no influence upon mobilist interpretations in marine geology until the 1960s. Mobilism had no bearing upon the development of radiometric dating at all. During the 1960s, research in paleomagnetism, marine geology, and geochronology came together in support of mobilism, and the remarkable effect that this had upon the reception of mobilism was partly due to the historical independence of these lines of inquiry. Historical focus on the development of a "mobilism debate" may thereby omit important details and may result in a somewhat skewed portrayal of the eventual uptake of mobilism and the contours and growth of scientific knowledge more generally.

At the end of Chapter 5, I review alternative attempts to make sense of the mobilism debate, with particular attention directed to Henry Frankel's *The Continental Drift Controversy,* the most extensively researched and detailed history of the mobilism debate.[57] Frankel conceives of science as a problem solving enterprise. Proposed solutions may confront difficulties such as discordance with data or theoretical incompatibility. A central feature of Frankel's interpretative framework is the notion of a *difficulty-free solution* wherein apparent difficulties that confront a proposed solution become adequately resolved for centrally involved researchers. When a difficulty-free solution is reached, outstanding difficulties no longer weigh against a proposed solution and instead become new problems. The analytic framework that I develop in Part 2 builds upon Frankel's notion of the difficulty-free solution. Whereas Frankel emphasizes the empirical matter of whether a difficulty-free solution is reached and historical events that contributed to consensus formation, I aim to account for *why* researchers might find a proposed solution sufficiently convincing as to form consensus.

Part 2 consists of three chapters and develops an account of the growth of scientific knowledge. Scientific knowledge may be represented as a set of accepted statements.[58] Different individuals and groups may endorse alternative sets of statements, and the set of statements representative of scientific knowledge may change. Some individual or group may form a new statement, or change an old statement, and may then try to convince others of this modification. However, scientists may disagree on which statements are best, and this may result in debate.

As a first approximation, the resolution of such debates may be conceived in the following way. All statements within the set of accepted statements are independently justified. Empirical statements

[57] Frankel, 2012

[58] I also refer to such statements or sets thereof as propositions, beliefs, assumptions, expectations, commitments, postulations, or convictions.

are self justified. Generalizations obtain justification in relation to empirical statements. Epistemic principles may then be developed that distinguish better or worse generalizations, such that some statements may be more strongly secured than others. The set of statements that comprise scientific knowledge may change over time as new observations are made, as new generalizations are formed, and as mutually exclusive statements jockey for epistemic superiority.

Across the three chapters of Part 2, I aim to challenge and elaborate upon this first approximation in order to develop an enriched account of the growth of scientific knowledge.[59] My general approach is to examine the epistemic bearing of predictive inter-relationships between statements.[60] When this is done, knowledge may still be conceived of as a set of commitments, but epistemic insights proliferate, and many features of the first approximation – like the distinction between empirical and non-empirical statements – fade away.

The analytic framework developed in Part 2 is highly flexible and broadly applicable. I do not specify a particular logical language, though I do rely on certain logical concepts such as entailment, conjunction, contradiction, and modus tollens.[61] I construe the nature of statements quite broadly. Statements may include empirical statements and theoretical statements, generalizations, hypotheses, expectations about reliability, notions of functionality of instruments, and so on.[62] Even when statements are not clearly stated, nor discussed, we may conceive of implicit statements, or knowledge that could be expressed as statements, even if that is not explicitly done. I also construe the nature of predictive relationships quite broadly. Many sorts of statements may be construed as forming predictive relationships, even if they are not typically treated as predictive. For example, measurements may be a constituent within an entailed relationship. Measurements themselves might also be construed as entailing prediction. Commitments pertaining to the functionality and reliability of a measuring instrument, along with some instrumental output, may entail some prediction pertaining to the presumed target of measurement.[63] Predictive relationships may also include virtual statements and

[59] It is not my contention that this first approximation is presently widely endorsed in the study of science. Rather, this first approximation is used for heuristic value.
[60] Though I emphasize the epistemic significance of prediction testing, epistemic principles may be developed around alternative relationships such as explanatory relationships or classificatory relationships.
[61] I use the term “entailment” to follow conventions in the philosophy of science when referring to prediction derivation. The more general terms “implication” or “logical consequence” may be substituted.
[62] Statements may also pertain to researchers, politics, morality, religion, and so on.
[63] A prediction entailed by measurement with a thermometer would thereby pertain to the presumed temperature of the target that is being measured. This entailed outcome could then be tested through alternative means or might enter into still other entailed relationships with other accepted statements.

may vary in precision. Some scientific pursuits may be described as more deductive than others and prediction may thereby be deemed to play a larger role in some areas of scientific knowledge than others. Philosophers or historians, for example, often describe geology and paleontology as historical or descriptive sciences, rather than deductive or predictive. Practicing scientists in one field may explicitly endorse a hypothetico-deductive methodology while hypothetico-deductivism may be eschewed elsewhere. Regardless, entailed relationships between accepted statements may be identified across all sorts of scientific pursuits, even those that may not be deemed to heavily rely on predictive methodology.

In Chapter 6, I argue that certain kinds of predictive relationships between statements have epistemic significance. I identify a model of prediction testing that I argue is responsible for several common assumptions in the epistemology of science, though these assumptions have been widely critiqued. I identify alternative models of prediction testing, based on the possibility of deductive falsification, which I call *Models of Discordance* and *Models of Concordance*. Within these models, a prediction may be entailed by a conjunction of statements, a prediction may be tested directly against an alternative statement (regardless of whether this statement is empirical), and a prediction may be tested against an alternative prediction. I then use these models to organize alternative collections of literature on epistemology of science. This chapter thereby offers a synthesis of diverse views in the epistemology of science, including theories of confirmation, predictive holism, problem solving, and certain views on consilience, coherence, and experimental knowledge. I argue that discordances between accepted statements result in ambiguous falsification, while concordances may increase the epistemic strength of commitments therein, given that these commitments have some independent epistemic support. When concordances partially overlap, they form structures that may be called *predictive networks.* This chapter thereby develops upon the first approximation of scientific knowledge by identifying certain sorts of relationships between commitments as having epistemic bearing upon those commitments. Sets of commitments may form networks of concordances, while discordances pose problems that require the modification of that network. Answers to epistemic questions may depend upon the way in which such a network is dissected and analyzed.

In Chapter 7, I develop an enriched account of the analysis of falsification and problem solving. The apparent ambiguity of falsification, first identified in Chapter 6, is typically examined with respect to empirical refutation and predictive holism. Due to this apparent ambiguity of falsification, problem solving is not logically determined. Consensus, however, is common in the history of science. Some non-logical factors are thereby often believed to be required to account for consensus formation and scientific change. I argue that when the ambiguity of falsification is clearly defined, it becomes apparent that this ambiguity can be logically narrowed in several ways. Most notably, if the statements contained within a falsification problem entail multiple predictions, then problem solving is not just accountable to a single refutation, but also to avoidance of any inseparable refutation as well. The Models of Discordance and Concordance are used to flesh out such notions of prediction and inseparable refutation. Upon initial emergence of a falsification problem, the ambiguity of falsification may be great, but restrictions upon this ambiguity may become apparent over time as problem solving efforts take place. These restrictions may limit the flexibility of a set of statements with respect to one another, which may result in problems that can only be solved by highly disruptive adjustments. Narrowing the ambiguity of falsification may, in the strongest cases, logically falsify specific statements, even grand theoretical frameworks. In weaker conditions, problem solving trajectories may be eliminated and the superiority of one proposed solution over an alternative may be definitively established. Accordingly, consensus formation during problem solving does not necessarily require non-logical factors. This chapter develops upon the first approximation of scientific knowledge by identifying how relationships between statements may constrain permissible problem solving and facilitate the isolation of falsification therein.

In Chapter 8, I argue that network structures can provide epistemic support to commitments therein, in excess to that which may be apparent when examining constituent commitments or concordances individually. When such structures form, they can be said to *snap together*. In the strongest possible cases, a network structure may preclude the falsity of some commitment(s) therein. Establishing such impossibility requires knowledge of the commitments and concordances that comprise the structure, as well as additional stringent conditions. In somewhat less stringent conditions, falsity within a network structure may be deemed implausible. Such implausibility is apparent when the formation of a small or large network structure requires coincidental concordance between distinct directions of network growth, as may be apparent when the ambiguities and debates involved in such research do not overlap. Thus, a full account of epistemic support within a set of accepted statements, requires consideration not only of the relationships between those statements, but also the relationships between those relationships. A holistic view of predictive networks can provide epistemic insight that would not otherwise be apparent.

Chapter 7 includes a case study that elaborates upon fixist interpretations of biotic disjuncts leading up to the 1960s. I show that attempted problem solving resulted in the accumulation of constraint within fixist frameworks, especially due to the study of marine geology in the 1950s. In Chapter 8, I examine the snapping together of mobilism during the 1960s. Together, I argue that some networks may grow and may snap together in series, as demonstrated by networks associated with mobilism in the second half of the 1960s. Other networks may grow, but as discordances arise and as problem solving takes place, these networks become less flexible and accumulate constraint, eventually requiring the modification of large sections of that network, as demonstrated by networks associated with fixism in the 1950s. Mobilism has privileged epistemic status because it was integral to a series of snapping together events, and this resulted in surging support for mobilism and eventual consensus formation.

The account developed in Part 2 thereby emphasizes the epistemic importance of independently supported concordant statements. Diversity and plurality in scientific research are conducive to the formation of this kind of epistemic support. Diversity and plurality in problem solving efforts also has bearing upon the accumulation of constraint and the capacity to isolate falsification and reach consensus. Additionally, identifying the range of ambiguities and debates within scientific research facilitates the recognition of implausible snapping together events. Accordingly, close attention to historical research context is required to apply and assess the expanded vision of scientific knowledge that I develop across this work. Richer accounts of research contexts may thereby facilitate more intricately detailed insights into epistemic support therein. Though I emphasize grand interpretative frameworks within this work (fixism and mobilism), the image of scientific knowledge that I develop may apply elsewhere.

Part 1: Origins of the Plate Tectonics Revolution

Chapter 2: Paleomagnetism

Introduction

Paleomagnetism is the study of fossil magnetism, used to provide insight into the history of the geomagnetic field. Several motivated researchers in the early 20th century undertook pioneering work in the field, but research was often limited by sparsity of data and crude instrumentation.[64] Following World War II, new research aims and related improvements to fieldwork techniques and measuring instruments facilitated rapid progress in the field. By the second half of the 1950s, a group of paleomagnetists specializing in historical reconstruction of the relative orientation of the geomagnetic field relied on mobilism as an interpretative framework.

Several historians portray paleomagnetism as reinvigorating the mobilism debate. In *The Rejection of Continental Drift,* Naomi Oreskes argues that new measurement methods in post-war Earth sciences, such as those employed in paleomagnetism, were amenable to fresh interpretations, freed from past research contexts that limited acceptance of mobilism.[65] In *Drifting Continents and Shifting Theories,* Homer Le Grande portrays paleomagnetism in the 1950s as thawing an otherwise frozen mobilism debate.[66][67] Henry Frankel devotes an entire volume of *The Continental Drift Controversy* to paleomagnetism.[68] Like Le Grande, Frankel portrays paleomagnetism as rejuvenating the mobilism debate during the 1950s. In this volume, Frankel catalogues problem solving efforts and associated debates in the field, culminating in the formation of a difficulty-free solution.[69] As noted in Chapter 1, a difficulty-free solution is formed when central researchers recognize that a proposed solution no longer confronts difficulties that are worthy of objection. The problem at hand is then treated as resolved, and any outstanding difficulties become new problems. Frankel suggests that mobilism obtained such

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[64] Basic field compasses were used in the study of rock magnetism well into the 20th century.

[65] Oreskes, 1999

[66] Le Grande, 1988

[67] Earlier histories of mobilism often address paleomagnetism only in relation to the important role played by field reversals in the 1960s. William Glen's *The Road to Jaramillo* and Allan Cox's *Plate Tectonics and Geomagnetic Reversals* pay much closer attention to geomagnetic field reversals than the endorsement of mobilism among paleomagnetists in the 1950s (Glen, 1982; Cox, 1973). Anthony Hallam's *A Revolution in the Earth Sciences* provides an introduction to mobilist interpretations in paleomagnetism in the 1950s, but also notes the limited effect of this research outside of the speciality (Hallam, 1973). Hallam attributes this to the opacity of published results to outsiders and the apparent frequency of anomalies.

[68] Frankel, 2012

[69] Frankel's account is mostly directed to post-war developments and those researchers centrally involved in the debate that resulted in the difficulty-free solution.

difficulty-free status among paleomagnetists in the second half of the 1950s. However, the strong support and endorsement of mobilism within paleomagnetism had limited influence outside the field.

This chapter follows a similar historical trajectory to that offered by Frankel. I focus on post-war developments in paleomagnetism, especially those that pertain to mobilist interpretations. I recapitulate some of the research and debates that are examined more-thoroughly by Frankel, but an important aim that I have in this chapter is to identify the range of ambiguities and debates involved in paleomagnetic research as it took place. Accordingly, I pay close attention to fieldwork practices as well as measurement methods and instruments. I also distinguish alternative subspecialties within paleomagnetism. By the second half of the 1950s, mobilism offered an interpretative framework that was favored by those researchers devoted to reconstructing the orientation of the geomagnetic field over time, but assent within this subspecialty was not universal. Alternatively, geomagnetic field reversals were treated as a distinct problem, and mobilism was virtually irrelevant to the development of this subspeciality during the 1950s, even though research on field reversals was ultimately far more important to the accelerating support for mobilism in the 1960s.

This chapter will proceed as follows. First, I introduce the research aims and related development of measuring methods and instruments leading into the 1950s. I then examine how mobilist interpretations developed to account for reconstructions of the past orientation of the geomagnetic field. Finally, I examine the alternative interpretations developed to account for the phenomenon of paleomagnetic polarity reversals.

Post-War Paleomagnetism

Patrick Maynard Stuart Blackett was a giant of 20th century British physics, and he played an important role in the maturation of paleomagnetism following WWII.[70] He graduated from Cambridge in 1921 and joined the Cavendish Laboratory under the directorship of Ernest Rutherford, where he worked with cloud chambers studying nuclear transmutation, antimatter, and cosmic rays. He was elected member of the Royal Society in 1933. That same year, he left Cambridge to head the physics department at Birkbeck College, London. He became head of the physics department at the University of Manchester in 1937. During World War II, Blackett made several contributions to British military

[70] Nye, 1999, 2004

sciences. Most notable was his development of operations research which applies advanced mathematics to goal-oriented decision making. In 1948, he received the Nobel Prize in physics. Blackett was a self-identified socialist. After the war, he worked with the Labour party, especially on matters of science and technology policy, and became a prominent advocate against British development of nuclear weapons.

In 1947, while researching cosmic rays, Blackett noticed that the magnetic moments of the Earth and the Sun are proportional to their angular momenta. This proportionality is a function of the gravitational constant G divided by the speed of light. This relationship also seemed to align with the first measurements of the magnetic field of a star.[71] Accordingly, Blackett hypothesized a lawlike regularity wherein magnetism is a fundamental property of rotating mass.[72] With some optimism, Blackett speculated, "perhaps this relation will provide the long-sought connexion between electromagnetic and gravitational phenomena".[73]

Blackett set out to measure the magnetic field produced by rotating masses in his lab at the University of Manchester. He designed and built a highly sensitive astatic magnetometer for this task. An astatic magnetometer consists of two (or more) oppositely polarized, horizontally oriented magnets which are set at a fixed distance from one another and suspended by a torsional wire. The opposing orientation of the magnets form a system that is unaffected by homogenous ambient magnetic fields or variations thereof. When exposed to a magnetic dipole, the torque in the torsional wire will be proportional to the dipole moment. This relationship can be established by calibration against objects of known magnetic properties.

Edward Bullard, a former student of Blackett's, proposed that Blackett's theory of magnetism could also be tested by geomagnetic measurements.[74] Bullard noted that, in Blackett's account of geomagnetism, the intensity of geomagnetism should decrease with depth.[75] German physicist Walter

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[71] Babcock, 1947
[72] Blackett, 1947
[73] Blackett, 1947, 658
[74] Bullard received his PhD in physics from Cambridge and joined the newly developed Department of Geodesy and Geophysics (Wilson, 1987). Bullard engaged in gravity and heat flow measurements and pioneering marine seismic refraction research. During WWII, Bullard worked with the British Admiralty on degaussing techniques to protect against magnetic mines. Following the War, he became a Professor of physics at University of Toronto before returning to the UK where he headed the National Physical Laboratory. He returned to Cambridge in 1956 and became head of the department of Geodesy and Geophysics in 1964. Bullard was made fellow of the Royal Society in 1941 and received the prestigious Vetlesen Prize in 1968.
[75] Frankel, 2012, Volume III, 8

Elsasser had proposed a mathematical theory of the Earth's magnetic field as a self-exciting dynamo at about the same time that Blackett proposed his alternative account. Measurements of the intensity of geomagnetism at depth could provide a crucial test to decide between these alternative accounts.

Keith Runcorn followed Bullard's suggestion and set out to measure variations in geomagnetic intensity in coal mines. Runcorn had entered the Faculty of Engineering at Cambridge in 1941, completing a two-year degree. In October 1946, he became an assistant lecturer in physics at the University of Manchester. He obtained his PhD from Manchester, under Blackett, in 1949.[76] Runcorn's early geomagnetic measurements seemed to support Blackett's contention that geomagnetic intensity would decrease with depth. Similar results were reported from South Africa.[77] Soon, however, Runcorn realized that these early results did not adequately account for confounding influence of nearby magnetic fields, like those produced from magnetized rock. In 1951, Runcorn reported that depth had no measurable effect on the Earth's magnetic field.[78]

Meanwhile, researchers in the Department of Terrestrial Magnetism at the Carnegie Institution, Washington DC, realized that Blackett's theory predicted that Earth's magnetic field should exhibit little directional variation over time. Furthermore, this prediction could be tested by the study of paleomagnetism, a field of research pursued at Carnegie since the late 1930s.[79]

Paleomagnetism is the study of fossil magnetism. In the 19th century, researchers determined that rock magnetism often aligned with the orientation of Earth's magnetic field. By the turn of the century, both field work[80] and laboratory experiments[81] demonstrated that magnetic minerals in cooling rock could permanently acquire an orientation that was parallel with an ambient field.[82] The temperature at which magnetism can become permanently locked into cooling rock is called the *Curie*

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[76] Runcorn became a fellow of the Royal Society in 1965 and won the Vetlesen Prize in 1970 (Collinson, 2002).
[77] Hales and Gough, 1947
[78] Runcorn, Benson, Moore, and Griffiths, 1951
[79] During World War II, paleomagnetic research at Carnegie stopped, as the Department's efforts were directed to serve the US Navy (Brown, 2004). This included the production and improvement of instruments, such as compasses and chronometers, and the development of magnetic maps. Magnetometers were developed and tested for use in submarine detection. Work pertaining to the description of magnetic fields of ships and submarines could also facilitate detection and camouflaging capabilities. Following WWII, the Carnegie group returned to work on paleomagnetism and the development of core fieldwork methodologies.
[80] Folgheraiter, 1899; David, 1904
[81] Curie, 1895
[82] J.G. Koenigsberger completed much important work on ferromagnetism and rock composition, beginning toward the end of the 1920s. For early work on thermo-demagnetization see Thellier, 1938 and Nagata, 1943.

point. For naturally forming rock, the strongest ambient field is typically the Earth's magnetic field. Studying rock magnetism may, thereby, provide insight in the history of Earth's magnetic field.

Several substantial challenges confronted the early study of paleomagnetism. First, different rocks have different magnetic properties. Igneous rock with high proportions of iron and/or titanium have high magnetic susceptibility. Magnetite is one of the most common minerals found in highly magnetic natural rock.[83] Igneous rocks were typically harder to date precisely, due to the absence of fossils. Alternatively, sedimentary rock could be dated more readily, but even in the best of cases, retained magnetism in sedimentary rock was found to be weaker and more difficult to measure.[84] Crystalline structures in consolidated rock may result in some tendency for magnetic minerals to align along certain axes, which may result in remanent magnetism that deviates from the ambient field.[85] Furthermore, rocks are often mineralogically complex, with variable magnetic properties, differing Curie points, and differing crystalline structures. Second, the *natural remanent magnetism* (NRM) –rock magnetism obtained during consolidation that is oriented parallel to the Earth's magnetic field – may not be permanent. Rocks are subject to great fluctuations in temperature, pressure, exposure, erosion, and may also be subject to chemical changes. The effect of these processes upon rock magnetism was not well studied. Additionally, consolidated rock may retain secondary magnetic properties which can obscure NRM. For example, lightning strikes can induce remanent magnetism in consolidated rock. Even weak magnetic fields that are sustained for long periods of time can induce remanent magnetism. Such *secondary magnetism* can obscure NRM. Third, magnetic minerals may hold magnetic properties temporarily when an ambient magnetic field is present. This is called *induced magnetism* and can pose a significant challenge to interpretation of field measurements. Fourth, rocks frequently change their position and orientation due to a host of physical factors. Entire formations may be folded, faulted, rotated, and even inverted. Finally, even though the Earth's magnetic field is presently dipolar and oriented toward the Earth's axis of rotation with slight secular variations, this may not have been the case in the past.

The Carnegie group used a spinner magnetometer to measure the magnetic orientation of samples. Spinner magnetometers are based on different principles of magnetism than astatic magnetometers.[86] A rock sample, typically cut into the shape of a disc, is rotated at a constant rate

[83] Koenigsberger, 1938
[84] Iling, 1943
[85] Koenigsberger, 1938
[86] Johnson, Murphy and Torreson, 1948

about a single axis, adjacent to a sensor. For early work at Carnegie, this sensor was an induction coil. The rotation of a magnetic body near the center of the coil induces an alternating current, the intensity and phase of which is proportional to the magnetic properties of the rotating sample. Only the magnetism vector that is perpendicular to the axis of rotation can be measured in this way, so the sample must be repositioned and measured multiple times. In 1948, researchers at Carnegie measured remanent magnetism of sedimentary samples of increasing age and concluded that Earth's magnetic field has remained highly stable for the past million years.[87] The next year, based on expanded sampling, this duration of stability was extended to 50 million years.[88]

In addition to these early measurements and inferences, the Carnegie group also developed significant tests for the stability of NRM. In 1949, John Graham[89] developed the fold test and conglomerate test.[90] If the orientation of remanent magnetism in folded strata follows the fold, such that hypothetical unfolding would result in parallel magnetic orientation, then the remanent magnetism predates the fold. Alternatively, if the orientation of remanent magnetism within conglomerates are randomly distributed despite the uniform magnetic orientation of the surrounding matrix, then the magnetism of the strata has remained stable since its formation. Graham employed these stability tests in his field work. In Maryland, he found a formation with magnetic inclination that was opposite to what should be expected of rock in the northern magnetic hemisphere, even though the formation passed the stability test.

Such reversals in polarity of fossil magnetism had been identified previously. In the first decade of the 20th century, Bernard Brunhes and Pierre David found reversals in lava flows of central France.[91] In the 1920s, Motonori Matuyama of Kyoto Imperial University determined that magnetic orientation of samples from Japan and Manchuria were either aligned with the Earth's present field, or opposite to it.[92] Correlating such reversals with stratigraphic position, Matuyama speculated that Earth's field may have

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[87] Ibid

[88] Torreson, Murphy and Graham, 1949

[89] John Graham was born in Boston (Doell, 1973). He graduated from Johns Hopkins University in 1940 with a degree in geology. He studied inorganic chemistry in his senior year. During WWII, he served as a commissioned naval officer, specializing in ordnance. He worked on radar development and counter measurers, proximity fuses, and guided missiles. Following WWII, Graham returned to Johns Hopkins for graduate studies in geology. In his second year, he received a fellowship with the Carnegie Institution. Graham left Carnegie in 1957 citing a lack of support in magnetic studies.

[90] Graham, 1949

[91] They also found that baked clays adjacent to these lava flows shared magnetic orientation with the lava flows (Brunhes, 1906).

[92] Matuyama, 1929

reversed during the Pleistocene epoch. In 1951, Jan Hospers,[93] then at the Department of Geodesy and Geophysics at Cambridge, argued from 1950 fieldwork on Icelandic lavas that paleomagnetic reversals are the result of global field reversals.[94] He found that normal and reversed lavas were sometimes situated atop one another and showed no differences in lithography. This made it difficult to suppose that factors such as heat, pressure, or lightning could significantly influence the magnetic orientation in one layer without influencing lower layers. Concurrently, Alexander Roche began a series of studies on French lava flows. He also identified numerous reversals and detected no difference in lithography between normal and reversed rock in laboratory tests.[95] Roche also identified reversals in baked clays adjacent to reversed lava flows, even though the wider clay formation was of normal polarity. The basic explanation, offered by Roche, was that the lava raised the temperature of adjacent clays past their Curie point, and when they cooled, they took on the polarity of the geomagnetic field at that time.[96]

Graham and his colleagues at Carnegie considered field reversals to be implausible. How, then, to proceed, given that Graham found a reversal in strata that passed the fold test? Confronted with this problem, Graham deemed his fold test to be unreliable. Based on apparent normal polarity in contemporaneous flat-lying sediments, Graham maintained that some unspecified process had re-magnetized the folded Maryland strata, but it is unclear why or how such re-magnetization should be so perfect as to following folding contours of the formation.[97] Alternative explanations for Graham's measurements, such as significant wandering of the geomagnetic field, or substantial motion of Earth's crust were not even considered.[98]

In 1951, Louis Néel hypothesized four possible mechanisms for self-reversals, wherein remanent magnetism could take on opposite polarity to the ambient field.[99] Within a few months, Takeshi Nagata and colleagues at the University of Tokyo identified self-reversals in laboratory studies.[100] Graham then endorsed the notion that his puzzling Maryland formation had undergone self-reversal prior to folding,

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[93] Jan Hospers was born in Groningen in 1925 (Frankel, 2012, Volume II, 25-26). He graduated from the University of Groningen in 1948 with a degree in geology and physics. He completed a master's degree at the University of Utrecht in 1950. It was during his time at Utrecht that Hospers engaged in fieldwork in Iceland, under leadership of M.G. Rutten.
[94] Hospers, 1951
[95] Roche, 1950, 1951, 1953
[96] Like the conglomerate test, baked clays could be used to infer stability of remanent magnetism, when the baked clay shares its orientation with adjacent igneous intrusions and deviates from that of surrounding clay.
[97] Graham and Torreson, 1951
[98] See Frankel, 2012, Volume II, for a more thorough examination of the research of the Carnegie group.
[99] Néel, 1951
[100] Nagata, Akimoto and Uyeda, 1951

thereby reclaiming the viability of his previous measurements.[101] However, in subsequent works, Hospers and Roche showed that reversals were not randomly distributed but seemed to occur in stratigraphically consistent succession. This was a strong challenge to the general adequacy of the self-reversal explanation.[102]

Still at the University of Manchester, Blackett was unable to measure magnetic fields induced by rotation and ended his experimental programme in 1952, turning to the study of paleomagnetism as an alternative means to test his theory. Blackett put John Clegg[103] in charge of the Manchester paleomagnetic research group. Runcorn also took up a research programme in paleomagnetism after completing his PhD and moving to the Department of Geodesy and Geomagnetism at Cambridge, joining Hospers. Runcorn brought on Ted Irving and Ken Creer. Both graduated from Cambridge in 1951. Irving specialized in geology,[104] while Creer read physics in his third year.[105] Runcorn tasked Creer with the construction of an astatic magnetometer for the Department which was completed in 1953.[106]

Apparent Polar Wander and Mobilism

Assuming that the geomagnetic field has remained dipolar and closely associated with the Earth's axis of rotation, magnetic inclination of NRM can be used to infer paleolatitude, the latitudinal position of a sampled rock at the time of consolidation. A sufficiently sensitive magnetometer can be used to establish the magnetic inclination, declination, and strength of a rock sample. These measurements can be used in a straightforward way to infer the relative position of the Earth's north magnetic pole at the time of rock consolidation. Such determinations are called *paleopoles*. Even when sampling and analytic practices limit the possible influence of confounding factors, variation can still be expected across contemporaneous samples due to slight differences in physical and chemical histories

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101 Graham, 1952
102 Hospers, 1953a, 1953b
103 Clegg received a B.SC. in physics from the University of Manchester in 1935 (Frankel, 2012, Volume II). During WWII he worked on radar research for the British Navy. He returned to the University of Manchester in 1946 and worked in radio astronomy, constructing radio telescopes and studying meteor showers. He received his PhD in 1949. Clegg brought on Mary Almond, who worked under him in radio astronomy. Blackett also hired Peter Stubbs, with a degree in geology from the University of Manchester. Clegg build a second magnetometer at Manchester I 1951, calibrating it against Blackett's. In the fall of 1953, Blackett became head of the Department of Physics at Imperial College, London. Clegg, Almons, and Stubbs joined him there.
104 Hyndman, 2015
105 Frankel, 2012, Volume II
106 Blackett's astatic magnetometer could measure 12 to 15 specimens a day. Creer's magnetometer, completed in 1953, could measure a sample in under 10 minutes (Frankel, 2012, Volume II).

of those samples, but also due to general margins of error in magnetometer measurements[107] and establishing contemporaneity. Hospers recognized that detailed analysis of paleomagnetic data from his Icelandic field work would require statistical analysis to account for variations therein. R. A. Fisher, already widely known for his work on population genetics, provided these statistical tools. Fisher developed a measure of the precision of a set of paleopole measurements and a means of determining cones of confidence for these pole positions based on averages and standard deviations of paleopole measurements on a spherical surface.[108] Hospers began using Fisher's statistics in 1951, arguing that Icelandic samples indicated that Earth's magnetic field remained closely linked to the pole of rotation. The geomagnetic field reverses, but the reversed field also tends to align with the Earth's pole of rotation.[109] In subsequent papers, Hospers argued that this relationship between magnetic and geographic poles is apparent from Icelandic lava for the past 20 million years.[110] He also calculated multiple paleopole cones of confidence from samples taken from Iceland, France, England, Scotland and Ireland, ranging from the Early Quaternary to the Eocene. Hospers used these calculations to argue that the geomagnetic pole tended to align with the geographic pole.[111]

Meanwhile, Irving engaged in extensive fieldwork in Scotland, sampling from Precambrian sedimentary formations. He initially measured his samples on Blackett's magnetometer, and later corroborated these measurements against the newly constructed Cambridge magnetometer. Irving found that the orientation of several stratigraphic sections deviated significantly from Earth's present geomagnetic field. He found reversals, too. Irving presented preliminary results in a 1954 conference in Birmingham.[112] Likewise, Creer reported preliminary results from Devonian, Permian, Triassic, and Eocene formations, showing deviations from the present field. Clegg presented results from his work on British red sandstones of the Upper Triassic, showing strong deviations from the present field. These results contrasted with some of the most detailed paleomagnetic work completed to that point which tended to present the Earth's magnetic field as highly stable. Runcorn thereby proposed that the orientation of the Earth's magnetic field changes over time. Such *polar wandering* could account for

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[107] Also, statistical averaging of such measurements may take place to average out the effects of secular variations.
[108] Fisher, 1953
[109] Hospers, 1951
[110] Hospers, 1953a, 1953b, 1954
[111] Hospers, 1955.
[112] Griffiths and King, 1954

Irving, Creer, and Clegg's measurements.[113] Alternatively, Clegg proposed that historical deviations in paleopole measurements might be explained by motion of Britain relative to the Earth's axis of rotation.

Subsequent publications bore out these preliminary results and hypotheses. In 1954, Clegg argued that deviation in inclination of Triassic rock sampled from various locations across England could be explained if Great Britain resided at a lower latitude during the Triassic.[114] Creer, relying primarily on his own extensive field work, located paleopole positions for Great Britain from the Precambrian through the Eocene.[115] By "connecting the dots" he produced an apparent path of polar wandering (APW), representing the motion of Earth's magnetic field relative to Britain over time. Whereas Clegg favored mobilist explanations for his paleomagnetic measurements, Creer considered polar wandering to be at least as plausible an explanation.

Under the interpretation of polar wandering, alternative landmasses should yield paleopole measurements that are mutually consistent. Under the interpretation of mobilism, paleopole positions should be discordant across landmasses that have undergone relative motion. Runcorn was convinced that further research across continents would show general agreement of paleopole positions, thereby supporting polar wander over mobilism. In 1954, he began field work in the Grand Canyon. Comparing his paleopole positions with Creer's pathway, Runcorn found discrepancies that he, at first, considered to be systematic and indicative of the effects of secondary magnetization.[116] Meanwhile, Irving found that preliminary measurements in India were indicative of mobilism, and the required scale and direction of relative motion closely aligned with paleogeographic reconstructions proposed by Wegener based on paleoclimate data.[117] Creer found that his polar wander pathway for Britain was discordant with Graham's Maryland measurements. In 1956, Runcorn determined that the discrepancy between his North American measurements and the British polar wander pathway could not be eliminated.[118] This convinced Runcorn of mobilism. Several other researchers engaged in similar projects around this time,

[113] There are two historically relevant notions of polar wandering. The first proposes that the geomagnetic field is not historically coupled with the earth's axis of rotation. The geomagnetic north pole may, simply, wander. The second proposes that the Earth's axis of rotation may wander, along with the geomagnetic pole. Runcorn favored this version of polar wandering prior to his acceptance of mobilism in 1956, but similar polar wandering hypotheses had been proposed previously, often by paleoclimatologists.
[114] The mean direction of samples deviated 41 degrees in inclination from the present geomagnetic field (Clegg, Almond and Stubbs, 1954).
[115] Creer, Irving, and Runcorn, 1954
[116] Runcorn, 1956a
[117] Frankel, 2012, Volume II
[118] Runcorn, 1956b

reaching similar conclusions. Clegg engaged in fieldwork in India.[119] Creer worked in South America.[120] South Africa,[121] Zimbabwe,[122] and the Caspian[123] were also sites of substantive field work. In 1954, Irving had moved to the Department of Geophysics at Australian National University[124] and began fieldwork in Australia.[125] Indian and Australian measurements were especially significant, as both indicated substantial decrease in latitude, with APW pathways highly divergent from British and American benchmarks.

To briefly recapitulate this rapid turn of events, it was 1954 when paleomagnetic measurements indicative of great changes in the geomagnetic field were first presented, though private recognition of the implications of such measurements would have taken place during fieldwork over the preceding couple years. By 1956, based on consistency of paleopole measurements within continents, and discordance of such measurements across continents, mobilism became the chief interpretative framework. British paleomagnetists, aware of the youth of their field and the broadly contentious nature of mobilism, directed much effort toward explicating and strengthening their methods and conclusions. They convened interdisciplinary conferences[126] and published in general science journals[127] to share results and renew debate on mobilism. They also published detailed descriptions of instruments, practices, methods, and ambiguities.[128]

By the end of the 1950s, assorted practices were routinely employed to limit the potential influence of confounding factors upon paleomagnetic measurements. Graham's stability tests and the baked clay test were widely employed. Another common test for stability consisted of the identification of symmetrical reversals in a sampled formation. Secondary magnetism or other possible confounding forces would be highly unlikely to result in or maintain such symmetry.[129] The most precise inferences could be made if many samples were taken at many locations. This diversification would limit the

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[119] Clegg, Deutsch and Griffiths, 1956
[120] Creer, 1958
[121] Graham and Hales, 1957
[122] Nairn, 1956
[123] Khramov, 1958
[124] Under the directorship of John Conrad Jaeger, the Department of Geophysics at ANU became an important center of paleomagnetism and rock dating in the 1950s and 1960s.
[125] Irving, 1956a
[126] Examples include the Imperial College Symposium of 1956 and the NATO conference at Newcastle in 1967.
[127] Runcorn, 1955; Clegg, 1956; Nairn, 1956; Opdyke and Runcorn, 1956; Irving, 1957a; Irving and Green, 1957a; Runcorn, 1959a; Chang and Nairn, 1959; Bull and Irving, 1960
[128] Irving, E. and S. K. Runcorn, 1957; Irving, 1957b; Creer, Irving and Runcorn, 1957; Collinson and Nairn, 1959; Blackett, Clegg and Stubbs, 1960
[129] Cox and Doell, 1960

possible influence of local disturbances to NRM, assuming that such disturbances would tend to vary across locations. Samples were often sought from multiple rock types as well and were preferentially taken from rock types known to produce consistent magnetic measurements. Oriented cores would be drilled in the field and returned to the lab where the core would be cut into discs and measured on a magnetometer. Magnetometer measurements could establish inclination, declination, and strength of samples. Paleomagnetic orientations were routinely averaged for short time intervals to account for secular variation of the geomagnetic field. Collections of concurrent averages could then be used to establish paleolatitudes or cones of confidence for paleopole positions. Sampled rock was sometimes subject to additional petrologic or magnetic testing. Toward the end of the 1950s, magnetic *cleaning* of samples was increasingly common. Such cleaning consisted of the application of temperature or magnetic variations of increasing strengths, capable of erasing secondary magnetism, thereby making the NRM more easily measured in the lab.[130]

The discerning researcher had to parse through the growing body of paleomagnetic literature to distinguish the reliability of data sets and associated inferences. Even when research practices were highly rigorous, ambiguities remained. Stratigraphic methods and fossils were the primary means of dating samples. Fossil dating could be fickle and crude, which could influence measures of concurrency.[131] Polarity reversals were often accounted by flipping the inclination and declination 180 degrees and proceeding as normal. Paleolatitudes could be inferred from measured inclination, but only based on the assumption that the geomagnetic field tends to align with the geographic axis of rotation. Alternatively, paleopoles did not necessarily require commitment to this same assumption, but paleopole positions can only be determined relative to a sampled landmass. Accordingly, it could be difficult to interpret paleopole discordances between or across multiple landmasses. Researchers considered such discordance to be indicative of relative motion, but this was based on the assumption that the geomagnetic field remained dipolar throughout history, and the precise character of inferred relative motions was multiply interpretable by various combinations of relative continental motion and/or polar wandering.

One of the most important higher-level methodological commitments within paleomagnetism was that corroboration could be used to establish practices capable of limiting ambiguities. In 1956, Graham argued that stress could induce or alter magnetization, so if the history of a formation is not

[130] Creer, 1958
[131] Radiometric dating in paleomagnetism became increasingly common only in the 1960s.

known, the NRM cannot be reliably established.[132] This might seem to be highly problematic to the study of paleomagnetism, especially to higher-level inferences based on data gathering practices that did not specifically take stress into account. However, several prominent researchers objected to the severity of Graham's challenges, based on the high degree of corroboration within paleomagnetic data.[133] The high-precision clustering of measurements across numerous contemporaneous sample sites, the tendency for such clustering to take place across landmasses or geological time, and the agreement of such clustering between independent researchers like Clegg and Creer in Britain, Creer and Irving in India, and Runcorn and Graham in North America, limited the potential influence of alternative unaccounted confounding factors upon the strongest paleomagnetic results. In 1961, Irving explicitly argued that if the rigorous practices of paleomagnetic data collection and interpretation are weakened, then such corroborations lose their significance.[134]

It is no surprise, then, that Irving spearheaded efforts to expand the corroboration of paleomagnetic inferences by integrating various palaeoclimatological data. Irving reasoned that if the geomagnetic field tended to align with Earth's axis of rotation, then palaeoclimatological inferences should corroborate paleopole measurements. This corroboration could then offer an additional line of support for mobilist interpretations and also limit the possible degree to which paleomagnetic measurements could be attributed to polar wandering. In 1956, Irving began correlating paleoclimate indicators – tillites indicative of glaciation and salt deposits indicative of tropical climate – with

[132] Graham, 1956, 1957

[133] Irving and Runcorn responded to Graham's objection by checking for concordance across rocks on the same continent, finding agreement in paleopoles. Surely, rock of different type, in different locations on a continent cannot be expected to share the same stress history (Du Bois et al, 1957). Blackett and Clegg cited corroboration between paleopole determinations of rock from the same region with differing stress histories (Blackett, Clegg and Stubbs, 1960). Irving likewise showed that corroboration of field samples challenged Graham's conclusions, even though lab studies showed stress could induce magnetism in some samples (Irving, 1959).

[134] "It is very probably that paleomagnetic work, if conducted with proper care, can solve satisfactorily the controversial problems of polar wandering and continental drift. But for this to be possible, all results must be firmly based on good stability evidence and on adequate sampling and uniform analysis; otherwise, it may be possible in a matter of years to find in the literature results to support almost any point of view." (Irving, 1961, 231)

paleolatitudes inferred from paleomagnetic measurements.[135] Runcorn,[136] Blackett,[137] and others pursued similar approaches while diversifying palaeolatitudinal indicators.

Corroborations were surely prized, but there were also notable instances of corroborative failure. In 1957, Allan Cox, a graduate student at Berkeley[138] published results from 1955 fieldwork on Eocene lava flows in Oregon.[139] He identified stability via the fold test and thermally cleaned his samples of secondary magnetism. He accumulated 57 samples from eight flows, distributed across 38 miles. His samples were dated by analyzing 30 different species of interbedded fossil. Cox's paleopole deviated significantly from previous work on North American polar wandering. Instead, his paleopole position was closer to Eocene paleopole position of Indian rock. Cox claimed that this deviation from other North American measurements could be explained if polar wandering took place quite swiftly, but the change in paleopole position was too substantial and too rapid for mobilism to be a plausible explanation. Furthermore, the disagreement between Cox's rigorous measurements and polar wandering pathways for North America established by Creer, Irving, and Runcorn called into question the strength of corroborative paleomagnetic measurements upon which mobilist hypotheses found their strongest support.[140] Cox thereby deemed paleopole measurements to be problematic and directed subsequent research to polarity reversals. In response, Irving agreed that rapid polar wandering might explain the measurements, but then, one would expect corroboration of this rapid change elsewhere.[141] Instead, Irving proposed that Cox's sampling region had undergone local rotation with respect to the rest of the continent, due to orogenic processes.[142][143] Corroborative failures could also be attributed to misidentification, and sloppy or incomplete work.[144]

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135 Irving, 1956b
136 In 1959, Runcorn worked with Neil Opdyke to corroborate paleopole positions with paleolatitude inferred from paleowind directions. Paleowind direction could be inferred from the shape of fossilized dunes whose crescent shape is oriented in relation to prevailing wind directions and wind direction relates to hemisphere and latitude. (Opdyke and Runcorn, 1959)
137 Blackett, 1961
138 Allan Cox was born in Santa Ana, California, in 1926 (Krauskopf, 1997). He received a PhD from Berkeley in 1959, and thereafter joined the USGS at Menlo Park. Cox became professor at the geophysics department at Stanford in 1967, and Dean of Earth Sciences in 1979. Cox shared the Vetleson Prize with Doell and Runcorn in 1970.
139 Cox, 1957
140 Creer, Irving and Runcorn, 1957
141 Irving and Green, 1957b
142 Irving, 1959
143 Similar conclusions were endorsed for Iberia relative to France, though this was not ad hoc (Clegg et al., 1957).
144 Another corroborative failure of note is the so-called "Squantum tillite" of the Boston area. Long interpreted as a glacial tillite of Permian age, the Squantum tillite did not align with Wegener's paleoclimate reconstructions. It

By 1960, with additional paleomagnetic data gathered form Antarctica,[145] South America,[146] Japan,[147] China,[148] and Europe,[149] consistency of measurements within continents, and discordance of measurements across continents continued to lend support to mobilist interpretations.[150] Of some note, when Mesozoic paleopoles of Antarctica, South Africa, South America, and India were recombined based in a mobilist reconstruction of the Gondwana, their widely scattered paleopoles became more tightly concentrated.[151] The sophistication of paleomagnetic reconstructions also increased as the differences between pairs of APW paths were analyzed.[152]

In 1960, Allan Cox and Richard Doell[153] wrote a highly influential review of the field.[154] They provided a general introduction for readers of the Geological Society of America Bulletin who may not have been familiar with the rather specialized and technical work of paleomagnetism. They also tabulated a massive set of prior paleomagnetic results and remarked on the reliability of the measurements. Cox and Doell argued that conclusions about the history of the geomagnetic field and associated inferences about mobilism and polar wandering required more detailed data before reliable conclusions could be reached. They claimed that existing measurements were often unreliable, and that multiple sample sites should be measured for each geological period on each continent in order to establish continental and cross-continental consistency of measurements. Such consistency would be

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also did not align with Permian paleolatitude measurements of North America. Irving proposed that the Squantum Tillite "is of small extent and may be the product of a mountain glacier" (Irving, 1956b).
Similarly, American paleontologist Francis Stehli argued that climatic zones inferred from Permian marine invertebrates around Eastern Asia contradicted supposed paleopole positions inferred from Europe (Stehli, 1957). In response, Runcorn and Irving suggested that the present distribution of the continents is not a suitable basis upon which to infer paleogeography. Paleopole positions are relative to a particular landmass, not to current global geography (Runcorn, 1959b; Irving, 1964).

145 Bull and Irving, 1960

146 Creer, 1962

147 Nagata, et al., 1959

148 Chang, and Nairn, 1959

149 Nairn, 1960

150 Nagata interpreted results mainly in terms of polar wandering coupled with rotation (Nagata et al., 1959).

151 Bull and Irving, 1960

152 Irving, 1958

153 Doell was born in Oakland California in 1923 (Dalrymple, 2016). He worked for United Geophysical, an exploratory geophysics company, prior to earning his undergraduate degree from Berkeley in 1952. He obtained his PhD from Berkeley in 1955. In his thesis, Doell described paleomagnetic measurements of the Grand Canyon. Finding different magnetic orientations in the Precambrian and Permian, and confident that no large-scale displacements of the landmasses had taken place, he attributed this to polar wandering. After receiving his PhD, Doell set up a paleomagnetic laboratory at the University of Toronto where he worked as a lecturer. He then became an assistant professor at MIT until 1958. In 1959, Doell returned to Menlo Park to work at the USGS. Doell shared the Vetleson Prize in 1970 with Cox and Runcorn.

154 Cox and Doell, 1960

indicative of historical continuity of the geomagnetic dipole, which would then warrant further inferences about mobilism or polar wandering. Cox and Doell were not swayed by the degree of corroboration across measurements. As previously noted, Cox had identified a deviant Eocene paleopole from Oregon in 1957. Cox and Doell thereby concluded that there was insufficient data to judge mobilism or polar wandering for much of Earth's history. However, they did note that paleomagnetic data from the Carboniferous and Permian offered the strongest support for mobilism, especially with respect to Australia, even though the data was not complete. Irregular polar wandering was an alternative possibility since there was some uncertainty in the historical relationship between the geomagnetic pole and the earth's axis of rotation.

Polarity Reversals

Following Hospers' and Roche's pioneering work in the early 1950s, polarity reversals were widely identified. Runcorn found Tertiary reversals in the Pacific Northwest.[155] Irving described reversals in Precambrian sedimentary rock.[156] Indeed, reversals were so common, that they were frequently used for testing stability of remanent magnetism. Attempts were made to correlate Iceland and French reversal data, [157] and rock of intermediate polarity between reversal periods was reported in 1957.[158]

As previously noted, geomagnetic field reversals were often hypothesized to account for such polarity reversals. Though initially opposed to the idea, Runcorn endorsed field reversals in 1956.[159] Hospers endorsed periodicity of field reversals at intervals of approximately 500,000 years.[160] Similar periodicity was endorsed in the work of Soviet paleomagnetist Aleksei Khramov in an influential work translated into English in 1960.[161] Alternatively, self-reversal was a possible explanation for this phenomenon, with a theoretical basis developed by Néel and empirically supported by Nagata and Uyeda. In 1954, additional self-reversing magnetite samples were described by Japanese researchers.[162] Magnetite is one of the minerals primarily responsible for fossil magnetism, so the description of self-reversal in magnetite was of notable significance. The researchers attributed this to a very slow process

[155] Campbell and Runcorn, 1956.
[156] Frankel, 2012, Volume II
[157] Einarsson and Sigurgeirsson, 1955
[158] Einarsson, 1957; Sigurgeirsson, 1957
[159] Opdyke and Runcorn, 1956
[160] Hospers, 1951
[161] Khramov, 1958
[162] Kawai, Kume and Sasajima, 1954a; Kawai, Kume and Sasajima 1954b

of differentiation and re-magnetization of magnetite and titanomagnetite from a homogenous parent. Also in 1954, American researchers James Balsey and Arthur Buddington argued that there was a connection between polarity reversals and oxidation and also described associated laboratory results of induced self-reversals.[163][164] This relationship between oxidization and reversals was subsequently pursued by others, including Blackett.[165] In 1957, Balsey and Buddington penned a review of polarity reversals, concluding that more research was needed on the mineralogical basis of self-reversals, and that too many researchers were over-eager to hypothesize field-reversals.[166] In 1955, Néel published additional theoretical mechanisms on self-reversals.[167] Also, Belgian geologist John Verhoogen,[168] developed a theoretical account of very slow self-reversals based on migration of cations within crystal lattices of consolidated rock.[169] This theorized mechanism of self-reversal was so slow that lab testing did not offer a plausible route for study.

By the end of the 1950s, Cox and Doell built a paleomagnetism laboratory at the USGS in Menlo Park, intent on distinguishing between field reversal and self-reversal hypotheses. They were disposed to the notion of field reversals and that rock of the same age should thereby show the same polarity across different locations. We will return to this research in Chapter 5.

Conclusion

During the 1950s, paleomagnetism became a highly fruitful, yet technical and specialized discipline. The initiation of important research programs in paleomagnetism around this time was driven by general curiosity pertaining to the history of the geomagnetic field, especially as this history informed alternative theories of geomagnetism. By the middle of the 1950s, efforts were primarily directed toward determination of paleopole positions and paleolatitudes, comparisons across times and landmasses, and deciding between alternative suitable interpretations. By the end of the 1950s, research was increasingly directed toward identification of independent corroborations to reinforce mobilist interpretations. A notable strand of paleomagnetic research pertained to the phenomenon of

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[163] Balsey and Buddington, 1954
[164] Balsey and Buddington also worked with Graham on magnetic effects of stress (Graham, Buddington and Balsley, 1959)
[165] Blackett, 1962; Wilson, 1965; Wilson and Haggerty, 1966; Wilson and Watkins, 1967
[166] Balsley and Buddington, 1957
[167] Néel, 1955
[168] Cox and Doell worked under Verhoogen during their doctorates.
[169] Verhoogen, 1956

polarity reversals in magnetic rock. Research on reversals developed alongside broader paleomagnetic research, and thereby utilized many of the same instruments and practices. However, reversals were not interpreted in terms of mobilism or polar wandering. Rather, the main interpretative structures pertained to ferromagnetic self-reversals and geomagnetic field reversals.

Though highly specialized, the field of paleomagnetism was also unavoidably interdisciplinary in nature. Geological maps, stratigraphic principles, fossil dating, and minerology were used to identify and correlate rocks. Geology also informed selection of sample locations. Paleoclimatology, itself an interdisciplinary field based on interpretation of diverse geological and paleontological traces, was often employed to corroborate paleopole and paleolatitude measurements. Electromagnetic theory was indispensable in the development and interpretation of magnetometry. Along with minerology, magnetic theory was also centrally involved in the development of NRM theory, magnetic cleaning techniques, and prospects of self-reversals.

Of course, ambiguity and debate were widespread. Alternative theories of geomagnetism spurred initial research. The reality of reversals was firmly established in the early 1950s, but the source of reversals was still subject to debate. Some argued for geomagnetic field reversals, often based on stratigraphic sequences of reversed and normal polarity and possible correlations thereof. Others argued for self-reversals, often based on theoretical accounts and a handful of laboratory results. During the early 1950s, measurements tended to show that the Earth's magnetic field remained highly stable over time, but this rapidly changed. Hospers argued that the geomagnetic field tended to align with Earth's axis of rotation, but proponents of polar wandering would continue to challenge this hypothesis well into the 1960s. Polar wandering found some support from paleopole measurements, but relative continental displacement also seemed to be a viable possibility. For much of the 1950s, researchers employed some combination of both these hypotheses to account for paleomagnetic measurements. By the end of the 1950s, paleomagnetism had convinced many researchers – especially in Europe – of the reality of mobilism. Conviction in the reality of mobilism was largely due to convergence of paleopole determinations within landmasses, and divergence of paleopole determinations between landmasses. Mobilist interpretations also obtained support by corroborations from paleoclimatology and elsewhere. Australian measurements were often considered to offer the very best evidence for mobilism, showing notable divergence in paleopole positions with other landmasses. However, mobilism was not universally agreeable, especially among American researchers. A general problem was that paleomagnetic results could be interpreted in many different ways, from lower-level assumptions involved in NRM stability to higher-level interpretations of diverging APW pathways. Corroboration of paleomagnetic measurements that satisfied a collection of standards was highly regarded as indicative of the general reliability of paleomagnetic measurements, despite the broad range of assumptions involved in paleomagnetic work. Still, notable corroborative failures were known.

Henry Frankel argues that mobilism obtained difficulty-free status among paleomagnetists in the 1950s.[170] Despite this difficulty-free status, researchers outside of paleomagnetism were not convinced. Paleomagnetic research in the 1950s did not result in widespread acceptance of mobilism. According to Frankel, this was largely due to the difficulties raised by Cox, Doell, and Graham which were echoed by Harold Jeffreys and other esteemed physicists who continued to maintain that mobilism was not physically possible. Due to these difficulties, researchers not intimately familiar with paleomagnetism were unable to recognize the difficulty-free status of mobilism. As for the difficulties themselves, Frankel claims that they were illegitimate, and that other centrally involved researchers recognized that these difficulties had already been resolved or were overly skeptical.

I am ambivalent about the difficulty-free status of mobilism among paleomagnetists in the 1950s. With respect to the limited influence of paleomagnetic evidence for mobilism upon the broader Earth sciences during the 1950s, I maintain that the eventual widespread uptake of mobilism in the 1960s was not due to research in any one field. Rather, it was the remarkable combination of multiple lines of research that ultimately mobilized the Earth sciences. In this regard, it will become apparent in Chapter 5 that research on polarity reversals became centrally involved in the mobilism debate during the 1960s, but only in combination with alternative research in marine geology and geochronology.

[170] Frankel, 2012, Volume I-II

Chapter 3: Marine Geology

Introduction

Prior to the 20th century, little data was available for the study of marine geology. During the early 20th century, and accelerating by World War II, measuring instruments and techniques diversified and improved, and manpower (or boat power) and funding directed to marine geology also increased, thereby greatly expanding data collection.

Most histories of the mobilism debate emphasize developments in marine geology, especially in the 1960s. In part, this is because marine geologists obtained the data that made it possible to test the maritime speculations of contractionists and permanentists. Additionally, many of the researchers who became the strongest and most influential proponents of mobilism during the 1960s were marine geologists. Though mobilist interpretations in marine geology were tentatively proposed toward the end of the 1950s, debate persisted into the second half of the 1960s. Researchers at the Lamont Geological Observatory of Columbia University, especially, were strongly opposed to mobilism, but beginning in about 1966 this quickly changed. Many long-standing opponents to mobilism in marine geology converted. At times, this transition was quite rapid, taking only a few days.

Frankel's account of the mobilism debate pays close attention to developments in marine geology during the 1950s and 1960s.[171] Like his volume on paleomagnetism, Frankel's history of marine geology is primarily concerned with problem solving efforts and associated debates and theoretical developments. According to Frankel, by the second half of the 1960s, these problem solving efforts culminated in three difficulty-free solutions: seafloor spreading, transform faults, and plate tectonics.[172] For the most part, these solutions were developed and debated by marine geologists and associated geophysicists.

In this Chapter, I examine developments in marine geology leading into the 1960s. Frankel's difficulty-free solutions are thereby beyond the scope of this chapter. As was the case in the previous chapter, I recapitulate some developments in marine geology that are examined more thoroughly by Frankel. However, I pay closer attention to the functionality and development of measuring apparatuses and associated contexts of data acquisition. Additionally, I emphasize the range of ambiguities and debates in the study of marine geology and the cautious resolve to work through uncertainty. I will show

[171] Frankel, 2012, Volume III-IV
[172] Frankel, 2012, Volume I

how mobilist connotations emerged in marine geology toward the end of the 1950s from tentative attempts to unify a growing body of knowledge about the seafloor. Such mobilist interpretations developed in specific research contexts, rife with ambiguities, and highly distinct from research contexts in paleomagnetism.

I will first introduce the development of important measuring instruments, methods, and associated contexts of data acquisition that contributed to the growth of marine geology in the 20th century. I then examine developments pertaining to the identification and categorization of structural features of the seafloor following World War II. Toward the end of the 1950s, many researchers developed speculative descriptions of physical history that might account for collections of patterns in marine geology. Finally, I describe attempts to account for the phenomenon of marine magnetic anomalies. Though marine magnetic anomalies were typically considered to be of secondary importance during the 1950s, the interpretation of these anomalies would become centrally involved in the eventual surging support for mobilism later in the 1960s.

Measurements in Marine Geology

Prior to the middle of the 20th century, the vast majority of knowledge pertaining to the Earth, its structure, and history, came from the study of continents. Early research in marine geology was therefore typically imbued with expectations from continental geology. The fundamental reason for this pre-eminence of continental geology is that the study of the ocean floor poses an excess of technical challenges. The growth of research in marine geology in the 20th century is closely tied to the development of instruments and techniques that could overcome these challenges. A brief introduction to some of these technical developments will be useful for more detailed consideration of advances in marine geology leading up to the 1960s.

Bathymetry is the measurement of underwater depth, often for the purpose of topographical mapping. Early bathymetric work consisted of measuring lengths of a submerged weighted line, but depth measurements became increasingly sophisticated in the 20th century. Echo-sounding bathymetry consists in the emission of a wave burst followed by the detection of an echo from the seafloor. Given the speed of sound in water, the measured time interval between the initial burst and the return of the echo can be used to calculate the distance of the echoing body. High frequency bursts can be used to measure the surface of the seafloor. Alternatively, low frequency bursts can penetrate surface levels to measure subsurface structures. This is called seismic refraction.

Initial echo-sounding work consisted of a sounding device, hydrophones, and a chronometer. The first practical fathometer was developed in the 1920s, automating much of the echo measurement process. This allowed for bathymetric measurements to be taken with greater frequency and while a ship was in motion. Recording devices, or fathograms, became widely employed by the 1940s.[173] Uneven echoing surface or floating debris could result in multiple echoes or measurements that were not representative of the seafloor. Such confounding factors could be mitigated through multiple measurement passes, or by crosschecking echo-sounding against physical line measurements when possible.[174]

Seismic refraction studies often employed explosives to produce sounding bursts at established distances and times, such that a ship could measure variation in delays between echoes due to subsurface changes in seismic propagation. For seismic refraction, complex substructures (topographically or compositionally) would not always produce clearly or uniquely interpretable results. Multiple ships (or buoys) could be used to limit possible interpretations of complex substructures, but variation in relative position of ships or buoys would influence measurements. Echo-sounding measurements would typically be repeated in series to produce a profile of the ocean floor.[175] Combinations of parallel profiles could be used to map ocean floor topography and substructure. This was time consuming work. During the 1950s and 1960s, wider areas of seafloor could be measured concurrently by fanning out the directions of echo measurement, thereby facilitating rapid bathymetric measurements.

Following World War II, the state of bathymetric knowledge included a handful of ocean ridges, trenches, and guyots. The Mid-Atlantic Ridge garnered much attention since its discovery in the 19th century, due to its vast length and the absence of similar structures along the breadth of the Atlantic. There was some speculation that this ridge extended around Africa to join the Carlsberg Ridge in the Indian Ocean. Trenches were associated with island arcs. Early seismic measurements seemed to indicate that ocean crust was thinner than continental crust.

[173] Hawley, 1928
[174] Carstens, 1954
[175] Ewing et al., 1950; Hill, 1952

Seismology is the study of earthquakes and wave propagation through the Earth. Notable advances in early 20th century seismology mainly took place in the USA and Japan. Technical advances pertained to instrumental sensitivity, seismography network formation and standardization, and interpretative methodologies. Theoretical advances pertained to the differentiation of kinds of seismic waves, and higher-level inferences about the interior of the earth based on the measurements and comparison of wave propagation. The electromagnetic seismograph was first developed in the early 20th century, but notably improved by Hugo Benioff in the 1930s.[176] His vertical seismograph consisted of a 100KG mass, suspended by a spring, providing a period of approximately 0.5 seconds.[177] A magnetic circuit was situated beneath this mass, with a small gap between the bottom of the mass and the magnetic apparatus, such that vertical motion of the mass would result in variation in the magnetic flux across this gap. This variation would then produce a current within a coil system which could be graphed with a galvanometer. Long-period seismometers, used to measure more-distant seismic events, were developed in the 1950s.[178]

Interpretation of seismic events is based on the physics of waves and a collection of assumptions and generalizations about the medium through which these waves propagate. Man-made explosions were used to independently test and calibrate some of these generalizations. Even so, data from a single seismograph is infinitely interpretable. This interpretative flexibility can be limited by comparing seismic measurements taken from multiple locations. By measuring the amount of time that it takes for seismic waves to reach different seismographs, the epicenter of an earthquake can be triangulated. This requires the synchronization of a seismograph network. The hypocenter, the focal point where motion first takes place beneath the epicenter, can be calculated by the time delay between arrival of surface and compressional waves.[179] Following abortive effects of World War I, effort to form a global network of seismic observatories was renewed in the 1920s. Concurrently, methods developed for the interpretation of seismograph measurements so as to determine the direction of fault motion that takes place during an earthquake.[180] The *first-motion* of compressional waves, as measured

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[176] Benioff was born in Los Angeles in 1899 (Press, 1973). During his undergraduate work at Pomona College, he worked as a summer assistant on solar astronomy at Mount Wilson Observatory. Following graduation, he studied stellar radial velocities at Lick Observatory, but quickly departed for an assistant physicist position with the seismological program of the Carnegie Institution, Pasadena, California. He received his PhD from the California Institute of Technology in 1935 and was appointed assistant professor in 1937. During WWII, Benioff worked on the development of radar and underwater sound propagation.
[177] Benioff, 1932
[178] Warner, 2014
[179] Jeffreys, 1952
[180] Nakano, 1923; Byerly, 1926

on a seismograph, will be directly toward or away from the epicenter of an earthquake. By comparing measurements from a set of seismographs with suitable locations, the pattern of first motions can be used to establish faulting direction.

Following World War II, the US invested in seismic networking and interpretation to detect the detonation of nuclear bombs.[181] This funding drastically increased toward the end of the 1950s, immediately prior to international nuclear test bans.[182] During the 1960s, the US Department of Defense funded the development of the World-Wide Standardized Seismograph Network to monitor international adherence to the Partial Nuclear Test Ban Treaty of 1963. This global network consisted of over 100 seismic observatories, uniformly calibrated with identical instruments.[183] By the 1940s, the Earth's crust was inferred to be 30 to 35 km deep in continental regions, based on changes in wave propagation below this depth.[184] Oceanic crust seemed to be notably thinner than continental crust. It was well known that earthquake epicenters were associated with faults, and the majority of earthquakes were known to take place near the surface of the crust, but some measurements indicated earthquake depth of several hundred kilometers.[185] In 1949, Hugo Benioff identified diagonally-trending hypocenter points under continental margins adjacent to ocean trenches.[186]

Gravimetry is the measurement of the strength of the Earth's gravitational field. During the 1920s, Felix Vening Meinesz designed a gravimeter for use at sea.[187] By measuring the relative motion of a pair of pendulums, swinging from the same frame and of the same amplitude but opposite phase, horizontal acceleration will have opposing effects upon each pendulum such that difference between the motion of the two pendulums well remain undisturbed.[188] This can be used to eliminate effects of horizontal acceleration upon the use of a pendulum (as would otherwise prohibit ship-borne gravimetry) to measure local acceleration due to gravity. Vening Meinesz added a third pendulum, which hung

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[181] Barth, 2003
[182] From 1959 to 1961, seismology funding from the Advanced Research Projects Agency (ARPA) increased by a factor of 30, spending approximately $250 million for improvement of seismic detection (U.S. Congress, 1971, 19).
[183] Each station included both short and long period seismographs. Radio-synchronized clocks maintained the synchronicity of the system. Data collection was centralized.
[184] Bullen, 1940
[185] Gutenberg and Richter, 1949
[186] Benioff, 1949
[187] Felix Vening Meinesz was born in Scheveningen, Netherlands in 1887 (Bruins and Scholte, 1967). He attended the Delft Institute of Technology, graduating in civil engineering in 1910. Joining the Netherlands Geodetic Committee, he was tasked with gravity measurements in the Netherlands, where he worked with pendulum gravimeters. He received a doctor's degree in 1915, contributing to theory of pendulum measurements. He was a professor at the State University of Utrecht from 1927-1957, and the Delft Institute of Technology from 1938-1957.
[188] Vening Meinesz, 1929, 1941

freely, to collect two sets of measurements. Deviation from vertical alignment, and mechanical vibration could influence gravimetry measurements in unpredictable ways.

In 1923, Vening Meinesz began the first gravimeter expedition at sea, aboard a submarine of the Royal Netherlands Navy. Submarines used electric motors when submerged and avoided the accelerations caused by surface waves, thereby facilitating accuracy of measurements that would not be possible by ship. He continued his gravimetry measurements through the 1930s. His goal, shared by many researchers in the early 20th century, was to establish the shape of the Earth. Following World War II, Vening Meinesz type gravimeters were widely utilized. [189] In the 1950s, the spring gravimeter was also developed for gravimetry measurements at sea.[190] In a spring gravimeter, two taut springs are aligned horizontally with a weighted beam at their junction. Vertical acceleration of the beam is measured while horizontal disturbance is mechanically inhibited. When the beam is at its neutral position, two photoelectric cells are equally illuminated, but differential illumination takes place based on vertical motion of the beam, thereby producing an electric current which is then recorded.

Gravity measurements are infinitely interpretable. This is because inconsistencies in the force of gravity at the surface of the Earth may be due to variations in densities and distances of (especially) underlying materials. By comparing gravimetry measurements with other data, like seismic measurements, interpretative possibilities can be greatly narrowed. Vening Meinesz attempted to correlate his measurements with bathymetric data and found that ocean trenches and island arcs showed negative gravity anomalies. He hypothesized a physical structure called a *tectogene* to explain this anomaly, wherein local down-buckling of the ocean crust displaces denser mantle.[191] Harry Hess endorsed and further developed upon the tectogene hypothesis.

Hess was born in New York in 1906.[192] He graduated from Yale with a B.S. in 1927, studying geology. After two years as an exploration geologist in northern Rhodesia, he began graduate studies at Princeton where he received a PhD in 1932. Hess worked with Vening Meinesz in 1932, making gravity and bathymetry measurements in the Caribbean Sea for the US Navy.[193] After a brief time at Rutgers and the Geophysical Laboratory in Washington D.C., Hess joined the faculty at Princeton where he remained. In 1937, he embarked on another gravimetric survey, this time with Maurice Ewing (who will

[189] Yoshibumi, 2010
[190] Graf, 1958
[191] Vening Meinesz, 1934; also see Kuenen, 1936
[192] James, 1973
[193] United States Hydrographic Office, 1933

be introduced shortly), aboard the USS Barracuda where he found negative gravity anomalies associated with trenches.[194] During World War II, Hess worked on submarine detection, and eventually served as Commander of an attack transport ship which participated in the landings on the Marianas, Leyte, and Iwo Jima. During his time as a Commander, Hess used the ship's sounding equipment to expand bathymetric knowledge of the Pacific. He is often credited with the discovery of guyots during the war, which he published in 1946.[195]

Hess expanded upon the tectogene hypothesis.[196] He claimed that convection currents in the mantle pulled the crust down around tectogenes, thereby sustaining isostatic disequilibrium and explaining deep-focus earthquakes associated with trenches. Sediments are forced together and accumulate at the mouth of the down-buckling crust. Compensatory uplift adjacent to the tectogene results in the formation of island arcs. Hess also proposed that island arcs evolve into mountain belts.

Growth in the study of marine geology during the 20th century was closely associated with the development of remote sensing instruments and techniques. Just as import was political and economic demand for detailed measurements and associated technical capacities to deploy remote sensing instruments in increasing numbers. This demand was largely, though not exclusively, due to various military applications.[197] The close ties between military and mid-century oceanography had a profound influence upon research directions and frequently limited the free exchange of research due to government classification.[198]

Ridges, Trenches, Faults

Edward Bullard, at the National Physical Laboratory in the UK, pioneered ocean floor heat flow measurements in the early 1950s. On continents, temperature increases with depth, implying a flow of heat from inside the Earth to the Earth's surface. This heat flow can be estimated by measuring the temperature gradient of rock with known thermal conductivity. Bullard used a watertight probe nearly

[194] Hess, 1937
[195] Hess, 1946
[196] Hess, 1937, 1938a, 1938b, 1939
[197] Petroleum exploration, telecommunications, and insurance underwriting were also financial drivers of research.
[198] For a thorough examination of the broad influence of military funding and classification on American oceanography (especially at Lamont, Scripps and Woods Hole Oceanographic Institution), see Naomi Oreskes' *Science on a Mission* (Oreskes, 2021)

five meters in length to penetrate seafloor sediments.[199] Paired thermocouples inside the probe would then produce an electric current proportional to the temperature gradient. A galvanometer paired with a camera recorded these measurements before the probe would be winched to the surface. Physical jarring of the probe could produce mechanical noise in measurements, and the probe was only capable of penetrating soft sediment. Physical deformation of the probe was not uncommon. In general, heat flow measurements tend to assume stability in boundary conditions of a system, but this is a notable simplifying assumption.

Establishing the thermal conductivity of seafloor sediment involved the collection of cored sediment samples, followed by laboratory experimentation. A coring tube would penetrate the sediment and collect a core several feet long which would then be winched back to the surface.[200] The core would be extruded and cut into manageable sections which were then stored in an air-tight containers, inhibiting moisture loss. Sediment cores may not be representative of broader formations. Additionally, sediment may be inconsistently pushed aside rather than collected due to friction during coring, resulting in a tendency toward samples that can misrepresent sediment ratios in a column. Measurements of thermal conductivity in the lab involves the application of a known quantity of heat at certain distances from which the transfer of heat can be measured. This facilitates the formation of tables of temperature conductivity for sediments of varying compositions. Such measurements of thermal conductivity often determined that water content of sediment was of greater importance than mineralogical constitution. Accordingly, questions arose pertaining to the generalizability of laboratory measurements to deep ocean conditions.[201] In-situ conductivity measurements only developed toward the end of the 1950s.[202]

Heat flow measurements in the early 1950s showed that ocean and continental heat flow were roughly the same.[203] Continental rock was typically presumed to contain more radioactive material than oceanic rock, so this measured similarity in heat flow was unexpected. The high oceanic heat flow could be accounted by adjusting assumptions about the distribution of terrestrial radioactive materials and/or adjusting assumptions about the large-scale transfer of thermal energy within the earth, often by

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[199] Bullard, 1954
[200] The development of the piston corer during the late 1940s greatly improved coring depth and orientation. A piston corer consists of a weighted hammer atop the coring tube which drives the tube deeper into the sediment with successive blows.
[201] Bullard, 1954
[202] Von Herzen and Maxwell, 1959
[203] Revelle and Maxwell, 1952; Bullard 1954

postulating the existence of internal convection currents.[204] Upwelling of convection currents was often cited as an explanation for high heat flow measurements, while downwelling explained low heat flow.[205] Continued heat flow measurements through the 1950s showed that lowest oceanic heat flow measurements often took place near trenches, while ridges showed unusually high heat flow measurements.[206] By the end of the 1950s, Bullard attributed these measurements to patterns of convection currents in the mantle.[207] By this time, convection currents were also widely employed as a causal mechanism to explain physical features of marine geology.

In addition to heat flow measurements, remote sensing data accumulated during the 1950s and 1960s which facilitated the identification and classification of structural features of the ocean floor. Though several additional notable centers of research contributed to this general effect, the two most notable institutions for data collection at this time were Scripps and Lamont.[208]

The Scripps Institution of Oceanography (Scripps) at UC San Diego, so named in 1925, grew from a research institute founded primarily out of interest in the study of marine life. Scripps received substantial funding from the National Defense Research Committee and then the Office of Scientific Research and Development immediately prior to and during WWII.[209] Submarine warfare and underwater sound garnered substantial attention. Several Scripps researchers were subsequently involved in research and consultation associated with oceanographic components of post-war atomic tests in the Pacific. Following the War, the Office of Naval Research provided substantial funding to Scripps. During the 1930s, the majority of graduate students at Scripps were in biological sciences, but military funding directed research toward physical oceanography. By the 1950's the curriculum at Scripps had notably shifted toward physical, chemical, and geological work.[210]

The Lamont Geological Observatory (Lamont), was established in 1949 at Columbia University, under the directorship of Maurice Ewing.[211] The methodology employed at Lamont, under Ewing's

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[204] Ibid.
[205] Bullard., Maxwell and Revelle, 1956
[206] In 1956, the highest heat flow values that Bullard identified were from the Albatross Plateau in the East Pacific which he assumed to be a relatively featureless region, later identified as a portion of the East-Pacific Rise.
[207] Bullard., Maxwell and Revelle, 1956; Bullard and Day, 1961
[208] Other institutions of note include Woods Hole Oceanographic Institution, the Naval Electronics Laboratory in San Diego, and Cambridge's department of Geodesy and Geophysics.
[209] Shor, 2003
[210] Oreskes, 2000
[211] Maurice Ewing was born in Lockney Texas in 1906 (Bullard, 1975). He received a B.A. (1926), M.A. (1927), and PhD (1931) from Rice University, where he worked on refraction seismology in the physics department. In 1929, Ewing became an Instructor of Physics at the University of Pittsburgh, and then worked at Lehigh until 1940. His

direction, was directed toward data collection, and the manufacture of cheap and effective instruments to improve global capacity for data collection.[212] Lamont obtained a 200' pleasure yacht from the US Navy (used as a training ship for the US Merchant Marines), that was refitted and installed with a modified navy echo sounding system and a seismic reflection system.[213] Initial funding came from the US Navy: Lamont was involved in setting up a sound surveillance system in Bermuda, useful for the US Navy in detection of submarine activity in the Atlantic.

In 1949, researchers at Lamont published bathymetric measurements of the Mid-Atlantic Ridge, primarily gathered during a 1947 expedition.[214] They reported on the flatness of the tectonically inactive Atlantic basin between Bermuda and the Azores and reported that the Mid-Atlantic Ridge consists of parallel sections. They also noted a seismically active east-west trending trough-like region, which they interpreted as a graben, indicative of tensional forces. Subsequent mapping of this trough region was classified by the US Navy.[215] The Mid-Atlantic Ridge was considered to be a product of folding and faulting.[216]

Meanwhile, Henry Menard[217] and Robert Dietz[218] were collaborating on research at Scripps and the US Navy Electronics Laboratory. They conducted a bathymetric survey of the northeast pacific in 1949, identifying new structures including ridges, but their work was classified. In 1952, they also

research mainly pertained to seismic prospecting until 1934, when he was asked by Dick Field, chairman of the Committee on the geophysical study of the ocean basins at the AGU, to apply his expertise on seismic refraction to study the continental shelf. Ewing thereby engaged in pioneering work on seismic refraction measurements at sea, and subsequently in deep water. Ewing joined Harry Hess on the USS Barracuda in 1937. In 1940, Ewing became a Research Associate at the Woods Hole Oceanographic Institution in Massachusetts. Obtaining contracts from the Bureau of Ordnance and the Bureau of Ships of the U.S. Navy, Ewing worked on remote sensing with possible wartime applications. Following WWII, Ewing took a position in the geology department at Columbia University and became the founding director of Lamont in 1949. Ewing was a giant of American marine geology. Along with Menard, at Scripps, Ewing might be called the most influential marine geologist or the 1950s.

[212] Bullard, 1980

[213] Gray, 1956

[214] Tolstoy and Ewing, 1949

[215] Frankel, 2012, Volume III, 372

[216] Tolstoy, 1951

[217] Menard was born in Fresno, California in 1920 (Menard, 1986). He received a degree in geology from Caltech in 1942 and joined the US Navy where he was trained as a photo interpreter. Following the war, Menard returned to Caltech for a M.S. in geology, and received a PhD from Harvard in 1949, studying sediment transport. He was hired as a lecturer at Scripps in 1950 and became an associate professor there in 1955.

[218] Dietz was born in New Jersey in 1914 (Koppes, 1998). He obtained a PhD in geology from the University of Illinois in 1941, during which time he mapped submarine canyons off California. Much of his graduate work was done at Scripps. During WWII he spent time photomapping in South America. Following the war, he joined the US Navy Electronics Laboratory in San Diego where he worked from 1946 to 1963. He was an adjunct professor at Scripps from 1950 to 1963.

identified a very long asymmetrical ridge in the Northeast Pacific which they named the Mendocino submarine escarpment.[219] The Eastern extent of this escarpment seemed to meet the San Andreas fault. They interpreted the asymmetry as a product of different rock type and density on adjacent sides of the escarpment.

In 1955, Robert Fisher,[220] at Scripps, provided a general theory of the evolution of ocean trenches.[221][222] Noting collections of negative gravity anomalies at trenches, association with deep focus earthquakes, and seismic refraction measurements indicative of oceanic crust, he argued that some force must pull the ocean crust downward at trenches. He appealed to low heat flow measurements as indicative of downwelling convection currents, which might provide this force. The deepest trenches contain virtually no sediments because the sediment is being pulled into the mantle along with crustal material. When convection ceases, the trenches will fill with sediment and then rise isostatically to produce island arcs. Later that same year, Fisher and colleagues at Scripps published seismic refraction measurements of the Tonga trench in the West Pacific, indicating that oceanic crust is about twice as thick below the trench than at adjacent regions.[223] A similar thickening of crust was found at the Puerto Rico trench by Lamont researchers the following year.[224]

Researchers at Lamont disagreed with those at Scripps on the fundamental nature of trenches. In the mid 1950s, researcher at Lamont argued that trenches were tensional features.[225] Tensional forces resulted in thinning of the ocean crust, where sediment accumulates. Gravity anomalies were interpreted to be the product of a thin crust overlain with sediment. The thinning of the crust results in isostatic imbalance, so the trench is eventually uplifted and deformed to produce island arcs. Against this view, Fisher pointed to the diagonal trend of earthquake foci adjacent to ocean trenches as indicative of continental overthrusting of oceanic crust due to compressional force.[226]

Hess continued to endorse the tectogene hypothesis. He argued that convection currents rise beneath continents, and tectogenes form when two adjacent convection cells meet and sink together.[227]

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[219] Menard and Dietz, 1952
[220] Fisher was born in Alhambra California in 1925. He graduated from Caltech in 1949 and received a PhD at Scripps in 1957 (UC San Diego, 2012).
[221] Fisher and Revelle, 1955.
[222] Fisher and Revelle wrote that their work offered "tentative answers" (Fisher and Revelle, 1955, 36).
[223] Raitt, Fisher and Mason, 1955
[224] Officer et al., 1957
[225] Worzel and Shubert, 1955; Ewing and Heezen, 1955
[226] Fisher, 1961
[227] Hess, 1951

Hess disagreed with the implication that diagonally-trending earthquake foci pointed to faulting. In previous work, he attributed this diagonal trend to the convection cell on the continental side of the trench. According to Hess, the tectogene structure is symmetrical, with a vertical fault down the middle. Against Lamont, Hess added the objection that isostatic rise due to tension should take place rapidly, and that Lamont provided no account for the origin of the tension.[228]

Returning to ridges, Maurice Hill,[229] at Cambridge, identified a valley at the center of the Mid-Atlantic Ridge in 1953.[230] This structure was also identified by Marie Tharp[231] at Lamont around this time, but her work was not published. Through most of the 1950s, Tharp worked under Bruce Heezen[232] at Lamont on projects relating to seafloor mapping, especially around the Mid-Atlantic Ridge. She identified a valley in the ridge, but Heezen was not immediately convinced of the generality of this feature.[233] In 1956, Heezen and Ewing associated this central valley with shallow focus earthquake locations.[234] Mapped earthquake epicenters indicated structural contiguity between the Mid-Atlantic Ridge into the Indian Ocean to the Carlsberg Ridge.[235] The Carlsberg ridge extends northward to the Gulf of Aden and into the Red Sea, which also marks the northern extent of the Great Rift Valley. Considering the structural and seismic similarities between the Great Rift Valley and the valley atop the Mid-Atlantic Ridge, Heezen proposed that ridge valleys were rift valleys, tensional structures, and suggested a common age and origin.[236] Furthermore, Heezen and Ewing proposed that this rift was contiguous from

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[228] Fisher and Hess, 1963

[229] Hill was born in 1919, the son of A.V. Hill, Nobel prize recipient in Medicine, and Margaret Keynes, John Maynard Keynes' sister (Bullard, 1967). After obtaining a degree from Cambridge, he become a research student in the Department of Geodesy and Geophysics, working under Bullard. He received a PhD in 1951, studying seismic observations. He remained at Cambridge and was elected Fellow of the Royal Society in 1962.

[230] Hill, 1955

[231] Born in Michigan in 1920, Tharp earned an MA in geology from University of Michigan (Tharp, 1999). She took a job with Stanolind Oil and Gas Company and obtained a degree in mathematics at the University of Tulsa attending night classes. She took a job at Columbia University under Ewing in 1948 and followed him with the formation of Lamont.

[232] Heezen was born in in Iowa in 1924 (Frankel, 2012, Volume III, 374). He attended the University of Iowa, studying physics, zoology and geology. He met Maurice Ewing (then at Woods Hill Oceanographic Institution) in 1947 and under Ewing's guidance, Heezen embarked on an expedition to obtain samples on the continental shelf of the eastern US. He obtained a degree from University of Iowa 1948, majoring in geology. He then joined Ewing at Woods Hole and participated in several expeditions in the Atlantic, including the first expedition to tow a magnetometer over the Mid-Atlantic Ridge (Heezen, Ewing and Miller, 1953). Heezen followed Ewing to Lamont, eventually obtaining his PhD from Columbia in 1957.

[233] Tharp and Frankel, 1986

[234] Ewing and Heezen, 1956a

[235] Rothe, 1954

[236] Heezen, 1956

the North Atlantic, around South Africa, to the Red Sea and Great Rift Valley.[237] By the end of the 1950s, due to accumulated bathymetric and seismic data, there was growing expectation that many large ocean ridges formed a globally continuous network, though the details were not always agreeable.[238]

Ridges vary in length, width, depth, and relief. Trenches, basins, guyots, and other seafloor structures demonstrate similar variabilities as well. Consequently, the practice of classifying and relating seafloor structures is fraught with ambiguity. Even though the Albatross Plateau was first bathymetrically surveyed in the 19th century, it was only widely identified as a ridge toward the end of the 1950s. Alternatively, in 1958, Menard identified a region in the Central Pacific called the Mid-Pacific Ridge to be part of the globally contiguous ridge system,[239] but he no longer considered the Mid-Pacific Ridge to be a ridge by 1965.[240] Bathymetric mapping off the West Coast of North America resulted in a prolonger period of uncertainty about the nature of several mapped structures through the 1950s and into the 1960s. The Mendocino Escarpment, for example, was first considered to be a product of different rock densities akin to continental margins,[241] but subsequent discovery of parallel escarpments resulted in alternative identification as unique fracture zones in the ocean crust.[242] Still later, the fractures were identified as faults.[243]

Establishing a contiguous rift system thereby required a willingness to generalize from highly incomplete and ambiguous data, and different researchers did not always agree on when this was warranted.[244] Correlations between data sets could facilitate such generalizations but establishing such correlations itself required a willingness to generalize. Again, not everyone agreed when this was permissible. For example, Menard and Heezen disagreed on the general correlation of rift valleys and ridges. This difference resulted in differential interpretation of bathymetric data. In 1958, Menard wrote to Heezen that the East Pacific Rise does not contain a rift valley. Looking at the same bathymetric data, Heezen claimed a rift valley was present.[245] Similarly, Menard's inclusion of a Mid-Pacific Ridge in a

[237] Ewing and Heezen, 1956b
[238] Ewing and Ewing, 1957; Menard, 1958a
[239] Menard, 1958a
[240] Menard, 1965a
[241] Menard and Dietz, 1952
[242] Menard, 1955
[243] Menard, 1960
[244] Hill, for example, noted that the continuity of the rift system was, "open to much doubt" (Hill, 1957).
[245] Menard, 1986, 103-105.

globally continuous rift system was in large part due to his expectation that this system tended to reside in a median position with respect to the continents, a generalization that he later amended.

Ultimately, not all ridges contain rift valleys. Also, ridge systems are not fully continuous, but are frequently interrupted by lateral offsets of varying scale. Even the Mid-Atlantic Ridge consists of many of these offsets between ridge segments. Early work at Lamont and Scripps identified such offsets, but they were not recognized to be a general feature interrupting the continuity of ridges until the 1960s. Interestingly, Heezen's correlation of seismically active rift valleys with ocean ridges – which prompted his endorsement of a globally continuous ridge system – facilitated the eventual identification of ridge offsets. Bathymetrically, ridges are often so wide that slight offsets are undetectable, whereas offsets in the rift valley can be identified more readily.

Higher-level theoretical accounts of the formation of ridges diversified in the second half of the 1950s. Prior to this point, it was common to suppose that the Mid-Atlantic Ridge was an ancient folded mountain belt, or the product of accumulated volcanic extrusions or intrusions.[246] In 1954, Hess considered three hypotheses on the origin of oceanic ridges.[247] First, a ridge may be an accumulation of extruded basaltic lavas along a line of fractures in the crust. Second, an upward convection current in the mantle may result in the intrusion of lower density rock into the ocean crust, resulting in isostatic uplift. Third, a downward convection current in the mantle could cause thickening and buckling of the crust. The following year, Hess claimed that rising convection currents could elevate the isotherm of an exothermic metamorphic process that decreases the density of crustal rock.[248] This change in the isotherm thereby increases local rock density, causing subsidence. Cessation of convection then lowers the isotherm, decreasing density via an exothermic metamorphic process, causing uplift. In 1957, Ewing measured low seismic velocity under the Mid-Atlantic Ridge and speculated that this was due to physical mixing of mantle with crustal material.[249] Appealing to Hess' 1954 work, Ewing claimed that upwelling convection produced mantle intrusions, and resulting extensional force resulted in the ridge valley. In 1960, Ewing elaborated that trenches ringing the pacific were compressional, and an alternative pattern of compressional and tensional belts in the Earth's crust provided strong evidence for convection currents in the mantle.[250] Ewing also speculated that when the Earth was molten, and prior to

[246] Hess, 1939, 1954; Tolstoy, 1951
[247] Hess, 1954
[248] Hess, 1955
[249] Ewing and Ewing, 1957
[250] Frankel (2012, Volume III, 412) cites Ewing, 1960.

solidification of the crust, convection currents may have broken apart a supercontinent, thereby accounting for the median position of ocean ridges.

The study of ocean floor sediment contributed to the interpretation of seafloor structures, including ridges, especially toward the end of the 1950s. Seismic measurements of the thickness of seafloor sediment following WWII showed that sediment cover was substantially thinner than previously expected.[251] If ocean basins were ancient structures – as presumed by permanentists – then the oceans should have accumulated sediment many kilometers thick.[252] Accumulating measurements through the 1950s indicated that ridges, especially, showed very thin sediments.[253] Perhaps most significantly, all dredged or cored fossils were found to be no older than the Cretaceous.[254] Several possible explanations for these features of seafloor sediments were proposed. Sediment may be underlain by consolidated sedimentary rock, though permissible thickness of this consolidated material would still be slight.[255] Sedimentation rates in the deep past may have been substantially lower than today. There may be some mechanism through which the seafloor is cleared of sediments, perhaps by consolidation into continental masses. Finally, the seafloor may be young.

Beginning at the end of the 1950s, several prominent researchers offered alternative accounts for the origin and evolution of ocean basins as relatively youthful features of the Earth. Menard developed a theoretical account for his seafloor mapping projects. Having identified large parallel fracture zones in the East Pacific, Menard tentatively hypothesized in 1955 that convection currents caused crustal shortening stresses, producing long parallel fractures.[256][257] In 1958, he compared the correlated oceanic ridges with lines bisecting oceans.[258] He argued that ridges evolve over time and offered an explanation for their origin in terms of convection currents. Upwelling takes place at the middle of oceans, producing oceanic ridges. The currents then move horizontally across ocean basins and sink near continental margins, producing trenches. Horizontal currents stress the crust, causing fracture zones. Also in 1958, Ronald Mason,[259] then at Scripps, identified offset magnetic anomaly

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[251] Hess, 1962; Ewing, 1963
[252] Kuenen, 1950
[253] Hill, 1960
[254] Hess, 1962
[255] Hamilton, 1959; Hamilton, 1960
[256] Menard, 1955
[257] Like many theoretical papers, Menard was explicit that his conclusions were tentative (Menard, 1959, 1149).
[258] Menard, 1958a
[259] Mason was born in Winsor, UK, in 1916 (Goble, 2009). During WWII he worked with the Corps of Royal Electrical and Mechanical Engineers. He graduated from Imperial College of Science and Technology with an MSc and became a lecturer in Geophysics at Imperial College in 1947. Mason became chair of Geophysics at Imperial

patterns (which will be introduced shortly) across Menard's fractures, indicating that these structures were, in fact, strike-slip faults rather than stress ruptures.[260] This spurred Menard to modify his account, and integrate lateral motion of the ocean floor.[261] In 1960, Menard claimed that the horizontal motion of convection currents causes the ocean crust to thin and stretch near ridges. Ocean floor stretches laterally, away from ridges, and variations in the strength of convection currents result in variable degrees of stretching, thereby producing strike-slip faults, and accounting for offset magnetic anomalies. Menard also speculated that this stretching might result in continental displacements.

Though most researchers at Lamont worked within a fixist framework, Heezen endorsed Earth expansion.[262] According to Heezen, ridge valleys are tensional features, expanding over time. They may begin as continental rift valleys. The continental crust is split apart, and underlying oceanic crust thins. Rift valleys thereby expand into seas and eventually oceans.[263] Material from the mantle rises to the rift, producing an oceanic ridge.[264] Heezen noted that Africa and Antarctica are surrounded on all sides by oceanic ridges.[265] He also noted that continental margins are typically surrounded by tensional features.[266][267] Accordingly, Heezen endorsed the notion that the radius of Earth had gradually increased, perhaps due to increase in the gravitational constant over time.[268][269]

In 1960, Hess offered an account of the formation of ocean basins that came to be called seafloor spreading.[270] He called his theory "geopoetry" and began the paper as follows: "The birth of the oceans is a matter of conjecture, the subsequent history is obscure, and the present structure is just beginning to be understood."[271] He argued convection currents rise at ridges and then move horizontally

College in 1967. During his time at Imperial College, Mason worked closely with researchers at Scripps. In 1952, Mason joined an expedition with Scripps researchers, where he participated in magnetic mapping and the survey of the Tonga Trench.

260 Mason and Raff, 1961

261 Menard, 1960

262 Heezen first endorsed Earth expansion in 1957 (Heezen, 1957). His position was not, however, definitive. In 1959, he wrote, "in view of the meager evidence now available, this may seem too drastic and may itself have other more serious objections of astronomical nature" (Heezen, 1959b).

263 Heezen, 1957, 1959a

264 Heezen, 1960

265 Heezen, 1962

266 Heezen, 1959a

267 Recall that Heezen considered trenches to be tensional.

268 Heezen, 1960

269 During the 1960s, Heezen's support for expansionism waned, and then he stopped endorsing expansionism altogether, re-contextualizing his expansionist leanings as support for mobilism (Heezen and Fox, 1966).

270 Hess only published this account in 1962 but circulated draft work in 1960 (Hess, 1962).

271 Hess, 1962, 599

under the crust, perpendicular to the orientation of the ridge. New seafloor is created at the center of oceanic ridges. Older ocean crust moves laterally away from the rift valley on both sides of the ridge, driven by lateral convection currents, carrying continents along for the ride. Dietz added that trenches are the location of convergence and downwelling convection currents, where dense oceanic crust plunges beneath continental crust.[272] Dietz also claimed that sediments along the edges of spreading seafloor become incorporated into continents. In a paper with Fisher in 1963, Hess expanded on seafloor spreading, arguing that oceanic crust descends into the mantle at ocean trenches.[273] Downthrusting crust releases water and magma which rises on the convex side of the trench. Fisher thought the oceanic crust descended diagonally toward continental margins, as indicated by diagonal trends in earthquake foci. Hess disagreed, citing first motion seismic studies showing horizontal displacement near vertical faults.

Magnetic Anomalies

In 1955, Scripps was commissioned by the US Navy to survey the West Coast of North America for the purposes of submarine operations. Magnetometers were attached to the survey vessels at minimal extra cost. In 1953, Lamont researchers had described a pattern of magnetic anomalies in the Atlantic between Dakar and Barbados.[274] The work at Scripps was far more extensive. Resulting measurements were compiled into maps, and a zebra pattern of magnetic anomalies was identified. In 1957, Lamont researchers measured a strong magnetic anomaly at the center of the Mid-Atlantic Ridge.[275][276] In 1958, by consulting magnetic anomaly maps, researchers at Scripps determined that the pattern of magnetic anomalies in the East Pacific was offset across Menard's great fractures.[277] In some locations, the offset spanned over 600 nautical miles.[278]

Such measurements were made with magnetometers either towed behind ships or, occasionally, behind airplanes. During World War II, Victor Vacquier developed an airborne fluxgate

[272] Dietz, 1961
[273] Fisher and Hess, 1963
[274] Heezen, Ewing and Miller, 1953
[275] Frankel (2012, Volume III, 71) cites Ewing, Heezen and Hirschman, 1957
[276] In 1953, Hill explicitly stated that the median valley of the Mid-Atlantic Ridge was not associated with any large magnetic anomaly (Hill, 1953)
[277] Mason, 1958
[278] Vacquier, Raff and Warren, 1961

magnetometer for the detection of submarines.[279] A fluxgate magnetometer consists of a ferromagnetic core wrapped by two coils. An alternating current is applied to one coil, thereby producing a magnetic field with alternating polarity. This induces an electric current in the second coil. If the core is oriented in the same direction of an ambient field, the induced magnetic field will be increased. If the core is oriented in the opposite direction of an ambient field, the induced magnetic field will be decreased. Two parallel cores with induced magnetization of opposite polarity will thereby alternate polarity slightly out of sync with one another in a manner that is proportional the ambient field. This phase shift produces a voltage that is proportional to the intensity of the ambient field.

Proton precession magnetometry was developed in the 1950s, initially for land based magnetometry surveys, but rapidly taken up by marine geologists.[280] Proton precession consist of a container filled with a proton rich fluid. A coil induces a magnetic field in this container such that the protons therein align. When the induced field ceases, the protons align with the ambient magnetic field and precess at a frequency that is proportion to the strength of the ambient field. The precession results in a fluctuating magnetic field around the coil which can be measured.

Typical magnetic profiling work consisted of a ship or airplane making a linear pass over some region or structure of interest. Often, only variation in the strength of the magnetic field would be measured. Fluctuations in the measurable magnetic field were recorded and often graphed to illustrate magnetic variations with respect to distance. This graph is called a magnetic profile. Numerous profiles taken from evenly spaced passes in a region can be compiled to produce a map of magnetic anomalies. Directional stabilization of the magnetometer is required for coherent measurements. Navigational precision must also be maintained. Diurnal variations in the ionosphere can influence local measurements. These effects can be subtracted out by identifying inconsistencies at crossover points, or with reference to a nearby stable magnetic observatory. Larger scale corrections involve subtracting out the effect of estimates of the Earth's dipolar field.

Following the discovery of magnetic anomalies over the oceans, the source of these anomalies was subject to much speculation. The stability of the measurements seemed to support the notion that the seafloor was the source of the anomalies. Researchers at Lamont initially attributed the effect to variations in the induced magnetization of the seafloor.[281] In 1958, Mason argued that the volcanic layer

[279] Vacquier, 1946
[280] Packard and Varian, 1954; Waters, 1955
[281] Heezen, Ewing and Miller, 1953

of seafloor – the most likely candidate for strong magnetic properties – was too thin for induced magnetism to account for the measured anomalies.[282] Accordingly, remanent magnetism obtained during rock formation was offered as an alternative explanation. The importance of remanent magnetism was further supported by the study of dredged and cored samples of seafloor rock. In 1960, Maurice Hill and colleagues at Cambridge found a high degree of remanent magnetism in dredged basalts.[283] Bullard and Mason described high variation in magnetic susceptibility of rock dredged in the North-East Pacific,[284] but subsequent work showed the natural remanent magnetism in dredged basalt to be one or two orders of magnitude greater than in continental basalt.[285]

If magnetic anomalies were due to remanent magnetism of the seafloor, then how to account for the striation pattern of these anomalies? According to Mason, the positive anomalies were due to NRM, while the negative anomalies were weakly magnetized rock. [286][287] He speculated on large lava flows in Earth's history that filled pre-existing troughs. The cooling rock retained its magnetic orientation aligned with the Earth's field, thereby resulting in the zebra pattern of positive anomalies. Alternatively, the negative magnetic anomalies could be due to reversals of remanent magnetism. Initial speculation on this interpretation suggested that intrusive material underwent self-reversal in-situ. Field reversals were deemed to be implausible due to the striation pattern of the anomalies which would not be expected of baked rock adjacent to intrusions.[288] In 1961, Mason suggested that magnetic anomalies may be due to fluctuations in thickness of the magnetic rock, or intrusions of magnetic material from the mantle.[289] Researchers at Lamont related depth of the curie point beneath the ridge crest to remanent magnetism.[290] Vacquier, at Scripps, attributed the striation pattern of magnetic anomalies to mineralogical differences caused by local variations in geothermal gradients producing blocks of basalt magnetized in the direction of the current field.[291]

[282] Mason, 1958
[283] Laughton, Hill and Allan, 1960
[284] Bullard and Mason, 1963
[285] Ade-Hall, 1964
[286] Mason, 1958
[287] In the Northern Magnetic Hemisphere, positive anomalies would be produced by a magnetic body oriented toward the magnetic North Pole. This is not the case near the magnetic equator or the Southern Magnetic Hemisphere. Unless stated otherwise, when I refer to positive and negative anomalies, I will be assuming a location in the Northern Magnetic Hemisphere.
[288] Girdler and Peter, 1960
[289] Mason and Raff, 1961
[290] Talwani, Heezen and Worzel, 1961
[291] Vacquier, 1962

Even if magnetic bodies on the sea floor are deemed to be the source of the magnetic anomalies, there are many possible ways to account for any given magnetic profile. The distances between alternatively magnetized bodies, their width, depth, shape, and the magnetic susceptibility, strike, distance, and inclination of the rock are all parameters that could be adjusted to fit a magnetic profile. The calculations involved are well defined, but with so many parameters, the utilization of computer modeling greatly sped up the process of identifying permissible states of the ocean floor that could account for magnetic profiles while also accommodating permissible variations in measured variables. Early computer programs assumed a two-dimensional space consisting of adjacent blocks of vertical-sided magnetic bodies. During the 1950s, researchers at Lamont developed computer programs to assist in the possible interpretation of magnetic profiles.[292] Edward Bullard developed such a program for the EDSAC 2 computer at Cambridge.[293]

Conclusion

In Chapter 2, a fairly linear narrative developed around a handful of mainly British paleomagnetists who developed an increasingly sophisticated set of practices, methods, and interpretative structures in the study of rock magnetism. The history of marine geology is not so readily told in chronological order, and culminations are not so apparent during the 1950s. Still, some relevant patterns can be highlighted.

Instruments were often subject to many known sources of error, and measurements sometimes disagreed. Many marine geologists tended to avoid high-level theoretical work, instead directing their efforts toward data collection and lower-level interpretations. Even so, collected data was often infinitely interpretable, and generalizations were often made from highly limited data sets. Correlations between different kinds of measurements facilitated generalizations and could impose restrictions upon interpretative flexibility. Some researchers indulged in high-level theoretical work, but such theorizing was typically highly speculative. The speculative nature of theoretical work was apparent in the language that researchers used, the tendency in publications to offer multiple hypotheses, and the tendency for particular researchers to rapidly modify their positions.

[292] Press and Ewing, 1952
[293] Bullard, 1961

By the early 1960s, it was widely recognized that marine geology is distinct from continental geology. The seafloor has unique structures – ridges, trenches, guyots, basins, and long linear parallel faults - and these structures seemed to be genetically connected across vast distances. Ridges were typically interpreted to be tensional structures, forming an extensive system, often in positions median to continents. Most, but not all, considered trenches to be compressional structures. There was strong indication from seafloor sediment that ocean basins were relatively young features, but this was not universally affirmed. Magnetic anomalies were often considered to be the product of magnetic properties of the ocean floor, and increasingly, this was attributed to remanent magnetism. Seafloor structures were sometimes accounted independently from one another but, toward the end of the 1950s, theories frequently attempted to relate structures to one another. The most ambitious accounts attempted to explain all unique seafloor features, while also remaining accountable to bathymetry, heat flow, gravimetry, and seismic measurements. Magnetic anomalies were often, at least implicitly, treated as secondary phenomena, not a primary feature of the seafloor requiring a place within unifying theories. Genetic theories frequently appealed to convection currents, but the nature of these postulated currents was quite variable. Alternative researchers proposed convection currents of varying size, location, number, and effect. The notion of mobilism was gaining some attention by the end of the 1950s, largely due to the absence of any pre-Cretaceous seafloor sediment and offset magnetic anomalies indicative of substantial lateral motion of the seafloor.

The 1950s is often portrayed as a decade of resurgence in the mobilism debate, wherein new geophysical data obtained by paleomagnetists and marine geologists became conducive to mobilist interpretation. This is surely an important time in the history of the mobilist debate, but by the beginning of the 1960s, amassed geophysical data was not itself sufficient to motivate widespread uptake of mobilism. Some paleomagnetists strongly endorsed mobilism, but even the strongest proponents recognized that specific mobilist interpretations were often complicated by the uncertainty of polar wander or accumulation of sloppy measurements. Alternatively, mobilist interpretations in marine geology were typically highly speculative. Even those marine geologists who endorsed mobilism were often not strongly convinced, and their convictions rapidly changed. With Oreskes, it may be most accurate to state that accumulated geophysical data in the 1950s was conducive to a multitude of possible interpretations, as measuring methods and accumulated data were not so strictly connected to the interpretative frameworks of prior research contexts with bearing on the mobilism debate.[294]

[294] Oreskes, 1999

Indeed, in Chapters 2 and 3 I have attempted to show how mobilist interpretations in paleomagnetism and marine geology arose from research contexts that were, initially, largely divorced from the mobilism debate.

It is also apparent from this and the previous chapter that research contexts in paleomagnetism and marine geology in the 1950s were largely distinct. Those paleomagnetists who endorsed mobilism in the 1950s were not particularly concerned with developments in marine geology. Likewise, those marine geologists who endorsed mobilism in the 1950s were not particularly concerned with paleomagnetism. As argued by Frankel, proponents of mobilism in marine geology sometimes appealed to paleomagnetic evidence in their work, but this was to bolster already-established hypotheses by diversifying apparent evidence.[295] Marine magnetic anomalies were increasingly interpreted as a product of rock magnetism, perhaps relating to remanent magnetism or self-reversals, but this area of overlap between marine geology and paleomagnetism was not particularly relevant to mobilist frameworks developed in either field.

[295] Frankel, 2012. For examples, see Hess, 1962; Menard, 1958b; Heezen, 1959b

Chapter 4: Geochronology

Introduction

Geochronology is the study of the age of rock. According to the principle of superposition in geology, younger rock is deposited atop older rock. Intrusions are younger than the rocks into which they intrude. Pebbles within conglomerates are older than those conglomerates. Faults are younger than the rocks through which they pass. General principles, such as these, are of fundamental importance to the relative dating of rocks and formations. In the early 19th century, British and French researchers determined that successive formations contained unique sets of fossil fauna which could be used to date and correlate formations across great distances. This method of relative dating was expanded by identifying overlaps in the range of different fossil species. Much work in geology during the 19th century pertained to the identification of geological formations and their relative ages by piecing together these stratigraphic and biostratigraphic dating indicators. By the end of the 19th century, the relative age of a series of major geological formations was well established in what is called the *geological timescale*.[296] Refinements continued thereafter. During the 20th century, such refinement of the geological timescale underwent a notable transformation as research in radiochemistry facilitated the determination of the absolute age of rock. This chapter examines the development of such absolute dating methods from the beginning of the 20th century to about 1960.

During the 19th century, various efforts were made to measure absolute geological time, either with respect to providing the absolute age or duration of a geological structure or event, or in broader attempts to measure the age of the Earth. In either case, these efforts typically consisted of the identification of some contemporary measurable physical process that was assumed to remain consistent or predictable over time. In 1833, Charles Lyell estimated contemporary lava accumulation rates on Mount Etna, and thereby concluded that a great amount of time would be required to account for Etna's size.[297] Additionally, underlaying Etna's lava flows were fossils of recent origin, implying that Etna is a relatively young feature of the Earth. Lyell's attempt at dating Mount Etna related to his broader endorsement of uniformitarianism, wherein contemporary processes (acting over vast expanses

[296] The development of the geological timescale is one of the chief accomplishments of 19th century geology, requiring international cooperation between several generations of geologists. The formation of the geological timescale required the application of various principles of relative dating, but also involved local and global interpretations of physical history. Consequently, the development of the geological timescale was hardly straightforward, and consensus needed to be forged through sometimes prolonged debate over ambiguous evidence that could be interpreted in multiple ways (Rudwick, 1985; Secord, 1986).
[297] Lyell, 1833

of time) are considered to be the key to understanding geological history. In the First Edition of *On the Origin of Species,* Charles Darwin estimated contemporary erosion forces to calculate the age of The Weald in Southeast England based on its presumed original extent.[298][299] He estimated the age of The Weald to be over 300 million years. Darwin was trying to show that the Earth is sufficiently ancient for the slow process of natural selection to account for the varieties of life.[300]

William Thomson (elevated to the House of Lords in 1892 as Lord Kelvin), one of the most respected physicists in the world, wrote extensively on the age of the Earth in the later half of the 19th century. Kelvin assumed that the Earth began in a molten state and cooled over time. The age of the Earth could be established by the principles of thermodynamics and contemporary measurements of heat gradients. In 1863, Thomson estimated the age of the crust of the Earth to be about 100 million years, or between 20 and 400 million years.[301] Kelvin updated his work several times over the next 40

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[298] Darwin, 1859

[299] Darwin's work on evolution also had bearing on geochronology. Darwin considered evolution by natural selection to be very slow process, wherein even slight changes of biological form required the passage of many successive generations. He also maintained that Precambrian time must be significantly longer than Cambrian time, to account for the diversity and sophistication of known Cambrian life forms via gradual evolution from a simple common ancestor. Toward the end of the 19th century, while geologists and physicists sought out specific physical clocks that could be used for absolute dating, many prominent biologists (especially those who endorsed natural selection) would surely have been receptive to greater expanses of time than these clocks allowed. However, absolute dating by measurement of biological evolution was not so readily developed (early work on molecular clocks began in the 1960s).

As noted in the introduction, evolutionary theory was also integral to the mobilism debate. Early mobilists often recruited global paleontological and biogeographical patterns, as interpreted by evolutionary theory, to argue for continental mobilism or against fixism. However, contractionists and permanentists often appealed to subsided land connections to accommodate this same data (see Chapter 7 for elaboration). Evolutionary theory also offered a theoretical underpinning for biostratigraphy, which had bearing upon correlating formations, relative dating, and (eventually) absolute dating. Such work was integral to relevant research programs in paleomagnetism and geochronology (as will be seen) and contributed to research in marine geology related to dating and correlations of seafloor sediments, guyots, islands and coastlines. Additionally, following the development of plate tectonics, the fields of biogeography and paleobiogeography were productively reorganized to accommodate paleogeographic reconstructions informed by plate tectonics.

Despite this broad relevance, Darwin is not prominently featured in this work. There are two main reasons for this. First, though evolutionary theory had bearing upon the historical cases examined herein, this bearing was often unrelated to a specific evolutionary mechanism. Second, the historical cases that are examined in this work serve to demonstrate the utility of the view of predictive networks and, especially, the notion of snapping together. For the sake of simplicity and clarity, certain historical boundaries had to be drawn when writing this history. Evolutionary theory and biological sciences could be more thoroughly integrated into the account developed here. Indeed, I suspect that evolution by natural selection snaps together in many interesting ways, including with other commitments and predictive networks examined herein. However, more thorough consideration of these topics is unnecessary for my purposes.

[300] His work on the age of The Weald was subsequently subject to much criticism. Darwin omitted these calculations from the Third Edition and all subsequent editions of *The Origin (Darwin, 1861).* Of course, Darwin was also deeply influenced by Lyell's uniformitarianism.

[301] Thomson, 1863a; also, see Thomson, 1862

years or so.[302] He explicitly directed his results against British geologists who endorsed uniformitarianism and a seemingly endless expanse of time which, Thomson claimed, contradicted the principles of physics. From about 1870 to the end of the century, Kelvin's estimates garnered significant attention across British science, including among prominent geologists.[303]

Kelvin's work coincided with and contributed to a self-conscious drive within geology in the second half of the 19th century to establish more quantified measurements and thereby raise the field from a descriptive to an exact science. In this spirit, geologists made several other attempts to measure the age of the Earth or the duration of certain geological periods. In 1871, Archibald Geikie popularized an important approach to such absolute dating methods.[304][305] By estimating contemporary denudation and sedimentation rates and assuming consistency in these rates over time, the amount of time required for the deposition of geological formations could, in principle, be established.[306] Geikie suggested that Kelvin's 100 million year estimate of the age of the crust was sufficient for all of geological history.[307] In 1878, Irish polymath Samuel Haughton estimated the Earth to be 153 million years old based on a similar method of sediment accumulation.[308] British geologist Thomas Mellard Reade studied chemical denudation rather than physical, and estimated that 95 million years had elapsed since the Cambrian.[309] In 1895, William Johnston Sollas, professor at Trinity College, calculated an age of only 17 million years since the beginning of the Cambrian using similar methods, but later derived an age between 148 and 103 million years.[310] In 1899, Irish physicist John Joly[311] attempted to

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302 Thomson, 1863b, 1871, 1872, 1882, 1889, 1895

303 Joe Burchfield argues that Kelvin's prestige, the broad power of his applications of thermodynamics to astronomical phenomena (including the sun's heat), and the expectation among geologists that their discipline conformed to the principles of physics all contributed to Kelvin's influence upon geologists (Burchfield, 1975).

304 Geikie, 1871

305 Geikie built upon methods first established by John Phillips in 1860 (Phillips, 1860).

306 Estimates of the maximum thickness of the full column of geological formations could then be used to establish the absolute age of formations since the Cambrian.

307 Alfred Russel Wallace was another early adherent to Kelvin's estimates (Wallace, 1870). Wallace believed that evolution by natural selection could take place much faster than Darwin supposed. In part, this was due to Wallace's expectation that periodic climate changes in Earth's history accelerated natural selection.

308 Haughton, 1878

309 Reade, 1893

310 Sollas, 1877, 1909

311 John Joly was born near Bracknagh Ireland in 1857 (Nudds, 1986). In 1876 he entered Trinity College, Dublin, where he would remain for the rest of his career. In 1883 he received a Bachelor of Engineering, with special certificates in Practical Engineering, Mechanics and Experimental Physics, and Mining, Chemistry, Geology and Mineralogy. He was appointed Assistant to the Professor Civil Engineering in 1882, and in 1891, he took a position as Assistant to the Professor of Experimental Philosophy in the Physics Department. His research was eclectic, including publications of geological descriptions, synthesis of chemical compounds and crystals, and description of numerous instruments for measurement of physical properties. He undertook pioneering work on expanding

establish the age of the oceans by measuring salination.[312] He presumed that oceans began as fresh water, and salination was the product of continental erosion and salt deposition via rivers. By estimating the global rate of salt deposition, Joly inferred that it would take about 100 million years to account for current ocean salinity. Joly later updated this approach and argued that the Earth's crust is between 87 and 117 million years in age.

By the end of the 19th century, several geological dating methods seemed to converge around 100 million years as the age of the Earth. However, estimates among physicists and astronomers, including updated calculations made by Kelvin, restricted the age of the Earth still further. Kelvin estimated the age of the Earth to be between 20 and 40 million years.[313] It was also increasingly apparent that several assumptions involved in Kelvin's calculations were problematic.[314] In 1895, physicist John Perry criticized many of Kelvin's starting assumptions.[315] Also, Kelvin assumed that the interior of the Earth was rigid, but geologists increasingly considered isostasy to be indicative of a fluid substratum. Several geologists, including Geikie, objected to further restrictions upon the age of the Earth, and seemed open to longer timelines.[316] By 1904, American Geologist George Becker, claimed that most geologists considered 100 million years to be sufficient time to account for geological phenomena.[317] Still, the prospect of more extensive timelines of Earth's history would likely not have confronted much opposition among geologists.[318]

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applications of photography, including the first successful method of producing colour photographs from a single plate. He also published on botany while in the Department of Physics. In 1897, Joly became the Chair of the Department of Geology. In the early 20th century, he took a sustained interest in radioactivity. He developed methods of measuring quantities of radioactive materials in rock samples, and thereby estimated quantities of radioactive material in the Earths crust. He also showed that the phenomenon of paleochroic haloes was due to radioactive crystals, and that properties of these halos related to the properties of the radioactive substances therein. Following WWI, when Joly directed his work to matters of defence, he undertook pioneering research on radium therapy for cancer treatment. Beginning in the 1920s, he also developed his theory of thermal cycles, very briefly examined in Chapter 1.

[312] Joly, 1899

[313] Chamberlin, 1899

[314] Thomas Chrowder Chamberlin criticized Kelvin on methodological grounds, in some ways similar to the criticisms subsequently levied against Wegener (Chamberlin, 1899).

[315] Perry, 1895a, 1895b

[316] Geikie, 1892

[317] Becker, 1904

[318] All of the geological clocks introduced here attempted to date subperiods of Earth's history, offering only minimal bounds. This work also was often completed in a self-conscious effort to abide by restrictions imposed by physicists. Additionally, Darwin and Wallace had both argued for relatively long periods of Precambrian time (Darwin, 1859; Wallace, 1870).

The previous paragraphs offer a brief introduction to the theory and methods of absolute dating in geology leading into the first decade of the 20th century. About this time, Kelvin's deductions were deeply problematized by research on radioactivity. Additionally, radioactive decay seemed to be a constant physical process that could, at least in principle, be used for absolute dating purposes.

As will become apparent in Chapter 5, radiometric dating methods were an indispensable part of the collection of research in the 1960s that contributed to the surging support for mobilism. However, the role of geochronology in the mobilism debate is often overlooked or marginalized.[319] The notable exception to this general tendency is William Glen's *The Road to Jaramillo*.[320] Whereas most histories of mobilism in the 1960s focus on developments in marine geology, Glen emphasizes the development of the geomagnetic reversal timescale and the pivotal role this research had in the confirmation of seafloor spreading. Glen's research draws from original interviews and includes detailed descriptions of measuring instruments and methods. In agreement with the general features of my own overarching historical argument, Glen argues that a confluence of evidence fired intense interest in mobilism during the 1960s.[321] This confluence of evidence was produced from independent lines of research, each with modest origins and no thought of serving the others.[322] He provides a history of the development of potassium-argon dating, a brief account of research on polarity reversals in rock magnetism, and a detailed account of the development of the geomagnetic reversal timescale and its role in the confirmation of seafloor spreading.

The development of the geomagnetic reversal timescale and its bearing upon seafloor spreading will be examined in the Chapter 5. This chapter examines developments in geochronology and radiometric dating leading into the 1960s. More than Glen, I emphasize ambiguities and debates, and thereby pay closer attention to formative research contexts in radiochemistry, spectroscopy, and the development of an absolute geological timescale. Research in geochronology was not at all concerned with mobilism. Additionally, leading into the 1960s, ambiguities and debates in geochronology were, for the most part, distinct from those involved in research in paleomagnetism and marine geology. I will first briefly introduce the origin and development of radiochemistry and the associated recognition of

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319 Frankel draws extensively from Glen when examining the development of the geomagnetic reversal timescale (Frankel, 2012, Volume II, 469). He also uses many of Glen's original interviews and compares some of these interviews with his own. Frankel does not pay close attention to 20th century developments in geochronology, however, because these developments do not take place in a context of debate on mobilism.
320 Glen, 1982
321 Glen, 1982, 10
322 Glen, 1982, 8

the possibility of radiometric dating. I will then examine early efforts to develop radiometric dating methods to construct an absolute geological timescale. I then describe developments in mass spectroscopy and associated diversification and improvement of radiometric dating measurements leading up to the early 1960s.

Radioactivity

The identification of X-rays by Wilhelm Roentgen in 1895 elicited a flurry of inquiry into epiphenomena. In 1896, speculating on some connection between X-rays and the phosphorescence of uranium salts, Henri Becquerel found that a photographic plate placed in a drawer with a specimen of uranium salt became fogged. [323] This reaction takes place even when opaque paper covers the photographic plate. Initially, Becquerel considered the phenomenon to be the result of secondary radiation, wherein the uranium gradually releases energy stored from exposure to sunlight. He soon determined that sunlight exposure had no influence on the intensity of the phenomenon. The uranium salts, themselves, seemed to be the source of the rays. He also found that these rays could pass through metal and ionize gas, and he measured the reflection, refraction, and polarization of these rays.

Marie and Pierre Curie began investigating Becquerel's rays in 1897.[324] Marie coined the term radioactivity for this phenomenon. They found that radioactivity did not seem to be a process of a chemical reaction and that intensity was only dependent on the amount of uranium present. The Curies also determined that thorium was radioactive, and excess radiation in pitchblende led to the discovery of polonium and radium, also radioactive elements.

Working under J.J. Thomson at the Cavendish Laboratory, Ernest Rutherford began experimental inquiry into radioactivity as an extension of his previous research on X-rays. In Rutherford's early work, he challenged Becquerel's claims of reflection, refraction, and polarization.[325] Additionally, Rutherford argued that radioactivity of uranium was not homogenous. As noted by Becquerel, Uranium rays could ionize gas and penetrate thin layers of metal. Rutherford found that by increasing the thickness of such metal layers, ionization remains stable until a certain thickness is reached, at which point the ionization decreases to another stable level. Rutherford identified the less-penetrative rays as α radiation and the

[323] Becquerel, 1896a, 1896b
[324] Curie, 1898; Curie and Curie, 1898; Curie, Curie and Bémont, 1898
[325] Rutherford, 1899

more-penetrative rays as β radiation. He speculated that β radiation was similar to X-rays and that α radiation was produced by the passage of β rays through a radiating body. In agreement with Rutherford, Becquerel rejected his previous measurements of reflection, refraction, and polarization.[326]

The Curies established that Rutherford's β radiation was susceptible to a magnetic field, thereby challenging Rutherford's interpretation.[327] Such magnetic deflection was indicative of a stream of charged particles akin to cathode rays. J.J. Thomson had previously determined that cathode rays were composed of negatively charged subatomic particles and could be deflected by a magnetic field. In 1900 Becquerel found that measurements of the charge-to-mass ratio of β particles was the same as that measured by Thomson for the electron.[328]

Also in 1900, the Curies found that - quite unlike other ionizing rays - the decreasing ionizing effect of α radiation increased with distance from the radiating source.[329] Thereafter, Robert John Strutt speculated that α radiation consisted of fast moving particles, but with such high mass as to inhibit magnetic deflection.[330] Following Strutt's suggestion, Rutherford subjected alpha radiation to strong electric and magnetic fields and thereby measured the deflection α radiation in 1902.[331] By incrementally increasing the strength of these induced fields, Rutherford calculated the charge-to-mass ratio of the α particles, finding a value much less than that of the electron.

Electrons were known to carry negative charge and comprise only a tiny fraction of the mass of an atom. Atoms were typically of neutral charge, so there was general expectation that the negative charge of the electron was balanced by some alternative positive charge. Due to the relatively small mass of the electron, atoms were sometimes conceived to contain thousands of these charged corpuscles. Having identified the positive charge of particles of α radiation, Rutherford supposed that these particles might constitute some portion of the atom robbed of some electrons.[332] Radioactive atoms may thereby be unstable and break apart, resulting in high-speed expulsion of atomic constituents in the form of α and β particles.

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[326] Becquerel, 1899
[327] Curie and Curie, 1900
[328] Becquerel, 1900
[329] Curie, P., 1900; Curie, M. S., 1900
[330] Strutt, 1901
[331] Rutherford, 1903
[332] Rutherford, 1904a

Concurrent with his work on α and β radiation, Rutherford also identified a peculiar radiation from thorium that seemed to be vulnerable to air currents.[333] Bodies upon which this radiation landed, also exhibited radioactivity themselves. This radioactivity decayed at an exponential rate, independent of the type of surface so activated. Also, this effect could be removed by scouring or with acid. Rutherford initially speculated that this effect was a secondary radiation, but he soon favored a particulate emanation from thorium.[334] Frederick Soddy, working with Rutherford, found that the emanation from thorium was chemically inert. The pair took this to be an indication of the transmutation of radium into a chemically inert, radioactive gas.[335] Accordingly, Rutherford and Soddy endorsed the notion that α and β radiation were the product of the disintegration of atoms, and that this disintegration resulted in atomic transmutation.

In 1901, Pierre Curie found a similar phenomenon while studying radium.[336] He disagreed with Rutherford and Soddy's conclusions, instead favoring an explanation based on secondary radiation.[337] Afterall, the radioactivity of the emanation decayed rapidly, which was unlike other known radioactive materials like Uranium, Radium, or Thorium.[338] Indeed, Rutherford and Soddy had found that half the radioactivity of their emanation decayed after just three days.[339] However, in 1903, Curie found that emanations from radium diffused and condensed like other gases.[340] Additionally, the previous year, Curie had determined that radium maintains a higher temperature than its surroundings, and this heat was maintained even when immersed below freezing.[341] Curie thereby considered subatomic transformation to be a possible explanation for this heat.[342]

In 1903, Soddy, along with William Ramsay, showed spectroscopically, that radium expelled helium.[343] Ramsay had previously isolated helium from rock samples, finding that helium was only contained in rocks with radioactive properties.[344] The following year, Ramsay also found that the emanation of radium produced additional spectroscopic lines which Ramsey took to be indicative of a

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[333] Rutherford and Owens, 1899; Rutherford, 1900
[334] Rutherford, 1900
[335] Rutherford and Soddy, 1902a, 1902b
[336] Curie and Debierne, 1901a, 1901b
[337] Curie, 1903a; Curie and Laborde, 1903
[338] Curie and Curie, 1902
[339] Rutherford and Soddy, 1902a, 1902b
[340] Curie and Danne, 1903
[341] Curie, 1902a, 1902b
[342] Curie and Laborde, 1903; Curie, 1903b
[343] Ramsay and Soddy, 1903
[344] Ramsay, 1895

new element.[345] By 1904, Marie Curie considered transmutation to be the most favorable explanation of radioactivity.[346]

In 1905, Rutherford measured the rate of decay of radium.[347] A known quantity of radium was spread in a thin layer across an aluminum plate. The face of a second plate was positioned a few millimeters away. The apparatus was contained in a vacuum, and a strong electrical field was oriented parallel to the plates. The electrical field would divert any electrons liberated from the plates, the vacuum would limit ionization of gas between the plates, and the thin layer of radium would limit the absorption of α radiation by the radium itself. Rutherford found that the second plate obtained a positive charge, as would be expected from the emission α particles, and from this charge he was able to calculate the amount of α radiation released from the radium. Rutherford then calculated a decay constant for radium which was much lower than the rapid decay of the emanation of radium.

During attempts to measure the magnitude of the charge of α particles, Rutherford found that α radiation was scattered when passing through other matter. By 1908, there was widespread expectation that the α particle was a doubly ionized helium atom, and the scattering effect garnered more attention. J. J. Thomson developed an explanation for scatter patterns based on the notion that highly scattered rays were the product of many instances of smaller deviations as particles passed through an alternative substance.[348][349] However, Rutherford developed an alternative explanation for scattering, based on the possibility that the scatter of α particles was the result of singular encounters.[350] He proposed that the atom is almost entirely empty space, but contains a central nucleus comprising most of the atom's mass. Because of this, α particles which are emitted with such high velocity from radioactive atoms are capable of penetrating through the empty spaces that constitute metals and other substances. The scatter of α radiation is due to α particles striking the relatively tiny nucleus. Subsequent development of this model of the atom by Neils Bohr resulted in notable successes.

Thus, near the end of the first decade of the 20th century, there was widespread recognition that radioactivity was the product of atomic disintegration. This disintegration results in the emission of radioactive rays and the transmutation of a radioactive element into a daughter element. Though other

345 Ramsay and Collie, 1904
346 Curie, M., 1904
347 Rutherford, 1905
348 Thomson, J. J., 1906, 1910
349 Thomson's work pertained mainly to β particles.
350 Rutherford, 1911

forms of radiation were identified, particles of α radiation were known to consist of helium ions ejected from a disintegrating atom at high speed. Different radioactive elements decay at different rates. The rate of decay is constant, such that proportions of parent and daughter element are predictable over time. Additionally, radioactive decay was known to produce a substantial amount of heat.[351]

Origins of Radiometric Dating

In 1905, Bertram Boltwood,[352] a chemist at Yale, postulated that lead was the final product of uranium decay.[353][354] Lead was generally present in rock containing uranium. Additionally, rock containing uranium from older formations consistently contained a greater proportion of lead than that from younger formations, and this ratio had a high degree of consistency between rocks of similar age.[355] Boltwood had previously conducted similar research on ratios of radium and uranium in diverse rock samples, supporting his conclusion that radium was a decay product of uranium.[356] Boltwood calculated the ratio between lead and helium produced via radioactive decay, based on the presumption that the uranium decayed only by α radiation.[357] He then compared this ratio to helium measurements in radioactive rock, but found such measured helium ratios to be highly inconsistent. Boltwood interpreted this to mean that helium retention due to α radiation was highly variable across rock samples, likely due to variations in mineral density.

The decay rate of uranium, relative to that of radium, is quite low. Consequently, in a given interval of time that is short in comparison to the half life of uranium, the amount of radium produced

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[351] Radiogenic heat challenged the fundamental assumptions that Lord Kelvin employed in his efforts to calculate the age of the Earth. John Joly and Ernest Rutherford were among the first to recognize this (Joly, 1903; Rutherford, 1904b). An extra source of heat internal to the Earth may retard cooling. The Earth may not even be cooling at all. Radiogenic heat also played a role in Arthur Holmes' endorsement of convection currents within the Earth which also challenge Kelvin's assumptions pertaining to interior heat distribution.

[352] Boltwood was born in 1870 in Amherst Massachusetts (Kovarik, 1929). He entered the Sheffield Scientific School at Yale in 1889, studying chemistry and physics, graduating in 1892. Thereafter, he studied inorganic chemistry at the Ludwig-Maximillian University of Munich. In 1894 he returned to Yale as an assistant in analytical chemistry and spent a semester studying physical chemistry in Leipzig under Wilhelm Ostwald. He became an instructor at Yale in 1896 and received a PhD from Yale in 1897, studying chlorides. His first work on radioactivity began in 1899. From 1900 to 1906 Boltwood headed a private laboratory in New Haven, consulting for mining engineers and chemists. In 1906 he became a professor of physics at Yale.

[353] Boltwood, 1905

[354] In the same paper, Boltwood identifies radium as a decay product of uranium (Boltwood, 1905a; Rutherford and Boltwood, 1905)

[355] Boltwood, 1907

[356] Boltwood, 1904, 1905b

[357] Boltwood, 1907

by uranium decay will equal the amount of radium that is decayed. This results in a nearly constant ratio or radium to uranium within radioactive minerals. Knowing this ratio, and the decay rate of radium, Boltwood estimated the decay rate of uranium.[358] Boltwood also compiled a list of measurements of lead content within rock containing a high percentage of uranium, from which he established lead to uranium ratios. Boltwood then used these ratios to calculate the age of the minerals based on his estimated decay rate of uranium. The oldest age measured by Boltwood was 2,200 million years.

Working under Robert John Strutt at Imperial College, Arthur Holmes[359] elaborated on Boltwood's work. For Holmes, in order to establish whether age inferences based on ratios of radioactive decay were reliable, the inferred ages needed to be corroborated against the geological age of the sampled formations.[360] Holmes thereby set out to produce an absolute geological timescale. Previous efforts to measure radiogenic ratios were performed on entire rocks. Rock samples would be crushed, and the homogenous product divided and differentially processed to measure the concentration of helium or lead in one division, and the concentration or uranium or radium in the other. Holmes pioneered efforts to subdivide a rock sample into its constituent mineral grains, and then process and measure these grains independently.[361] Crystalline mineral grains might trap the products of radioactive decay and thereby provide more accurate ratio measurements. Additionally, different mineral grains collected from a single rock sample could be used to establish a set of ratios which could then be checked against one another or averaged.

Holmes crushed rock samples and separated mineral grains. He fused mineral grains with borax and dissolved the fusion in hydrofluoric acid which he then boiled to liberate radium. Measuring radon emissions with an electroscope, Holmes could calculate radium concentration, which he then used to calculate uranium concentration based on the presumed constancy of their ratio. Lead was isolated by mixing powdered mineral grains with a fusion mixture, then dissolving in hydrochloric acid and

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[358] Boltwood, 1907

[359] Holmes was born in Hebburn, UK, in 1890 (Dunham, 1966). He entered Imperial College on a scholarship in 1907. He received a B.Sc. in 1909 and joined the laboratory of Robert John Strutt at Imperial College for post-graduate work. For financial reasons, he joined an expedition to Portuguese East Africa, organized by a mineral company. His health suffered due to Malaria and blackwater fever which he contracted on the journey. In 1912 he returned to London where he was appointed Demonstrator in Geology at Imperial College, where he worked until 1920. He was unfit for military service during WWI due to his health. Again, due to financial motivations, Holmes took a position as chief geologist of an oil exploring company operating in Burma. In 1925, Holmes became professor at the University of Durham where he stayed until 1943, at which point he became chair of geology at Edinburgh University.

[360] Holmes, 1911

[361] Holmes, 1913

evaporating to dryness multiple times until silica was filtered off, leaving a clear solution from which lead sulphide was precipitated by the addition of ammonium sulphide. The precipitate was filtered out, dried, and ignited. The residue was treated with nitric acid and boiled to convert lead to nitrate. Sulphuric acid was added, and the solution was heated. The precipitate was filtered again, washed with alcohol, dried and ignited, and finally weighed. Given the decay rate of uranium, and the measured ratio of uranium and lead, Holmes calculated the age of a Devonian rock sample. In 1913, Holmes updated Boltwood's work and added his own measurement of Devonian rock to construct an absolute geological timescale which he published in a book title *The Age of the Earth*.[362] Holmes dated the Carboniferous at 340 million years, the Devonian at 370 million years, the Silurian at 430 million years, and the Precambrian between 1,000 and 1,650 million years.

Holmes recognized that in order to calculate the age of a rock, several assumptions had to be defended. The decay product(s) of a parent element had to be known, the decay constant of the parent element had to be known, and the ratio of parent and daughter element had to be measured. Additionally, inferring the age of a rock assumed that the rock contained no daughter element upon its formation, that all daughter element was the product of radioactive decay of the parent element, that no parent element nor daughter element were added or removed from the rock following its formation, and the decay constant of the parent element remained constant over time. These measurements and assumptions could be problematized in many ways.

Geophysical processes are complex and varied. Material may be added to or removed from rock after its formation. Subterranean waters may circulate through porous rock resulting in leaching. Such water may contain dissolved minerals and gases of variable constituents and concentrations, and this can result in precipitations. Rocks may be subject to high fluctuations in temperature and pressure, capable of resulting in crystalline and chemical changes. Surface rock may be eroded and exposed to the atmosphere, sunlight, and surface water. Different minerals may be differentially vulnerable to such processes. Any of these processes could contribute to the addition or removal of constituents within a decay chain and thereby compromise dating accuracy.

Radioactive decay may produce a daughter element that is, itself, radioactive. Radioactive elements may thereby produce a chain of daughter elements. Boltwood and Holmes thought there was good reason to suppose that uranium ultimately decayed into stable lead which would not itself be lost

[362] Holmes, 1913

to decay. Accordingly, the presumed accumulation of lead made uranium-lead ratios amenable to dating inferences. However, knowledge of the extent and constituents of radioactive decay chains was known to be incomplete. This posed two notable challenges to the viability of dating rock by radioactive decay. First, even if uranium and lead seem to be retained in rock, such that the addition or subtraction of these materials over the life of some rock sample was known to be minimal, this would not necessarily be the case for other elements in the decay chain. Elements in a decay chain my have different physical and chemical properties and may thereby be differentially vulnerable to processes that may compromise the retention of radioactive decay products. Second, uncertainty in constituents of decay chains also raises the possibility that decay chains of different radioactive elements might overlap. If lead were a final decay product of another radioactive element not contained within the uranium decay chain, then accumulation of lead in some rock sample may not be due solely to the decay of uranium. Accordingly, even if the uranium-lead decay chain were known in its entirety, and it was certain that no parent or daughter element were added or removed from a sample upon its formation, it might still be the case that lead within that sample accumulated from the decay of some other radioactive element.

Decay constants were often inferred or estimated indirectly based on rapidly developing assumptions about decay chains and radiation theory. Laboratory measurements suggested that decay rates were constant, but geophysical processes may involve great expanses of time, heat, pressure, or other variables that may not be reproducible in a laboratory setting. Additionally, different radioactive elements were known to have notably different rates of decay. Slowly decaying elements, like uranium, may be conducive to dating older rock with low resolution, but this limited dating capacity to only the most ancient formations.

The measurement of uranium-lead ratios required chemical processing of a large amount of rock. Holmes processed 100 kilograms of rock to obtain sufficient zircon that would yield measurable amounts of uranium and lead.[363] Prior methods often required processing even larger quantities. This demand for a large amount of rock compounded the more general problem of locating uranium and lead rich rock that could be geologically dated. Igneous rock is the most obvious candidate for dating by radiometric decay as it would be difficult for secondary rock to satisfy the requirement that parent and daughter elements are neither added nor removed over time. Igneous rock, however, can be difficult to date, as geological age is largely established through analysis of sedimentary formations. Only maximum possible geological age can be attributed to igneous intrusions, and only minimum possible geological

[363] Lewis, 2001

age can be attributed to igneous pebbles within a conglomerate. Furthermore, the most common igneous rocks, such as basalt and granite, typically did not contain sufficient uranium and lead to be effectively dated. Finally, establishing the geological age is not itself without challenges. The history of geochronology in the 20th century is beset by attributions of improper geological age and subsequent revision.

In 1913, Frederick Soddy argued that products of uranium decay previously considered to be distinct were chemically inseparable.[364] Also, Soddy claimed that uranium was a constituent in the decay chain of uranium. To make sense of his results, Soddy claimed that atoms may be chemically identical, yet have different atomic mass. He called these *isotopes*. In 1914, Holmes argued that lead was the final product of thorium decay, and he determined this lead to be a different isotope from that derived from uranium.[365] A third lead isotope was considered to be non-radiogenic in origin.[366] Holmes quickly recognized that such lead isotopes would compromise dating inferences unless accounted in some way.[367] The prospect of undiscovered isotopes also posed a challenge for the overall reliability of rock dating by radioactive decay.

Due to these challenges, rock dating by measuring the products of radioactive decay often produced inconsistent results. The great expanses of time endorsed by Holmes, Boltwood, and others also contrasted sharply with other approaches to geochronology. Accordingly, many geologists and physicists were skeptical of the viability of such rock dating. In 1908, American geologist George Becker,[368] dated rock below the Cambrian between 1.5 and 11.5 billion years.[369] Becker cited this inconsistency and the great expanse of time as a reason to reject dating by radioactive decay. He also argued that if radiogenic heat were sufficient to extend the age of the Earth, then this would require highly implausible adjustments to sedimentation and salination rates in Earth's history. In 1918, F.W. Clarke, chief chemist to the USGS, echoed Becker's objections and added that isotopic lead invalidated

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[364] Soddy, 1913
[365] Holmes and Lawson, 1914
[366] Holmes and Lawson, 1914; Richards and Lembert, 1914
[367] Holmes, 1914
[368] George F. Becker was born in New York City in 1847 (Merrill, 1927). He entered Harvard in 1864 and upon graduation he studied at the University of Heidelberg where he received a PhD in 1869. Returning to America, he worked as a construction engineer and then an instructor in mining and metallurgy alongside Clarence King. King was a highly regarded geologist who also worked on geochronology during the 19th century. Becker received an appointment from the USGS in 1879, working in California, Utah, Nevada, and elsewhere. During his time with the USGS, Becker developed the Geophysical Laboratory at the Carnegie Institution of Washington. He served as president of the Geological Society of America in 1914.
[369] Becker, 1908

previous measurements.[370] In 1925, Joly argued that radioactive decay rates may change over time and also highlighted inconsistency across rocks dated by radioactive decay.[371] By this time, Joly favored an age of the Earth between 150 and 250 million years.[372]

Uranium-lead ratios were not the only ratios that were measured to establish rock age, even in early work on radiometric dating. Helium, thorium, radium and actinium were also measured with varying levels of approval or apparent success. Often, different methods of rock dating produced highly divergent results. Holmes, for one, considered uranium-lead dating to be the most reliable method available. Strutt had argued that radiogenic helium diffused into the atmosphere, thereby making sense of Boltwood's previous difficulties.[373] Holmes noted that helium measurements routinely underestimated rock age and claimed that laboratory tests demonstrated helium loss by diffusion.[374] Holmes also argued that thorium-lead dating was inconsistent, since obtained dates were inconsistent for rock of the same geological age.[375] He speculated that percolating water effected thorium derived lead more than uranium derived lead, resulting in this discrepancy. For Holmes, the efficacy of a dating method could be judged by yielding absolute ages that consistently agreed with relative geological dating. Holmes championed uranium-lead dating because even in its earliest applications (though subsequently and frequently revised) this dating method produced results that were consistent with relative geological age. Subsequently, consistency with uranium-lead dating became a viable means of establishing the suitability of alternative dating methods.

Radiometric Dating and Mass Spectroscopy

Following the early work of Boltwood and Holmes, radioactive rock dating became increasingly connected with centers of pioneering work on spectroscopy. In 1913, J. J. Thomson passed beams of

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[370] Clarke, 1918
[371] Joly, 1925
[372] Joly had previously completed pioneering geochronology work based on radioactivity. Along with Rutherford, he studied pleochroic haloes (Joly and Rutherford, 1913). Rutherford calculated the amount of radiation required to produce various intensities of staining in artificially induced haloes of various sizes, and Joly compared these standards to Devonian samples, finding an age estimate of no less than 400 million years. At the time, Joly considered the discordance with his own work on salination to be inexplicable. If rates of radioactive decay changed over time, then Joly could make sense of the discordance between traditional geological dating methods and the more recently developed methods of radiometric dating and pleochroic halo dating.
[373] Strutt, 1908
[374] Holmes, 1913
[375] Holmes, 1926

ionized gases through electric and magnetic fields, finding that this differentiated the streams based on the mass-to-energy ratio of the particles therein.[376] Working under Thomson at the Cavendish Laboratory, Francis Aston[377] became interested in improving the separation of ionized beams, as this would facilitate inquiry into isotopes. In 1919, he constructed a mass-spectrograph.[378] Ionized gas was propelled from a chamber through a collimating slit system and passed though an electric field oriented perpendicular to the rays. Passing particles would be scatted by an angle proportion to mass, velocity, and charge. A section of the resulting spectrum was then passed through a diaphragm and deflected by a perpendicular magnetic field, deflecting particles in the opposite direction and onto a photographic plate. Alternative isotopes of an element would impinge upon the photographic plate at different locations. By calibrating the spectrograph against ions or compounds of known mass and charge, Aston was able to discover many new isotopes. Variations in the intensity of the resulting imagine on the photographic plate could also be used to estimate the relative abundance of isotopes.

In 1918, Arthur Dempster,[379] at the University of Chicago, constructed a mass spectrometer which measured ionic beams with an electrometer instead of a photographic plate. [380] In Dempster's design, ionized gas is accelerated to near uniform energy levels by a differential electrical charge and then introduced into a vacuum chamber. The vacuum chamber is curved in a semi-circle and situated between two poles of a magnet producing a uniform magnetic field across the curve. Particles of different mass will be differentially deflected by the magnetic field as they travel through the vacuum chamber. At the opposite end of the vacuum chamber, individuated ionic beams are measured quantitatively be electrometer. There are several methods through which ionized gas can be produced. The energy of the admitted ion beams can also be adjusted. These variables – along with non-

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[376] Thomson, J. J., 1913

[377] Aston was born in Harborne, UK in 1877 (Downard, 2007). He graduated from Mason College in 1898 specializing in organic chemistry. Upon graduation, Aston worked as a brewer's chemist for three years. He returned to Mason College as part of the University of Birmingham and researched cathode rays. In 1910, Aston joined Thomson at Cambridge to assist on cathode ray experiments. During WWI, Aston conducted experiments to advance aeronautical coatings and aerodynamics. Aston remained at Cambridge until his death in 1945.

[378] Aston, 1919

[379] Dempster was born in Toronto, Canada in 1886 (Allison, 1950). Most of his career was devoted to the discovery of isotopes and their relative abundances. He obtained an M.A. from the University of Toronto in 1910. He then spent a semester at the University of Munich, and another at Gottingen, then studied at the University of Wurzburg for two years where research was conducted on the deflection of ion beams. In 1914 he fled Germany and took a position at University of Chicago where he obtained a doctorate in 1916. He briefly served in the US Army, and in 1919, he returned to the University of Chicago as an assistant professor of physics.

[380] Dempster, 1918; Dempster, 1921

spectroscopic analyses upon samples prior to ionization – facilitate additional analysis of the chemical source of spectroscopic measurements.

Mechanical vibrations and unaccounted electromagnetic fields can influence spectroscopic measurements. Impurities contained within or introduced to the vacuum chamber can result in spectral readings that are not representative of the presumed ionic source. Due to the minute quantities of ionized gas involved in these measurements, even the slightest source of contamination can compromise measurements. Efforts to improve the reliability and functionality of spectroscopes often limited sources of contamination by soldering joints whenever possible and heat-proofing the entire apparatus so that it could be baked for outgassing. Fluctuation in the energy of the introduced ionic beams, or fluctuation of the induced magnetic field could perturb ionic beams and thereby limit the instrument's resolution. The introduction of feedback mechanisms which translated such beam fluctuations into compensatory adjustments elsewhere thereby improved measurement precision.

Prior to 1933, isotopic abundances were primarily measured by spectrograph.[381] During the 1930s and 1940s, increased utilization of electronic ion collectors, and improvements thereof, resulted in increasing reliance on mass spectrometry for isotope abundance measurements. Many notable developments in mass spectrometry during this time were due to the work of Alfred Nier.[382] In 1940, Nier designed a sector field mass spectrometer. Instead of relying on a semi-circle design, Nier's vacuum chamber was only 60 degrees, with a permanent magnet located directly at the apex of the curve.[383] This decreased the required strength of the magnetic field, thereby reducing the spectrometer's size and production cost and increasing its availability and utility.

In 1939, Enrico Fermi inquired with Nier if he could separate uranium isotopes for the purpose of experimental inquiry into nuclear fission. Nier was able to isolate sufficient amounts of uranium-235 by replacing his electric collector with a metal strip, upon which uranium accumulated.[384] During WWII, many mass spectrometers of Nier's design were produced for various uses relevant to the Manhattan

[381] Nier, 1955
[382] Nier was born in St. Paul, Minnesota in 1911 (Reynolds, 1998). He tinkered with radio technology as a child. He graduated from the University of Minnesota in 1931 studying electrical engineering. He then studied physics during graduate work where he focused on the use of mass spectrometry to study isotopic composition. Nier then received a fellowship at Harvard from 1936 to 1938. He returned to the University of Minnesota where he spent the remainder of his career with the exception of a two-year stint in New York City from 1943 to 1945 in relation to work on the Manhattan Project.
[383] Nier, 1940
[384] Nier, at al., 1940a, 1940b

Project, such as monitoring purity of uranium and identification of trace contaminants or leaks. Post-war improvements to vacuum technology and electronics further improved the performance of Nier's design.[385] In the early 1950s, Nier and one of his students developed a double focusing mass spectrometer, which selects and deflects ionic beams with electric and/or magnetic fields in such a way that variations in velocity and divergence angle of the beam upon entry into the vacuum chamber come to be focused at the collector.[386] This improved the resolution of the mass spectra, facilitating more precise mass measurements.[387]

In the early 1950s, John Reynolds[388] designed a static-mode mass spectrometer.[389] In all previous mass spectrometers, the sample introduced into the vacuum chamber would be quickly exhausted. Strong spectra could be produced only for samples of sufficient volume. However, trace isotopes may not produce a strong reading. In Reynolds' static-mode spectrometer, the sample was sealed off within the spectrometer envelope, allowing cumulative effects of the spectra to be analyzed over time. This improved the capacity to measure minute samples. This was especially useful in the analysis of argon gas. Also in the 1950s, researchers began using known quantities of non-radiogenic isotopes as a means of setting a quantitative benchmark against which small quantities of isotopes could be measured. For geochronologists, quantities of some radiogenic isotope within a sample could be precisely established by measuring its ratio with a known quantity of non-radiogenic isotope.[390] This practice of isotope dilution was particularly useful in measuring trace amounts of lead.

Spectroscopy facilitated the identification of isotopes, measurements of atomic mass, and measurements of isotopic ratios. In turn, such measurements facilitated the identification of radioactive and radiogenic isotopes and decay chains, improved the precision of decay constant estimates, and facilitated the precise measurement of even tiny quantities of radiogenic isotopes, thereby improving

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[385] Nier, 1947, 1991
[386] Johnson and Nier, 1953
[387] Nier and Roberts, 1951
[388] Reynolds was born in Cambridge Massachusetts in 1923 (Price, 2004). Like Nier, Reynolds tinkered with radios as a child. He graduated from Harvard in 1943 with a degree in electronic physics. He entered active service in the US Navy in 1943 as an ordnance officer and worked on antisubmarine projects. Following the war, Reynolds studied physics at the University of Chicago. He completed his PhD in 1950, studying isotopes and radioactive decay via mass spectroscopy and thereafter accepted an assistant professorship at Berkeley. John Verhoogen of the Berkeley Geology Department thought it would be useful to develop isotope spectroscopy within the department. Reynolds built a spectroscopy lab at Berkeley with the aim of improving rare gas mass spectrometry, with applications to geochronology. Reynolds also investigated meteorites and thereby inferred the existence of an extinct radioisotope, Iodine-129.
[389] Reynolds, 1956
[390] Inghram, 1954

the precision of rock dating and expanding the domain of rock that could be effectively dated. Spectroscopy also limited the amount of physical material required for analysis and increased the speed and ease with which radiometric measurements could be made. For these reasons, radiometric dating became increasingly connected with centers of spectroscopy.

Geochronology to 1960

Beginning near the end of the 1920s, Holmes took a sustained interest in the development of helium dating methods. Researchers had recently increased the sensitivity with which trace amounts of helium could be measured in terrestrial rock. A rock sample would be crushed and constituent gases liberated by application of heat, the use of flux, or by dissolution in acid.[391] Liberated gases would be collected in a vacuum chamber which would inhibit atmospheric contamination.[392] The collected gases would then be exposed to activated charcoal which would absorb the gases with the exception of helium, neon, and hydrogen. Remaining hydrogen could be removed by injection of pure oxygen and heat, converting the hydrogen into water which, along with excess oxygen, would be absorbed by the charcoal. The remaining gas could then be examined spectroscopically to ensure the purity of helium, and its volume could then be measured. Holmes supposed that rock containing trace amounts of helium may be less susceptible to atmospheric diffusion that compromised previous helium dating methods.[393] Measuring ratios of helium against radioactive parent isotopes might thereby expand the set of rock that could be reliably dated to include those rocks with limited lead content.

Meanwhile, in 1929, Aston at Cambridge examined the isotopic content of radiogenic lead and measured the atomic mass of the three known lead isotopes.[394] However, Aston found that the ratio of the presumed non-radiogenic lead with that derived from thorium decay was higher in uranium minerals. This led Rutherford to infer that the presumed non-radiogenic lead was, in fact, the product of decay of an alternative uranium isotope.[395] Based on variations in the ratios of this lead, Rutherford claimed that the two uranium isotopes decayed at different rates. Uranium-lead dating could thereby be made more precise by accounting for these alternative uranium and lead isotopes. The prospect of two

[391] Goodman and Evans, 1944
[392] Urry, 1933
[393] Dubey and Holmes, 1929
[394] Aston, 1929
[395] Rutherford, 1929

independent uranium decay constants also raised the possibility that uranium-lead dating could be applied twice to one rock sample. Additionally, since different lead isotopes are produced at different rates from their parent isotopes, the ratio of the alternative lead isotopes could also be measured to establish the age of the rock sample. In principle, a single rock sample could thereby be dated by measuring three different isotopic ratios.

In 1935 at the University of Minnesota, Nier identified a radioactive isotope of potassium with atomic mass 40.[396] Slight radioactivity of potassium was discovered by J.J. Thomson in 1905,[397] but identified isotopes were not radioactive.[398] The following year, researchers at the California Institute of Technology found that potassium-40 constituted a small proportion of naturally occurring potassium, but was the source of all potassium's radioactivity.[399] Additionally, calcium and argon isotopes with atomic mass 40 were postulated to be alternative stabled products of potassium's decay. Argon-40 detected in the atmosphere was thereafter speculated to be the product of potassium decay in terrestrial rock.[400]

In 1938, German radiochemists isolated radiogenic strontium from rubidium-rich mica of southeastern Manitoba that had been lead dated.[401][402] Rubidium was long known to be radioactive, and following a period of theoretical debate in the mid-1930s over the atomic mass of the radioactive rubidium isotope,[403] rubidium-87 was found to be the source of all rubidium's radioactivity.[404] The radiogenic strontium from the Manitoba rock was found to be nearly pure strontium-87, and researchers were thereby able to infer a decay constant of rubidium by working backwards from the age of the rock derived by the lead method.[405] The high proportion of radiogenic strontium in the rock sample and the estimated decay constant for rubidium-87 facilitated subsequent development of rubidium-strontium radiometric dating. The inferred decay constant of rubidium was substantially longer than that of uranium isotopes, so rubidium-strontium dating would likely not improve the

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396 Nier, 1935
397 Thomson, J. J., 1905
398 Aston, 1921
399 Smythe and Hemmendinger, 1937
400 Von Weizsacker, 1937
401 Straßmann and Walling, 1938
402 Fritz Straßmann also worked with Otto Hahn on the detection and theory of nuclear fission. After learning of their work, Niels Bohr and Enrico Fermi took interest in this subject, resulting in the enquiry to Nier to obtain pure uranium isotope samples.
403 Klemperer, 1935; De Hevesy, 1935; Nier, 1936; Hahn, Straßmann and Walling, 1937
404 Hemmendinger and Smyther, 1937
405 Straßmann and Walling, 1938

absolute geological timescale for relatively young rock. However, unlike uranium-lead dating, rubidium-strontium decay consists of only a single transformation, thereby limiting the number of possible steps within a decay chain that may be subject to differential loss/gain effects of geological processes. Additionally, rubidium minerals are plentiful in feldspars and micas, thereby increasing the set of rock that could be dated by radioactive decay.

In 1939, Nier measured the relative abundance of Rutherford's inferred uranium isotopes and thereby refined estimates of their alternative decay constants.[406] Around this time, Nier also began a programme of geochronological dating, favoring the measurement of lead-lead ratios which could be measured readily and precisely via spectrometer.[407] Typically, measurements of uranium or other parent isotopes were not done spectroscopically, but through chemical isolation or indirect measurement of radioactivity. Accordingly, uranium concentration was often a factor that limited the availability of datable rock. By the 1940's Nier's geochronological research was considered world class, in part due to his reputation as a global leader in spectrometry.

Toward the end of the 1930s, Holmes' interest in the development of helium dating bore fruit. Helium content of a rock sample could be measured by the methods identified previously, while minute uranium and thorium content could be estimated by chemical isolation and measurement of radioactive emanation with the use of an ionization chamber and electrometer.[408] Background radiation and secondary effects can compromise such measurements, but the use of a control ionization chamber can facilitate measurement of differential ionization. Such measurements could be made absolute by calibration against standard solutions of radium. Given the measurement of ionization, and theory of the radioactive decay chains of uranium and thorium, such measurements could be used to infer the quantity of uranium or thorium within a sample. Holmes found that rock dating inferred by uranium-lead measurements aligned remarkably closely with the helium dating of 39 samples measured at the Massachusetts Institute of Technology.[409] Problems, however, soon became apparent. Another researcher at MIT developed an alternative method for measuring radioactive parent content by using a vacuum tube electrometer to count alpha rays emitted into an ionization chamber. This alternative method of measuring helium ratios produced results that were consistent with relative geological age of samples, yet inconsistent with the absolute age established by previous helium measurements. In 1939,

[406] Nier, 1939
[407] Nier, 1941
[408] Urry, 1933; Lane and Urry, 1935; Urry, 1936
[409] Holmes, 1937

it was determined that the results that seemed so promising to Holmes were compromised by inaccurate calibration of the ionization chamber device.[410] A 1941 review of the helium method noted that helium ratios consistently produced younger ages than lead ratios, and that retentivity of radiogenic helium was a major source of uncertainty in these measurements.[411]

In 1947, Holmes published an updated geological timescale.[412] He included only five rock samples, dated by Nier, for which geological age could be well established. Holmes also relied on a method that he had developed previously, wherein relative duration of geological periods could be estimated based on variations in the thickness of the corresponding sediment column. When calibrated against Nier's dated samples, Holmes placed the Eocene at 58 million years, the Permian at 203 million years, the Carboniferous at 255 million years, the Silurian at 350 million years, and the Ordovician at 430 million years. The Carboniferous, Devonian, and Silurian were thereby deemed to be slightly less ancient than indicated in his first geological time scale published over 35 years earlier. Holmes also noted that the oldest reliable dates were about 2 billion years old, a value that he later increased to 2.5 billion.[413] Earlier in the century, geochronologists often favored an age of the Earth on the order of 100 million years, while Holmes routinely argued that the Earth was over a billion years old. By 1949, there was little doubt that Holmes was right in his long-time effort to push back the age of the Earth.

By the 1950s, centers for the study of rock dating by radioactive decay expanded, as did the volume of measurements, especially in the USA. In part, this was due to increasing availability and precision of spectrometers, increased funding for atomic science and technology and the availability of separated isotopes from the U.S. Atomic Energy Commission, and development of new chemical methods and associated refinement and systematization of dating programmes. Zircon crystals became more prominent in lead dating techniques. The atomic radius of zirconium differs from lead, resulting in speculation that zirconium crystallization would exclude non-radiogenic lead. In principle, this could increase the reliability of lead dating.[414] Zircon, however, typically contains a very small proportion of uranium, thereby requiring that a great amount of zircon be processed to yield a few milligrams of uranium, sufficient for spectroscopic analysis. Accordingly, uranium content of zircon was frequently inferred by direct counting of alpha emissions, the basic principles of which were described previously in

[410] Evans, et al., 1939
[411] Goodman and Evans, 1941; also, see Evans and Goodman, 1941
[412] Holmes, 1947
[413] Holmes, 1948, 1954
[414] Larsen, Keevil and Harrison, 1952

relation to helium dating. As isotope dilution techniques developed in the 1950s, smaller quantities of uranium could be measured by spectrometer.

The initial promise of the helium method declined due to growing recognition that helium diffusion following rock formation greatly compromised dating consistency.[415] Alternatively, a programme of radiocarbon dating developed based on the production and decay of carbon-14, and the appeal of strontium dating methods increased. By 1950, strontium and radiocarbon dating programmes were clearly established.[416] Due to the short half life of carbon-14, radiocarbon dating could only be used to date young quaternary (and typically organic) samples and therefore could not be cross checked against lead dating. Alternatively, estimates indicated that rubidium decays substantially slower than uranium and was therefore most useful for dating Precambrian rock. Still, rock dated by strontium could be corroborated against lead dating. Strontium dating seemed to be mostly consistent with geological age determinations.[417] Agreement with lead dates were taken as further indication of the viability of this dating method and offered an opportunity to cross check inconsistent lead dates against an independent dating method, ostensibly limiting ambiguity. Disagreements between strontium and lead dates were typically attributed to either laboratory analysis, uncertainties in decay constants, or confounding geophysical processes.[418]

The approximate decay constant of rubidium that was estimated in 1938 remained in use through the 1940s. Though the potential error was known to be high, the decay constant obtained independent support by direct counting in 1946.[419] In the early 1950s, strontium dating began to employ isotope dilution methods for more precise spectroscopic measurements. Dates obtained in this way were consistently greater than those obtained by lead dating.[420] By modifying the previously employed decay constant of rubidium, researchers at the Carnegie Institution of Washington D.C. could bring strontium and lead dating into closer agreement.

In 1948, Nier introduced various potassium minerals into a high temperature vacuum furnace, and collected vapors were measured by mass spectrometer. He found that these minerals contained a higher proportion of argon-40 than is present in the atmosphere and argued that this confirmed prior

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[415]Keevil, Jolliffe and Larsen, 1942; Hurley and Goodman, 1943; Hurley 1950; Hurley 1954
[416] On radiocarbon, see Libby, Anderson and Arnold, 1949; Arnold and Libby, 1949; on strontium dating, see Ahrens, 1949
[417] Ahrens, 1949; Aldrich, 1956; Jeffery, 1956
[418] Ahrens, 1949; Aldrich, 1956; Wetherill, et al. 1956; Tilton and Nicolaysen, 1957
[419] See the account of rubidium decay provided by Ahrens, 1949
[420] Aldrich, 1956

speculation on the decay of potassium-40 into argon-40. Based on the measured ratios, and a range of possible decay constants for potassium, Nier estimated the branching ratio of potassium decay.[421][422] The following year, Nier obtained pure samples of argon-36 and argon-40.[423] He mixed these in known proportions to produce a standard against which relative abundance measurements from his spectrometer could be calibrated. This allowed for more precise measurements of isotopic ratios of argon and potassium and facilitated a more precise determination of the decay constant of potassium-40. Nier's work raised the possibility of the development of a potassium-argon dating method. Since the half life of potassium is shorter than uranium and a wide range of rock contains potassium, this offered some promise for the dating of rock too young to be reliably dated by lead. However, in 1950, German researchers found that different mineral grains seemed to retain variable proportions of argon, hypothesizing that radiogenic argon is lost from these minerals by diffusion.[424]

As previously noted, John Reynolds constructed a static-mode spectrometer at Berkeley with the aim of improving measurements of rare gases like argon. Along with colleagues, he developed a programme for potassium-argon dating in the early 1950s.[425] Isotope dilution with a known quantity of pure argon-38 was used to measure trace amounts of argon-40. Alternatively, potassium analyses were made by flame photometry, wherein a mineral was introduced to a flame and resulting spectra analyzed to infer constitution. Initial age inferences were made on Precambrian rock that were dated by lead methods, but large variations in apparent retention of radiogenic argon was evident. Micas seemed to retain more argon than feldspars. Diffusion of argon would be a possible explanation, but so too would be metamorphic processes. Still, potassium-argon dating was argued to be effective, especially for micas and when corroborated by lead dating.

Contributions to potassium-argon dating were made elsewhere - most notably at the University of Chicago and the Department of Terrestrial Magnetism at the Carnegie Institution in Washington D.C., and later at the USGS, Menlo Park – but following the work of Reynolds, Berkeley became the foremost center of research on potassium-argon dating into the early 1960s. Berkeley was home to Reynolds' spectrometer, designed to measure trace amounts of argon. Berkeley also had a paleontology department with specialists in mammalian biostratigraphy. Finally, researchers at Berkeley obtained a

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[421] Aldrich and Nier, 1948
[422] These measurements were revised in the 1950s (Wasserburg and Hayden, 1955; Wetherill, Wasserburg, et al. 1956)
[423] Nier, 1950
[424] Smits and Gentner, 1950
[425] Folinsbee, Lipson and Reynolds, 1956

sizable grant from the Shell Development Company followed by a National Science Foundation grant to further develop potassium-argon dating.

By measuring the diffusion of argon in a vacuum furnace, Jack Evernden[426] and Garniss Curtis[427] at Berkeley determined that argon is retained in potassium-rich micas at temperatures less than 300°C but diffuses more rapidly at higher temperatures.[428] Accordingly, potassium-rich micas whose local geology did not show signs of substantial heating following deposition (such as metamorphosis or presence of igneous intrusion) could be preferentially sampled. Later, argon loss was also found to be a function of lattice changes and mineral grain size, such that substantial argon diffusion was possible even at temperatures below 300°C.[429] Accordingly, preferred samples would be large-grained and show no signs of alteration. This line of inquiry into argon diffusion also produced a peculiar result. Spectroscopic analysis indicated that the isotopic ratio of liberated argon was, at relatively low temperatures, identical with the isotopic ratio of atmospheric argon. As temperature in the vacuum furnace increased, the proportion argon-40 in the liberated gas also increased. Evernden and Curtis hypothesized that atmospheric argon could contaminate rock samples, but this argon is concentrated near surface sites which are more readily liberated by heat.[430] The possibility of the absorption of atmospheric argon following rock formation or after sampling could compromise argon measurements. Accordingly, cleaning techniques were developed to remove the ostensibly atmospheric argon. One method was to “bake-out” the atmospheric argon at sustained low temperatures. However, if the temperature was too low, atmospheric argon may remain in place for subsequent analysis, whereas if the temperature was too high, radiogenic argon may be lost during cleaning. An alterative cleaning method, established by Evernden and Curtis after 1960, was to use an acid wash which would liberate gases from surface layers of a sample.[431] After cleaning, a rock sample could then be processed to

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[426] Evernden was born in 1922 (Evernden, 1998). He obtained a BS in mining geology from Berkeley, and obtained a PhD in geophysics in 1951, studying seismology. He then worked for Stanolind Oil and Gas for two years as a geologist before returning to the Berkeley in 1953 where he worked until 1965. John Reynolds introduced him to potassium-argon dating which Evernden later described to be more fun than seismology. Evernden worked on potassium-argon dating for four years before returning to work in seismological problems associated with monitoring a comprehensive nuclear test ban treaty.
[427] Curtis was born in 1919, in San Rafael, California (UC Berkeley). He obtained a B.Sc. in mining engineering from Berkeley in 1942. He then worked as a geologist for Christmas Copper Corp. and Shell Oil before returning to Berkeley where he completed his PhD in geology in 1951. Joining the faculty at Berkeley, he studied volcanoes. His interest in establishing the age of lava flows led to collaboration with Reynolds and Evernden, where Curtis directed the potassium-argon dating methods to geological problems.
[428] Evernden, Curtis and Lipson, 1957
[429] Evernden, et al., 1960
[430] Evernden, et al. 1960
[431] Evernden and Curtis, 1965

liberate radiogenic argon for spectroscopic analysis. Using these methods, along with fastidious spectroscopic standards that avoided contamination from atmospheric argon and other sources, Evernden and Curtis attempted to push the lower limits of potassium-argon dating toward the upper bounds of radiocarbon dating by establishing absolute dates for mammalian biostratigraphy and hominid evolution. Near the lower bounds of potassium-argon dating which required highly precise argon measurements, results could be cross-checked against relative dating by mammalian biostratigraphy.[432]

Conclusion

During the 19th century, many geologists and biologists considered the Earth to be very ancient, and its age indefinite. By the beginning of the 20th century, several lines of inquiry seemed to agree that the Earth was around 100 million years in age. Chief among this research was the work of renowned physicist William Thomson. Subsequent development of geochronology in the 20th century demonstrates a decline in perceived utility of thermodynamics to geochronology. With respect to thermodynamics, radiogenic heat challenged the simplistic cooling earth model, and so too did development of the notion of internal convection currents. Discovery of nuclear fusion also challenged other assumptions in Thomson's dating work.

Even the earliest radiometric dating efforts hinted that the Earth may be over a billion years in age. However, it was only closer to the middle of the 20th century that radiometric dating methods became the principal authority on absolute dating, and this raised another notable conflict between two fields of inquiry. In 1929, Edwin Hubble measured the expansion of the universe and, working backwards, the time elapsed since the origin of the universe could be inferred. In this way, the age of the universe was estimated to be about 1.8 billion years. By 1950, however, Holmes considered the oldest reliably dated rock to be over 2 billion years old. Meteorites were also dated by radiometric methods to be around 4.5 billion years old. This apparent discordance between radiometric dating and the age of the universe was resolved during the 1950s, when it was determined that Hubble underestimated distance in his measurements, and with suitable corrections, the universe may be around 10 billion years in age, or more.[433]

[432]Evernden, et al., 1964; Evernden and Curtis, 1965
[433] Brush, 2001

The history of the development of radiometric dating is a history of identifying and reacting to ambiguity. This history includes successes and failures. Lead was used in the earliest radioactive dating efforts and became the gold standard endorsed by Holmes because such measurements corresponded with relative geological age. Helium dating also developed quite early and declined after confronting serious challenges. Those challenges seemed to be resolved in the 1930s, resulting in a resurgence in helium dating, only for the problem of diffusion to appear once more. It is interesting to note that the difficulty of diffusion contributed to the decline of helium dating, yet inconsistencies in lead dating were often attributed to radon diffusion, and diffusion of argon was addressed by focused sampling techniques and the development of cleaning techniques.

Some of the central confounding factors involved in radiometric dating could be controlled, isolated, and studied. In much of the literature prior to the 1960s, authors express optimism at the prospect of subsequent technical improvements that may facilitate more precise measurements. However, some of the central assumptions involved in radiometric dating – such as the presumption that radioactive decay is the only source of variation in the proportion of parent and daughter isotope over time – largely remained well outside the capacity of technical control. To the contrary, contamination and the escape of radiogenic decay products were widely recognized to be a potential source of error, and often cited as a possible cause for discrepant measurements.

Early on, Holmes employed a methodology of consistency to deal with the many ambiguities involved in radiometric dating. As a minimal criterion of effectiveness, a dating method could be deemed reliable if dates derived by that method agreed with relative geological age. Dating methods could also be corroborated against one another. Despite the methodological importance of corroboration in the history of radiogenic dating, inconsistencies were plentiful. Even alternative uranium-lead methods routinely diverged from one another and from lead-lead dating. Sometimes, such discordance resulted in the rejection or modification of individual measurements or entire dating methods. Other times, discordance resulted in other interpretative changes. For example, all dating methods yielded some measurements that disagreed with geological age determinations. Quite often, this would be attributed to some error or weakness in the work or methods of the geochronologist. Indeed, through most of the history provided above, geological age was the arbiter of radiometric dating, even though geological age was known to be ambiguous and frequently subject to modification. Toward the 1960’s, though, geochronologists were sufficiently confident in their methods to increasingly fault geological age when such discordance was found.

Mobilism had virtually no bearing upon geochronology research leading into the 1960s. Traditional relative dating methods informed by stratigraphy and biostratigraphy were centrally important to the development of radiometric dating methods and interpretations of paleomagnetic measurements. However, the ambiguities and debates involved in the alternative uses of such relative dating methods did not overlap. Geochronologists and paleomagnetists did not engage in widespread debate over interpretations of the geological timescale. Paleomagnetic methods did not contribute to the relative dating methods involved in the formation of an absolute geological timescale. Alternatively, radiometric dating methods contributed to the refinement of the geological timescale which had broad bearing across the Earth sciences, including within marine geology and paleomagnetism. However, such refinements only became practically relevant in these areas during the 1960s and thereafter. Paleomagnetists who endorsed mobilism in the 1950s typically relied on biostratigraphy to establish contemporaneity and sequence within paleomagnetic measurements. Similarly, marine geologists had relied on dredged fossils to establish the relative age of seafloor sediments. Only in the 1960s, were strands of research in geochronology, paleomagnetism, and marine geology combined.

Chapter 5: Surging Support for Mobilism

Introduction

Paleomagnetism developed rapidly during the 1950s. By 1960, most major researchers outside of the USA who worked on historical reconstruction of the geomagnetic field endorsed mobilism. Paleolatitude determinations and APW facilitated the reconstruction of continental displacements. Researchers also sought out independent means of measuring paleolatitudes to corroborate paleomagnetic inferences. The phenomenon of polarity reversals in rock magnetism was well known, but it was uncertain whether these reversals were the product of geomagnetic field reversals or ferromagnetic properties of certain rocks.

During the 1950s, distinctions between marine and terrestrial geology became increasingly apparent. The seafloor was not simply subsided continental crust. By 1960, continental displacements seemed plausible due to large offsets in magnetic anomalies across faults in the pacific, but the extent and nature of this displacement was equivocal. Harry Hess endorsed the notion of lateral motion of the seafloor, wherein new seafloor is created at ridges and destroyed at trenches. For Hess, this process of seafloor spreading was driven by convection currents within the mantle. Like other grand attempts to genetically relate the distinctive features of marine geology, seafloor spreading was widely recognized to be highly speculative. Marine magnetic anomalies were known, but typically considered to be of secondary importance in grand accounts of seafloor evolution.

Geochronology underwent notable transformation in the 20th century, driven by shifting expectations of which physical processes are most consistent and reliably measurable. By the 1950s, lead or strontium ratios routinely indicated that the Earth was several billion years in age. However, most of the well-defined geological history of Earth – within the past 500 million years or so - was too young to be effectively dated by lead or strontium methods. Potassium-argon dating held promise for such intermediate age determinations, but notable ambiguities included the often-unknown effect of argon diffusion and atmospheric argon contamination within the history of a sampled rock. Even into the 1960s, new methods to account for such diffusion and contamination were needed to facilitate the dating of younger rock. By the early 1960s, potassium-argon researchers considered their work to be authoritative, but this was not necessarily the case outside of that community.[434]

[434] This is unsurprising given the generally limited availability of dated rock and inherent uncertainty in dating sedimentary formations via igneous intrusions. See comments in Evernden et al., 1965.

In each of the three preceding chapters, the historical narrative paid close attention to unique sets of aims, instruments, objects of study, problems, methods, theories, people, and places. A central function was to sketch the contours of the diverse ambiguities and debates involved in the development of three strands of scientific inquiry. Each of the three narratives provided in previous chapters ended around 1960. Except for a handful of paleomagnetists who accepted mobilism in the 1950s, mobilism was widely considered to be, at best, highly speculative around this time. By the end of the 1960s, however, a revolution was well underway across the Earth sciences, as mobilism formed a central part of an emerging framework used to address diverse and wide-ranging problems in paleomagnetism, marine geology, and geochronology, but also in seismology, orogeny, biogeography, paleontology, geodesy, and beyond.

It is during the 1960s that certain strands of research within paleomagnetism, marine geology, and geochronology became intertwined. In this chapter, I offer an account of their intertwining. Previous chapters aimed to identify sources of ambiguity and contours of debate. Though some ambiguities will be highlighted in this chapter, the central focus of this historical account shifts around 1965, at which point a sort of conceptual coherence that I call *snapping together* takes center stage.[435] Whereas previous chapters aimed to account for the breadth of ambiguity and debate within a field, this chapter focuses on a narrower series of intertwining research related to the geomagnetic reversal timescale, the Vine-Matthews hypothesis, and corollaries.[436] I begin by introducing the geomagnetic reversal timescale. In order to test the geomagnetic reversal hypothesis, potassium-argon dating was used to establish the contemporaneity of reversal events and thereby construct an absolute reversal timescale. I then introduce the Vine-Matthews hypothesis which combined the seafloor spreading hypothesis with the geomagnetic reversal hypothesis to account for marine magnetic anomalies. Finally, I examine efforts to model magnetic profiles about mid-ocean ridge axes by using the geomagnetic reversal timescale to establish parameters within the Vine-Matthews hypothesis. Magnetic profiles

[435] The idea of snapping together will be examined in Chapter 8.

[436] As noted in Chapter 4, Glen claims that independent lines of research produced a confluence of evidence that fired the mobilism debate in the 1960s (Glen, 1982). Glen emphasizes the role of geochronology and the geomagnetic reversal timescale in his account. Alternatively, Frankel identifies three difficulty-free solutions in the mobilism debate during the 1960s (Frankel, 2012). These solutions took place in quick succession. The first, is the Vine-Matthews hypothesis which was proposed in 1963 and obtained difficulty-free status in 1966 following the identification of the Reykjanes Ridge magnetic profiles as well as the Eltanin profiles about the Pacific-Antarctic Ridge. Tuzo Wilson's 1965 hypothesis of the transform faults obtained difficulty-free status in 1967, following seismic measurements of Lynn Sykes. Finally, plate tectonics was difficulty-free when first proposed by Morgan, McKenzie, and Parker in 1967, and further developed by Le Pichon and others. Frankel doesn't provide a reason for why these difficulty-free solutions were clustered together.

obtained from many different ridges aligned with this modeling and facilitated the determination of seafloor spreading rates. Such seafloor spreading rates, in turn, facilitated the demonstration of predictive utility and global coherence of plate tectonics.

The Geomagnetic Reversal Timescale

In the early 1960s, Allan Cox and Richard Doell were working at the USGS in Menlo Park on paleomagnetic reversals. The pair published a review of paleomagnetism in 1960,[437] a test of Earth expansionism by paleomagnetism,[438] an analysis of magnetic properties of oceanic basalt retrieved from the Mohole project,[439] and an examination of sources of error in paleomagnetic methods based on measurements of lava flows which cooled in know fields.[440] In 1962, Cox and Doell met Brent Dalrymple, a graduate student at Berkeley working on Cenozoic potassium-argon dating.[441] Potassium-argon dating at Berkeley had begun producing results of high consistency under the leadership of Reynolds, Curtis, and Evernden, and their methods allowed for dating of Quaternary rock. This presented an opportunity to develop a research programme that could distinguish between self-reversals and geomagnetic field reversals as alternative possible explanations for polarity reversals in rock magnetism. Previous attempts had been made to correlate reversals in lava flows, but potassium-argon dating could provide an independent means of establishing contemporaneity across lava flows that might avoid some of the ambiguities involved in stratigraphic dating methods. If polarity reversals were due to geomagnetic field reversals, and potassium-argon dating were sufficiently reliable and precise, it should be possible to produce a coherent global reversal timescale. Such a timescale could be constructed from many different sample locations. Alternatively, if polarity reversals were largely due to self-reversals, it would not be possible to construct a consistent timescale across multiple sampling locations.

Meanwhile, in 1960, John Jaeger, the director of the Department of Geophysics at Australian National University in Canberra, sent Ian McDougall to Berkeley for a one-year post-doctoral position in order to learn potassium-argon dating. Jaeger became director of the Department of Geophysics in 1952

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[437] Cox and Doell, 1960
[438] Cox and Doell, 1961
[439] Cox and Doell, 1962
[440] Cox and Doell, 1963
[441] Dalrymple was born in Alhambra California in 1937 (Petersen, 2012). He obtained a B.A. in geology from Occidental College in 1959. He completed his PhD at Berkeley in 1963 and joined Cox and Doell at the USGS.

and was deeply committed to making ANU a center for emerging geophysical research.[442] McDougall[443] spent a year at Berkeley, and Evernden travelled from Berkeley to ANU to assist in the construction a potassium-argon dating laboratory. After completing his postdoc, McDougall travelled to Hawaii to collect volcanic samples in 1961. He was initially interested in whether the young Hawaiian basalt could be effectively dated by the potassium-argon method. While engaged in this fieldwork, McDougall met Don Tarling,[444] also from ANU, who was collecting Hawaiian basalts for his doctoral research on paleomagnetic secular variations.

Prior to 1960, potassium-argon dating was mainly carried out on minerals with high potassium content, with comparatively little work completed on basic igneous rocks that contain little potassium. In 1961, McDougall attempted to establish the relative argon retention of feldspar minerals commonly found within basic igneous rock for this purpose, building upon the work of MIT researchers.[445] He also found that the whole-rock measurements of the chilled margin of an igneous intrusion produced consistent ratio measurements. McDougall came to favor whole rock measurements of lava flows: Based on known superposition and their recent origin, samples could be taken that almost certainly had not been subject to high temperatures or stresses following crystallization which might otherwise result in argon diffusion.[446] When such rocks were not available, McDougall would isolate minerals with the best-known argon retentivity.

In 1963, Cox, Doell, and Dalrymple published a timescale of geomagnetic reversals based on six dated and magnetically sampled sites from lava flows in California.[447] Three of these sites were dated by Evernden, Curtis, and others at Berkeley, while two were dated by Dalrymple. The radiometric dates were obtained first and not for the initial purpose of paleomagnetic inquiry. Only subsequently did Cox and Doell obtain paleomagnetic samples from these dated sites based on published descriptions of locations and unpublished field notes. Recently formed rock was known to be of normal polarity. Cox

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[442] Paterson, 1982
[443] McDougall was born in Hobart, the capital of Tasmania, Australia in 1935 (Zeitler, et al., 2019). He obtained a B.Sc. in geology at the University of Tasmania in 1957 where he attended classes from Samuel Carey. He then entered graduate studies at the Australian National University in the Department of Geophysics under director John Jaeger. He obtained his PhD in 1961. At Jaeger's suggestion, McDougall changed his research focus from petrology to geochronology.
[444] Tarling obtained a bachelor's degree in Geology & Geography form the University of Keele in 1957 and a Master's in Geophysics from Imperial College London in 1959 (Frankel, 2012, Volume II, 471-472). He then attended ANU under Ted Irving, studying geomagnetic secular variation. He received his PhD from ANU in 1963.
[445] McDougall, 1961
[446] McDougall, 1963
[447] Cox, Doell and Dalrymple, 1963a

and Doell found that rock dated at 0.98 million years ago also had normal polarity, while rock dated at 0.99 million years was reversed. Two more sampling sites consisted of reversed polarity, between 1-2 million years in age. Finally, rock dated at 2.6 and 3.2 million years were of normal polarity. Cox and Doell were predisposed to the notion that field reversals were periodic in nature. Accordingly, they offered two timescale interpretations for these measurements. The first consisted of polarity reversals every 0.5 million years. The second consisted of polarity reversals every million years. They then showed that these timescales were consistent with three additional data points from a European study.

Shortly thereafter, McDougall and Tarling published their own timescale.[448] McDougall and Tarling had gathered samples together in Hawaii. Accordingly, paleomagnetic and radiometric measurements were sometimes made on the same sample, or samples from the same outcrop. Still, stratigraphic methods often had to be employed to infer dates or establish maximum or minimum ages. Based on 59 sampling sites, the pair identified the youngest reversal to be 1.15 million years in age, with nine younger sites showing normal polarity.[449] Following twenty-eight sites of reversed polarity, the next youngest site of normal polarity was dated to 2.76 million years. Eight sites dated around 2.95 million years were of reversed polarity, and normal polarity was measured at 3.27 million years. McDougall and Tarling thereby discarded the notion that field reversals were periodic. They also distinguished a lengthy normal period, preceded by a lengthy reversed period, with relatively rapid polarity reversals around 3 million years ago. The measurements of Cox, Doell and Dalrymple were also included for comparison, with the only notable discrepancy coming from one of the European data.

Cox, Doell, and Dalrymple published a second timescale in 1963. [450][451] This study integrated 10 new data points from California and one from the Olduvai Gorge in Tanganyika (Tanzania). The youngest dated reversal was about 1 million years in age. This was preceded by a period of reversed polarity lasting between 0.8 and 1 million years in duration. This reversed period was preceded by a period of normal polarity. There was no indication of reversals over 2 million years in age. However, the Olduvai sample was of normal polarity and dated at 1.85 million years, and normal polarity was found in several

[448] McDougall and Tarling, 1963
[449] Another sample for which only a minimum age bound of 0.86 million years was established was also found to be reversed.
[450] Cox, Doell and Dalrymple, 1963b
[451] It is interesting to note that in both their 1963 timescales, Cox, Doell and Dalrymple emphasize petrological variations between sample sites to limit the possible influence of self-reversals. This approach makes sense in their first paper, as sample sites are mainly limited to California. As more data accumulated, this approach became unnecessary.

samples dated just over 3 million years. These results compelled the trio to discard the presumed periodicity of geomagnetic reversals.

The second timescale of Cox, Doell, and Dalrymple was published in the same month as McDougall and Tarling's first timescale. Significant discrepancies between these two timescales were apparent. Both groups indicated that recent rock is of normal polarity, with the most recent transition to reversed polarity taking place about 1 million years ago. Both groups also identified an extended period of reversed polarity prior to 1 million years ago. However, McDougall and Tarling marked a transition to normal polarity around 2.5 million years ago. Alternatively, Cox, Doell and Dalrymple marked this transition around 1.8 million years ago. The USGS group also indicated that this period of normal polarity stretched to the limit of their timescale at about 3.25 million years. McDougall and Tarling, however, alternated normal and reversed periods in their timescale, due to a reversed polarity data point at 2.95 million years.

In 1964, Cox, Doell, and Dalrymple identified errors in their previous measurements.[452] In their first paper, the trio used stratigraphic correlation with known locations of radiometrically dated rock to date their paleomagnetic measurements. Upon reading the results of McDougall and Tarling, the USGS group returned to the locations of their fieldwork and identified errors in stratigraphic correlation. In one case, a dated outcrop was not deemed ideal for paleomagnetic sampling, so samples were taken from an outcrop assigned to the same formation 2km away. Upon re-examination, the polarity of these two outcrops were inconsistent. In another case, the paleomagnetic and radiometrically dated samples were obtained only a few meters apart but turned out to be from different lava flows. The USGS group revised their data, such that Californian samples dated at 2.2 and 2.3 million years, initially reported as normal polarity, were actually reversed.

To this point, Cox, Doell, and Dalrymple relied on the potassium-argon laboratory at Berkeley for radiometric dating. Cooperation with researchers at Berkeley, however, became somewhat strained as potassium-argon dating was in high demand and geochronologists at Berkeley had their own research projects. Additionally, researchers at Berkeley who contributed radiometric dates to the USGS research group were not listed as co-authors on their work. A potassium-argon dating laboratory at the USGS became operational in 1964.[453]

[452] Cox, Doell and Dalrymple, 1964a
[453] The USGS laboratory only came into being when Cox and Doell threatened to leave the USGS for not being able to satisfy the conditions of an NSF grant supporting their research (Glen, 1982, 204)

In 1964, McDougall and Tarling updated their timescale, integrating data from the second timescale of the USGS group.[454] McDougall and Tarling split their timescale into three periods. A period of normal polarity spanned 1 million years ago to the present day. Prior to this, a reversed period began 2.5 million years ago. Another normal period preceded this with uncertain duration as available data ended about 3.25 million years ago. Their scale included three aberrant data points. The Olduvai sample showed normal polarity at 1.8 million yeas ago, a sample from Europe showed normal polarity at 2.4 million years, and a Hawaiian sample from their first timescale exhibited reversed polarity at 2.95 million years. The European sample was considered suspect as polarity measurements were determined in the field by compass rather than rigorous laboratory measurements. McDougall and Tarling also called for re-examination of the Olduvai sample and stated that their reversed sample dated at 2.95 million years may have been misdated due to argon diffusion and should also be re-examined. At this point, the ANU group was disposed to the notion that geomagnetic reversals were infrequent and were thereby willing to critique aberrant data points.

Later that year, Cox, Doell, and Dalrymple updated their timescale yet again.[455] This time, they integrated McDougall and Tarling's data and added additional measurements from North America. All samples under 1 million years of age were of normal polarity. This was denoted as the *Brunhes normal epoch*. An extensive period of reversed polarity called the *Matuyama reversed epoch* spanned from approximately 1 million years to 2.5 million years. A brief period of normal polarity around 1.8 million years was named the *Olduvai event*. This was supported by an additional measurement from North America. By pushing the end of the Matuyama epoch to 2.5 million years, the USGS aligned their timescale with that of the ANU group. A normal period called the *Gauss normal epoch* preceded the Matuyama and lasted until about 3.4 million years ago. The Gauss normal epoch was interrupted by a brief period of reversed polarity about 3 million years ago, called the *Mammoth event*. This was supported by an additional measurement from North America. The Gauss epoch was preceded by a reversed epoch of unknown duration. There was some inconsistency in the data points near the boundary of the Matuyama and Gauss epochs, but this could be because correlational and dating methods were not sufficiently precise to provide perfect dating resolution. The USGS group thereby deviated from the ANU group by allowing for very brief periods of geomagnetic reversals to punctuate longer epochs.

[454] McDougall and Tarling, 1964
[455] Cox, Doell and Dalrymple, 1964b

By 1965, such attempts to construct a geomagnetic reversal timescale revealed a high degree of consistency across reversal sequences in Hawaii and North America. Such consistency offered very strong support to the hypothesis of geomagnetic field reversals. For those most familiar with the USGS and ANU research, self-reversal was no longer a plausible explanation for polarity reversals in general, but only a possible confounding factor that might produce local noise in otherwise global patterns.[456] The consistency between reversal patterns also demonstrated the high precision and reliability of potassium-argon dating of basic rock, even as efforts to construct an absolute geological timescale from potassium-argon dating was underway at Berkeley.

It is interesting to note, however, that the USGS group seemed overeager to emphasize the consistency of data across three continents in 1963, since earlier that month the ANU group had published data that disagreed with the USGS timescale. Confronted with such discordant data, the USGS group returned to the field and reversed the polarity of two data points at the center of this disagreement. This change helped align the USGS group with ANU measurements but retrospectively diminished the internal consistency of their 1963 timescale. Thereafter, European data seemed to disagree with both the USGS and ANU timescales. Though the European measurements were indispensable to the early USGS publications,[457] the incompatible data was summarily questioned by the ANU group for substandard measurement techniques. The ANU group also called for re-examination of data points that seemed to disagree with somewhat arbitrarily generalized periods of normal or reversed polarity. Yet, the USGS group was willing to weaken such generalizations to allow for shorter reversal periods within longer epochs.

The Vine-Matthews Hypothesis

In 1962, Drummond Matthews, a research fellow at Cambridge, led a survey of the Carlsberg Ridge in the Western Indian Ocean. Researchers at Lamont and Scripps were already engaged in large-scale magnetic surveying, so to provide some novel utility, Matthews aimed to produce the most detailed magnetic survey that he could over a relatively small area. He thereby obtained bathymetric and magnetic measurements over an area 50 by 38 nautical miles which, based on seismic data,

[456] This conclusion was also supported by contemporaneous work on polarity of baked contacts (Wilson, 1962; Irving, 1964)
[457] The European data provided measurements from a second landmass, against which measurements from California could be compared.

contained a segment of the Carlsberg Ridge.[458] Survey passes were taken at intervals of one nautical mile. Beacons were anchored to facilitate position fixing of survey passes via visual bearing and radar. Sometimes dead reckoning or star sighting was used to determine geographical position as well. Including cross-tracks, two thousand nautical miles of profiling data was then used to construct a bathymetric and magnetic map of the seafloor. Matthews found that the central median valley was not well developed and that a fault seemed to offset the ridge by 10 nautical miles. He also found a strong negative magnetic anomaly associated with the ridge and magnetic anomalies associated with seamounts flanking the ridge.

The following year, Frederick Vine,[459] a student of Matthews', offered an explanation for the pattern of magnetic anomalies found in this survey.[460] Vine considered the negative anomaly at the crest of the ridge to be the effect of a body magnetized in the present direction of the Earth's field, but at a low magnetic latitude in the southern magnetic hemisphere. The positive anomalies flanking the crest were considered to be reversely magnetized. Vine and Matthews used a computer program to illustrate that blocks of normal and reversed magnetism produced a more-accurate model of obtained measurements than that produced by uniform normal magnetization. Computer modelling was used to produce possible magnetic profiles based on variations in field strength (itself a product of thickness and magnetic susceptibility), inclination, and profile bearing. Vine endorsed seafloor spreading, wherein new seafloor is created at ocean ridges from cooling of upwelling mantle material while progressively older seafloor extends away from the ridge. If, upon its formation, new seafloor acquired and retained the magnetism of the geomagnetic field, and if geomagnetic field reversals take place, then, Vine argued, a pattern of differentially magnetized adjacent blocks of seafloor could be created.[461] In effect, Vine combined seafloor spreading with the geomagnetic reversal hypothesis in order to account for marine magnetic anomalies.

Upon publication, the Vine-Matthews hypothesis did not garner significant attention. The Matthews profile showed that the central anomaly was the most pronounced. Magnetic anomalies were known to present as a linear pattern, but it was unclear if these stripes tended to be parallel to ocean ridges. Matthews' survey was on too small a scale to even make this conclusion with respect to the

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[458] Matthews, Vine and Cann, 1965

[459] Vine was born in West London in 1939 (Vine, National Life Stories). He graduated from Cambridge in 1962 with a degree in Natural Sciences. He was Drummond Matthews' first research student and received his PhD from Cambridge in 1965.

[460] Vine and Matthews, 1963

[461] Vine and Matthews cited Cox, Doell, and Dalrymple's 1963 timescale in support of the notion of field reversals.

Carlsberg Ridge. The most thoroughly mapped magnetic anomalies were in the Northeast Pacific, but there was no known ridge in the area. Accordingly, the Vine-Matthews hypothesis didn't seem to offer an adequate account of the best available data. Seafloor spreading was a moderately popular theory, but not universally endorsed, and it was widely recognized to be highly speculative. Likewise, even among paleomagnetic researchers at this time, the field reversal hypothesis was not universally endorsed. Additionally, even if seafloor spreading and field reversals did take place, seafloor magnetism would have to be sufficiently strong for remanent magnetism to account for the measured anomalies, but, due to uncertain physical processes and characteristics, Vine and Matthews did not fully describe how this might happen. Based on seismic refraction measurements, the seafloor was thought to be comprised of several layers, but the formation, composition, magnetic susceptibility, or relative thickness variations of these layers was mostly unclear.

The speculative nature of the Vine-Matthews hypothesis is also illustrated by the previous experience of Lawrence Morley, who also tried to explain marine magnetic anomalies by connecting seafloor spreading with field reversals. Based on contemporaneous expectations of proponents of field reversals, Morley supposed that reversals take place at regular intervals of 1 million years.[462] By measuring the distance of anomalies away from a ridge, Morley could estimate seafloor spreading rate. Morley submitted his paper to the journal *Nature*, before Vine and Matthews, but Morley's paper was rejected. After revisions, the paper was rejected from the *Journal of Geophysical Research* as well.

As examined in the previous section, subsequent work from the ANU and USGS on reversal timescales seemed to strengthen the hypothesis of field reversals. However, seafloor spreading did not fare quite so well. In 1964, George Backus,[463] at Scripps, suggested a test of Vine-Matthews.[464] If seafloor spreading displaced continents, then the historic spreading rate in the South Atlantic should be greater than the historic spreading rate in the North Atlantic. This is because continental displacement is greater in the South. Now, if the spreading rate is greater in the South than in the North, and Vine-Matthews is correct, then the width of magnetic anomalies in the South Atlantic should be greater than in the North Atlantic. Backus predicted what this difference in width ought to be, based on the notion

[462] Morley, 2018
[463] Backus was born in Chicago in 1930 (Frankel, 2012, Volume IV, 202). He obtained a B.Phil. (1947), a B.Sci. (1948), an M.Sci. in mathematics (1950), an M.Sci. in physics (1954), and a PhD in physics (1956), all from the University of Chicago. During his PhD he studied the origin of Earth's magnetic field under Subrahmanyan Chandrasekhar. Between 1957 and 1960, Backus worked as a physicist at Princeton and then an assistant professor of mathematics at MIT. He then accepted a position at Scripps in 1960 where he remained.
[464] Backus, 1964

that North America and South America drifted apart from Europe and Africa and that this relative motion could be explained as a rotation about a sphere, wherein the pole of rotation exhibits no motion while the equator of rotation exhibits the greatest rotation. Backus applied to the National Science Fund to execute this test of Vine-Matthews, but his application was rejected as too speculative.[465]

Research into the magnetic properties of seafloor rock, obtained by dredging and coring, indicated that oceanic basalt retains sufficient remanent magnetism to account for magnetic anomalies. In 1964, Jim Ade-Hall, a PhD student in the Geology Department at Imperial College under Ronald Mason, found that retained natural magnetism is systematically much greater in submarine basalts than continental basalts.[466] The layer(s) of the oceanic crust, and possible thickness thereof, capable of producing measured magnetic anomalies remained subject to speculation.[467] Vine had initially attributed remanent magnetism to crustal blocks consisting of the entire oceanic crust beneath upper sediments, extended to a depth 11km below the center of the ridge and 20km below sea-level over the deep ocean.[468] The measurements obtained by Ade-Hall, and subsequent petrological work, expanded the range of permissible depths and thicknesses of magnetic rock within the ocean crust that would be capable of accounting for magnetic anomalies.

In 1964, researchers at the Department of Geodesy and Geophysics at Cambridge dated basalt from near the crest of the Mid-Atlantic Ridge. Using the potassium-argon method, the basalt was dated at 29 million years in age.[469] Similarly, researchers at Lamont, identified Miocene fossils within dredged rock and also within several sedimentary cores taken near the Mid-Atlantic ridge.[470] Though sedimentation was nearly absent near the ridge crest, sediments beyond the crest in the South Atlantic seemed to be of constant thickness.[471] They argued that this precluded the possibility of seafloor spreading for at least the past 20 million years. Others at Lamont argued that convection currents were of insufficient magnitude to cause seafloor spreading, based on heat flow measurements, especially in the Atlantic.[472] Heat flow measurements did show higher heat flow near mid-ocean ridges. However, based on a heat flow model of the ocean floor which assumed seafloor thickness, conductivity, density,

[465] Menard, 1986
[466] Ade-Hall, 1964
[467] Hess, 1965; Cann and Vine, 1966
[468] Vine and Matthews, 1963
[469] Baker, P.E., et al. 1964
[470] Saito, Ewing and Burckle, 1966
[471] Ewing, Le Pichon and Ewing, 1966
[472] Langseth, Le Pichon and Ewing, 1966

base temperature, and similar parameters, it was determined that the magnitude of this heat flow was too low, and the areas of high heat flow were too narrow. In addition to this general trend, heat flow measurements also yielded many puzzling data points, such as low heat flow measurements at some ridge locations or large variation in heat flow across short distances.[473] In 1966, Lamont researchers explicitly claimed that this data was inconsistent with continuous continental displacement in the Atlantic during the Cenozoic.[474]

In a 1965 paper, Lamont researchers reviewed magnetic profiles across the Mid-Atlantic Ridge, identifying the anomalies as running parallel to the ridge axis.[475] They distinguished between anomalies near the ridge which diminish in amplitude farther from the ridge crest, from those farther from the ridge that show greater wavelength and amplitude. Vine had previously accounted for the diminishing amplitude in anomalies near the ridge, based on the notion that the potential effect of secondary volcanism (producing magnetically atypical rock) increases with age and distance from the ridge crest. However, researchers at Lamont highlighted that the Vine-Matthews hypothesis did not account for the increased amplitude and wavelengths of anomalies farther from the ridge crest. Instead, they endorsed the notion that these anomalies had an alternative mechanism of origin.

In general, researchers at Lamont remained opposed to the possibility of mobilism, especially in recent history. However, seafloor spreading garnered support from alternative lines of inquiry pursued elsewhere. Some of the most impressive work of this kind came from Canadian geophysicist, Tuzo Wilson.[476] In 1963, Wilson argued that measured ages of certain oceanic islands supported the seafloor

[473] Von Herzen and Langseth, 1965
[474] Langseth, Le Pichon and Ewing, 1966
[475] Heirtzler and Le Pichon, 1965
[476] Wilson was born in Ottawa, Canada in 1908 (Garland, 1995). He graduated from the University of Toronto in 1930 where he studied physics and geology. He engaged in geological field work during summers. Wilson then obtained a Massey Fellowship and enrolled at Cambridge for a second B.A. There, he met Harold Jeffreys and Teddy Bullard. Wilson attended Princeton for his PhD where he met Harry Hess. He joined the Geological Survey of Canada in 1936 where he engaged in mapping efforts in Nova Scotia, Quebec, and the Northwest Territories. During WWII, Wilson served overseas in the Royal Canadian Engineers where he worked on tunneling for defensive purposes in Britain, rising to the rank of Colonel, Director of Army Operational Research. In 1946, Wilson joined the University of Toronto as Professor of Geophysics in the Department of Physics. At the time, Wilson was the only professor of geophysics in Canada. He instituted a uranium-lead dating laboratory and began a programme dating the Canadian Shield, writing on the growth of fixed continents. Wilson also published an influential paper on global orogeny produced by contractionism in 1950. Wilson was one of the premier geophysical theoreticians during the 1960s. No doubt, this was due to his interdisciplinary training and work experience and his experience dealing with large-scale features of cratons, mountains, and glaciers. By the early 1960s, Wilson abandoned contractionism to endorse mobilism.

spreading hypothesis.[477] If seafloor spreading takes place, then oceanic crust should increase in age with distance from mid-oceanic ridges. Wilson suggested that some islands form at mid-oceanic ridges and are then carried away from the ridge by seafloor spreading. The age of such islands should, then, increase with distance from mid-ocean ridges. Upon dating islands based on their oldest identified fossils, Wilson found that island age indeed correlated with ridge distance. He considered this to be supportive of seafloor spreading. However, the pattern did not hold in the Pacific, and there were also several problematic data points. Iceland, for example, is quite close to a ridge, but also very old. Wilson attributed this to a slower rate of spreading in the North Atlantic, but such variations in spread rate were not explained in his analysis. One notable difficulty with this approach was that fossil dating only provides a minimum possible age of an island. Additionally, islands may not form only at ridges. Though islands cannot be older than the seafloor upon which they are located, young islands could still be quite distant from ocean ridges. Regardless, based on the correlation between island age and ridge distance, Wilson proposed that seafloor spreading may take place at a rate of about 3.5cm per year, driven by internal convection cells residing on either side of the ridge in long, irregular cylinders.[478]

Late that year, Wilson offered an account of island ages in the Pacific that was consistent with seafloor spreading.[479] He claimed that relatively slow-moving central regions of convection cells could produce local sources of lava that extrude through the oceanic crust above. If seafloor spreading takes place, the oceanic crust may change in relative position to this lava extrusion and island chains may thereby increase in relative age in the direction of seafloor spreading.

In 1965, Wilson published a remarkable paper in the journal *Nature*.[480] He argued that Earth's crust is divided into several large rigid plates that are not readily deformed except at their edges. The boundaries between these plates form a global network of what Wilson called *mobile belts*, where relative movement between the plates takes plates. These mobile belts consist of three distinct features. Mid-ocean ridges were deemed to be tensional features, where new seafloor is created.[481] Island arcs were deemed to be compressional features. Wilson also identified a new class of fault called

[477] Wilson, T., 1963a
[478] Menard argued that when dated seamounts and seafloor cores were added to Wilson's work, this eliminated any relation between age and ridge distance (Menard, 1965b). He also argued that ages inferred by fossils may be imprecise and that the best-dated sections of seafloor do not align with Wilson's supposed trend. Rather, the oldest datable material at the crest of the Mid-Atlantic Ridge is about the same age as the oldest dated material at the Canary Islands.
[479] Wilson, T., 1963b
[480] Wilson, T., 1965a
[481] Wilson did not use the term "seafloor spreading" in this paper.

the *transform fault*. The transform fault is a location where adjacent plates move past one another. He claimed that at the termination of any of these three features, the mobile belt is transformed into one of the other two features. Wilson highlighted ridge offsets at the Mid-Atlantic Ridge as representative of transform faults. These offsets were previously interpreted to be transcurrent faults, wherein faulting produced offsets in a ridge that was initially continuous. According to Wilson, the direction of relative motion at these faults would be precisely opposite to previous expectation. Based on his general framework, Wilson suspected that an unidentified ridge must be located off of Vancouver Island, between the San Andreas Fault and a submarine fault, seismically identified by Benioff, adjacent to the Aleutian Arc.

In 1965, Wilson and Hess joined Vine at the Department of Geodesy and Geophysics at Cambridge for a period of a few months.[482] Hess was in England for a lecture tour. Wilson was on sabbatical and planned on delivering lectures, including one at Newcastle upon the invitation of Keith Runcorn. It was during this time at Cambridge that Wilson developed the idea of transform faults. In a 2003 retrospective, Vine claims that Wilson was explaining how his system required that an undiscovered ridge reside off the coast of Vancouver Island.[483] Harry Hess then reminded Wilson and Vine that the Northeast Pacific was one of the few areas of seafloor that had been magnetically mapped in detail. If Wilson's postulated ridge existed, and if the Vine-Matthews hypothesis was correct, then the ridge location should be discernable in the magnetic map. Vine retrieved this map from a volume of the *Bulletin of the Geological Society of America* and laid the map in front of Wilson and Hess.

> *All three of us stared at it in amazement. Not only were there linear magnetic anomalies paralleling the trend of Tuzo's putative ridge, but there was also a symmetry to the pattern of anomalies about the ridge crest.*[484]

In a 1979 interview with Henry Frankel, Vine recalled:

> *The real flash in a way was the symmetry of the Juan de Fuca. It was the first thing I saw. That was '65 when Tuzo and Harry and I realized there was a ridge in that area – that is an incredible story as to why it had not been recognized before – but the magnetics over it was symmetrical, and we just stood there looking at it with out mouths open. There it was. The thing had been in the literature for four years; nobody had seen it. When you went to*

[482] Frankel, 2012, Volume IV, 257-258
[483] Vine, 2018
[484] Vine, 2018, 60

look for it, it just stood out. That was the first thing. The Juan de Fuca was a tremendous revelation in that it showed that the records could be sufficiently clearly written to generate symmetry and the boundaries were quite sharp and well defined.[485]

Vine later said that Wilson considered this to be "one of the best bits of work he ever did."[486]

Modeling Magnetic Anomalies

In 1965, Wilson reported on this discovery in the journal *Science* and named the ridge the Juan de Fuca Ridge.[487] In an accompanying paper, Vine and Wilson also reported on the magnetic anomalies associated with this ridge.[488] They provided three magnetic profiles. They also provided a mirror image of one of these profiles to demonstrate symmetry about the ridge.[489] Assuming a constant rate of seafloor spreading, they modeled the magnetic profile that would be expected, given the 1964 reversal timescale produced by Cox, Doell, and Dalrymple. As was the case in Vine's 1963 paper, this modeling was done on a computer program that could model magnetic profiles based on a set of parameters including distances between alternatively magnetized bodies, their width, depth, strike, and magnetic inclination and susceptibility.

These models did not clearly align with measured profiles.[490] However, Vine and Wilson argued that a constant rate of seafloor spreading was unlikely, and thereby attempted to produce a best-fitting model of the Juan de Fuca magnetic profile by incorporating variations in the spreading rate. They produced two such models. The first model assumed that remanent magnetism within the ocean crust extended from a depth of 3 to 11km below sea level. The second model, based on a suggestion by Hess, assumed that remanent magnetism within the ocean crust was confined to a thinner yet more strongly magnetized region from 3.3 to 5km below sea level. The model profile generated from Hess' suggestion

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485 Frankel, 2001, Volume IV, 300-301.
486 Vine, National Life Stories
487 Wilson, T., 1965b
488 Vine and Wilson, 1965
489 They selected the profile with the clearest symmetry for this task.
490 Vine later stated, "I'd took longer than [Wilson] would have liked in writing it up, and the problem was from my point of view I was sort of – had to include simulations and so on, magnetic computations, the problem was that these anomalies did not reflect the reversal timescale as we know it [laughs]". Also, "unfortunately with the timescale that I had, the Cox, Doell and Dalrymple timescale at the time, it wouldn't fit. So… you know, I had spent a lot of time trying to explain this away [laughs] in the paper cause a crucial thing was the symmetry" (Vine, National Life Stories).

seemed to align more closely with measurements. However, an obvious issue with Vine and Wilson's approach was that, by adjusting spreading rates, *any* symmetrical magnetic profile could be aligned with *any* reversal timescale.

Researchers at Lamont independently discovered the Juan de Fuca and other ridges in the Northeast Pacific based on bathymetric, seismic, and heat flow data.[491] They published a magnetic profile of the region and compared this with profiles of the southern extension of the East Pacific Rise and the Mid-Atlantic Ridge, emphasizing apparent correlations. They also published profiles of the Reykjanes Ridge, emphasizing linearity and symmetry of the magnetic anomalies. Still, Lamont researchers claimed that the Vine-Matthews hypothesis did not account for the systematic differences in the character of magnetic anomalies distant from the ridge axes.

In November of 1965, Vine attended the annual meeting of the Geological Society of America in Kansas City. Brent Dalrymple was also in attendance. Dalrymple showed Vine an updated reversal timescale based on work that the USGS had completed in the summer.[492] In their 1964 timescale, the USGS group identified the transition from the Brunhes normal epoch to the Matuyama reversed epoch as the most recent reversal event, taking place about 1 million years ago. However, in early summer of 1965, the USGS trio re-examined radiometric dates for a sample location considered to be representative of the Brunhes-Matuyama boundary. Revised dating placed these samples at about 0.7 million years ago, thereby resulting in greater ambiguity with respect to the timing of the Brunhes-Matuyama boundary. Later that summer, Doell and Dalrymple engaged in fieldwork in New Mexico, aiming to date the Brunhes-Matuyama boundary more precisely.[493] They published their results in 1966, identifying both normal and reversed samples between 0.7 and 1 million years. Three samples dated at about 0.7 million years were reversed, one sample at 0.88 million years showed intermediate polarity, one sample at 0.89 million years was of normal polarity, and one sample at 1.04 million years showed reversed polarity. One possible explanation they offered for these results was that the precision of potassium-argon measurements is not sufficiently high to distinguish ages of samples so close in age. Since global correlations were not identified, another possible explanation was that the apparent reversals were the product of self-reversal. Yet another possibility was that some unnamed reversal event took place between 0.7 and 1 million years ago. The USGS group considered this third option to be

[491] Talwani, Le Pichon and Heirtzler, 1965
[492] This timescale was not yet published.
[493] Doell and Dalrymple, 1966

most likely. They isolated the Brunhes-Matuyama boundary to 0.7 million years, with a reversal event of normal polarity around 0.9 million years, which they dubbed the *Jaramillo event*. The Jaramillo event was subsequently corroborated in samples from Reunion by the ANU group in 1966.[494] The ANU group also extended the timescale to the *Gilbert Reversed Epoch* ending at 3.35 million years ago and added two additional short reversal events, one around 1.6 million years in the Matuyama epoch and another at 2.8 million years in the Gauss epoch.

At the GSA meeting in November, Dalrymple provided Vine with a preliminary version of this updated timescale. The change to the Brunhes-Matuyama boundary and the Jaramillo event would influence Vine's profile modeling. Vine claimed that this information, "provided me with the last piece of the jigsaw puzzle and enabled me to make a convincing and essentially unarguable case for the validity of the Vine-Matthews hypothesis".[495] Retrospectively, Vine claimed to immediately recognize that the revised timescale would allow for a model of constant seafloor spreading to fit the magnetic profile of the Juan de Fuca Ridge.[496] He said, "to me, at that instant, it was all over, bar the shouting".[497] Before addressing Vine's 1966 work that integrated the updated reversal timescale into magnetic profile modeling, some attention needs to be directed to concurrent work taking place at Lamont by Neil Opdyke and Walter Pitman.

Neil Opdyke took a position at Lamont in 1964.[498] Director, Maurice Ewing, wanted to develop paleomagnetic research at Lamont, and Opdyke was an accomplished terrestrial paleomagnetist. He previously worked with Keith Runcorn on the use of paleowind directions to corroborate inferences derived from paleomagnetism.[499] Ewing suggested that Opdyke study the paleomagnetism of deep-sea sediment cores that were often obtained during Lamont's expeditions. At first, Opdyke was not interested. Seafloor sediments did not seem to be a promising research direction for a paleomagnetist interested in mobilism. A central challenge was that seafloor sediments were often loosely consolidated and fragile, with very weak magnetic properties. Still, sparse previous work on paleomagnetism of

[494] McDougall and Chamalaun, 1966
[495] Vine, 2018, 61
[496] Vine, National Life Stories
[497] Vine, 2018, 61
[498] Opdyke was born in New Jersey in 1933 (Glen, 1982, 324-325). He was raised by a single mother, his grandparents, and his uncle. Opdyke studied geology at Columbia University where he met Keith Runcorn while visiting. Opdyke joined Runcorn as a summer student, engaged in fieldwork in Arizona. Runcorn then assisted in Opdyke's admission to Cambridge, where he completed a PhD in 1958.
[499] Opdyke also worked with Ted Irving at ANU in 1959 (Glen, 1982, 325).

seafloor sediments had identified variations in remanent magnetism and reversals.[500] A graduate student at Lamont constructed a spinner magnetometer that became functional in 1965 and soon measured magnetic reversals within the core column.[501] This spurred Opdyke to examine the large collection of cores at Lamont in more detail.

By measuring the reversal patterns within sediment cores and correlating patterns across cores of varying thickness from different locations, Opdyke could establish relative dating and duration of geomagnetic reversals. Terrestrial work on the reversal timescale at USGS and ANU faced notable difficulties and ambiguities associated with historical inconsistency in the deposition of rock that could be paleomagnetically and radiometrically measured. Both groups were well aware that such inconsistency could result in gaps in the available data, thereby contributing to interpretative ambiguities in the construction of the reversal timescale. Opdyke's work on seafloor sediments seemed to avoid this difficulty. He would cut a core into sections and measure the polarity of each piece. Polarity changes through the length of the core could then be identified. Opdyke found that the pattern of polarity reversals through the length of a core, corresponded with patterns obtained from other cores of varying depths and locations. Additionally, these patterns corresponded with the reversal timescale constructed from terrestrial studies. Opdyke thereby inferred that seafloor sedimentation is not as vulnerable to gaps in deposition and associated interpretative ambiguities as are terrestrial studies. Though his approach faced other challenges, like the lack of direct radiometric dating,[502] or inconsistency in sedimentation rates resulting in ambiguities when inferring relative dating or duration, the lack of gaps in deposition allowed Opdyke to extrapolate beyond terrestrial timescales and identify new reversal events.

Walter Pitman[503] joined Lamont as a technician in 1960 and became a graduate student the following year. In 1965, Pitman participated in an expedition aboard the Lamont ship *Eltanin* and converted magnetometer data into magnetic profiles at the end of the year. Pitman quickly noticed strong bilateral symmetry about the ridge and clear correlation between profiles of the Pacific-Antarctic Ridge and the Juan de Fuca.

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[500] Keen, 1960; Harrison and Funnell, 1964

[501] Glen, 1982, 326-327

[502] Though, Opdyke could establish absolute dates based on fossils.

[503] Pitman was born in Newark, New Jersey in 1931 (Coakley, Cande and LaBrecque, 2020). He graduated from Lehigh University in 1956, studying electrical engineering. He then worked with the Hazeltine Corporation, a defense electronics company, before joining Lamont as a graduate student where he studied magnetic anomalies.

I pinned up all the profiles of Eltanin 19, 20, and 21, on Opdyke's door and went home for a bit of rest. When I came back the guy was just beside himself! He knew that we'd proved seafloor spreading! It was the first time that you could see the total similarity between the profiles – the correlation, anomaly by anomaly. The bilateral symmetry of Eltanin 19 was the absolute crucial thing. Once Opdyke saw that he said, "That's it – you've got it!"[504]

Based on the Eltanin 19 profile, Opdyke inferred the existence of reversal events in the Gilbert epoch.[505][506] Opdyke was able to corroborate these reversals by measuring cores taken during the *Eltanin* cruise. He recalled, "it was one of the most thrilling experiences that I have ever had in science. We were overjoyed and totally convinced that the observations had proved that the magnetic anomalies were accurate records of the reversals of the field".[507] Pitman later claimed, "that was the first confirmation we had of our sequence. Until then, we'd been showing that our pattern matched someone else's. This time, it was the other way around."[508]

Vine visited Lamont in early 1966 and Pitman showed him the Eltanin profiles. Vine also met with Opdyke, who was working on his first paper on seafloor sediment cores. Vine had learned of the USGS group's revision of the Brunhes-Matuyama boundary and the Jaramillo event only a few months before, and Opdyke was unaware of the ongoing revisions at the USGS at this time. From his work on seafloor sediments, Opdyke had identified a short reversal event, immediately preceding the Brunhes-Matuyama boundary which he called the *Emperor Event*. Opdyke informed Vine of this discovery.

Vine recounted what followed to Frankel:

Neil said, "Look, Fred, fantastic we just discovered a new event. We call it such-and-such event." I said, "Oh, yes, I hate to tell you this Neil, but Cox, Doell and Dalrymple have discovered that event and they have named it and presented it." He was just astounded. And I said, "Yes, Neil it is called the Jaramillo. Moreover, here it is on the Eltanin-19

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504 Glen, 1982, 334-335
505 Opdyke, 1985
506 Note that the Eltanin-19 profile was just one of many profiles obtained by the *Eltanin*. It was deemed to be the least distorted profile, but this assumption was based on alignment with modeled expectations. Home Le Grande notes that much data in marine geology, including the magnetic profiles that turned out to be so important, were subject to manipulation and selection in order to align with expectation (Le Grande, 1988).
507 Opdyke, 1985, 1182
508 Wertenbaker, 1974, 205-206

profile." They [Opdyke and Pitman] both looked at Eltanin and looked back at me. They said, "My God!"[509]

The Eltanin profiles were presented at the AGU meeting in Washington D.C. in April 1966. Cox recalled seeing the Eltanin 19 profile as, "a truly extraordinary experience".[510][511]

The Eltanin 19 profile really has everything on it that we found in all our work on reversals. When it came out, it had things I knew were there and things I thought were probably there, including short polarity intervals slightly older than the Olduvai event. The potassium-argon dates at the beginning of the Olduvai event are more inconsistent than they should be, in view of what we know of the dating errors. This led me to suspect that one or more short events, slightly older than the Olduvai, were fuzzing up the boundary. Eltanin 19 shows a big event, the Olduvai, and then slightly older than that on both flanks of the Rise, two little blips come in – I think they're both real. I said so in an article shortly afterward. There was so much happening all at once. That was the most exciting year of my life because in 1966, there was just no question any more that the seafloor spreading idea was right.[512]

Opdyke recalled:

I saw the stunned look on Dick Doell's face; he was sitting in the lab outside my office that April in 1966 when we gave those talks in Washington. Doell looked at the magnetic stratigraphy in the cores and at the Eltanin 19 profiles and said, 'It's so good it can't possible be true, but it is'.[513]

Opdyke's paper on seafloor sediment cores was published in October 1966.[514] Opdyke emphasized the correspondence between the USGS reversal timescale and the reversal record in seafloor sediments. He also estimated sedimentation rates based on variations in depths of the reversal pattern, and thereby claimed that locations with higher sedimentation rates will provide more detailed insights into short reversals, while locations of lower sedimentation rates will extend the reversal

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[509] Frankel, 2012, Volume IV, 376
[510] Glen, 1982, 337
[511] On Menard's experience seeing the Eltanin-19 profile for the first time, Pitman claimed that Menard "sat and looked at Eltanin 19, didn't say anything, just looked and looked and looked" (Wertenbaker, 1974, 218).
[512] Glen, 1982, 339
[513] Glen, 1982, 339
[514] Opdyke, et al., 1966

timescale beyond the Gilbert epoch. Pitman's paper on the Eltanin profiles was published in December.[515] He presented the profiles in a manner similar to that found in Vine and Wilson's 1965 work on the Juan de Fuca Ridge. Pitman presented a mirror image of the Eltanin 19 profile to demonstrate symmetry about the ridge axis. He also produced a model profile based on the USGS 1966 timescale with an assumed seafloor spreading rate of 4.5 cm/yr, illustrating the striking correspondence between this model and the Eltanin 19 profile. The USGS timescale extended only to about 4 million years, so Pitman extrapolated a reversal timescale back to 10 million years, based on the Eltanin 19 profile. Pitman then applied this extrapolated timescale to model a profile of the Reykjanes Ridge assuming a spreading rate of 1cm/yr, which he then compared to measured profiles, again showing striking correspondence.

Later that same month, Vine published a review of new evidence.[516][517] When the Vine-Matthews hypothesis was first proposed in 1963, several serious difficulties were readily apparent. Vine began his review by ameliorating some of these difficulties. In 1963, magnetic anomalies in the Northeast Pacific were not known to be associated with an oceanic ridge and, though anomalies typically formed linear patterns, there was no clear instance of linearity and symmetry about a ridge axis. Following the work of Vine and Wilson (as well as those at Lamont) ridges were identified in the Northeast Pacific. The magnetic anomalies in this region paralleled ridges and showed symmetry about the ridge crest. The same can also be said of a detailed magnetic survey of the Reykjanes Ridge, completed in 1963. Vine then compared magnetic profiles about the Reykjanes Ridge to modeled profiles based on the 1966 reversal timescale of the USGS group. Assuming a spread rate of 1 cm/yr per limb of the Reykjanes Ridge, the USGS timescale matched the Reykjanes profile. Vine took the Reykjanes Ridge as a relatively clean case of seafloor spreading, resulting in mobilism. Radiometric dating of igneous formations in Scotland and Greenland that Vine considered to be representative of the initiation of relative continental displacement yielded an age of 60 million years, exactly agreeing with the spreading rate inferred from the magnetic profile.

The modeled spread rate was 2.9 cm/yr for the Juan De Fuca Ridge, 4.4 cm/yr for the East Pacific Rise, 1.5 cm /yr for the Carlsberg Ridge in the Northwest Indian Ocean, and 1.5 cm/yr for the Mid-Atlantic Ridge in the South Atlantic. Vine provided mirror images of the Juan De Fuca and the East

[515] Pitman and Heirtzler, 1966
[516] Vine, 1966
[517] Frankel identifies Vine's paper as the point at which the Vine-Matthews hypothesis became difficulty-free (Frankel, 2012).

Pacific Rise profiles to illustrate their symmetry. Based on the Eltanin 19 profile, Vine suggested that the Mammoth event may be more complex than previously presented. He also showed that the width of anomalies is greater in the South Atlantic than at the Reykjanes Ridge, noting that this aligned with Backus' expectations in 1964. Having shown the correlation between magnetic profiles and models based on the USGS reversal timescale, Vine then assumed a constant rate of seafloor spreading and extrapolated a reversal timescale to 11.5 million years.

Vine also addressed the objection that researchers at Lamont had previously emphasized. Anomalies nearest the ridge crest tend to have lower amplitude and wavelength than anomalies farther from the ridge. Researchers at Lamont previously argued that this difference in character was indicative of a difference in origin. Vine claimed historical variation in the geomagnetic field could account for these differences in the character of magnetic anomalies. If field reversals were more frequent in the recent past than the distant past, then this would result in narrower blocks of differentially magnetized seafloor closer to the ridge. Sufficiently narrow blocks of this sort my not be individually measurable and may thereby tend to lower the overall amplitude of the magnetic anomalies. If changes in the character of magnetic anomalies were, in fact, due to such historical changes in the frequency of reversal events, then it could be expected that the change in anomalies would take place at alternative distances from alternative ridges due to differential seafloor spreading rates. More precisely, anomalies adjacent to slower spreading ridges should exhibit the change in anomaly character nearer to the ridge than anomalies adjacent to faster spreading ridges. Vine claimed that currently available data supported this interpretation.

In addition to these major arguments, Vine also found that slight deviations from constant spreading rates in the East Pacific Rise and the Juan De Fuca Ridge were exactly analogous. He proposed that the Mammoth event may include a period of normal polarity. This was confirmed by McDougall and Chamalaun. He also showed that the magnetic anomalies over the red sea could be modeled, supporting the notion that the red sea is the location of a young but active ridge. He also offered an interpretation of complex seafloor spreading history in the Northeast Pacific. Finally, Vine noted that anomaly patterns can be used to distinguish between active and inactive ridges and identified an inactive ridge in the Labrador Sea.[518]

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[518] The presence of such a ridge was previously subject to some debate due to the lack of seismic activity. The continental fit paper by Bullard, Everett, and Smith (which will be introduced soon) indicated that the continental shelves of North America and Greenland fit together well (Bullard, Everett and Smith, 1965).

The following month, in December 1966, McDougall published an updated reversal timescale.[519] This timescale included a newly identified reversal event shortly after the Mammoth, during the Gauss epoch, as anticipated by Vine's reading of the Eltanin 19 profile. Additional reversals events in the Gilbert epoch, identifiable in the Eltanin 19 profile were later confirmed by Opdyke in sediment cores and Cox and Dalrymple in terrestrial lava flows.[520] Cox later compared reversal timescales derived from terrestrial studies, seafloor sediment studies, and magnetic profile analysis.[521] He claimed that the three methods very strongly agree on the reversal timescale over the past 4 million years, though reversal events of short duration may not be readily identifiable. Opdyke's continued work on marine sediment cores allowed him to identify older reversal events and thereby corroborate extended reversal timescales established from magnetic profiles.[522] Dating was precarious at times since extrapolations via seafloor spreading assumed constant spreading rate or calibration against some constant standard and dating from sediments assumed constant sedimentation rates or calibration against fossil dating. Still, the correspondence even across 25 million years was apparent.

In Wilson's 1965 paper on transform faults, he appealed to the work of Lamont graduate student Lynn Sykes[523] who had isolated shallow earthquakes in the Pacific to ridge offsets. In subsequent work, Sykes analyzed first-motion data gathered from the World-Wide Standardized Seismography Network.[524] In 1967, Sykes found that fault motion at ridge offsets agreed with Wilson's prediction. Less than 1% of the data used by Sykes was inconsistent with inferred first-motions. Thus, active spreading regions could be distinguished not only by the pattern of magnetic anomalies but also through seismic activity.[525]

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519 McDougall and Chamalaun, 1966.

520 Hays and Opdyke, 1967; Cox and Dalrymple, 1967

521 Cox, 1969

522 Opdyke, 1972

523 Sykes was born in 1937 in Pittsburgh, Pennsylvania (Sykes, 1996). He entered MIT in 1955, and graduated with a B.S. and M.S. in 1960, studying geophysics. Thereafter, he pursued a PhD at Lamont, where he studied surface waves caused by shallow earthquakes. He obtained his PhD in 1965 and took a position at Lamont.

524 Sykes, 1967

525 Frankel claims that Wilson's transform fault became a difficulty-free solution following confirmation by Sykes (Frankel, 2012). No serious objections to the transform fault hypothesis were raised after 1967. For Frankel, the Vine-Matthews hypothesis and the transform fault hypothesis were corollaries of seafloor spreading. Though Frankel isn't explicitly clear on the matter, he seems to think that the difficulty-free status obtained by these two hypotheses also pertained to the seafloor spreading hypothesis.

Rachel and Larry Laudan identify the Vine-Matthews hypothesis and transform faults as two confirmed novel predictions that had significant influence upon perceived epistemic standing of mobilism during the 1960s (Laudan and Laudan, 1989).

Also in 1967, Jason Morgan at Princeton,[526] extended Wilson's concept of rigid crustal plates and transform faults to a spherical surface.[527][528] As Backus had previously inferred with respect to relative thickness of marine magnetic anomalies, the relative motion of rigid plates[529] on a spherical surface can be described as a rotation about a pole, such that the relative displacement of the blocks is greatest at the equatorial region of rotation and decreases nearer the poles. Two parameters are required to locate this pole of relative rotation, and one parameter is required to specify the magnitude of angular velocity. Taking Wilson's delineation of mobile belts, Morgan divided the earth's crust into about twenty large blocks. He then set out to describe the motion between the African and South American blocks. Assuming, as Wilson had argued, that plate motion is parallel to the strike of transform faults, Morgan identified great circles running perpendicular to the strike of ridge offsets, determined bathymetrically, along the Mid-Atlantic Ridge. He identified the pole of rotation at the intersection of these great circles,[530] and cross-checked this pole position against the seismic data obtained by Sykes. Having identified the pole of rotation, Morgan then calculated a model spreading rate for the Mid-Atlantic Ridge, wherein spreading would be least near the pole of rotation and most near the equatorial region of that pole. He compared this model to measured spreading rates of the Mid-Atlantic Ridge determined by collections of magnetic profiles calibrated against Vine's 1966 timescale, finding strong agreement. Morgan then examined the motion of the Pacific block relative to the North American block, and the Antarctic block relative to the Pacific block. Having established these poles of rotations and their agreement with measured seafloor spreading rates, Morgan was able to derive the pole of rotation and relative motion of the Antarctic block relative to the African block. At the time, no magnetic profiles clearly showed spreading to support this relative motion. Accordingly, Morgan looked to the spreading rate of the Carlsberg Ridge and the Mid-Indian Ocean Rise (now the Southeast Indian Ridge) located between Antarctica and Australia to infer the seafloor spreading rate between Africa and Antarctica.[531] Morgan claimed that the agreement of his models with measurements of seafloor spreading supported

[526] Morgan was born in 1935 in Savannah, Georgia (Frankel, 2018). He graduated from Georgia Institute of Technology in 1957 and thereafter spent two years in the U.S. Navy. He then joined Princeton's geosciences department where he obtained a PhD in geophysics in 1964. He took a postdoc position in the Department of Geology at Princeton under Walter Elsasser. Hess was at Princeton. Vine visited Princeton in 1965 and shared an office with Morgan.
[527] Morgan, 1968
[528] Morgan presented the core idea of applying Wilson's transform faults to a spherical surface in 1967, but his first paper on the subject was published in 1968.
[529] Wilson used the term "plate" while Morgan used the term "block".
[530] The great circles would be analogous to meridians and the strike of fault lines would be analogous to lines of latitude for such pole positions.
[531] Also, see Morgan and Johnson, 1970

the assumed rigidity of crustal blocks. Such rigidity implied that upper portions of the Earth's mantle and the Earth's crust acted as a single rigid unit. He also argued that ridges may not be created by the upwelling of convection currents. Instead, whatever forces might contribute to relative crustal displacements may split a block along some line of weakness and this fracture will then be filled with mantle materials.

In 1967, Dan McKenzie[532] and Robert Parker at Scripps independently developed similar ideas to Morgan.[533] They also applied Wilson's delineation of mobile belts to a spherical surface.[534] McKenzie noted that transform faults are always parallel to the relative velocity vector between two plates. They also emphasized triple junctions, locations where three plates meet. If plates are rigid, when velocity vectors of two plates are known, the third can be calculated (as Morgan did to infer the motion of Antarctica relative to Africa). McKenzie and Parker used seismic data to infer rotational pole positions of

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[532] McKenzie was born in Cheltenham England in 1942 (McKenzie, 2007). He attended King's College, Cambridge where he studied physics, chemistry, and geology. In his final year he studied geophysics, working under Hill, Matthews, and Bullard. He began graduate work in geophysics under Bullard in 1963. At first, he studied seismic velocities in the mantle, and then mantle viscosity in general. During his graduate work, McKenzie spent several months at Scripps. He received his PhD in 1966. He then took a fellowship at Caltech, then went back to Scripps for six moths, and then back to Cambridge in 1968 where he stayed.

[533] McKenzie and Parker, 1967

[534] Morgan, McKenzie, and Parker relied on Euler's fixed point theorem, which states that in three-dimensional space, every rotation has an axis wherein a point on the rotating body remains fixed. When applied to a sphere, any displacement of a rigid body upon on the surface of a sphere can be described as a rotation about some axis. Morgan, McKenzie, and Parker were not the first to apply spherical geometry to interpretations of motions with bearing on seafloor spreading or mobilism. In 1964, Backus noted that the rate of seafloor spreading in the North Atlantic should be less than the rate of seafloor spreading in the South Atlantic (Backus, 1964). Also, in 1965, Bullard, at Cambridge, along with Jim Everett and Alan Smith published an influential paper on the fit of continents around the Atlantic (Bullard, Everett and Smith, 1965). Wegener, Du Toit, and Carey had previously emphasized the remarkable fit between the coastlines or continental shelves of continents, especially South America and Africa, but a common refrain from prominent fixists like Harold Jeffries was that this supposed fit was highly exaggerated. Bullard, Everett, and Smith thereby aimed to identify the best fit mathematically, and then compare this fit to other reconstructions and known disjuncts. For this task, they digitized geometrical data of the continental shelves of South America and Africa, and North America, Greenland, and Europe. They then used the EDSAC 2 computer to identify the best fit using a least-squares method. Euler's theorem was used to describe the motion of the continents on a sphere, and thereby establish the pole of rotation that gave the least misfit between continental shelves. The best fit between South America and Africa contained slight overlaps in some areas like the Niger Delta but was otherwise remarkably close. Iceland and the Faroes Ridge were known to be Cenozoic structures, and removed from the best fit of North America, Greenland, and Europe. A best fit across the entire Atlantic Ocean was also provided, but Central America, Mexico, the Gulf of Mexico, The Caribbean Sea, and the West Indies were not included. Additionally, the Iberian peninsula significantly overlapped with Africa and was thereby rotated and shifted. Bullard, Everett, and Smith noted that the best fit also seemed to align with disjuncts identified by Du Toit and others, and also argued, "if the continents were once joined, then not only the shapes but the ages, structures and petrology of the rocks must match across the joins; if they do, the probability that the fits are due to chance is negligible. The importance of the geometrical fits is that they position the continental blocks with an accuracy of the order of a degree and leave little room for adjustments to fit other evidence" (Bullard, 1965, 50).

the Pacific plate relative to North America. Having established this pole position, they noted that all slip vectors along the margin of the Pacific and North America must be parallel to lines of latitude about this pole. They then compared this prediction to broader seismic data from the World-Wide Standardized Seismograph Network, finding agreement in 80% of data. Areas of disagreement in their data resulted in McKenzie and Parker proposing the existence of a small plate in the Northeast Pacific bounded by the Juan de Fuca and Gorda Ridges and a hypothesized subduction zone along the coast of Oregon.[535] This would account for deviant seismic data in the area, as there would not be a single pole of rotation if a third plate were involved.

McKenzie later wrote:

> *What made plate tectonics so immediately convincing was that it was principally designed to account for sea floor spreading, continental drift, and magnetic anomalies. With no further input, it also accounted for the distribution of earthquakes, which in the oceans lie in narrow bands on plate boundaries.*[536]

Also, like Morgan, McKenzie argued that ridges need not have deep structures.[537] Available heat flow data could be accounted by excess heat produced by upwelling mantle material filling fissures caused by seafloor spreading, rather than upwelling convection currents.

In 1967, Xavier Le Pichon,[538] at Lamont, elaborated upon Morgan's work.[539][540] Le Pichon developed a simplified model of the earth's crust, consisting of six rigid blocks, and he identified the poles of rotation for the opening of the South Pacific, Atlantic, Arctic, North Pacific, and Indian oceans. With parameters of poles of rotation obtained by magnetic profile models and the strike of seismically active ridge offsets, Le Pichon showed that his global model was geometrically consistent. He emphasized that the global pattern of block motions was interrelated. Seafloor spreading at one mid-ocean ridge was the sum of spreading at other ridges. Having established the internal geometrical consistency of seafloor spreading within his model, Le Pichon then used this model to infer historical

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[535] This area is now called the Cascadia Subduction Zone.
[536] McKenzie, 2018, 186
[537] McKenzie, 1967
[538] Le Pichon was born in 1937 in French Vietnam (Le Pichon, 1998). His family moved to Cherbourg, France following WWII. He began graduate studies at Lamont in 1959 on a Fullbright fellowship. In 1960 he returned to France, where he served in the Navy for two years. He returned to Lamont in 1963, studying the Mid-Atlantic Ridge.
[539] Le Pichon, 1968
[540] His paper was published in 1968.

continental displacements into the Paleocene. He argued that convection currents were an implausible explanation for seafloor spreading.[541] Afterall, seafloor spreading rates diminish predictably toward poles of rotation. If seafloor spreading were the result of internal convection currents carrying the crust above, then this would result in the onerous requirement that internal convection currents also diminish near the poles of rotation. Like Morgan, Le Pichon endorsed the notion that seafloor spreading is the result of a thick lithosphere that breaks apart along lines of weakness.[542]

Le Pichon later recalled:

> *Even now, it is difficult for me to forget my extraordinary excitement the day I realized that my six plate model worked, and that it could indeed account as a first approximation for the broad geodynamic pattern. I remember coming home early in the morning for breakfast after a night at the computer and telling my wife: 'I have made the discovery of the century.*[543]

By the end of the 1960s, Hess' notion of thin ocean crust riding atop internal convection currents was replaced by a thick lithosphere, capable of supporting crustal rigidity. Internal convection currents were not integrated into these kinematic models. Indeed, internal convection was critiqued as an implausible driver of seafloor spreading. Vine's modeling of magnetic profiles and Wilson's notion of rigid crustal plates moving parallel to the strike of transform faults were used to model the surface of the Earth. These models assumed spherical geometry which could be used to illustrate the internal consistency of seafloor spreading rates and derive the relative motion of alternative plates. Subsequent directions of research pertained to the elaboration of these simplified models by identifying additional plates and integrating additional empirical data.[544] The location and magnitude of plate deformation,

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[541] During the 1950s, convection currents were often recruited to account for phenomena of marine geology. Through much of the 1960s, it was often supposed that convection currents drove seafloor spreading. By the end of the 1960s, however, convection currents were deemed unnecessary in the kinematic theory of plate tectonics.
[542] Frankel claims that the work of Morgan, McKenzie, and Parker offered another difficulty-free solution in the mobilism debate (Frankel, 2012). Subsequent work, including that of Le Pichon, extended and further confirmed plate tectonics. Unlike the difficulty-free solutions of the Vine-Matthews hypothesis and transform faults, plate tectonics entailed mobilism.
[543] Le Pichon, 2018, 216
[544] In 1968, Sykes, along with Bryan Isacks and Jack Oliver at Lamont, integrated global seismology with plate tectonics (Isacks, Oliver and Sykes, 1968). They argued that global seismic activity is concentrated in narrow, continuous belts that bound seismically stable areas. The stable areas may be taken as plates of lithosphere, and the seismically active areas their boundaries. At ridges and transform faults, seismic activity is shallow. At zones of convergence, intermediate and deep activity is sometimes apparent, indicative of underthrusting (recall that Benioff previously described a diagonal trend in earthquake hypocenters adjacent to ocean trenches) (Benioff, 1949). This is most apparent when at least one oceanic plate is involved. Convergence of continental plates are

variations in spreading rates over time, changes to rotational pole positions, and changes in plate boundaries became relevant research questions. Paleogeographic reconstructions of the Earth's surface also became increasingly common, and integrated research on abnormalities and variations in marine magnetic anomalies, updating of marine and terrestrial reversal timescales, and paleomagnetism and paleoclimatology.

Conclusion

In the early 1960s, radiometric dating methods were used to test the hypothesis of geomagnetic reversals by constructing a geomagnetic reversal timescale. This work was spearheaded by researchers at USGS and ANU. Shortly thereafter, yet independently, researchers at Cambridge attempted to account for marine magnetic anomalies by combining the geomagnetic reversal hypothesis with the seafloor spreading hypothesis. Each of these projects combined historically distinct strands of scientific research that were, themselves, uniquely historically contingent and loaded with ambiguities. A series of remarkable successes, relevant to these two projects, took place between 1965 and 1967.

In 1965, Tuzo Wilson hypothesized the existence of a ridge in the Northeast Pacific from a generalization of seafloor spreading and the assumed rigidity of the Earth's crust. The Vine-Matthews hypothesis was then utilized to discover the Juan De Fuca Ridge. Additionally, geomagnetic reversal timescales were used to model magnetic profiles based on the Vine-Matthews hypothesis. The 1966 discovery of the Jaramillo event by paleomagnetic and radiometric study of terrestrial lava flows resulted in a magnetic profile model that agreed with measurements obtained from the Juan de Fuca Ridge, the Pacific Antarctic Ridge, the Mid Atlantic Ridge, the Carlsberg Ridge, the Reykjanes Ridge, and more. Some magnetic profiles were so clean, that they could be used to infer undiscovered reversal events. Paleomagnetic measurements of seafloor sediments corroborated the terrestrial reversal timescale and confirmed extrapolated reversal events from magnetic profiles. By comparing dated magnetic anomalies with their distance from the ridge crest, seafloor spreading rates could be inferred. In 1967, Morgan, McKenzie and Parker, and Le Pichon applied Wilson's notion of transform faults to a

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more complex. First motion analysis indicates the relative motion of plates, and this agrees with inferences from seafloor spreading and transform fault trends as represented by Le Pichon (Le Pichon, 1967). Additionally, the lengths of deep seismic zones, the maximum depth of these zones, and the frequency of large earthquakes and tsunamis and the presence of volcanism also correspond to the rate of underthrusting.

spherical surface. Morgan and Le Pichon showed that seafloor spreading rates inferred by magnetic anomalies were self-coherent when applied to a spherical surface.

Subsequent works showed that spreading centers and inferred historical spreading rates were consistent with paleomagnetic reconstructions of relative continental displacements.[545] Paleopole determinations of radiometrically dated terrestrial rock on opposing sides of a spreading ridge could be used to estimate the absolute age of ocean basins, agreeing with inferred historical spreading rates.[546] Radiometric dating also showed age correlations between geological disjuncts across landmasses separated by spreading ocean basins.[547] The Cascadia Subduction Zone, hypothesized by McKenzie and Parker, accumulated independent support from gravimetric, heat flow, bathymetric, sedimentation deformation, and seismic measurements, but also from analysis of magnetic anomalies in the region and tectonic modeling.[548] The degree of relative motion between Africa and Antarctica, hypothesized by Morgan, agreed with subsequent magnetic profiling about the Southwest Indian Ocean Ridge.[549]

Researchers recognized that something important was taking place in the second half of the 1960s. Prior to 1967 or so, large-scale theories in marine geology were typically recognized to be highly speculative, but around this time, increasing numbers of researchers were definitive in their support of seafloor spreading, transform faults, crustal plates, and mobilism. In 1962, Harry Hess dubbed seafloor spreading “geopoetry”, but by 1968 he wrote that the Vine-Matthews hypothesis had proved seafloor spreading correct.[550] Frederick Vine, who previously recognized the Vine-Matthews hypothesis to be speculative, became convinced that it was correct. Henry Menard, at Scripps, who was opposed to seafloor spreading even into 1966 fully endorsed the idea by the end of the year.[551] Dan McKenzie became convinced of seafloor spreading in November 1966 after attending conference presentations of Vine’s 1966 review paper and Sykes’ paper on transform faults.[552] Despite previously mounting objections to mobilism, Cox and Doell, along with Dalrymple, converted. Cox claimed that this took place

[545] See Heirtzler, et al., 1968.
[546] See Dalrymple, Gromme and White, 1975
[547] See Hurley, et al., 1967
[548] See Riddihough and Hyndman, 1976
[549] See Norton, 1976
[550] Hess, 1968
[551] Menard, 1986
[552] Menard, 1986, 275

in a flurry of exciting research in 1966.[553] Dalrymple later described the experience as an "aha" moment.[554]

Lamont, an institution that was nearly universally opposed to mobilism in the early 1960s, rapidly changed allegiance after the events of 1966. Several researchers recognized the significance of magnetic profiles shortly after seeing the Eltanin 19 profile. Others, like Maurice Ewing, took more time to make their endorsements known.[555]

Le Pichon retrospectively identified 1966 as a turning point.

> *The presentation of the magic profile at the AGU stunned everybody. The 600-mile (1,000 kilometers)-long profile revealed a perfect symmetry with respect to the axis of the mid-ocean ridge crest. Furthermore, it could be interpreted simply and perfectly with the sea floor spreading model, using the Earth magnetic field reversals chronology obtained by the young Lamont paleomagnetic group (led by Neil Opdyke) by measuring the magnetic polarity of oceanic sediment cores. In particular, the magnetic anomaly profile as well as the sediment cores revealed the presence of a new magnetic event that Richard Doell and Brent Dalrymple, at the U.S. Geological Survey (USGS), had just independently identified. They called it the Jaramillo event, a short duration of normal magnetic field. With this new event, the correlations from one ridge crest to the other became evident. Suddenly, the balance of phenomena explained or left unexplained by the sea floor spreading hypothesis appeared positive, and acceptable without serious reservation to any scientist familiar with the whole picture. The massive move toward mobilism was then inevitable.*[556]

In, *Plate Tectonics and Geomagnetic Reversals,* one of the earliest books on the plate tectonics revolution, Cox claimed, "in a remarkable series of articles written between 1962 and 1968… many of the main threads of geological research were brought together to form the fabric of plate tectonics".[557] Cox identified four independent lines of research that came together in the 1960s: mapping of the seafloor, measurements of magnetic anomalies, the geomagnetic reversal timescale, and the capacity to isolate seismic epicenters and hypocenters. Cox expands that, "their coming together to form the

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[553] Glen, 1982, 339
[554] Dalrymple, 2016
[555] See Menard, 1986, 272-273, 287
[556] Le Pichon, 2018, 212
[557] Cox, 1973, 2

observational basis for plate tectonics must surely constitute one of the classic examples of serendipity in the history of science. *It is difficult to imagine a central committee responsible for planning research in tectonics that would have had the imagination to foresee the unlikely path of development of this major scientific advance*".[558]

Robert Dietz later called plate tectonics a "revolution in geology and geophysics", recruiting the terminology of Thomas Kuhn's timely work on scientific change.[559][560]

Making Sense of the Mobilism Debate

Henry Frankel's *The Continental Drift Controversy* is the definitive history of the mobilism debate.[561] At over 2000 pages, this four-volume work is the culmination of over 35 years of research by one of the foremost experts on the history of mobilism. Frankel published extensively on philosophical and historical aspects of mobilism from 1976 until his death in 2019. As his interest in the detailed history of mobilism developed, he engaged in extensive correspondence with many of the surviving scientists who contributed to the debate. This correspondence (along with original interviews and archival work) served as the foundation for his 2012 work, by far the most detailed history of the mobilism debate ever written.

Frankel divides the history of the mobilism debate into three phases. The first phase, beginning with the work of Alfred Wegener (and Frank Taylor), spanned from 1910 to 1950 or so. During this time, fixism reigned in the study of the Earth, and mobilism was endorsed by only a handful of prominent (and

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558 Cox, 1973, 2 (my italics)
559 Dietz, 1977
560 In highlighting the contemporaneous and retrospective recognition of the compelling case for mobilism that had developed by 1967, I do not yet intend to imply any overarching framework or conclusion. Indeed, two cautionary points should be made about this supposedly compelling period. First, researchers tend to be concerned with their own fields of research and their own research projects within that field. Just because evidence is compelling to some researcher or group, it does not mean that this has much bearing on the attitudes of broader communities. Also, researchers are often inclined to situate their own research at the center of retrospective accounts of developing ideas and are sometimes willing to present themselves and their research in a flattering manner. Second, an important feature of the compelling nature of evidence for mobilism by 1967 pertains to the changing convictions of researchers at Lamont. On the face of it, evidence that changes convictions may seem to be more compelling than evidence that reinforces views that are already held. Though typically opposed to mobilism prior to 1966, Lamont researchers also made important contributions to eventual widespread acceptance of mobilism. Historical accounts of the development of plate tectonics, and especially retrospectives written by Lamont alumni, sometimes conflate these two roles, such that Lamont is sometimes portrayed as the institution that convinced the world of mobilism.
561 Frankel, 2012

mainly non-American) researchers. Wegener established the main set of problems that were debated during this time: the fit of continental margins, biotic and geological disjuncts, Permo-Carboniferous glaciation, the origin of tertiary mountain belts, and the physical processes of continental drift. The second phase of the debate began with the maturation of the study of paleomagnetism in the early 1950s, initially in the UK. Paleomagnetists developed rigorous standards to measure and interpret rock magnetism, and by the end of the 1950s, mobilism was an indispensable component of the interpretative framework within the field, especially for researchers outside of the USA. Paleomagnetism rekindled faded interest in mobilism. The third phase began near the middle of the 1950s, with the accumulation of new geophysical data about the seafloor. This data was amenable to both fixist and mobilist interpretation. The seafloor spreading hypothesis spawned two important corollaries, Wilson's transform fault hypothesis and the Vine-Matthews hypothesis. Subsequent confirmation of these corollaries (along with plate tectonics) contributed to widespread endorsement of mobilism toward the end of the 1960s.

In addition to this historical periodization, Frankel also provides an analytic framework with which to structure and interpret this history. For Frankel, science is a problem solving activity. A problem is something that is recognized to need a solution. Such problems may include anomalous observations or sets of unexplained facts. Solutions are designed to solve one problem, while theories may be designed to solve many problems in a common way. Wegener's theory of continental drift, for example, was designed to solve multiple problems. There are two stages to problem solving. The first stage consists of the identification of some problem for which some hypothesis is offered as a solution. Second stage problems pertain to the entities or processes invoked to solve first stage problems, often with respect to causal/mechanical elaboration.

Proposed solutions to problems may confront difficulties. According to Frankel, "difficulties were objections that were raised against these proposed solutions and theories, obstacles that were in their way all along or placed there later by opponents."[562] Frankel categorizes these as data difficulties and theoretical difficulties. Data difficulties consist of unreliable data, anomalous data, and missing data. Theoretic difficulties consist of external incompatibility with non-competing beliefs and internal inconsistencies. Debates over the viability of proposed solutions within a scientific community pertain to the identification and elimination of such difficulties.

[562] Frankel, 2012, Volume I, 5

Problems arose when scientists became puzzled by phenomena they could not explain. Sometimes more data were gathered to establish the legitimacy of a problem and to clarify it. A solution was then offered. Difficulties were usually raised by scientists with opposing views. They were also raised by supporters in the same camp and even by scientists themselves against their own solutions. Difficulties were removed either by amending the flawed solution or theory, or by showing that the raised difficulty was itself unfounded, a phantom difficulty.[563]

Frankel claims that some solutions to problems can, after some consideration, achieve difficulty-free status, at which point, reasonably conceived difficulties are absent, and the problem is deemed to be adequately resolved.[564] Theories associated with difficulty-free solutions may still confront anomalies or inconsistencies, but these will be viewed as distinct problems that can be resolved in turn.[565] According to Frankel, a difficulty-free solution can be identified when those involved in a debate form a consensus that some solution has resolved all reasonably conceived difficulties. He states, "if after scrutiny a solution was acknowledged to be difficulty-free by its opponents, debate ceased and the solution was accepted."[566] Those who may deviate from this consensus are marginalized in subsequent debate.[567]

Frankel identifies four difficulty-free solutions within the mobilist controversy. By the end of the 1950s, mobilism offered a difficulty-free solution to interpretation of paleomagnetic measurements. According to Frankel, the difficulty-free nature of this solution was not widely recognized. Researchers not sufficiently knowledgeable in paleomagnetism were not aware of the difficulty-free status of mobilism and thereby clung to out-dated difficulties that had already been accounted. Three other

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[563] Frankel, 2012, Volume I, 5

[564] Furthermore, "difficulty-free solutions that have not been examined by opponents are given no special status" (Frankel, 2012, Volume I, 14).

[565] "What is to stop defenders of a solution from removing difficulties by fiat, simply reclassifying them as unsolved problems? Nothing! However, once again, there is no guarantee that opponents will agree, thereby continuing the controversy." Frankel continues, "the reclassification of a former difficulty as an unsolved problem is permissible if there are difficulty-free solutions already associated with the theory in question" (Frankel, 2012, Volume I, 15).

[566] Frankel, 2012, Volume I, 13

[567] Even though Frankel delineates the difficulty-free solution by the formation of consensus, he also hints toward rational standards as bearing upon the delineation of difficulty-free status. Frankel claims that researchers may raise unreasonable difficulties. For example, though Frankel claims that mobilist interpretations in paleomagnetism obtained difficulty-free status in the 1950s, several prominent paleomagnetists did not join this consensus, but Frankel claims that the difficulties raised by these holdouts were unreasonable. Throughout his work, Frankel identifies problems, solutions, difficulties, and debates in detail, but he does not offer an explicit generalized account of when or why some difficulties might be reasonably conceived, while others might be deemed unreasonable.

difficulty-free solutions were reached between 1966 and 1967. The Vine-Matthews hypothesis became difficulty-free in 1966, following several years of debate. The transform fault hypothesis became difficulty-free in 1967, following seismic measurements of Sykes. Plate tectonics was immediately difficulty-free upon its initial formulation by Morgan, McKenzie, and Parker in 1967. These difficulty-free solutions contributed to the growing support for mobilism and resolution of the mobilism debate.

During the mobilism controversy, alternative research strategies were employed in order to improve the relative problem solving capacity of one proposed solution over a competitor. Frankel identifies three of these strategies. The first strategy was to expand the problem solving effectiveness of a solution. Often, this consisted of overcoming difficulties in a general effort to reach a difficulty-free solution. Contrary data could be challenged as unreliable, anomalies could be accounted with slight modifications to a proposed solution, new problem solving capacities of a proposed solution might be identified, and the relation to accepted background knowledge may be strengthened. The second research strategy was to decrease the problem solving effectiveness of a competing solution. This strategy consisted of identifying difficulties confronting opposing solutions. The final research strategy was to compare the problem solving effectiveness of competing solutions. Frankel claims that this was done to highlight how one solution could solve a difficulty that confronted a competing solution, or to illustrate that one solution solved a broader collection of problems than a competing solution. Throughout his historical account, Frankel identifies the role of these research strategies in structuring the mobilism debate.

To be a researcher in Earth science, especially in the second and third stages of this controversy, required a great amount of specialized training in field work, instrumentation, and interpretation. Researchers tended to be most interested in research within their own specialty. Additionally, Earth science research was often regionally focused. Field work was often time consuming and expensive and was therefore often completed locally. Frankel claims that geologists, in particular, often distrusted reports from geographical regions outside their own region of familiarity since fieldwork could often be interpreted in multiple ways and at multiple levels. Thus, Earth science consisted of numerous subgroups, structured by unique peculiarities of objects of study, methods and standards, training and education practices, and even regional physical history.

According to Frankel, research strategies my have differential impact across these subgroups. Specialists are better able to evaluate the legitimacy of difficulties than are non-specialists who may not be as readily swayed by debates outside of their own specialty. Non-specialists may also consider illegitimate difficulties to be more substantial than they actually are and may be more inclined to accept or reject research outside of their home specialty based on its alignment with their own preconceptions. Frankel applies this lesson to the tepid reception of mobilism outside of paleomagnetism in the 1950s. He also claims that some subgroups, such as paleoclimatologists of the southern hemisphere, were disposed to mobilism quite early, and the strongest proponents of mobilism prior to the late 1960s were generalists who examined global data sets and sought global explanations.

Frankel's account of the numerous factors that contribute to the delineation and evolution of subgroups within a scientific controversy bears some resemblance to the work of Naomi Oreskes in *The Rejection of Continental Drift*.[568] Taking a broader interest in the social settings of the mobilism debate, Oreskes argues that changes in scientific methods - motivated by diverse pragmatic concerns including, but not limited to, access to funding, prestige, authority, and research fruitfulness and ease - result in changing notions of what is and is not scientific. She claims that American geologists in the early 20th century endorsed Baconian induction, emphasizing field work and plurality of hypotheses which aligned with values of American democracy and the protestant work ethic. This methodological tradition, along with traditions of uniformitarianism and Pratt isostasy, conflicted with early arguments for continental drift, resulting in the rejection of continental drift as unscientific among American researchers. These methodological traditions were gradually undone and replaced by standards that privileged the objectified, quantified, geophysical measurements that offered support to mobilism in the 1960s, thereby resulting in the uptake of mobilism by American researchers. This was driven by pragmatic decisions and increased utilization of new measurement methods that opened prospects of new theoretical interpretations. Like Frankel, Oreskes maintains that diverse socially embedded factors constrain scientific decision making, but Oreskes' account mainly focuses on developments within the subgroup of American geology.[569]

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[568] Oreskes, 1999

[569] In *The Ocean of Truth*, Henry Menard offers a first-hand account of developments in marine geology leading up to the end of the 1960s and inquires why the chief marine geologists of the 1950s weren't centrally involved in the major developments of the 1960s (Menard, 1986, 297-298). Menard argues that data gathering expeditions were the mainstay of the chief marine geologists of the 1950s. Expeditions were the basis for publications, tenure, and research grants. The largest centers of marine geology had fleets of research vessels. Graduate students were required to participate in these expeditions and master the instruments and challenges involved in fieldwork at sea. Expeditions were also central to the marine geologists' adventurous attitude. Yet, Menard argues, this centrality of the expedition in the life of marine geologists in the 1950s promoted data acquisition and inhibited large scale theoretical speculations. For another similar account to Oreskes, see Pellegrini, 2019.

According to Oreskes, the mobilism debate is largely a story of American opposition and eventual conversion to mobilism. Europeans, she claims, viewed plate tectonics as a "pleasing confirmation of a long-suspected notion".[570] Likewise, in *Earth's Deep History* (2014), Martin Rudwick claims that the greatest contrast in the mobilism debate can be established between the United States and the rest of the scientific world.[571] As noted in the introduction to this work, prior to the 1950s, many prominent American researchers could be listed as opponents to mobilism, while very few could be listed as proponents. Even into the 1950s and 1960s, the strongest opposition to mobilism often came from American researchers. While most paleomagnetists in the UK endorsed mobilism by the end of the 1950s, several notable Americans objected. Additionally, The Lamont Geological Observatory at Columbia University in New York was a stronghold for mobilist opposition in marine geology well into the 1960s. Though it is surely the case that American researchers in general were less sympathetic to mobilism than the rest of the world, several important historical facts should not be overlooked. Several long-time influential opponents to mobilism, including Harold Jeffreys, were European. Much of the research that contributed to the surging support for mobilism in the second half of the 1960s involved collaboration between American and non-American researchers. Perhaps most importantly, mobilism was not widely endorsed outside of the USA by the end of the 1940s. Throughout *The Continental Drift Controversy*, Frankel thoroughly examines the shifting dispositions toward drift of the many researchers involved in the mobilism debate. Moreso than Americans, non-American researchers may have had greater exposure to mobilism during their education and professional work leading into the 1950s and 1960s, but this exposure was typically quite limited. Mobilist ideas were not centrally involved in the formal education or training of core researchers. Additionally, the use of mobilist ideas in professional research contexts only began near the middle of the 1950s in paleomagnetism, and somewhat later in marine geology. Thus, increased conviction in mobilism during the 1950s and 1960s was not just an American phenomenon.

Oreskes also emphasizes the role of geophysics in the eventual acceptance of mobilism. According to Oreskes, the shift in acceptance of mobilism was largely attributable to the rise of geophysics during the 20th century, which contributed to methodological shifts across the earth sciences, including an increased emphasis upon laboratory work, instrumentation, hypothetico-deductivism, and large-scale theorizing. Though it is surely the case that geophysical research was

[570] Oreskes, 1999, vii
[571] Rudwick, 2014

important to the resurgent interest in mobilism in the 1950s and 1960s, the relative importance of geophysics as compared to geology or paleontology over the duration of the mobilism debate is more equivocal. The mobilism debate was sparked by both geophysical and geological arguments. Geophysical and geological data challenged central assumptions of contractionism, while permanentism's great weakness was in its incapacity to account for certain geological and paleontological patterns across continents. According to Frankel, Wegener's geological and paleontological evidence was likely his best, while some of the strongest early arguments against mobilism pertained to Wegener's description of physical processes. As noted in the introduction of this work, due to multiply interpretable geological and paleontological evience, support for mobilism waned in the 1930s and 1940s, even as accounts of physical processes of mobilism improved. Geophysics was surely important to the post-war revival of the mobilism debate, but the relevance of geology and paleontology in this revival should not be discounted. Paleomagnetic research was predicated upon traditional geological dating methods. Research in marine geology during the 1950s called into question the capacity for geological and paleontological evidence to be accommodated within a fixist framework (see Chapter 7 for elaboration). During the 1960s, seafloor spreading was associated with mobilism largely because of the old geological and biotic evidence emphasized by Wegener, Du Toit, and Carey. Geological and paleontological dating methods were also indispensable to much of the research centrally involved to the surging support for mobilism in the second half of the 1960s.

Histories of the mobilism debate often account for why mobilism languished for decades prior to the 1960s. Several of these accounts celebrate the tectonics revolution as an episode of rational change in the history of science. Some argue that mobilism should have been accepted sooner, but biases of established experts[572] or cultural barriers[573] within scientific communities inhibited uptake. Others argue that acceptance of mobilism only became rational in the 1960s. This is often attributed in some way to new bodies of data obtained in the 1950s and 1960s. Sometimes such data is presented as a straightforward impetus of theory change, or a neutral arbiter of the dispute that accumulates unproblematically.[574] Alternatively, frameworks related to Larry Laudan's research traditions are often

[572] In *A Revolution in the Earth Sciences,* Anthony Hallam claims that opposition to continental drift by T.C. Chamberlin was representative of conservative prejudice against Wegener (Hallam, 1973, 131). Robert Newman adds that other leading American geologists were prejudiced against the ideas of foreigners (Newman, 1995).
[573] Ted Nield points to the disruptive influence of WWI especially to German science, the interdisciplinarity of Wegener's work, and the culture of multiple hypotheses in American science as inhibiting uptake of continental drift earlier in the 20th century (Nield, 2015).
[574] This sort of approach to the historical rise of mobilism is most common in brief histories offered in textbooks or works written for popular consumption.

endorsed by historians of science to account for the rational uptake of mobilism in the 1960s.[575] In Laudan's framework, data is not a straightforward impetus of theory change, nor is data neutral.[576] Rather, a rational choice can sometimes be made between alternative sets of commitments based on relative problem solving capacities. Even those historians who don't clearly fall under Laudan's umbrella often emphasize that there is much more to scientific decision making than consideration of data. Debates and sub-debates pertaining to mobilism were not only influenced by data, but also by a variety of other commitments that shifted over time. Thus, Oreskes emphasizes pragmatic methodological shifts resulting in changing views of good science, and Frankel emphasizes the importance of theoretical difficulties and consilience and highlights shifting problem sets and strategies over the course of the debate.[577] For example, a major difficulty confronting Wegener's account of continental drift was that he lacked a plausible mechanism, yet the eventual uptake of mobilism in the 1960s was not due to the discovery of an adequate mechanism. Plate tectonics was a kinematic theory.

Building on Frankel

In an effort to describe scientific knowledge as both historically contingent and epistemically reliable, historians and philosophers often endorse relativistic frameworks. In the case of the mobilism debate, such relativistic accounts make no distinction between the epistemic standing of mobilism at the end of the 1960s and the epistemic standing of fixism at the beginning of the 20th century.[578] There is, however, good reason to suppose that by the end of the 1960s mobilism occupied an epistemic position that was never attained by fixism. In the second half of the 1960s, support for mobilism expanded rapidly. Tentative defenders of mobilism, like Hess, Wilson, and Vine became more strongly

[575] Rachel Laudan argues that it was rational to entertain but not to pursue mobilism prior to the middle of the 1950s (Laudan, 1987). To this point, mobilism's problem solving capacity was stagnant. After this point, however, pursuit of mobilism became rational as problem solving capacity improved relative to rival traditions. Rachel Laudan and Larry Laudan argue that Wilson's transform faults and the Vine-Matthews hypothesis yielded the first confirmed novel predictions entailed by continental drift, and that such confirmed novel prediction convinced opponents of drift (Laudan and Laudan, 1989). In *Drifting Continents and Shifting Theories*, Le Grand claims that the problem solving capacity of drift improved in the 1960s, but also emphasizes gradual rather than rapid changes, regionalism, and hierarchies between such subgroups (Le Grand, 1988).
[576] Laudan, 1977
[577] Oreskes, 1999; Frankel, 2012
[578] Laudan's approach to scientific change may serve as an example. Those following Laudan's framework might claim that uptake of mobilism in the 1960s was rational, because the problem solving capacity of mobilism became more progressive than fixism around this time. Still, the epistemic standing of mobilism at the end of the 1960s is based on relative progressiveness which previously favored fixism. In principle, mobilism could become stagnant and be replaced in the future, or fixism could be become progressive and favored yet again.

convinced. Long standing opponents of mobilism in paleomagnetism and marine geology converted. Such conversions were often clustered around certain research results, especially between 1966 and 1967. Centrally involved researchers describe being astonished by such research, especially in relation to the discovery of the Juan de Fuca ridge and the corroboration of modeled magnetic anomalies by magnetic profile measurements and seafloor sediment measurements. Support for mobilism in other areas of the Earth sciences also rapidly expanded into the 1970s.

Frankel recognizes that something special took place in the second half of the 1960s. Specifically, the Vine-Matthews hypothesis, transform faults, and plate tectonics achieved difficulty-free status between 1966 and 1967. For Frankel, difficulty-free solutions were exceptionally rare and the "Holy Grail of researchers during the mobilism debate".[579] Early in the mobilism debate, continental drift offered no difficulty-free solutions. Also, Frankel does not attribute difficulty-free solutions to fixism. Endorsements of mobilism rapidly expanded toward the end of the 1960s because mobilism was associated with several mutually supportive difficulty-free solutions by this time. Frankel writes that even previously steadfast fixists, "accepted plate tectonics because they recognized it contained several compatible difficulty-free solutions that were mutually supportive." Furthermore, according to Frankel, "once these had been established there was no going back."[580][581] Thus, Frankel's notion of the difficulty-free solution challenges some of the relativism endorsed in alternative historical accounts.

Frankel's idea of the difficulty-free solution also resolves an important dilemma related to the privileged status of mobilism. On the one hand, there is clearly a significant change in the reception of mobilism in the 1960s that is amenable to some sort of special epistemic distinction, yet it was not the case that all previously raised difficulties against mobilist hypotheses were resolved. This is apparent in some of the "loose ends" in the history provided in this and previous chapters. Radiometric dating sometimes seemed inconsistent with seafloor spreading. Antarctica is surrounded by ridges. Miocene fossils were identified near ridge crests. Sediments at ostensible locations of subduction did not show

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[579] Frankel, 2012, Volume I, 13
[580] Frankel, 2012, Volume I, 15
[581] Frankel also writes:

> *It certainly is sometimes very challenging to tell for sure if a solution is difficulty-free. If a solution has no difficulties when proposed, or even when accepted, it may encounter them later; new discoveries may create difficulties for a solution that up to then had none. Difficulties may lurk in strange places. Whether a solution is difficulty-free is determined relative to what is known at the time* (Frankel, 2012, Volume I, 14).

Thus, for Frankel, a single difficulty-free solution is rare and may have bearing on the resolution of a controversy, but its difficulty-free status may be overturned. Alternatively, Frankel seems to indicate that sets of mutually supportive difficulty-free solutions may be unimpeachable or nearly so.

signs of deformation. Sediment thickness did not tend to increase with distance from mid ocean ridges. In the first half of the 1960s, researchers at Lamont (especially) claimed that these difficulties eliminated the possibility of seafloor spreading. Even when Lamont researchers eventually endorsed seafloor spreading, these issues remained unaccounted.

Xavier Le Pichon was opposed to mobilism in the early 1960s. He published papers on the crustal structure at mid-ocean ridges, magnetic anomalies, fracture zones and seafloor sediments, often offering interpretations inconsistent with seafloor spreading and mobilism. In his dissertation, he argued that seafloor spreading was wrong. In 1966, Le Pichon endorsed seafloor spreading, thereby challenging many of the assumptions that shaped his previous work. Le Pichon subsequently described his conversion to mobilism as follows:

> *This extremely painful 'conversion' experience has been crucial in shaping my own vision of what science is about. During a period of 24 hours, I had the impression that my whole world was crumbling. I tried desperately to reject this new evidence, but it had an extraordinary predictive power! Why then was the heat flow three times smaller than expected for sea floor spreading? Why were the magnetic anomalies so different over the flanks of the ridge? Why was the sediment fill in the trenches undisturbed? I did not know, but I was progressively forced by the convincing power of the magnetic anomaly profiles to assume that in all these unexplained observations, there must have been hidden parameters that had not yet been taken into account.*[582]

Frankel's notion of the difficulty-free solution allows for this juxtaposition of strong convictions despite persistent difficulties. When a difficulty-free solution takes place, persistent difficulties are transformed into new problems. Thus, when Le Pichon recognized the Vine-Matthews hypothesis to be difficulty-free, he became strongly convinced of seafloor spreading, and the persistent difficulties that confronted seafloor spreading and the Vine-Matthews hypothesis became new problems.[583]

For Frankel, difficulty-free solutions are identified empirically, from the behavior of researchers. Difficulty-free status is achieved when researchers form consensus that a problem has overcome all relevant difficulties. Those who continue to raise difficulties are excluded from subsequent debate.

[582] Le Pichon, 2018, 212

[583] Kuhn's distinction of revolutionary and normal science, or changes in Lakatos' research programmes or Laudan's research traditions tackle this same dilemma (Kuhn, 1962; Lakatos 1970). Frankel, however, does not differentiate between large paradigmatic commitments in his account of difficulty-free solutions.

Frankel does not, however, account for *why* researchers might find a solution to be sufficiently convincing as to form consensus in the first place.

The central aim of the remainder of this work is to develop such an account. Whereas Frankel's approach emphasizes the empirical matter of whether or when a difficulty-free solution is reached, I aim to account for *why* researchers might form consensus despite persistent difficulties. Like Frankel's notion of the difficulty-free solution, the account that I develop also accommodates privileged epistemic convictions that are receptive to the historical contingencies of scientific research.

I argue that knowledge creation involves the formation of conceptual networks. The suitability of alternative concepts within a network has bearing upon their epistemic strength and judgements thereof. Network relationships also facilitate strong deductive conclusions pertaining to problem solving and the isolation of falsification. Sometimes the perfect suitability of concepts within a network can become apparent during the formation of certain network structures.

My general historical argument is that perfect suitability became apparent in the coming together of paleomagnetism, marine geology, and geochronology during the 1960s. Frankel's historical account focuses on developments in a long-lasting debate over mobilism, spanning a period of 60 years or so. For Frankel, paleomagnetism and marine geology research in the 1950s and 1960s were phases of this broader debate. The history that I have provided is organized somewhat differently. I examined paleomagnetism, marine geology, and geochronology, not as phases within a broader debate on mobilism, but rather as independent lines of research. There was occasional overlap between institutions, researchers, and theoretical structures, but these different lines of inquiry confronted unique sets of ambiguities and debates. Mobilism was of general relevance in marine geology only toward the end of the 1950s with an increasing willingness to form and defend global hypotheses. The study of the ocean floor had relevance to the mobilism debate, but this does not mean that marine geologists were much concerned with mobilism while obtaining and interpreting data. Initial aims in the study of paleomagnetism typically pertained to the nature and history of the Earth's magnetic field. The hypothesis of geomagnetic reversals was not clearly related to the mobilism debate until the development of the Vine-Matthews hypothesis in 1963. Mobilism was not particularly relevant to the development of geochronology, though Arthur Holmes was a proponent of mobilism and radioactive decay was sometimes related to arguments for convection currents in the mantle. I argue that the manner in which these independent strands of research came together during the 1960s had a profound effect on the reception of mobilism.

More specifically, between 1965 and 1967 a series of events demonstrated the perfect suitability of the geomagnetic reversal timescale and the Vine-Matthews hypothesis. Shortly before the identification of the Jaramillo event, a host of ambiguities could be called upon to contest the geomagnetic reversal timescale or Vine-Matthews. A sufficiently motivated critic could argue that special fitting was involved in the ongoing research of the USGS and ANU groups. Reversed polarity may be due to self-reversals rather than geomagnetic field reversals. Potassium-argon dating might be argued to be insufficiently precise to produce a reliable reversal timescale. Furthermore, by arbitrarily manipulating spreading rates, Vine could align *any* magnetic profile with *any* geomagnetic reversal timescale. Magnetic anomalies over the ocean might be due to self-reversals, or differential petrological character of rocks, or variation in thicknesses of magnetic material. Additionally, seafloor spreading confronted numerous difficulties and was just one of many possible interpretations of seafloor structures. Shortly after the identification of the Jaramillo event, these arguments could no longer be made. The geomagnetic reversal timescale and the Vine-Matthews hypothesis could no longer be separated into two uniquely ambiguous hypotheses, independently vulnerable to systematic error or special fitting. Rather, their perfect suitability eliminated the plausibility of previously identifiable ambiguities. Miocene fossils near ridge crests were still puzzling, but such difficulties no longer weighed against an ambiguous line of research. Instead, such difficulties weighed against a perfectly suited network which offered a pledge that persistent difficulties would eventually be resolved.

The coming together of multiple strands of research in support of mobilism, was both epistemically and socially significant. In this case study, not only did perfect suitability within a growing network confer strong epistemic support to that network, but it also aligned the research of previously independent groups. The snowballing acceptance of mobilism toward the end of the 1960s was not initiated by a single specialty or in a single region. Instead, recognition of a profoundly convincing case for mobilism developed nearly simultaneously across multiple specialties and regions, and subsequent network growth integrated still more subgroups.[584]

This historical argument can account for some phenomena that are often left unaccounted in alternative historical work. My historical argument can account for the way that centrally involved researchers described the growing appeal of mobilism in the 1960s. Researchers typically did not appeal

[584] Paleomagnetists, geochronologists, marine geologists, geophysicists, and seismologists were at the center of things. Researchers were often located in USA and the UK but also Canada, Australia, Japan, and Europe, and data collection was often global.

to consensus as a reason for strengthening convictions or conversion. Rather, they often appealed to striking coherence of predictive successes. For some, these predictive successes were exciting, and almost too good to be true. For others, these predictive successes were shocking and painful, but impossible to ignore. Additionally, my analytic framework can account for *how* predictive successes become bunched together. For Frankel, it is coincidental that three difficulty-free solutions developed between 1966-1967. In my view, certain kinds of predictive successes provide a framework that is highly conducive to further predictive success.

Part 2: Predictive Networks and Consensus Formation

Chapter 6: Predictive Networks

Introduction

In this chapter, I argue that the growth of scientific knowledge involves the formation of predictive networks. These networks are accrued from simple entailed relationships within a set of accepted statements. I identify general models of these relationships, which I call Models of Discordance and Concordance.

Prediction testing is often recognized to have epistemic significance. I argue that attempts to account for this epistemic significance often rely on a limited conceptualization of prediction testing. This limited view of prediction testing contributes to a general impression of the epistemology of science, wherein a sequence of observations weighs upon some hypothesis, and scientific knowledge accrues linearly. The Models of Concordance and Discordance, I argue, provide an enriched view of prediction testing and thereby challenge these expectations about the development of scientific knowledge. These challenges are not new. Rather, the novelty is in the approach that is taken to mount these challenges.

Here is the approach. I argue that the Models of Discordance and Concordance model prediction testing. Models of Discordance are identified from their capacity to result in deductive falsification, as informed by literature on falsification, predictive holism, and problem solving. This facilitates the identification of symmetrical Models of Concordance wherein entailed relationships concord rather than discord. I argue that the Models of Concordance have positive epistemic significance. Since Models of Concordance model prediction testing, this epistemic significance can be mapped onto theories of confirmation. Additionally, the epistemic significance of concordance can be identified in other notions of epistemic support in the philosophy of science, such as consilience and coherence. I examine William Whewell's claim that consilience is a pledge of truth. Most contemporary accounts of consilience reject this strong claim. I argue that the Models of Concordance can model consilience, thereby facilitating the recognition that there are different kinds of consilience, and that the epistemic significance of consilience comes in degrees. In the strongest possible circumstances, such consilience may justify realism, an argument that I eventually relate to Ian Hacking. On coherence, I pay particular attention to Paul Thagard's notion that coherence within a set of propositions is built up from simple explanatory relationships between proposition pairs. His account of explanatory coherence demonstrates close affinity to the Models of Concordance. Thus, the Models of Discordance and Concordance are deductively founded and synthesize alternative notions of epistemic support.

Epistemology of Prediction Testing

In this section, I identify general features of prediction testing in science and related attempts to account for the epistemic significance of prediction testing.

Prediction formation and testing is a part of everyday life. Some predictions are obvious. A political pundit may predict that a public figure will run for office. A gambler may predict that a ball will land on a certain number of a roulette wheel. Other predictions may be more subtle, like those implicitly involved in planning for future events. Setting an alarm clock, for example, implicitly predicts that past regularities will apply in the future. One such regularity involved in setting an alarm clock is that loud noise will interrupt sleep.

Scientists make predictions. Unlike colloquial usage, scientific predictions do not necessarily contain a temporal dimension. Scientific predictions may include outcomes that are already known to obtain. For example, it may be claimed that Wegener's hypothesis of continental drift predicts paleontological and geological disjuncts across continents even though these disjuncts were identified prior to the formation of Wegener's hypothesis. Even when scientific predictions have a temporal dimension, they do not necessarily pertain to future events. Historical sciences, for example, frequently entail conclusions about historical events. Scientific prediction may permit certain states, they may be quantitatively precise or qualitatively specific. Alternatively, scientific prediction may prohibit certain states, they may be quantitatively imprecise or qualitatively vague.

Scientific predictions are expectations that follow when some commitment(s), like a hypothesis, are accepted as true. For example, Alfred Wegener hypothesized that the coastlines of South America and Africa were once conjoined. If this hypothesis were true, several additional expectations may follow. One such prediction may be that the Atlantic Ocean is younger than the South American and African continents. Scientific predictions are often considered to be particularly important in the process of scientific knowledge formation. In some cases, scientific predictions are deemed to be particularly authoritative or reliable, even if those predictions are not yet tested.

A distinguishing feature of scientific prediction is the potential capacity for testing. The truth of a prediction is not guaranteed from the commitments from which that prediction is derived. This capacity for testing can distinguish prediction from other sorts of derived conclusions, but this distinction can be quite blurry. For example, the age of a rock sample may be derived, when a set of radiometric data and other assumptions are given. This derived age of the rock might be considered a measurement, while others may consider the age of the rock to be an inference that makes sense of radiometric data, and inferences or measurements may not have the same epistemic role as predictions. However, the age of the rock in this example is undoubtedly derived, and there is still a sense in which the age could be tested. Supposing that the radiometrically dated rock is found to contain a fossil of deviant age, this may compel some geochronologists to discard the radiometric age and question the validity of the commitments therein. Perhaps, then, distinguishing scientific prediction from other derived conclusions may involve intentionality, wherein predictions are intended to test hypotheses, while other derived conclusions may not be intended for this function.

Scientific predictions are often believed to be tested empirically. Of course, sense experience is not forthright, so comparing prediction against empirical statements may be ambiguous and subject to debate or change over time. Empirical tests of scientific prediction may even be filtered through many layers of theoretical interpretation. For example, based on a clustered paleopole determinations from Jurassic rock in Northern Asia, paleomagnetists may predict a certain paleopole position for untested Jurassic rock in Central Asia. Testing this prediction involves assumptions pertaining to field work methods, magnetometry, rock dating, natural remanent magnetism, and the Earth's magnetic field. Thus, what makes empirical testing useful is not its unassailability. Rather, empirical tests are, in principle, readily and agreeably identifiable as concordant or discordant with prediction. They also offer a means of testing wherein the methods or theories involved in the test are independent from those involved in prediction formation.

From this very brief typology, it is apparent that the delineation of scientific prediction and testing is closely associated with notions of epistemic function. We now turn to alternative attempts in philosophy of science to identify the epistemic role of prediction testing.[585]

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[585] Carl Hempel developed an influential account of confirmation in the 1940s (Hempel, 1943, 1945). Central to Hempel's account of confirmation was Nicod's criterion, which Hempel characterized as stating that a generalization of the form "all Fs are Gs" is confirmed by the observation of an object that is both an F and a G and that objects that are not Fs are of no confirmatory significance. Hempel also identified additional plausible confirmation criteria. Combinations of these criteria, however, seemed to result in problems such as the Ravens

Hypothetico-deductive confirmation is based on the intuition that the justification for belief in a hypothesis is related to testing predictions derived from that hypothesis. When prediction entailed by a hypothesis is confirmed, scientists sometimes seem to suppose that this supports the hypothesis. When prediction entailed by a hypothesis is refuted, scientists sometimes seem to suppose that this undermines the hypothesis. These general observations, and the intuitions that they reflect, may be accounted by the following criteria, wherein *confirmation* denotes an increase in epistemic support.

Some evidence E confirms hypothesis H if and only if $H \rightarrow E$

Some evidence E disconfirms hypothesis H if and only if $H \rightarrow \sim E$

Scientific knowledge then grows in two general ways. Some hypothesis H may be tested against a growing collection of evidence. Each piece of evidence that satisfies the confirmation criteria then has bearing upon the justification of H. In the strongest possible cases, the refutation of prediction entailed by some hypothesis falsifies that hypothesis. Epistemic support conferred by confirmation is not as strong since confirmation of the consequent does not logically necessitate the antecedent. Thus, hypothesis H may accumulate support as more evidence is examined, yet it may also lose support or suddenly become falsified. Scientific knowledge may also grow by establishing the relative epistemic strength of alternative hypotheses. Some competing hypotheses H1 and H2 may both be tested against a growing collection of evidence. H1 may entail a set of predictions that partially overlaps with those entailed by H2, but these shared predictions are not epistemically significant. Rather, differential epistemic support can only be conferred to such alternative hypotheses when predictions diverge. Some piece of evidence E1 may confirm H1 yet not satisfy confirmation criteria with respect to H2. In such circumstances, H1 may obtain excess support over H2. In alternative circumstances often considered to be particularly important, contradictory predictions may be entailed by competing hypotheses H1 and H2, and a crucial experiment may then be completed to decide between them.

Thus, hypothetico-deductive confirmation provides a framework that can account for the epistemic support of a single hypothesis and relative epistemic support between alternative hypotheses. In the later case, divergent predictions have greatest epistemic significance. Within a hypothetico-deductive framework, scientific knowledge consists of many known hypotheses that entail predictions.

Paradox. One of the criteria identified by Hempel (the consistency condition) required that confirmatory evidence cannot support two hypotheses that are inconsistent with one another. This criterion is typically not included in contemporary works on confirmation, where confirmation pertains largely to prediction testing with recognition that a single piece of evidence may confirm multiple hypotheses.

Scientific knowledge grows by accumulating new evidence, which confers epistemic support to some hypotheses and culls others. The best hypotheses are the ones that have accumulated the most support.

Intuitively, some confirmed predictions may confer a greater degree of epistemic support than others. The basic confirmation criteria of hypothetico-deductive confirmation, introduced above, do not provide a clear way to delineate such relative epistemic significance.[586] One possible approach is to argue that some tested predictions are more significant than others, and that these tests have greatest relevance to the justification of belief. Significant tests may then be characterized in alternative ways.

One prominent approach is to argue that novelty is an important criterion for delineation of epistemic significance.[587] This is based on the notion that the confirmation of a predicted novel phenomena can be, at least intuitively, highly impressive. Additionally, episodes of novel prediction in the history of science sometimes seem to carry notable convincing power among scientists. Arthur Eddington's measurements of gravitational deflection of starlight is often cited as an example. With respect to continental drift, transform faults and the Vine-Matthews hypothesis might be candidates for highly convincing novel predictions.[588] During the 19th century, William Whewell argued for the significance of novel predictions, while John Stuart Mill disagreed.[589] In the 20th century, the work of Imre Lakatos revived this debate. Lakatos argued that alternative research programmes are judged by their relative progressiveness, as measured by excess confirmed novel prediction.[590] Novelty of scientific prediction is also sometimes considered to be the greatest success of scientific inquiry, indicative of the realism of scientific knowledge.[591]

Rather than emphasizing the epistemic significance of novelty, per se, Deborah Mayo endorses the notion that hypotheses can be subjected to tests of varying severity and that more severe tests are more epistemically significant.[592] For Mayo, a severe test is one where the probability of an outcome

[586] Two other senses of epistemic significance have already been introduced with respect to hypothetico-deductivism. First, falsification may warrant stronger conclusions than confirmation. Second, divergent predictions are more significant than shared predictions when judging relative epistemic support. The sense of epistemic significance identified here pertains to variation in the confirmatory support conferred by alternative sorts of confirmed prediction.
[587] Often, these additional distinctions of epistemic significance are identified within broader arguments of epistemic significance and are not always clearly defined as amended confirmation criteria.
[588] This much is explicitly argued in Laudan and Laudan, 1989.
[589] Whewell, 1858; Mill, 1843
[590] Lakatos, 1970. Also see Zahar, 1973; Worral, 2014
[591] Psillos, 1999; Barnes, 2008
[592] Mayo, 1991, 1996

would be very low if the hypothesis of interest were false. She thereby endorses the following confirmation criterion.

Some evidence E confirms hypothesis H if and only if $H \rightarrow E$ and $P(E|\sim H) \ll 1$

Explanation is also sometimes cited as an additional criterion of confirmatory significance. In this case, significance of a test is established by the quality of the relationship between the hypothesis and the evidence. Some hypothesis is more supported by confirmed prediction when the hypothesis not only entails the prediction but also explains it. Those who endorse the epistemic significance of explanation typically favor some sort of causal theory of explanation.[593]

Prediction also has an important epistemic role within Bayesian confirmation. Unlike hypothetico-deductive confirmation and the related theories introduced above, Bayesian confirmation adheres to the laws of probability and is based on the notion that the justification for belief can be expressed as such.

The probability of a hypothesis H, given evidence E, may be expressed by Bayes' theorem.

$$P(H|E) = P(E|H) \cdot P(H) / P(E|H) \cdot P(H) + P(E|\sim H) \cdot P(\sim H)$$

The confirmation criteria in Bayesian confirmation are as follows.[594]

Evidence E confirms hypothesis H if and only if $P(H|E) > P(H)$

Evidence E disconfirms hypothesis H if and only if $P(H|E) < P(H)$

Bayesian confirmation can also provide a clear definition of the degree of support conferred by a piece of evidence upon a hypothesis.

The degree to which evidence E confirms hypothesis H is $P(H|E) - P(H)$[595]

[593] Such theories of causal explanation are not necessarily clearly elaborated by those who endorse this epistemic significance (see Lipton, 2000, 2004). In the deductive-nomological view of scientific explanation, a phenomenon is explained just when that phenomenon is entailed by a general law combined with particular conditions. Accordingly, deductive nomological notions of explanations are not conducive to this type of strengthened confirmation criteria.
[594] There is an alternative possible interpretation of Bayesian confirmation wherein a certain probability threshold might be identified, at which point a hypothesis might be deemed to be justified.
[595] This is an obvious statement of the gross epistemic support conferred to a particular hypothesis. There are other possible characterizations of epistemic significance within Bayesian confirmation (see Crupi, Tentori and Gonzalez 2007).

As a representation of scientific knowledge, Bayesian confirmation recruits the notion of subjective probabilities, wherein probabilities are deemed to reflect a subjective degree of belief of an ideally rational agent.[596] The laws of probability can thereby be used to represent the sequential updating of degrees of belief in some hypothesis as evidence is accrued. A single piece of evidence may be probabilistically relevant to multiple hypotheses but may support or refute these hypotheses to varying degrees. Accordingly, given sufficient accumulation of relevant evidence, sequential updating may result in collective convergence of probabilities even if there are alternative possible ways to assign a starting or "prior" probability to some hypothesis H.[597] Hypotheses that are most probable will be those that scientists endorse. At a given point, multiple hypotheses may have similar probabilities, so scientists may disagree about which available hypothesis is preferable. The accumulation of evidence, however, should result in probabilistic differentiation. Among favored hypotheses, in science, some may be more strongly secured than others. The best hypotheses are the ones that have the highest probabilities, having accumulated the most and/or best support.

In Bayesian confirmation, prediction may be probabilistic or deterministic. Refutation is not necessarily absolute but may instead lower the probability of a hypothesis. Additionally, Bayesian confirmation includes a broader range of epistemic phenomena than hypothetico-deductivism, since confirmation criteria are not necessarily only satisfiable by predictions directly entailed by a hypothesis. For example, Harold Jeffreys considered measurements of the solidity of the Earth to weigh against continental drift, but continental drift presupposes fluidity rather than entailing it. Accordingly, the fluidity of the Earth does not easily satisfy hypothetico-deductive confirmation criteria with respect to continental drift but may still satisfy Bayesian confirmation criteria. Additionally, from Bayes' theorem, it is apparent that if $H \rightarrow E$ such that $P(E|H) = 1$, then $P(H|E)>P(H)$ (assuming all probabilities are values between 0 and 1). This is akin to the hypothetico-deductive confirmation criteria. From Bayes' theorem it is also apparent that the lower the value of $P(E|{\sim}H)$, the greater the increase to $P(H|E)$, all else being equal. This is akin to the notion of the severe test.[598]

[596] In the subjectivist view, the only constraint upon establishing probability is probabilistic coherence. There are alternative possible constraints that may be added by objective Bayesians.
[597] As indicated in Bayes' theorem, P(H) is a term that needs to be assigned a value in order for Bayesian updating to take place. How the starting value of this term should be determined is subject to debate.
[598] Bayesian confirmation can also find some affinity to Hempel's confirmation criteria as well, and ostensibly offers some means by which paradoxes of confirmation can be avoided. The general utility of Bayesianism to make sense of these alternative theories of confirmation is often presented as a strong reason to support Bayesianism (Sprenger and Hartmann, 2019).

Karl Popper argued that the confirmation of prediction is not itself epistemically significant.[599] Conclusions pertaining to the truth of some hypothesis do not deductively follow from the confirmation of prediction. Alternatively, the refutation of prediction entailed by a hypothesis deductively falsifies that hypothesis. Accordingly, Popper developed an epistemology of science grounded upon falsification. He argued that the defining feature of scientific hypotheses is falsifiability. Scientific hypotheses entail predictions that can be tested with a legitimate possibility of refutation. Should a prediction be refuted, the hypothesis is falsified and will either be modified or replaced. Should a prediction be confirmed, the hypothesis garners some epistemic support by virtue of avoiding falsification. Popper called this *corroboration*.[600][601] Popper's confirmation criterion may be stated as follows.

If $H \rightarrow E$ and $\sim E$ is a real possibility, then evidence E confirms hypothesis H

For Popper, good science aims to falsify hypotheses. Accordingly, novel predictions are of greater epistemic significance than predictions that are already known to obtain. The very best scientific hypotheses are those that make the most remarkable predictions and have withstood the most testing. Still, even the most strongly secured hypotheses may be falsified by subsequent tests.

In Popper's view, science is fundamentally a problem solving activity. A refutation produces a problem. Solving the problem consists of the incorporation of that refutation into the set of corroborative evidence entailed by some hypothesis.

The problem solving process proceeds as follows. A scientific hypothesis H1 may entail a set of corroborated predictions E.

$$(H1 \rightarrow E) \cdot E$$

Hypothesis H1 may also entail a novel prediction E1 that is refuted by observation E2, thereby falsifying H1.

$$(H1 \rightarrow E1) \cdot \sim E1$$

$$\sim H1$$

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[599] This is an important distinction between hypothetico-deductivism and Popperian views of epistemic significance of prediction testing. Popper's deductive approach and his emphasis on problem solving is why he is given separate consideration, rather than being grouped in with hypothetico-deductive confirmation, even though he has closer affinity to hypothetico-deductivism than to Bayesianism.

[600] Popper, 2002a, 2002b

[601] Note that I will soon use the term *corroboration* quite differently with respect to Models of Concordance.

The falsified hypothesis H1 may be replaced by some alternative hypothesis H2. Popper argues that the superseding hypothesis should avoid the refutation that confronts H1 and entail all the corroborated predictions of H1.

$$(H2 \rightarrow E) \cdot E$$

$$(H2 \rightarrow E2) \cdot E2$$

Hypothesis H2 may also entail additional predictions in excess to those noted above, facilitating further testing.

Alternatively, Popper claims that refutation may result in the modification of H1 to H1'. Such modifications of a falsified hypothesis, however, can easily accommodate a refutation when constructed specifically for that task, and this may result in the degeneration of a scientific hypothesis into an unfalsifiable hypothesis. Accordingly, Popper stipulates that such modifications to a hypothesis must also entail an excess of falsification opportunities. This may be denoted as E'.

$$(H1' \rightarrow E) \cdot E$$

$$(H1' \rightarrow E2) \cdot E2$$

$$H1' \rightarrow E'$$

If the conditions for successful problem solving identified above are not satisfied, then hypothesis H1 may still be retained despite its logical falsification since there is no superior hypothesis to take its place. In practice, Popper recognized that this is often the case.

Models of Prediction Testing

Though these theories of confirmation are different, common ground can be identified. Logically entailed prediction has an important role in the growth of scientific knowledge as represented within these theories of confirmation, though the nature of this role is somewhat variable. Alternative theories of confirmation are developed from alternative confirmation criteria. For Bayesians, entailed prediction only has relevance with respect to conditional probabilities. However, entailment is a necessary feature of the confirmation criteria of hypothetico-deductivism and Popperian falsification. In hypothetico-deductive and Bayesian confirmation, the epistemic strength of a hypothesis may be increased by confirmation of a logically entailed prediction. For Popper, confirmed prediction does not confer epistemic support. Instead, avoidance of falsification increases the epistemic strength of a hypothesis.

Predictions are typically considered to be empirically tested, though the nature of this testing is also somewhat variable. Theories of confirmation share part of their heritage with the logical empiricists who divided the language of science into theoretical and observational terms, a distinction that was subsequently problematized by increasing awareness of the theory-dependence of observation and the capacity for diverse and sustained debates about ostensible observation statements. Popper offered his own characterization of empirical testing through, what he called, basic statements. Basic statements are assertions of empirical events that are intersubjectively agreeable. He recognized that such statements are vulnerable to debate and modification but also claimed that conventional agreement on basic statements can be reached, allowing for prediction testing. Whereas Popper offered a detailed account of the character of the evidence against which hypotheses are tested, the character of evidence is often left vague in hypothetico-deductive and Bayesian confirmation. Still, these theories of confirmation typically conceive evidence to be empirical, and at least in principle eventually agreeable.[602]

Additionally, at least in the simplest possible cases, hypotheses are typically considered to be individually testable. Popper explicitly argued that hypotheses can be tested and falsified individually. Hypothetico-deductive and Bayesian confirmation do not necessarily carry the same general commitment toward epistemic individuation of scientific hypotheses. However, these theories of confirmation are often applied to or illustrated by simple examples wherein singular hypotheses are analyzed against a piece of evidence.

These commonalities can be represented by fundamental models of prediction testing that will be called Model 0 and Model 0'.

[602] Bayesian confirmation does not prescribe what the nature of evidence ought to be. However, in practice, such evidence is often taken as an empirical event with a probability of 1. Bayesian evidence is also sometimes described as testimony. A generalized view of probability updating may allow for evidence, itself, to be partitioned into alternative probability states that must be probabilistically coherent (Jeffrey 1965). Such evidence is most frequently treated as empirical. Additionally, Bayesian updating may be applied to raise or lower the probability of evidence, but even in such applications some basic evidence is taken unproblematically.

Model 0:

$(H \rightarrow E) \cdot {\sim}E$

${\sim}H$

Model 0’:

$(H \rightarrow E) \cdot E$

In these models, H denotes a scientific hypothesis and E denotes an empirical statement.[603]

In Model 0, the prediction E is refuted by some alternative statement ~E. The refutation of the consequent falsifies the antecedent. Popper’s approach to scientific knowledge is largely developed from this general model of prediction testing. Model 0 is symmetrical with Model 0’, though in Model 0’, prediction E is confirmed rather than refuted. Conclusions are not logically entailed from Model 0’. However, additional conclusions may be reached if Model 0’ is coupled with alternative confirmation criteria identified previously.

Models 0 and 0’ are models of prediction testing. They prescribe certain types of statements and relations between these statements, from which certain conclusions may follow. Specific instances of scientific practice may satisfy these prescriptions, thereby allowing insights from the general model to inform the specific circumstance. Additionally, these models of prediction testing are clearly amenable to the alternative accounts of the growth of scientific knowledge developed in theories of confirmation.[604]

Even in their general form, Models 0 and 0’ carry with them certain assumptions that influence broader conceptualizations of confirmation and the epistemic significance of prediction testing. One such assumption is that observation is the sole arbiter of theory change. In Models 0 and 0’, prediction testing consists of the comparison of a prediction against empirical evidence, though the character of this empirical evidence may be conceived in different ways. This primacy of empirical testing has a lengthy history in attempts to characterize the epistemology of science, even across groups and

[603] The distinguishing character of empirical statements and hypotheses is left intentionally vague to accommodate alternative possible distinctions.
[604] That is, those conditions that satisfy Model 0 or Model 0’, are also conditions that can be analyzed by alternative theories of confirmation.

individuals with otherwise varying commitments.[605] Afterall, scientists often put great effort into patient and detailed collection of observation and development of new methods to obtain or induce empirical outcomes.

Another assumption built into Models 0 and 0’ is that hypotheses and evidence are functionally distinct. Predictions are derived from hypotheses, then prediction is tested against empirical evidence in order to reach conclusions about that hypothesis.

Models 0 and 0’ also assume that scientific hypotheses obtain their epistemic support through a sequence of tested predictions. The corpus of scientific knowledge is the product of accumulated empirical tests. This general view of linear accumulation of evidence, and sequential analysis of hypotheses also has a lengthy history in the philosophy of science. The problem of induction is relevant, here. In his seminal account of the problem of induction, David Hume questioned the justification for establishing general causal relations based on repeated observation of contiguity.[606] He argued that attempts to justify such induction will rely on induction and thereby presuppose the justification, making any such argument circular.[607] Hume concluded that the causal relations that we ascribe to the world are psychological associations formed in the mind, and induction is not rational and unavoidably fallible. The problem of induction is a question of when or how instances justify generalizations, or when or how one such generalization may be deemed superior to another.[608] Theories of confirmation are, fundamentally, attempts to address the problem of induction.[609] It is therefore unsurprising that theories of confirmation tend to conceive of scientific knowledge as built-up from a sequence of tests.

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[605] Pioneering efforts to constrict a logic of confirmation were undertaken by logical empiricists (Hempel, 1945; Carnap, 1950; Reichenbach, 1959). The logical empiricists distinguished scientific statements to be those that could, at least in principle, be verified, and comparison with observation was one method of verification. The justification of a statement is what makes it scientific, and the justification is empirical. Additionally, in the history of the philosophy of science, empirical evidence was typically given a prominent role in the formation of scientific hypotheses as well (Bacon, 1605; Herschel, 1830; Mill, 1843; Whewell, 1858). In general, the manner in which empirical evidence is characterized is linked with the proposed epistemic role of such evidence within alternative epistemic systems.

[606] Hume, 1739

[607] Consult Chapter 8 for an argument on the justification of knowledge despite circularity.

[608] In principle, induction does not need to be characterized in a sequential, cumulative manner. This is, however, a forthright way to think about induction, because inductions are often reached in everyday life by repetition of instances.

[609] Hypothetico-deductive confirmation and Bayesian confirmation are attempts to identify general rules of induction that might be capable of identifying when one generalization is stronger than another. Alternatively, Popper rejected induction and aimed to develop a deductive approach to the growth of scientific knowledge.

In the next section, additional models of prediction testing will be introduced. These additional models challenge the assumptions built into Models 0 and 0'.

Predictive Holism and the Model of Conjunction

In his 1906 work, *The Aim and Structure of Physical Theory,* Pierre Duhem argued that predictions in science are not entailed by singular hypotheses, but by collections of commitments.[610] Some prediction E may be entailed by the conjunction of a hypothesis H with auxiliary hypotheses A. Such predictive conjunctions may be called predictive systems.

$$(H \cdot A) \rightarrow E$$

Duhem claimed that predictive systems are readily apparent from the history of science.[611] For example, Tuzo Wilson's prediction of the relative direction of motion at oceanic ridge offsets was derived from several commitments, including a hypothesis of seafloor spreading and that the Earth's crust is comprised of rigid plates.

Duhem also argued that interpretation of experimental results often required utilization of theory as well.[612] These interpretative hypotheses may also be considered to be part of the system involved in prediction testing. In this regard, Lynn Sykes tested the relative motion of crust on adjacent sides of transform faults through first-motion studies that inculcated assumptions about seismic waves, temporal and spatial distribution of a seismograph network, consistent mechanical functioning of seismographs within that network, and the reliability of data collection and graphical representations.

In Model 0, the refutation of prediction entailed by a hypothesis logically falsified that hypothesis. The refutation of prediction entailed by a conjunction, however, logically falsifies that conjunction without isolating falsification to any component therein**.**

$$((H \cdot A) \rightarrow E) \cdot \sim E$$

$$\sim(H \cdot A)$$

[610] Duhem, 1991
[611] Duhem favored the history of physics.
[612] Duhem also claimed that the degree of such theory involved in experimental interpretation increases as a science matures.

In such circumstances, the refutation of E is ambiguous with respect to the falsification of H. According to Duhem, "the only thing the experiment teaches us is that among the propositions used to predict the phenomenon and to establish whether it would be produced, there is at least one error; but where this error lies is just what it does not tell us."[613]

Auxiliary hypotheses have been characterized in different ways. Typically, auxiliaries refer to interpretative hypotheses, starting conditions, ceteris paribus assumptions, or other commitments that are deemed to be supplementary to some tested hypothesis. However, the ambiguity of falsification identified above applies to the refutation of prediction entailed by any conjunction of propositions, regardless of their supposed primary or secondary importance. Additionally, discussion of auxiliary hypotheses as supplemental may result in the erroneous assumption that systems can only contain a single scientific hypothesis. To the contrary, prediction may be derived from systems that contain several hypotheses. For example, Frederick Vine's prediction of symmetrical magnetic anomalies across ridge crests was the product of a conjunction of the seafloor spreading hypothesis with the hypothesis of geomagnetic reversals. It should also be highlighted that a predictive system may contain empirical statements. In this regard, initial conditions are often considered to be auxiliary hypotheses. For example, the age of a rock may be derived from a system that includes the measured ratio of radioactive potassium to radiogenic argon.[614] Additionally, a predictive system may include an untested empirical prediction from which additional predictions may be derived. For example, based on radioactive theory, Bertram Boltwood used the measured decay rate of radium to estimate the decay rate of Uranium. This estimate was later directly measured. Prior to this, however, Boltwood's estimate was a constituent within a predictive system that was used to derive some of the earliest radiometric ages.

Duhem's predictive system, consisting of hypothesis and auxiliaries, may be further generalized to reflect the flexible character of constituent propositions contained within a predictive system. This generalized model of prediction testing will be called the Model of Conjunction.

Model of Conjunction:

$((P1 \cdot P2) \rightarrow E) \cdot {\sim}E$

${\sim}(P1 \cdot P2)$

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613 Duhem, 1991, 185

614 Note that this derived relationship may also work in reverse. If the age of a rock is known, the ratio of potassium to argon may be derived.

Additional Examples of the Model of Conjunction:[615]

- Researchers at Lamont argued that potassium-argon dating and fossil dating near the crest of the Mid-Atlantic Ridge was too old to accommodate recent seafloor spreading. Hess claimed that the methods used to obtain these results were suspect, thereby attributing falsification to particular work involved in obtaining the refuting evidence and preserving seafloor spreading.
- Radiometric dating in the early 20th century routinely produced results that indicated the Earth was billions of years in age. John Joly recognized that the dates obtained by Holmes, Boltwood, and others challenged his views on the relatively youthful age of the Earth. Joly attributed falsification to the assumption that radioactive decay remains constant over time. By allowing variation in decay rates, this would preserve the viability of radiometric measurements but invalidate related age inferences, and thereby allow Joly to maintain a relatively young age for the Earth.
- During the 1950s, it became increasingly apparent that the seafloor was significantly younger than many continental formations. Fossils predating the cretaceous were not found on the seafloor, and sediment thickness as measured by seismic refraction was deemed to be thinner than anticipated given estimates of marine sedimentation rates. One possible reaction was to accept the relatively young age of the seafloor, which would seem to imply some dynamic process of seafloor formation. This was the route pursued by Hess, Dietz, Menard, Heezen, and others. This was not a logically necessary conclusion. Alternatively, consolidated sediment my reside under unconsolidated layers, some mechanism may clear seafloor sediments over time, or sedimentation rates may be lower in the past.
- Alfred Wegener proposed that paleontological disjuncts between South America and Africa were problematic to prevailing fixist expectations, and continental drift resolved this problem. Alternative researchers endorsed sunken continents, isthmian links, island hoping, or rafting to account for these disjuncts.

[615] Historical examples provided in this section and the next section are intended to illustrate relevant models of prediction testing while also highlighting the ambiguity apparent in resulting problem solving efforts. Often, conditions that satisfy the Models of Discordance (this term will be introduced shortly) result in uncertainty and disagreements among scientists.

The Ambiguity of Falsification and the Consensus Problem

The extent of the ambiguity of falsification can be more clearly delineated. Suppose that a refuted system is comprised of two propositions, P1 and P2.

System S:

$((P1 \cdot P2) \rightarrow E) \cdot \sim E$

$\sim(P1 \cdot P2)$

The refutation of E unambiguously falsifies the conjunction of P1 and P2 but fails to isolate falsification therein.[616] Still, this does not detract from the fact that there are a finite number of alternative ways in which falsification can be isolated within this system. In this case, P1 may be falsified, P2 may be falsified, or both P1 and P2 may be falsified.[617] The set of alternative ways in which falsification can be isolated within a system will be referred to as the *field of choice*.

System S field of choice:

(P1) - (P2) - (P1)&(P2)

The refutation of a system necessitates the falsification of one and only one option in the field of choice of that system. The ambiguity of falsification is ambiguity within a field of choice.

A refuted system may be modified in some way to eliminate the refutation and thereby resolve the falsification problem. However, due to this ambiguity of falsification, there is no logical obligation to retain or modify any component within the falsified system. The resolution of a falsification problem must merely attribute falsification to one and only one option within the field of choice. This is a notable

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[616] Typically, this is the full extent to which the ambiguity of falsification is articulated. On occasion, it is also claimed that the negation of a conjunction is equivalent to the disjunction of the negations.

$\sim(P1 \cdot P2) \leftrightarrow (\sim P1) \vee (\sim P2)$

[617] Identifying the possible alternatives is a simple matter of constructing a truth table of the conjunction of P1 and P2.

P1	**P2**	**P1 · P2**
T	T	T
T	F	F
F	T	F
F	F	F

As indicated, there are three alternative ways in which the truth values of P1 and P2 can result in the negation of the conjunction of P1 and P2. The size of a theoretical system is proportional to the size of that system's field of choice. A system that is comprised of three propositions has a field of choice consisting of seven options. If n is the number of propositions within a system, then the size of the field of choice is $2^n - 1$.

challenge confronting Popper's programme of falsification. When prediction is entailed by a system of propositions, Popper's ostensibly deductive basis for the epistemology of science becomes ambiguous as there is a degree of choice involved in problem solving.

Consider the following falsification problem, wherein observation E2 falsifies a system comprised of P1 and P2.

Problem S:

$((P1 \cdot P2) \rightarrow E1) \cdot \sim E1$

$\sim(P1 \cdot P2)$

For Popper, problem solving consists of the modification of a falsified hypothesis to accommodate a refutation, but in this general example, it is unclear which component of the falsified system should be modified.

Solution 1:

$((P1' \cdot P2) \rightarrow E2) \cdot E2$

Solution 2:

$((P1 \cdot P2') \rightarrow E2) \cdot E2$

Consequently, Duhem claimed that there is no such thing as a crucial experiment that can determine the superiority of one hypothesis over a competitor. Instead, experiment can only decide between alternative systems, and a beleaguered hypothesis may be retained by directing falsification to alternative commitments within that system. W.V.O. Quine, in his highly influential 1951 work *Two Dogmas of Empiricism,* claimed that any statement can be retained despite any observation whatsoever, by making sufficiently drastic modifications elsewhere.[618]

Popper was aware of this difficulty, but his response to the ambiguity of falsification was inadequate. He claimed that background knowledge which is shared between alternative systems

[618] Quine, 1951

facilitates the attribution of falsification to the differences between these systems.[619][620] This claim is sometimes problematic. Consider the following two systems.

System 1:

$((P1 \cdot P2) \rightarrow E1) \cdot {\sim}E1$

${\sim}(P1 \cdot P2)$

System 2:

$((P3 \cdot P2) \rightarrow E2) \cdot E2$

It may be supposed that this circumstance would falsify P1, but this is not necessarily the case. It is entirely possible, in this example, for the falsification of System 1 to be isolated to P2, which just so happens to entail a confirmed prediction in conjunction with P3.[621] This sort of simplistic analysis of differences between partially overlapping systems simply does not suffice.

Alternatively, Popper also appealed to the continuous testing of background knowledge as a means to isolate falsification.[622] There may be something to this claim, but Popper did not provide details on how such analysis could proceed.[623] Instead, he appealed to scientific practice of provisionally accepting some background knowledge as true in order to isolate tests to independent hypotheses. That is, Popper's response to the ambiguity of falsification was to claim that, in practice, scientists behave as if hypotheses are tested individually.[624] Of course, this appeal to practice does nothing to overcome the logical ambiguity identified by Duhem.

The ambiguity of falsification and associated ambiguity of problem solving outlined above does not change the fact that scientists often consider refutation to be important. Additionally, it is often the case that despite this apparent ambiguity of falsification and problem solving, scientists often reach agreement that one resolution of a falsification problem is superior to alternatives. For example, toward the end of the 19th century, many physicists and some geologists considered the age of the Earth to be a

[619] Popper, 2002b, 150
[620] Background knowledge is also often recruited in hypothetico-deductive and Bayesian confirmation. Confirmation is thereby sometimes said to be relative to some background knowledge. Attempts to challenge the ambiguity of falsification will be examined in the next chapter.
[621] A relevant notion called "special fitting" is introduced in Chapter 8.
[622] Popper, 2002b, 322-325
[623] See Chapter 7 for my account of such analysis.
[624] Popper, 2002b, 323

around 100 million of years in age. Radiometric dating results beginning in the early 20th century, and accelerating by mid century, refuted this hypothesis. In the early 20th century, some geologists such as George Becker and John Joly attributed falsification to assumptions involved in radiometric dating. However, geochronologists eventually reached a consensus that the suitable resolution to this problem was to modify the hypothesized youthful age of the Earth, even though, due to the apparent ambiguity of falsification, this course of problem solving was not logically determined.

This tendency for scientists to reach agreement despite the ambiguity of falsification may be called the *consensus problem*. Much notable work in the study of science over the past 60 years has sought some way to reconcile this apparent tension between logical ambiguity and social consensus. Duhem claimed that the choice between alternative solutions to a falsification problem is directed (though not logically determined) by diverse and imprecise principles of reasoning that he called "good sense".[625][626] Quine claimed that conservatism of change and simplicity of commitments direct the suitable response to refutation.[627] In *The Structure of Scientific Revolutions*, Thomas Kuhn argued that the history of a science can be periodized by a series of grand, normatively relevant commitments that he called paradigms.[628] Paradigmatic commitments may confront refutations, but the ambiguity of falsification allows for scientists to preferentially retain the paradigm during problem solving. Furthermore, the retained paradigm may direct the modification of other commitments. Accordingly, when a paradigm is in place, small-scale problem solving therein may be highly determined. Some attempts at problem solving may fail, and as failure persists or accumulates the preferentially retained paradigm commitments may change. Kuhn considered such paradigm change to be highly underdetermined. The choice between alternative possible paradigms is directed by values within scientific communities including accuracy, consistency, scope, simplicity, and fruitfulness.[629] According to Kuhn, these values are typical to science, but they may conflict with one another, and their meaning and relative importance may be influenced by paradigm change. Following Kuhn, consideration of the role of values in science diversified. Feminist epistemologists and others emphasize a role for socially contextual values (that do not have normative bearing upon the aims of science) in scientific decisions

[625] Duhem, 1991
[626] The notion of "good sense" is not fully fleshed out in Duhem's work, so it is difficult to clearly articulate what he means by the term. Still, contemporary interpretations of "good sense" often relate this concept to virtue epistemology (Stump, 2007; Ivanova, 2010; Fairweather, 2012).
[627] Quine's measure of conservatism and simplicity is related to his view of a fabric of scientific knowledge wherein some changes may be more or less disruptive (Quine, 1951).
[628] Kuhn, 1962
[629] Kuhn 1962, 1977

and change.[630] Notable works in the sociology of scientific knowledge claim that social forces may direct underdetermined decisions.[631] In the strongest such accounts, scientific change is no different from any other sort of social change: politics, economics, religion, identity, or some other preferred measure of social division establishes a socially contextual environment wherein alternative interested parties may conflict or cooperate. In weaker accounts, sociological context limits or constrains diversity of thought within science, thereby facilitating change in some directions rather than logically possible alternatives.[632]

In *Falsification and the Methodology of Scientific Research Programmes,* Imre Lakatos built upon Popper's account of problem solving in an attempt to construct a rational account of scientific change that was receptive to the ambiguity of falsification and the consensus problem.[633] Like Kuhn, Lakatos differentiated between grand normatively relevant commitments in science and secondary commitments that are preferentially adjusted during problem solving. Lakatos called these grand commitments the *hard core* of *research programmes*. A research programme consists of a series of problems that confront the hard core and their solutions. Lakatos claimed that there are always competing research programmes within a science. For Lakatos, science is a progressive venture, so scientific change is directed by the relative progressiveness of alternative research programmes. A research programme is progressive when a series of problems are resolved and those resolutions yield novel confirmed prediction. Alternatively, a research programme is degenerative when problems are not resolved or when the resolution of problems fails to produce an excess of novel confirmed prediction. Degenerative research programmes become unscientific, and the most progressive research programme will obtain the most support within a scientific community.[634]

For Lakatos, scientists do not judge the relative epistemic strength of alternative hypotheses, but the relative strength of alternative research programmes. A singular refutation my be ambiguous, but the relative progressiveness of research programmes is not.

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[630] Longino 1990; Haack 1998; Douglas 2000; Anderson 2004
[631] Bloor 1976; Collins 1981
[632] Oreskes' argument in *The Rejection of Continental Drift* may fit here (Oreskes, 1999).
[633] Lakatos, 1970
[634] For Lakatos, ad hoc hypotheses are those that may be added to the protective belt of a research programme that do not predict any novel facts, or those that do predict novel facts, but those facts are not corroborated. He also adds that ad hoc hypotheses may add novel predictions, some which my even be confirmed, yet if the hypothesis is arbitrary, it is still ad hoc. For Lakatos, the hard core establishes the sort of auxiliary hypotheses that are suitable, and deviation from this suitability may result in predictive success, but in a patchwork way that scientists will not find convincing.

In summary, the Model of Conjunction is a model of prediction testing that deviates slightly from Model 0 by allowing prediction to be entailed by a conjunction of propositions. This poses a notable challenge to theories of confirmation that are built upon Model 0 because, in the Model of Conjunction, falsification and problem solving are ambiguous. This ambiguity has had significant influence in the study of science, often associated with the problem of consensus formation.

Conceptual Problems and the Models of Entailment and Corroboration

In his 1977 work, *Progress and its Problems,* Larry Laudan expanded upon the work of Lakatos in a manner that was both highly influential and relevant to the present matter of distinguishing models of prediction testing.[635] Laudan added that the preferentially retained commitments that guide research traditions are not as rigid as Lakatos or Kuhn suggest and emphasized that problems sets may be differentially delineated across concurrent research traditions. Also, Laudan distinguished between empirical problems and conceptual problems. Solving conceptual problems, he claimed, is "at least as important in the development of science as empirical problem solving".[636]

According to Laudan, conceptual problems include logical inconsistences or self contradictions within the set of hypotheses that comprise a research tradition. There are also external conceptual problems, wherein a hypothesis conflicts with some alternative hypothesis that is also deemed to be rationally founded. External conceptual problems include inconsistency or joint implausibility between alternative hypotheses across scientific domains, some hypothesis and methodological prescription, or some hypothesis and prevalent world view.[637] Laudan anchors this discussion of conceptual problems to an alternative model of prediction testing that can be generalized as the Model of Entailment.[638]

Model of Entailment:

$P1 \rightarrow \sim P2$

$\sim(P1 \cdot P2)$

[635] Laudan, L., 1977
[636] Laudan, L., 1977, 45
[637] Laudan, L., 1977, 55-64
[638] See Laudan, L., 1977, Chapter 2, especially page 54.

Laudan claims that such conceptual problems bear symmetrically upon the commitments involved.[639] Like empirical problems, conceptual problems are ambiguous and demand no specific action. Indeed, it is apparent from the Model of Entailment, that the conclusion is logically equivalent to the conclusion within the Model of Conjunction. The Model of Entailment thereby produces a field of choice and, like problem solving in the Model of Conjunction, problem solving in the Model of Entailment is ambiguous. However, unlike the models of prediction testing introduced previously, the Model of Entailment does not involve empirical testing. Empirical testing is not required for a problem to be identified or, at least in principle, for problem solving to take place.

Examples of the Model of Entailment:

- A frequent objection to Wegener's theory of continental drift was that the Earth was not sufficiently fluid for such drift to take place. The solidity of the Earth was deemed to eliminate the possibility of continental drift. Harold Jeffreys endorsed this objection. Additionally, this solidity was frequently deemed to have stronger support than continental drift, so the discordance weighed heavily against Wegener's claims.
- Patrick Blackett proposed that magnetism is a fundamental property of rotating mass. Geomagnetic field reversals were hypothesized to account for paleomagnetic polarity reversals. Blackett's theory of magnetism could only account for geomagnetic field reversals if Earth's spin changed directions. Accordingly, Blackett was an early opponent of the geomagnetic field reversal hypothesis.
- In the early 1950s, the oldest radiometrically dated terrestrial rock was over 2 billion years in age and meteorites were dated to 4.5 billion years. However, astronomers deemed the entire universe to have an age of only 1.8 billion years, based on the measured rate of expansion of the universe. The inferred age of the universe thereby precluded the inferred age of the Earth. Astronomical measurements were modified in the 1950s, increasing the age of the universe and thereby accommodating these radiometrically determined ages.

[639] Laudan adds that different conditions may make conceptual problems more or less severe, and that the symmetry my not be perfect. For example, differential epistemic strength may result in problems that are differentially severe to the hypotheses involved.

- Alfred Wegener argued that the concept of isostasy prohibited the hypothesis of sunken continents. This compelled Wegener to reject the adequacy of the contractionist resolution to the problem of paleontological disjuncts across continents.

Laudan limits his description of conceptual problems to the Model of Entailment, but there is an additional model of conceptual problem that can be identified. This will be called the Model of Corroboration.

Model of Corroboration:

$(P1 \rightarrow E) \cdot (P2 \rightarrow \sim E)$

$\sim(P1 \cdot P2)$

In the Model of Corroboration, conceptual discordance is indirect and borne from empirical predictions entailed by the two hypotheses. Like the Model of Entailment, this problem bears symmetrically upon the commitments involved. The entailed conclusion within the Model of Corroboration is the same as that contained within the Model of Entailment and the Model of Conjunction. Though the source of discordance, in this case, is between two empirical statements, definitive empirical testing is not required for a problem of this sort to be identified.

Examples of the Model of Corroboration:

- In 1957, Allan Cox published results from fieldwork on Eocene lava flows in Oregon. His inferred paleopole positions were inconsistent with those published by Creer, Irving, and Runcorn. Cox thereby deemed paleopole measurements to be problematic and directed his research toward polarity reversals. Alternatively, Irving proposed that Cox's sampled region had undergone local rotation relative to the rest of North America, thereby accounting for the discordance.
- In the early 20th century, George Becker found great inconsistencies in age determinations derived from radiometric rock. He thereby argued that radiometric dating was unreliable.
- Arthur Holmes was a proponent of the helium radiometric dating method during the 1930s, in large part because early measurements agreed with those obtained from lead dating methods. Researchers at MIT, however, found that helium dating generally resulted in younger age determinations than those obtained by the lead method.

Thereafter, Holmes, along with most radiometric dating specialists, considered Helium dating to be unreliable.

- Paleomagnetists compared inferences about paleolatitude and paleopole positions against inferences in paleoclimatology. The Squantum tillite – inferred to be a Permian glacial tillite near present day Boston - seemed to contradict Permian paleolatitude inferences for North America. To account for this problem, Irving proposed the Squantum tillite was the product of a mountain glacier.

Models of Discordance

To this point, three additional models of prediction testing have been identified as elaborations of Model 0. These models will be collectively referred to as Models of Discordance.

Model 0:

$(H \rightarrow E) \cdot {\sim}E$

${\sim}H$

Conjunction:

$((P1 \cdot P2) \rightarrow E) \cdot {\sim}E$

${\sim}(P1 \cdot P2)$

Entailment:

$P1 \rightarrow {\sim}P2$

${\sim}(P1 \cdot P2)$

Corroboration:

$(P1 \rightarrow E) \cdot (P2 \rightarrow {\sim}E)$

${\sim}(P1 \cdot P2)$

The Models of Conjunction, Entailment, and Corroboration deviate from Model 0 by presupposing a set of accepted statements. Model 0 presupposes only a singular hypothesis. Each Model of Discordance includes an entailed prediction and some discordance between that prediction and an alternative accepted statement.[640] In each case, P1 and P2 may be said to be discordant. The Models of Discordance each produce an equivalent field of choice, and each problem may be solved by modifying one option within the field of choice to eliminate the discordance.

The Models of Discordance challenge assumptions that are built into Model 0.

In Model 0, empirical testing is the sole arbiter of theory change. In the Models of Discordance, problems may be produced from discordance between prediction and empirical outcome, but may also be the product of conceptual discordance, or from discordance between alternative predictions.[641]

In Model 0, hypotheses and evidence are functionally distinct. Hypotheses entail empirical prediction, and empirical tests then weigh upon the hypothesis. The Models of Discordance dissolve any strong functional distinction between hypothesis and evidence. The notation employed in these models retains a distinction between empirical and non-empirical prediction in order to highlight alternative possible sources of discordance (as identified in the previous paragraph). This distinction, however, is not made with respect to the commitments from which prediction is entailed. As previously noted, empirical statements (tested or untested) may be among those commitments from which prediction is entailed.

In Model 0, scientific knowledge grows through sequential analysis of accumulated evidence. In the Models of Conjunction, Entailment, and Corroboration, discordance weighs upon a set of accepted statements, rather than a single hypothesis. This raises the prospect of systems of commitments or lineages as epistemic units in science, which are not amenable to sequential analysis.[642]

Like Model 0, the Models of Discordance are models of prediction testing. They are simple and fundamental to prediction testing within a set of accepted statements, when minimal logical operations and rules are given. They are intended to provide some insight into approaches to the epistemic

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[640] The notion of accepted, used here, is quite flexible. Statements that are considered plausible candidates for acceptance may be included, as may statements that are speculative, with the corresponding recognition that there may be concurrent, mutually inconsistent sets of statements that may contain alternative sets of problems.
[641] Popper considered prediction testing to be epistemically significant due to the possibility of falsification. As clearly illustrated in the Models of Entailment and Corroboration, this epistemic function may be satisfied even without empirical testing.
[642] However, a general tendency toward sequential analysis sometimes remains intact in accounts of problem solving, where problems are often deemed to be independent from one another and resolvable in turn. Problems are sometimes deemed to be units of analysis.

significance of prediction testing within the philosophy of science, but also to specific instances of scientific practice.

Models of Concordance

Like Model 0 and 0’, each of the Models of Discordance has a symmetrical model of prediction testing wherein prediction is confirmed rather than refuted. These will be called Models of Concordance.

Model 0’:

$(H \rightarrow E) \cdot E$

Conjunction’:

$((P1 \cdot P2) \rightarrow E) \cdot E$

Entailment’:

$P1 \rightarrow P2$

Corroboration’:

$(P1 \rightarrow E) \cdot (P2 \rightarrow E)$

Like the Models of Discordance, the Models of Concordance apply to a set of accepted statements, P1 and P2. Each model includes some entailed prediction and a concordance between that prediction and some alternative accepted statement. In each of these models, P1 and P2 may be said to concord, and I contend that such concordance has confirmatory significance.

Like Model 0’, conclusions are not logically entailed from the Models of Corroboration. Additional conclusions may be reached, however, if these models are coupled with confirmation criteria. Now, the application of alterative theories of confirmation to Model 0’ is forthright since, as I have argued, this is the model of prediction testing typically assumed within theories of confirmation. Applying confirmation criteria to the Models of Concordance is not always so easy.[643] One possible route

[643] The relationships modeled by the Models of Concordance may result in conditional probability relationships between propositions therein. This may allow the Models of Concordance to be mapped onto Bayesian

through which these theories of confirmation may be applied to the Models of Concordance is by dispensing with the strong functional distinction between hypothesis and empirical testing. The relevant arguments, to this end, have already been introduced. Hypotheses are not unique in entailing prediction and the epistemically relevant features of prediction testing that are satisfied by comparing an empirical prediction against an empirical outcome can also be satisfied by comparing an empirical prediction against alternative empirical prediction or by comparing alterative accepted statements directly.

Some additional preliminary claims may be made about the confirmatory significance of the Models of Concordance. In the Models of Conjunction' and Corroboration', epistemic support will be conferred upon both P1 and P2, regardless of the confirmation criteria that may be employed. In each case, substitution of P1 with P2, or vice-versa, results in a logically equivalent expression. The Model of Entailment' only exhibits this symmetry in the special case wherein (P1 → P2) and (P2 → P1). However, in the general case, P1 → P2, alternative confirmation criteria may not attribute confirmation to both P1 and P2.[644]

The differential support of alternative accepted statements may influence the manner in which epistemic support is conferred when the models of concordance are satisfied. For example, at the outset of his pioneering work on radiometric dating, Arthur Holmes considered relative dating of geological ages to be very strongly secured in the corpus of scientific knowledge. In order to establish the reliability of the uranium-lead method as a means of rock dating, Holmes dated rock samples of known geological age. He found that the ages obtained by uranium-lead ratios were corroborated by the relative geological age of the rock samples. This raised Holmes' confidence in the uranium-lead method. By the 1930s, Holmes considered the uranium-lead method to be highly reliable and thereby judged the adequacy of alternative radiometric dating methods against results obtained by uranium-lead ratios. When the helium method seemed to concord with lead dating, Holmes considered this to be remarkable support for the helium dating method, since the lead method was already strongly secured. In this case, a more strongly secured set of commitments conferred epistemic support to a less strongly secured set

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confirmation quite readily. This is not so clearly the case in Hypothetico-Deductivism or Falsificationism, which tend to rely more strongly upon the distinction between hypothesis and empirical test.

[644] As indicated in the previous section, the Model of Entailment symmetrically falsifies P1 and P2. Accordingly, for Popper, since the risk of falsification is symmetrical, the confirmatory effect may be as well. Assuming that, when P1 → P2, P(P1|P2)>P(P1) and P(P2|P1)>P(P2), Bayesian confirmation criteria may confirm both P1 and P2. However, hypothetico-deductive confirmation may only confirm P1.

of commitments, by virtue of corroborated predictions. Of course, helium and lead methods were later identified to be discordant.

The manner in which epistemic support is conferred when Models of Concordance are satisfied may also be influenced by the remarkableness of the concordance. Remarkableness is related to the possibility of the concordance taking place despite falsity of beliefs therein and is thereby a product of the diversity of possible alternative beliefs and the specificity of the concordance.[645] Remarkableness may be defined in different ways. As one example, remarkableness may be expressed probabilistically. In the Model of Conjunction', it may be the case that $P(E|\sim(P1 \cdot P2)) << 1$, such that E strongly confirms the conjunction of P1 and P2.[646] In the Model of Entailment', it may be the case that $P(P2|\sim P1) << 1$, such that the entailed concordance of P1 and P2 is highly remarkable. In the Model of Corroboration', given the negation of P1, it may be highly unlikely that P2 would entail E.[647]

Historical examples also support this notion of remarkableness of concordance. In 1956, John Graham argued that stress could induce or alter rock magnetism, so if the history of a formation is not known, the remanent magnetism could not be reliably established. Other prominent paleomagnetists objected to Graham's conclusion by arguing that the corroboration of paleomagnetic data across rock type, age, location, and research group was *only possible* if the assumptions involved in such paleomagnetic research were immune from Graham's claims. In this case, the collection of concordances within paleomagnetic measurements were so remarkable that many researchers deemed the very notion of widespread unaccounted sources of error to be impossible.

The confirmatory significance of the Models of Concordance may also be influenced by the independence of accepted statements. As a minimal condition, in order for the Models of Concordance to have confirmatory significance, the basis upon which proposition P2 comes to be accepted cannot be

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[645] Specificity may include conceptual and empirical dimensions, depending upon the model of concordance at hand. The diversity of alternative possible beliefs pertains to the epistemic basis upon which relevant beliefs are accepted, independent of consideration of their concordance. This may be conceived in different ways. In some situations, it may be easy to establish a limited set of alternative possible beliefs in some domain and thereby have some informed basis upon which the possibility of concordance despite falsity of beliefs therein could be estimated. In many scenarios, however, this is not possible and an intuitive expectation of the range of alternative possible beliefs might be more relevant in judgements of remarkableness.

[646] An alternative possible approach to remarkableness in the Model of Conjunction', may analyze accepted propositions P1 and P2 individually, to establish how likely it is that, if P1 were false, the conjunction of P2 with a possible P1 alternatives would entail E.

[647] Even if Models of Concordance are symmetrical, remarkableness of the concordance may not be. Thus, the strength of epistemic support conferred by concordance may not be symmetrical.

solely based on its concordance with P1.[648] Stronger notions of independence may also have relevance. For example, it may be supposed that independent empirical support for alternative statements would have bearing on the epistemic significance of their concordance.

Historical examples also support the notion that independence has epistemic relevance to concordance. Prior to 1950, convection currents within the mantle were occasionally proposed as a mechanism to account for island arcs, mountain ranges, and even continental drift. The hypothesis of convection currents thereby found some concordance with other accepted hypotheses and evidence. In 1951, Edward Bullard claimed that the convection current hypothesis would be more convincing if independent evidence could be found.[649] This was one motivating factor behind his work on heat flow measurements of the seafloor.

In summation, I have argued that Model 0 and 0' are the most frequently assumed models of prediction testing in systematic accounts of confirmation and general epistemology of science. These models assume that prediction is entailed only by individuated hypothesis, that prediction is tested only by empirical evidence, and that scientific knowledge is obtained by sequential analysis of accumulated evidence. I have also argued that there are alternative ways to model prediction testing within a set of accepted statements. The Model of Conjunction pertains to the empirical testing of prediction entailed by a conjunction. The Model of Entailment pertains to direct testing of one accepted statement by another. The Model of Corroboration pertains to indirect testing of accepted statements via empirical prediction. Models of Discordance pertain to refutation of prediction, from which conclusions deductively follow. Models of Concordance pertain to confirmation of prediction, from which epistemic conclusions may follow when some confirmation criteria are accepted. Discordance between accepted statements results in ambiguous falsification. Concordance between accepted statements has confirmatory significance. These alternative models of prediction testing challenge assumptions that are embodied by Model 0 and 0'. Prediction may be entailed by multiple commitments which may include hypotheses but also other sorts of commitments including empirical statements or other predictions. Prediction may be tested against empirical evidence but may also be tested directly or indirectly against other accepted statements. The sequential analysis of accumulating empirical evidence may omit the epistemic significance of concordance between alternative accepted statements.

[648] Note that novel predictions would have such independent support.
[649] Bullard 1951, 520

These arguments are not new. Though Models 0 and 0’ of prediction testing are commonly assumed across varied accounts of the epistemic relevance of prediction testing, the Models of Discordance and Concordance that I identified have also been identified elsewhere.[650] Additionally, assumptions inherent in Models 0 and 0’, and the widespread influence of these assumptions within the epistemology of science, have also been critiqued from many directions. A small sample of such work, pertaining to the ambiguity of falsification, has already been introduced. In the next section, I identify additional collections of literature that challenge the assumptions embodied by Models 0 and 0’ and reinforce the epistemic significance of the Models of Concordance.

Consilience

William Whewell’s *Philosophy of the Inductive Sciences* was published in 1840 in two volumes, with a second edition in published in 1847. For the third edition, published between 1858 and 1860, the work was split into three volumes – *The History of Scientific Ideas, Novum Organon Renovatum, and On the Philosophy of Discovery* - with large additions.[651] Mostly across these works, Whewell described an inductive method of science.

According to Whewell, there are fundamental ideas which are required for scientific knowledge. Such ideas may include notions of space, time, and cause. They structure empirical knowledge. These fundamental ideas are a result of an activity of the mind, independent of experience in their origin, yet constantly combined with experience in exercise.[652] Whereas Hume claims that induction is not possible, Whewell claims that the fundamental ideas are an a priori source of knowledge, independent of experience, which may facilitate induction. Whewell also did not think that Kant’s ideals could account for the formation of empirical induction.[653] For Whewell, the fundamental ideas are of divine origin, yet also allow for objective knowledge. This objective knowledge is required because, Whewell argues, though fundamental ideas may be an a priori source of knowledge, they are only grasped through the actual working of science.

[650] The Models of Conjunction and Entailment were approached from literature on predictive holism and problem solving. The Models of Concordance (and the epistemic significance of such concordance) have also been approached from other directions in philosophy of science, as will be shown.
[651] Whewell, 1840, 1847, 1858a, 1858b, 1860
[652] Whewell, 1858a, 91
[653] Whewell, 1860, 312

There are two general processes of scientific induction which Whewell refers to as colligation and explication. Colligation refers to the mental operation wherein empirical facts are united by a conceptual generalization which consists of the identification of a property that unifies members of a class. Colligations are not guessed at, nor derived solely from observation, but are special processes in the mind that allows one to infer more than what is seen. Colligations are reached through a process of explication. The explication of conceptions consists of making a concept explicit and comparing the concept to known facts to see if the concept colligates those facts. Failure to colligate, results in further refinement of the concept.

These inductive processes are not immediately perfect. Once some induction is reached, Whewell claims that it must be tested. Such testing may include prediction of new facts of the same kind employed in the initial induction. Whewell often recruited Kepler's laws as an illustrative historical case. Kepler colligated known facts on the position of Mars with the concept of elliptical motion. New observations of the position of Mars could be used to test this concept.[654]

Better yet, Whewell claims, is when a concept is able to predict facts of a different class from those employed in the formation of the initial induction. There may be false hypotheses that account for facts of one class but, according to Whewell, it is not possible for such false hypotheses, adjusted to facts of one class, to also predict facts of a different class to which the hypothesis was not adjusted.

> *The instances in which this has occurred, indeed, impress us with a conviction that the truth of our hypothesis is certain. No accident could give rise to such an extraordinary coincidence. No false supposition could, after being adjusted to one class of phenomena, exactly represent a different class, where the agreement was unforeseen and uncontemplated. That rules springing from remote and unconnected quarters should thus leap to the same point, can only arise from that being the point where truth resides.*[655]

Whewell refers to this as the consilience of inductions. Only the very best-established theories in the history of science demonstrate such consilience. He provides an example of consilience from Newtonian gravitation. From Kepler's third law, Newton found the inverse square law, which also explained Kepler's first and second laws, though no connection between these laws was apparent beforehand. Further, Newton's inverse square law of gravitation was reached as a colligation of facts

[654] Whewell, 1858b, 87
[655] Whewell, 1858b, 88

pertaining to perturbations of the moon and planets, but it also explained the precession of the equinoxes.

> *No example can be pointed out, in the whole history of science, so far as I am aware, in which this Consilience of Inductions has given testimony in favour of an hypothesis afterwards discovered to be false.*[656]

Whewell also adds that hypotheses that are true become more coherent as the hypothesis is extended. This, Whewell claims, is an extension of the test of consilience over time. True hypotheses will remain simple as they unify more classes of facts. False hypotheses will become incoherent as more modifications are made to accommodate more classes of facts as the hypothesis is extended.[657]

A precise formulation of Whewell's notion of consilience is not fully clear from his presentation. He claims that consilience pertains to predictions of a class of facts not involved in the initial induction. This may be represented as follows, where Ei is evidence that is colligated by some induction H1, and » represents Whewell's process of induction.

(Ei » H1) · (H1 → E1) · E1

Whewell also claims that consilience pertains to cases in which inductions from different classes of facts jump together. This may be represented by a different model of consilience, wherein independent sets of facts result in the same induction.

(Ei1 » H1) · (Ei2 » H1)

Whewell's example of the relation of Newtonian gravitation to Kepler's laws, raises yet another model of consilience, wherein one induction unifies other inductions.

(Ei1 » H1) · (Ei2 » H2) · (H1 → H2)

or

(Ei1 » H1) · (Ei2 » H2) · (H1 · H2 » H3)

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[656] Whewell, 1858b, 90

[657] Whewell treats theories as singular inductions that remain singular as they are extended. Alternatively, Kuhn treats theories as sets of commitments, some of which may be adjusted as a theory is extended during normal science. In some sense, this idea of systems is implicit in Whewell's characterization, but by treating inductions as singular, his characterization of simplicity and unification is not so easily translated to systematic views.

Yet, in other examples, Whewell emphasizes that some induction H1 is superior to some alternative induction H2, because H1 entails the colligated facts of H2, while H2 does not entail the colligated facts of H1. This notion of consilience seems to be akin to a measure of relative empirical content.

Whewell's presentation of consilience has garnered much attention, often in relation to his claims of particularly strong epistemic significance. However, Whewell's lack of clarity on the concept of consilience has contributed to some inconsistencies across subsequent works which makes the task of pinning down a precise definition of consilience all the more difficult.

Robert Butts claimed that consilience occurs when a theory explains more than it was originally devised to explain.[658] Larry Laudan claimed that consilience occurs when a theory is shown to be capable of successfully explaining different classes of facts or surprising facts.[659][660]Mary Hesse characterized Whewell's consilience as follows: Some theory T (perhaps reached by induction), entails empirical laws L1, L2, L3, in conjunction with additional premises A. Hesse takes L1 to be given, and L2 and L3 to have no direct evidence in their favor. Whewell is then taken to inquire about the increase in epistemic support conferred to T, when L2 is confirmed.[661] Hess further inquires with respect to confidence in L3 given confirmation of L1 and L2.[662]

It is often argued that repetitions of the same sort of evidence yield diminishing returns to a scientific theory. In a Bayesian framework, if hypothesis H entails E, repetition of evidence E yields diminishing confirmatory returns upon hypothesis H, since later repetitions of E may be deemed to be more probable (independent of H) than earlier occurrences of E. New evidence that is probabilistically independent of E, therefore, confers more epistemic support than repetitions of old evidence. This is sometimes taken as a reflection of Whewell's claims of epistemic benefits of new evidence.[663] An alternative approach to consilience within Bayesian confirmation equates consilience with a type of evidential unification. Two empirical phenomena that are deemed to be probabilistically independent may, given a certain theory, become positively relevant to one another. In such circumstances, the degree of support conferred by these two pieces of evidence includes the confirmation of the first piece

[658] Butts, 1968, 18
[659] Laudan, L., 1971
[660] These different classes being distinguished by their apparent lack of relation prior to the realization of consilience.
[661] Hesse, 1968
[662] Hesse, 1971
[663] Niiniluoto, 2016

of evidence, the confirmation of the second piece of evidence, and some additional degree of unification of these two pieces of evidence achieved by the hypothesis.[664][665]

Consilience has thereby been delineated in alternative ways, and it can be difficult to find a common thread across alternative accounts. Additionally, Whewell's strong claims about the epistemic significance of consilience are often criticized by offering examples that satisfy the conditions of a particular formalization of consilience while only intuitively providing minimal epistemic support.

I do not think that consilience can be captured by a single formalization.[666] Instead, consilience encompasses an intuition that may be satisfied under various conditions and to varying degrees. Additionally, much of Whewell's account of consilience, including his very strongest conclusions, I consider to be correct and re-expressible in a manner that can be more clearly articulated and defended. Some of this re-expression will be accomplished in subsequent chapters, but an important piece will be elaborated presently.[667]

One idea that Whewell has in mind in his account of consilience is that unexpected coincidences across independent routes of inquiry are epistemically significant. Additionally, this unexpectedness is related to the possibility that the commitments involved in the coincidence *could have been* non-coincidental. Indeed, they would be expected to be non-coincidental if one or both routes of inquiry were misdirected in some way. This much is clear from some of Whewell's claims that have already been addressed and also from the following illustrative cases.

> *I may compare such occurrences to a case of interpreting an unknown character, in which two different inscriptions, deciphered by different persons, had given the same alphabet. We should, in such a case, believe with great confidence that the alphabet was the true one*.[668]

> *The testimony of two witnesses on behalf of the hypothesis; and in proportion as these two witnesses are separate and independent, the conviction produced by their*

[664] Myrvold, 2003, 2017; Shupbach 2005
[665] For non-Bayesian accounts of consilience as unification see Wilson, 1999; Snyder, 2005.
[666] This is somewhat similar to Laudan, who associates consilience with three circumstances: when hypothesis H explains known classes of facts or laws L1 and L2, when hypothesis H successfully predicts cases of a kind different from those contemplated in the formation of the hypothesis, and when H predicts or explains a phenomenon which would otherwise not be expected (Laudan, L., 1971).
[667] The notion of "snapping together" described in Chapter 8 may also be viewed as an account of consilience.
[668] Whewell, 1860, 274-275

agreement is more and more complete. When the explanation of two kinds of phenomena, distinct, and not apparently connected, leads us to the same cause, such a coincidence does give a reality to the cause, which it has not while it merely accounts for those appearances which suggested the supposition.[669]

This intuition about the epistemic significance of unexpected coincidence is also captured in my assertion that the Models of Concordance have epistemic significance. It is my contention that the Models of Concordance may thereby be conceived as models of consilience. Unlike Whewell's approach, the Models of Concordance do not stipulate that concordant routes of inquiry be of a certain inductive character. Instead, the Models of Concordance only require that concordant concepts be accepted statements with some independent basis for that acceptance. The Models of Concordance also allow for identification of unexpected coincidences across a broader range of circumstances than Whewell may have recognized.

I have drawn upon theories of confirmation to briefly elucidate the potential epistemic significance of the Models of Concordance. Often, these theories of confirmation distinguish the epistemic significance of testing outcomes based on the plausibility of the confirmation taking place, were the tested hypothesis false. When it is recognized that prediction testing can be modeled in alternative ways, this distinction of epistemic significance may be applied not only to empirical testing outcomes, but to alternative types of concordances as well. In the previous section, I described the remarkableness of concordance as related to the possibility of concordance taking place despite the falsity of beliefs therein. This, in turn, depends upon the specificity of the concordance, but also the diversity of alternative possible beliefs. In some cases, there may be known sets of alternative possible beliefs that fashion this judgement of remarkableness. For example, the Vine Matthews hypothesis combined seafloor spreading with the geomagnetic field reversal hypothesis to account for patterns of marine magnetic anomalies. Seafloor spreading offered an account of the origin of ocean basins, but there were several known alternatives.[670] There were also alternatives to the geomagnetic field reversal hypothesis that could account for apparent polarity reversals in remanent magnetism of a lava flows.[671]

[669] Whewell, 1847, 285
[670] Alternatives included Menards stretching hypothesis, Heezen or Carey's expansionsm, general permanentism, intermittent spreading, and even the notion that ocean basins were produced by impact events.
[671] Self-reversals could account for apparent polarity changes. The Earth's magnetic field may not have been dipolar in the past. Local magnetic effects might also distort natural remanent magnetism.

Alternatives to seafloor spreading did not seem to concord with the geomagnetic reversal hypothesis.[672] Alternatives to the geomagnetic reversal hypothesis did not seem to concord with seafloor spreading. Alternatives to both seafloor spreading and field reversals typically seemed entirely ambivalent with respect to one another. Finally, there were many ways in which marine magnetic anomalies could be accounted in a manner that did not involve seafloor spreading or field reversals. This made the eventual confirmation of the Vine-Matthews all the more compelling: If seafloor spreading and/or geomagnetic field reversals were false, their concordance might seem inexplicable.

The notion of the underdetermination of scientific theory may also explicate this notion of alternative possible beliefs. In the strongest possible sense of underdetermination, there may be infinitely many alternative beliefs that may account for a body of evidence equally well. Underdetermination is often considered to be a significant obstacle to strong epistemic claims, such as scientific realism. Underdetermination is also often considered to be unavoidable, as is the case when underdetermination is associated with the problem of induction. Now, if alternative beliefs are independently underdetermined, then their concordance may be highly unexpected. In the strongest possible circumstances, such concordance would be infinitely unlikely, were the concordant beliefs false. Thus, one of the most significant obstacles to strong epistemic claims may actually facilitate the strongest possible prediction testing conditions, thereby justifying some of Whewell's strongest conclusions when conditions are right.

During the 1960s, two research groups, one at USGS and one at ANU, developed a geomagnetic field reversal timescale. Rock samples were obtained from recent lava flows in Hawaii, North America, Europe, and Africa. These samples were obtained from rock dated by the potassium-argon method. This research was conducted primarily out of interest in testing the viability of the geomagnetic field reversal hypothesis, and in developing a precise timescale that could be used to correlate research in geology and paleontology. The main concerns in the development of this research pertained to the precision and reliability of the potassium-argon method, the rigor of fieldwork practices to effectively correlate dated rock with magnetically measured samples, and limited data points which allowed for multiple interpretations of reversal events and durations.

Also, during the 1960s, Frederick Vine, and others, established an alternative means of identifying the history of geomagnetic field reversals. Marine magnetic anomalies across ridge axes

[672] A possible exception would be in the case of Earth expansion, which may be viewed as a variant of seafloor spreading in this case.

were measured by towed fluxgate or proton precession magnetometers. The anomaly patterns could be interpreted as the product of past geomagnetic field reversals and seafloor spreading. This research was primarily conducted by marine geologists and geophysicists, to better understand the structure and evolution of the seafloor. The main concerns in the development of this research pertained to the association of anomalies with ridges, the general linearity of anomalies and their parallel orientation to ridge crests, symmetry across ridge crests, global correlations of anomaly patterns, and unknown physical parameters which allowed for multiple interpretations of magnetic data.

At Lamont, Neil Opdyke established yet another way to study the timing and relative duration of geomagnetic field reversals. Using a spinner magnetometer with a slow spin rate, the magnetic properties of discs of cored seafloor sediment could be measured. Changes in magnetic polarity within a cored column, along with assumptions on seafloor sedimentation rates, could be used to establish relative dates and durations of reversals. The main concerns in the development of this research pertained to the weak remanent magnetism of seafloor sediments and the sensitivity of the magnetometer, global correlations across cored sites, and variations in global and local historical sedimentation rates.

By the end of the 1960s, these three means of establishing historical geomagnetic field reversals were frequently and effectively corroborated against one another. The study of terrestrial lava flows was corroborated by seafloor sediment samples. Discoveries made by Opdyke could be checked against magnetic profiles. Magnetic profiles could be used to identify undiscovered reversal events that were then corroborated by terrestrial samples. The terrestrial study of field reversals in lava flows preceded research of seafloor sediments and magnetic profiles. The USGS and ANU results were known to Vine and Opdyke when they began their research. However, each of these three strands of inquiry include unique sets of researchers, institutions, instruments, objects of study, fieldwork practices, sampling locations, data processing methods, and background theories. It is also apparent from a close examination of the historical contexts of this work, that these trajectories of research were largely directed by independent sets of concerns. Additionally, it is also apparent that independent strands of research could have yielded alternative sets of beliefs that would not have concorded. For example, marine magnetic anomalies could be interpreted as petrological differences in the seafloor, entirely unrelated to field reversals.

Contemporary researchers were explicit that the agreement between these three trajectories of research conferred significant epistemic support to the geomagnetic field reversal hypothesis and also to seafloor spreading. In his Bakerian Lecture in 1967,[673] Edward Bullard claimed that the agreement between seafloor sediments and terrestrial studies was striking and, "the evidence compels belief in reversals of the Earth's field as the cause of these reversals of magnetization*",* in part because,

> *No two substances could be more different or have more different histories than the lavas of California and the pelagic sediments of the Pacific… If these two materials tell the same story then it must be the story of an external influence working on both and not a story of recurrent synchronous change in the two materials.*[674]

The concordance of marine magnetic anomalies, Bullard said,

> *is a claim so exorbitant in a subject where things are rarely clear-cut or predictable, that it is necessary to be very sure that we are not deceiving ourselves. The agreement of the calculated and observed curves is so good that it seems impossible to ascribe it to chance.*[675]

Bullard added that it might be possible for such concordance to take place if there were many parameters that could be arbitrarily adjusted to manifest a concordant pattern.[676] Since Vine and others working on magnetic profiles were aware of the terrestrially derived reversal timescale, they might be able to manipulate their results to fit established commitments.[677] However, when examining the standards employed by Vine and others in their work on magnetic anomaly profiling, Bullard concluded that this was not possible, and that this verified the Vine-Matthews hypothesis.[678][679] Note, that Bullard's concern, here, relates to the matter of independent support for concordant beliefs. If some parameter or set of parameters is adjusted only for some desired concordance, then this would violate the minimal demand for independent epistemic support.[680]

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[673] Bullard delivered the Bakerian Lecture as the 1967 recipient of the Bakerian Medal of the Royal Society.
[674] Bullard, 1968, 489
[675] Bullard, 1968, 497
[676] He said, "anything can be fitted convincingly to almost anything if enough arbitrary constants are available" (Bullard, 1968, 498).
[677] In Chapter 8, I refer to this as "special fitting".
[678] Bullard, 1968, 499
[679] Bullard notes that the arbitrarily adjusted parameters in magnetic profiling include fixing the horizontal scale, the vertical scale, and the increased magnetization of the central block. Recall, however, that Vine's 1965 paper included an additional arbitrarily manipulated parameter pertaining to variations in spreading rate.
[680] A warning to the same effect was articulated by Ted Irving in 1961, pertaining to reliance upon corroboration in paleomagnetism. Only when rigorous practices were upheld would such concordance have epistemic relevance (Irving, 1961).

I have argued that the concordance of independently supported beliefs has epistemic significance. Additionally, some concordances are more epistemically significant than others. In the strongest cases, highly independent research trajectories are found to concord in a manner that would be highly unlikely if one or both of the concordant beliefs were false. In such circumstances, which may include concordance between independently underdetermined beliefs, some of Whewell's strong claims about the epistemic strength of consilience may be justified.

Coherence

Attempts to systematically represent the epistemic bearing of prediction testing and accounts of scientific knowledge in general often identify some privileged type of commitment(s) as indisputable or self-justified. For empiricists, the self-justified beliefs are empirical facts or states of affairs. For rationalists, self-justified beliefs may include self-evident analytic statements or beliefs deduced from other self-evident truths. A set of self-justified beliefs may then serve as a foundation upon which additional beliefs, such as scientific hypotheses, may be justified. Such *foundationalism* envisions all knowledge as chains of justification anchored to self-justified beliefs.

A general problem with this approach to justification of belief is that it is quite difficult to unproblematically delineate self-justification. For example, the logical empiricists attempted to combine empirical and rational notions of self-justification, wherein analytic statements could be true a priori, and empirical statements could, at least in principle, be verified by a neutral, agreeable observational language. However, in 1951, W.V.O. Quine argued against the analytic-synthetic distinction, and by the 1960s, Norwood Hanson, Thomas Kuhn, and Paul Feyerabend argued against the supposed objectivity of empirical evidence.[681][682]

Quine considered the apparent distinction between analytic and synthetic statements to be a product of the notion that statements can be justified independently.[683] Alternatively, Quine envisioned scientific knowledge as a web of belief. Statements within the web are logically related to other statements. The web of belief confronts observation, but only as a corporate body. Recalcitrant observation is ambiguous and can be accommodated by modifying the web in many different ways.

[681] Quine, 1951; Hanson, 1958; Kuhn, 1962; Feyerabend, 1975
[682] Popper likened the foundation of knowledge to driving piles into a swamp (Popper, 2002a, 94). Neurath argued that scientific knowledge lacks a foundation and acts like a raft (Neurath, 1973).
[683] Quine, 1951

Indeed, the ambiguity is so great that any individual statement can be maintained "come what may" by sufficiently drastic modifications elsewhere. Even the rules of logic and mathematics that structure the web are themselves part of this web.

Mounting objections to the foundationalism of the logical empiricists contributed to increasing recognition of the relevance of coherence within a set of beliefs to the justification of those beliefs. In the weakest sense, foundationalism could be amended by proposing that the special class of self-justified beliefs may somehow be enhanced or diminished in relation to other beliefs. For example, empirical evidence may be deemed more or less reliable based on method of acquisition, or empirical evidence that contradicts well established theory may warrant greater skepticism. In the strongest sense, the distinction of privileged self-justified beliefs could be discarded entirely if coherence within a set of beliefs is itself considered to be justificatory in some way.

In his 1985 work, *The Structure of Empirical Knowledge,* Lawrence Bonjour developed a coherence theory of empirical knowledge.[684] He argued that justification is often presumed to be linear in character, involving a one-dimensional sequence of beliefs wherein self-justified beliefs serve as the foundation of justification, but that this presumption of linearity is incorrect.[685] Linearity is typically presumed of the justification of singular empirical beliefs, but Bonjour claims that justification actually pertains to entire systems of belief.[686] Justification within systems is non-linear. There is no privileged set of beliefs and no singular direction in which justification moves. Rather, justification of beliefs within a system is reciprocal. It is the coherence of the system that justifies belief. In this view, empirical statements are not self-justified, but justified by coherence with a system, though the causal origin of such empirical belief may be independent from that system and even non-inferential. Additionally, a system of knowledge may include the belief that empirical evidence obtained under certain conditions tend to be coherently justified.

Bonjour claims that coherent systems must be logically consistent, and the degree of coherence is proportional to the degree of probabilistic consistency. However, coherence is not just about consistency. It is also about positive epistemic connection. Coherence of a system is increased by the number and strengths of inferential connections between component beliefs.[687] Alternatively,

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[684] Bonjour, 1985
[685] Bonjour, 1985, 90
[686] Indeed, the apparent self-justification of some statements is predicated upon the acceptance of such a system.
[687] For Bonjour, such inferential connections are any relation of content within the beliefs such that the truth of one belief (or a set of beliefs) could serve as the premise of a justification argument for an alternative belief.

coherence of a system is decreased to the extent to which the system is divided into unconnected subsystems, and in proportion to the presence of unexplained anomalies within the system.[688]

In *Evidence and Inquiry,* Susan Haack attempts to develop a *foundherentist* account of justification.[689] Unlike foundationalism, Haack argues that there are no basic beliefs, and justification is not unidirectional. Unlike coherentism, Haack aims to make justification responsible to experience. She distinguishes between causal and evaluative features of experiential evidence. An individual has a set of belief states (sensory and introspective states) which are of causal relation to some belief p in that these states my sustain or inhibit the individual's belief in p. The same individual also has a set of belief contents with respect to p, expressible as propositions, some representing belief states, and others representing the contents of those belief states. How justified an individual is in believing that p, depends on how good the belief contents are with respect to p (which in turn also inculcates the set of belief states that causally sustain or inhibit belief in p). Haack models the goodness of the belief contents upon a crossword puzzle. When solving a crossword puzzle, entries are justified by how well they match a corresponding clue, but also by intersections with other entries, how reasonable those other entries are independently, and how much of the crossword has been completed. Haack claims that the clues are like experiential evidence, and intersecting entries are like background beliefs. She does not provide a detailed description of the relationships between beliefs that could be described as "intersections", though she does claim that such intersections may be conceived of as explanatory connections.[690] A full account of justification thereby does not privilege the role of empirical evidence nor coherence. Indeed, those propositions expressing belief states stand in some coherent explanatory relation to other propositions.

Numerous attempts have been made to identify a Bayesian measure of coherence, with a general question of when or whether such coherence is truth conducive.[691] Toji Shogenji defined the degree of coherence within a set of beliefs P1 and P2 to be P(P1|P2)/P(P1) which equals

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Bonjour claims that such a measure of coherence comes in alternative strengths. In the strongest case, each belief within a system my entail, and may be entailed by, the rest of the system. In weaker characterizations, the antecedent probability of any one belief within a system will be increased if the remainder of the set is given.

[688] In this case, anomalies are facts or events which are claimed to obtain by one part of a set of beliefs, but incapable of being predicted by other beliefs.

[689] Haack, 1993

[690] I use the model of a crossword puzzle in Chapter 8 with concordances acting as crossword "intersections".

[691] An alterative approach to the epistemic bearing of coherence in a Bayesian framework inquires when or whether the coherence of evidence increases the transmission of probabilistic support to a hypothesis. Often, these distinct ideas are conflated in Bayesian literature. There is also some overlap, at times, with Bayesian approaches to unification or consilience.

P(P1·P2)/P(P1)·P(P2).[692] Intuitively, this measures the degree to which two beliefs are more likely to be true together than individually. Olsson (2002) and Glass (2002) offered alternative measures of the degree of coherence of two beliefs P1 and P2 as P(P1·P2)/P(P1vP2).[693] Intuitively, this measures the degree to which the total probability of either P1 or P2 resides at the intersection of P1 and P2. By holding other possible variables within alternative sets of beliefs constant, a set of beliefs that has a greater measure of coherence – by either of the measures noted above – will also have a higher probability than an alternative set with a lower measure of coherence.[694] There are alternative possible Bayesian measure of coherence.[695] It is important to note that such alternative probabilistic measures may distinguish the relevance of alternative features of a set of statements in different ways. For example, in Shogenji's measure of coherence, the greater the number of individual beliefs, the lower the total individual strength of the information set will be,[696] thereby increasing the measure of coherence. Alternatively, the measure of coherence offered by Olsson and Glass is insensitive to the gross number of beliefs.[697]

Across a series of publications spanning over twenty years, Paul Thagard developed a particularly thorough account of coherence based on explanatory relationships.[698] Thagard argues that both the coherence of a set of propositions and the coherence of a single proposition within a set are derived from fundamental explanatory coherence relations between pairs (or small sets) of propositions. He identifies these relations as follows:

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[692] Shogenji, 1999
[693] Olsson, 2002; Glass, 2002
[694] What is to be held constant is subject to debate and is dependent upon the definition of coherence provided. Shogenji, for example, argues that prior probabilities should be held constant (Shogenji, 1999). Bovens and Hartman argue that reliability of information sources and joint probabilities should be held constant (Bovens and Hartmann, 2003). Olsson inquires as to whether any other epistemically relevant variables should be held fixed, including degrees of independence, specificity, set size, or the size of possible states of affairs (Olsson, 2005). Shupbach adds that such ceteris paribus concerns influence whether Bayesian coherentism is possible (Shupbach, 2008). In my view, this interest in ceteris paribus conditions is based on an attempt to define coherence while also arguing that coherence *in general* is truth conducive. This seems a problematic approach, especially since alternative accounts of the epistemic bearing of coherence do not necessarily imply that coherence is or should be preferable ceteris paribus. Accordingly, I question the general relevance of the Bayesian approach to coherence beyond strictly Bayesian research goals.
[695] Fitelson, 2003; Shupbach, 2011
[696] This is what Shogenji claims the denominator of his measure of coherence measures.
[697] Other features of a belief set that may or may not have bearing upon Bayesian measures of coherence of that set include prior probabilities, specificity of individual beliefs, independence of beliefs, reliability of means by which beliefs are established, size of the set of possible states of affairs with respect to the locus of belief (Olsson 2005).
[698] Thagard 1989, 1992, 1998, 2000, 2005, 2007, 2012

- Explanation
    - If P explains Q, then P and Q cohere
    - If P1 and P2 together explain Q, then P1 and P2 cohere
- Analogy
    - If P1 explains Q1 and P2 explains Q2 and P1 is analogous to P2 and Q1 is analogous to Q2, then P1 and P2 cohere and Q1 and Q2 cohere
- Competition
    - If P1 and P2 both explain Q, and if P1 and P2 are not explanatorily connected elsewhere, then P1 and P2 incohere

The explanandum "Q" in the explanatory relationships identified above, may be satisfied by empirically derived evidence or another hypothesis. Additionally, explanatory coherence comes in degrees. The more hypotheses that are required to explain something, the lower the degree of coherence.

In addition to these relationships of explanatory coherence, Thagard identifies several additional principles relevant to explanatory coherence.

- Contradiction
    - If P1 contradicts P2 then P1 is incoherent with P2
- Symmetry
    - If P and Q cohere, then Q and P cohere
    - If P and Q do not cohere, then Q and P do not cohere
- Data Priority
    - Propositions that describe results of observation (data) have a degree of acceptability on their own
- Acceptance
    - Acceptability of a proposition with a system depends upon its coherence with that system

Thagard conceives of scientific knowledge as a weak foundationalist explanatory network, wherein data has some degree of self-justification, but other propositions are justified by coherence within the system. A new belief my replace a competing belief if the new belief better coheres with the broader system of commitments. However, a candidate for incorporation into a system may cohere with some constituent commitments and incohere with others. Additionally, competing propositions may cohere and incohere with the system in alternative ways. Greater explanatory coherence cannot thereby be measured by simply counting coherent relationships, since the acceptability of a new belief, in part, depends upon the acceptability of other beliefs in the system (and vice versa).

Thagard thereby develops a computational model of coherence evaluation. A network of explanatory relationships is mapped, with nodes representative of beliefs (including hypotheses and data), and connections between nodes representative of coherent and incoherent relationships. The nodes carry with them a level of "activation" which is defined in relation to coherent connections which are excitatory, and incoherent connections which are inhibitory. Data nodes are given a starting activation value as they are presumed to be self-justified to some degree.[699] Activation then spreads from the data nodes throughout the network. As coherent connections boost node activation and incoherent connections inhibit activation, different nodes within the network obtain alternative activation levels. The activation of data nodes will be altered based on their coherence or incoherence with other nodes in the network. This cycle of activation updating may then be repeated until stability of activation is reached. For Thagard, this activation represents the acceptance or rejection of the belief and thereby models scientific change and decision making, in general agreement with inference to the best explanation.

Thagard has applied this model to assorted case studies in the history of science, including the early dispute over continental drift between Alfred Wegener and his opponents.[700] Wegener's argument for continental drift consisted of a set of evidence and a set of explanatory hypotheses. Wegener also explicitly compared his evidence and hypotheses against the beliefs of his opponents. Some of the evidential claims that Wegener endorsed were accepted by his opponents while others were in dispute. For example, Wegener argued that oceanic crust and continental crust were of different composition, but contractionists, against whom Wegener directed his argument, seemed to maintain that oceanic crust was merely subsided continental crust. Wegener also recognized that his opponents endorsed explanatory hypotheses that could account for much of the available evidence, yet contradicted Wegener's favored hypotheses.

[699] This starting value is based on presumed security of data inferences, such that some data nodes, even in their initial state, may be more strongly activated than others.
[700] Thagard and Nowak, 1988

Thagard maps out explanatory coherence relationships between all these propositions in an attempt to make sense of why Wegener favored continental drift over alternative explanations offered by his opponents. The resulting blueprint of Wegener's argument is then subjected to the computational model of coherence evaluation. Thagard also assumes that different individuals or groups may differentially delineate the set of evidence demanding explanation and associated explanatory hypotheses. Accordingly, though Wegener's delineation of evidence and hypotheses results in higher explanatory coherence for his favored hypotheses, Thagard claims that this was not the case for his opponents who found greater explanatory coherence in alternative hypotheses. The general conclusion that Thagard seems to defend is that scientific arguments and decision making is fashioned to maximize explanatory coherence.

In summation, the coherence of scientific belief is often recognized to have epistemic significance, though coherence and the nature of that epistemic significance can be approached in different ways. Bonjour aims to provide an account of justification that does away with the privileging of self-justified empirical statements, while Haack attempts to do away with the directionality of foundationalism, arguing that justification can move in all directions. For Quine, the approach to coherence is through semantic logical relations, where terms obtain meaning in relation to other terms. For Bayesians, of course, coherence is characterized by conditional probabilities between a set of facts or beliefs. This may include entailed relationships or otherwise. Finally, Thagard defines coherence in relation to explanatory relationships, which may be entailed relationships or otherwise.

Despite their different approaches, these varied accounts of coherence present scientific knowledge in a manner that does not cohere with the assumptions embodied in Models 0 and 0' of prediction testing. Accounts of coherence challenge the functional epistemic distinction between hypothesis and evidence and thereby challenge the notion that observation is the sole arbiter of scientific change. Additionally, the growth of scientific knowledge cannot be modeled simply by the sequential analysis of accumulating observation. Rather, sets of accepted propositions are epistemically relevant to one another.

The Models of Concordance, identified previously, may be conceived as modeling a certain kind of entailed coherence between commitments.[701] Indeed, the Models of Concordance already find some

[701] Additionally, the Models of Discordance, identified previously, have obvious bearing upon general notions of logical incoherence. As I argued previously, these models are deductively valid given minimal commitments.

expression in other works on coherence, as illustrated in Thagard's explanatory coherence.[702] Though Thagard considers explanatory relationships to be coherent, entailed relationships can be explanatory. Accordingly, there is a clear possibility that explanatory coherence may overlap with notions of entailed coherence, and some of this overlap is apparent when the Models of Concordance are compared with Thagard's principles of explanatory coherence. Thagard's principle of explanation states that if P explains Q, then P and Q cohere. This is clearly isomorphic with the Model of Entailment. If P and Q are taken as accepted statements and P explains Q by entailing Q, this principle of explanatory coherence encompasses the Model of Entailment. Thagard's principle of explanation also states that if P1 and P2 explain Q, then P1 and P2 cohere. This is clearly isomorphic with the Model of Conjunction. If P1 and P2 are accepted statements, and their conjunction explains Q by entailing Q, then this principle of explanatory coherence also encompasses the Model of Conjunction.

With respect to possible explanatory relationships, the Model of Corroboration can be conceived in different ways. Some proposition P1 may explain Q, and some alternative proposition P2 may also explain Q. If P1 and P2 are not explanatorily connected elsewhere, then Thagard would consider these to be competing explanations that, together, incohere. Since entailed relationships may also be explanatory, this would seem to contradict the notion that the Model of Corroboration has epistemic significance in at least some circumstances. However, it should be recognized that alternative true explanations may be given for the same explanandum and Thagard's principle of competition could be amended to reflect this: explanations are only in competition when they are discordant. Additionally, in suitable circumstances, the explanans in an explanatory relationship may itself be entailed by the explanandum. For example, a collection of propositions pertaining to radiometric dating and measurements of a radiometric ratio may be used to derive the age of a rock sample, and this derived age may then explain the measured radiometric ratio. Accordingly, if P1 entails Q and P2 entails Q, it may be the case that Q explains P1 and Q explains P2. Thagard's principle of explanation does not have it that this relationship would cohere P1 and P2, though such a principle does not seem out of line with those that he does provide.[703] Yet another possibility is to consider epistemic significance of the Model

[702] By highlighting these similarities, I do not intend to equate the Models of Concordance with Thagard's coherence or with the common notion that sets of propositions that cohere are preferable to sets of propositions that do not.

[703] It is odd that in the principle of analogy Thagard allows coherence of an explanandum with another explanandum, but this is not addressed in the principle of explanation. Thagard's chief example of the principle of analogy pertains to Darwin's analogy between artificial and natural selection. Yet, even in this chief example, there is no clear analogy in the explanandum between the analogical explanans. I am thereby compelled to think that Thagard's principle of analogy overlaps with the Model of Corroboration. For example, a set of assumptions about

of Corroboration to be a related to explanatory coherence of a meta-hypothesis that pertains to the reliability of P1 and P2. Regardless, even if Thagard's principle of explanation is not isomorphic with the Model of Corroboration, it is still apparent from the sum of Thagard's principles that propositions that satisfy the Model of Corroboration would cohere indirectly.

In the previous section, I argued that the Models of Concordance also model consilience and that the epistemic significance of consilience comes in degrees. In this section, I claimed that the Models of Concordance and associated commitments overlap with general tendencies in literature on coherence and notions of coherent relationships. This relation between consilience and coherence, mediated by the Models of Concordance, facilitates some relevant insights. A set of concordant commitments may be said to cohere, even when the epistemic significance of consilience within that set is inconsequential. Additionally, the tendency within literature on coherence to examine gross coherence between sets of propositions may overlook the epistemic relevance of consilience between members of those sets. That is, some coherent relationships (of high consilience) may be more epistemically significant than others.

Experimental Knowledge and Realism

The models of prediction testing identified previously have affinity to still other collections of literature.

The Model of Corroboration is apparent in certain notions of robustness. Early explication of robustness aimed to identify strong knowledge within sets of commitments known to be incomplete or incompatible.[704] If some data permits only a limited set of possible beliefs, and each of these beliefs agree on some entailed outcome, then that outcome is certain even when the beliefs are not.[705] An alternative approach to robustness supposes that something epistemically significant takes place when experimental, or measured, or entailed outcomes agree, despite variations in the conditions or commitments through which those outcomes are reached. There are two general ways to think about

radioactive decay, along with the age of a certain rock sample may explain multiple radiometric ratios across alternative dating methods. Such alternative radiometric measurements would seem to be analogically similar, such that the principle of analogy would suggest that these alternative measurements cohere. Of course, as previously noted, the age of a rock sample is itself derived from such radiometric measurements.

[704] Levins, 1966

[705] Woodward, 2006

this epistemic significance. The first, is that variations allow for alterative possible sources of error to influence an experiment, or measurement, or entailed outcome, and some analysis of these variations my thereby identify or eliminate such sources of error.[706] The second, is that agreement between experimental, measured, or entailed outcomes may confirm a particular hypothesis if that agreement should not take place if that hypothesis was false. For example, a causal hypothesis may associate a particular phenomenon with a particular cause, such that variation in conditions not deemed to be causally relevant to the phenomenon should be expected to have no effect on the phenomenon. The robustness of such experimental outcomes – in this case, the stability of the phenomenon across variations in experimental conditions – would thereby confirm the causal hypothesis.[707] Similar notions of robustness are sometimes also applied to agreement between predictions entailed by alternative sets of commitments, even when those predictions are not empirically tested.[708]

The epistemic significance of robustness is often applied to the locus of agreement, thereby increasing confidence in a robust prediction or phenomenon, but robust results are also sometimes considered to have epistemic bearing upon the means by which the robust agreement come about. For example, literature on epistemology of calibration sometimes draws upon robust results to argue for the reliability of measurement procedures.[709] Such procedures, including instruments, are often calibrated against certain data, even though these data are often delineated by alternative measurement procedures. It is frequently argued that calibration of a novel measurement procedure against an entrenched measurement procedure can increase confidence in the novel approach when results are coherent. Such calibrations of measurement procedures result in calibration networks that can link different procedures together. Such networks are often described as coherent or consilient, in that measurements from alternative adequately calibrated procedures ought to produce measurements that agree to some degree of accuracy and precision.[710] Incoherence within such a network may result in re-calibrations, and this may even eliminate the reliability of some measurement procedures in order to retain network coherence. Calibration is thereby sometimes presented as an iterative process, wherein one change to a calibrated network may require reconsideration of multiple measurement procedures, which may require reconsideration of still others.[711]

[706] A similar sentiment will be elaborated in Chapter 8, with respect to the contextuality of "special fitting".
[707] Shupbach, 2018
[708] Lloyd, 2010; Parker, 2011
[709] Basso, 2017
[710] Bokulich, 2020: Tal, 2017
[711] Chang, 2004; Bokulich, 2020

Ian Hacking asks, “do we see through a microscope?” and argues that robust results across microscopes of variable functionality facilitates the elimination of aberrations and thereby justifies great confidence in microscopy.[712][713][714] Additionally, Hacking argues that manipulation or intervention in the ostensible entities of microscopic inquiry facilitate still stronger conclusions about entity realism.[715] Hacking’s argument for entity realism is well known, but not clearly formulated. With respect to subatomic entities, Hacking claims, “if you can spray them, then they are real”.[716] He suggests that we often set out to build new devices that illicit anticipated effects based on knowledge of causal properties associated with theoretical entities like electrons. Furthermore, these efforts often successfully produce the anticipated results, and can be used to study other phenomena. Hacking’s argument for entity realism can be construed in many ways.[717] One possible interpretation is that the capacity to reliably predict outcomes of novel arrangements of physical conditions and theoretical entities demonstrates a degree of causal knowledge that justifies belief in the realism of those theoretical entities. Such predictive success would be an impossible coincidence otherwise. This interpretation of Hacking’s argument for entity realism maps onto the Model of Conjunction and the notion of highly consilient concordance therein.[718] Hacking’s argument may then be generalized to apply to highly consilient conjunctions elsewhere. Accordingly, it is my contention that the epistemic basis for Hacking’s notion of entity realism applies far more broadly than just to circumstances wherein manipulation of physical conditions induce predictable outcomes.[719] Some approaches to realism in historical sciences emphasize the epistemic significance of historical entities or events that facilitate continuous associations and

[712] Hacking, 1985
[713] Hacking often endorsed a view of experimental knowledge as being distinct from theoretical knowledge and, at least at times, free from systematic direction or obvious interpretation. This is slightly at odds with the previous paragraph on calibration which highlights certain commitments as integral to the development of experimental knowledge. By applying the Models of Concordance to literature on experimental knowledge, I am endorsing the general notion that experimental knowledge involves conceptual commitments and prediction testing.
[714] Hacking directed part of his discussion on microscopes to Van Fraassen’s constructive empiricism. Van Fraassen is interested in distinguishing between observables and unobservables to portray science as an empirically adequate venture, rather than a venture that aims toward truth. Hacking’s question about seeing through microscopes is then a question of the distinction between observables and unobservables, and the possibility of robustness allowing for what might seem to be unobservables becoming observable.
[715] Hacking, 1983, 1985
[716] Hacking, 1983, 22
[717] Resnik, 1994; Reiner and Pierson, 1995; Miller, 2016
[718] Note that this is one possible interpretation of Hacking’s argument for realism. A strong version of Hacking’s argument could also be pulled from the argument I develop in Chapter 8.
[719] Manipulation of physical arrangements is a challenge in many areas of scientific inquiry, including plate tectonics.

insights.[720] This sort of approach to realism in historical sciences may also be interpreted as an appeal to highly consilient conjunction, but with Hacking's manipulability replaced by a continuous capacity to provide additional insights into expanding lines of inquiry.[721]

Literature on scientific realism often endorses the notion that certain pieces of scientific knowledge that re-appear across episodes of scientific change are of particular epistemic importance.[722] In the broader context of the realism debate, this argument is directed against the pessimistic induction from the history of science. Often, this re-appearance of scientific knowledge is exemplified by historical case studies in physics, wherein components of a defunct science are entailed by some set of commitments of successor science. A classic example of this is the similarity in mathematic structures between Maxwell's electromagnetism and Fresnel's defunct optics which incorporated a luminiferous ether. This defense against pessimistic induction parallels the more general disposition in the study of scientific change, that successor science ought to explain the successes of defunct science. These approaches to the epistemic significance of re-appearing scientific knowledge exemplify the Model of Entailment.

This section on experimental knowledge and realism, along with the previous sections on coherence and consilience provide a fairly detailed case for a rather modest conclusion. The conclusion is that the Models of Concordance have epistemic significance, and recognition of this epistemic significance is not new. To reach this conclusion, I showed that the assumptions embodied by Models 0 and 0' of prediction testing have been challenged from many directions. Additionally, the Models of Concordance can aid in the interpretation of collections of literature pertaining to the epistemic support of both experimental knowledge and higher-order theoretical structures.

Conclusion

In the first half of this chapter, I identified two models of prediction testing – Model 0 and 0' – and I claimed that these models are often presupposed in attempts to account for the epistemic

[720] Stanford, for example, argues that contemporary processes that can be projected into the past limit the possibility of unconceived alternatives to the hypothesis of the organic origin of fossils (Stanford, 2010).
[721] Turner argues that there is an asymmetry in manipulability between small objects like electrons and historical objects (Turner, 2007). Other approaches to realism in historical sciences include questioning the distinction of the "observablility" of historical entities like dinosaurs, as compared to theoretical entities like electrons (Cleland, 2002; Carman, 2005).
[722] Psillos, 1994; Cordero, 2011; Harker, 2013; Vickers, 2013

significance of prediction testing. These models carry certain assumptions with them, and these assumptions have had widespread influence in the philosophy of science. I identified additional models of prediction testing that apply within sets of accepted statements.[723] The Models of Discordance model circumstances wherein prediction is refuted, while the Models of Concordance model circumstances wherein prediction is confirmed. Sets of statements may thereby be said to be concordant or discordant.

The Models of Discordance were approached from literature on falsification, predictive holism, and problem solving. Discordance unambiguously falsifies the set of discordant propositions, but the isolation of this falsification is ambiguous within a field of choice. The Models of Concordance are symmetrical with the Models of Discordance and represent an important class of solved problems, wherein a discordant set of commitments is modified to produce a concordant set of commitments. I argued that concordance within a set of commitments has epistemic significance, and I supported this argument from two directions. First, since the Models of Concordance are models of prediction testing, I claimed that concordance can be mapped onto theories of confirmation. Second, I showed how the Models of Concordance align with other collections of literature on epistemic significance. The Models of Concordance and Discordance thereby facilitate the integration of diverse collections of literature into a framework of prediction testing, while also challenging problematic assumptions that are frequently associated with epistemology of prediction testing.

On route to making this argument, many examples from Part 1 of this work were used to illustrate concordance and discordance. These historical examples illustrated the importance of discordance and associated ambiguity of problem solving in directing scientific inquiry. Practicing scientists also recognized the epistemic significance of concordance. Sometimes, this concordance took place within a certain strand of research, as illustrated by the prominent role that corroboration had in reinforcing methods and convictions in paleomagnetism and radiometric dating. Concordance also took place between broader strands of research as well. During the 1960s, three independent lines of inquiry – paleomagnetism, geochronology, and marine geology - became concordant in several notable ways, and these concordances influenced the reception of mobilism. Concordances with respect to the corroboration of dating geomagnetic field reversals was already examined in this chapter. Other notable

[723] Though I related the Models of Concordance to theories of confirmation, I remained agnostic with respect to the manner in which statements come to be accepted. Theories of confirmation surely do not encompass all that can be said about epistemic support.

concordances between these strands of research can also be identified. Confirmation of the Vine-Matthews hypothesis made seafloor spreading concordant with the field reversal hypothesis. Success of USGS and ANU researchers made the field reversal hypothesis concordant with commitments involved in potassium-argon dating. Their success weighed upon the reality of field reversals and the precision and reliability of potassium-argon dating. Toward the end of the 1960s, relative continental displacement established from paleomagnetic field reconstructions could be corroborated by seafloor spreading rates established by marine magnetic profiling. Despite the historical significance of these examples, other examples provided previously illustrate that concordance is not infallible, nor are all concordances equally convincing.

The historical examples highlighted in this chapter were among the simplest examples of concordance and discordance that I could identify from Part 1. The simplicity of the historical examples helped to illustrate each Model of Concordance and Discordance. However, most historical examples of concordance and discordance are not so easily modeled by only one Model of Concordance or Discordance. Still, more complex circumstances can be modeled by the elaboration and combination of the Models of Concordance and Discordance.

The individual Models of Concordance and Discordance can be elaborated. For example, some proposition P1 may concord with P2 and P3 in the Model of Conjunction.

$$((P1 \cdot P2 \cdot P3) \rightarrow E) \cdot E$$

Similarly, some propositions P1 may concord with P2 and P3 in the Model of Corroboration.

$$(P1 \rightarrow E) \cdot (P2 \rightarrow E) \cdot (P3 \rightarrow E)$$

Additionally, Models of Concordance or Discordance may be combined. For example, some statement P1 may entail prediction that is negated by prediction entailed by the conjunction of P2 and P3.

$$(P1 \rightarrow E) \cdot ((P2 \cdot P3) \rightarrow {\sim}E)$$

$${\sim}(P1 \cdot P2 \cdot P3)$$

This example of discordance consists of a combination of the Model of Conjunction with the Model of Corroboration.

As an alternative example, some prediction E1, entailed by a conjunction of P1, P2, and P3, may, in conjunction with P4, entail alternative accepted statement P5.

$$((P1 \cdot P2 \cdot P3) \rightarrow E1) \cdot ((P4 \cdot E1) \rightarrow P5)$$

This example of concordance consists of the combination of the Model of Conjunction, with the Model of Entailment. In this case, prediction E1, derived from a conjunction of P1, P2 and P3, constitutes part of the predictive system that is concordant with P5.

In addition to the possible elaboration and combination of Models of Concordance and Discordance, individual propositions may be concordant or discordant with other propositions in alternative ways. For example, a single proposition P1 may concord with P2 in the Model of Conjunction and may also concord with P3 in the Model of Corroboration.

$$((P1 \cdot P2) \rightarrow E1) \cdot E1$$

$$(P1 \rightarrow E2) \cdot (P3 \rightarrow E2)$$

In this case, P1 is concordant with P2 and P1 is concordant with P3, but P2 and P3 are not directly concordant with one another. P1 may even face discordance that does not confront P2 or P3.

$$(P1 \cdot P4) \rightarrow E3) \cdot \sim E3$$

$$\sim(P1 \cdot P4)$$

As indicated, concordances may be quite complex and inculcate many propositions. Similarly, discordance can be modeled in complex ways, further illustrating the challenges that may be involved in problem solving.

A single proposition may be concordant with many other propositions in many different ways. Some of these concordances may be quite simple, while others may be more complex, inculcating large sets of propositions. A single proposition may also be discordant with many other propositions. The Models of Concordance and Discordance thereby facilitate a networked view of scientific knowledge, consisting of overlapping concordant and discordant relationships. Discordance results in problem solving, wherein propositions are modified in an effort to eliminate the discordance while concordance may have positive epistemic significance. The growth of scientific knowledge involves the formation of concordances and identification and elimination of discordances. Judgements pertaining to the modification of a network will be influenced by the concordances and discordances that result from that change.

More detail on the analysis of these *predictive networks* will be introduced in the next two chapters. For now, it is important to note that a network of scientific knowledge, as represented in this chapter, can be dissected in multiple ways. A predictive network may be described as being comprised of individual propositions, each proposition having quasi-independent epistemic support. Particular entailed predictions within such a network may facilitate the delineation of predictive systems. Certain propositions may be contained within many such predictive systems that partially overlap, and this may be deemed akin to Kuhnian paradigmatic commitments. The evolution of such partially overlapping systems may facilitate the identification of Lakatosian research programmes or Laudan's research traditions. Different kinds of propositions may be distinguished based on their variable tendencies within a predictive network. Some commitments may confront empirical statements directly, while others may not. Some commitments may *only* confront observation in conjunctions, while other commitments may be more readily individuated. Some commitments may form many concordant links within a network, while others may form only a few. In the broadest view, something akin to Quine's web of belief may also be identifiable, wherein a modification to one proposition may require a series of other modifications, and commitments that have the fewest concordant links may be modified with less overall disruption to the network than centrally integrated propositions.

Answers to important epistemic questions in the philosophy of science may be influenced by the network structures to which these questions are posed. In Chapter 7, I argue that the ambiguity of falsification and the consensus problem can be overcome by including relevant network structures into analysis of falsification and problem solving. In Chapter 8, I argue that certain network structures, representative of an elaborated view of consilience, can yield highly reliable knowledge.

Chapter 7: Analysis of Falsification and Problem Solving

Introduction

The ambiguity of falsification, the consensus problem, and the notion of a “field of choice” were introduced in the previous chapter.

Problem S:

$((P1 \cdot P2) \rightarrow E) \cdot {\sim}E$

${\sim}(P1 \cdot P2)$

This ambiguous falsification problem includes a field of choice that consists of the set of alternative possible ways in which falsification could be isolated to this system. During problem solving, falsification is attributed to one and only one option within the field of choice.

Problem S field of Choice:

(P1) - (P2) - (P1)&(P2)

Due to this ambiguity of falsification, the suitable response to refutation is not logically determined, so there is a great degree of choice involved in problem solving. This results in the consensus problem, the apparent contradiction between logical ambiguity of falsification and the tendency toward consensus in scientific practice.

In this chapter, I argue that the orthodox understanding of the ambiguity of falsification and problem solving is incomplete. Refutations are typically considered to be epistemic units, and problem solving is construed as a sequential process wherein individuated problems are solved as they arise. Such traditional approaches to falsification and problem solving overlook relevant features of predictive networks.

In the previous chapter, the Model of Conjunction was identified from literature on predictive holism, but I also identified additional Models of Discordance capable of modeling falsification problems. I argued that prediction testing does not require a strong distinction between empirical and theoretical statements. Predictions may be entailed by sets of propositions that include tested or untested empirical statements. Additionally, alternative Models of Discordance show that predictions may be tested empirically but may also be tested against alternative predictions or against alternative accepted statements. I also claimed that the Models of Discordance can be elaborated and combined.

From these features of predictive networks, I will develop an enriched programme for the analysis of falsification and problem solving that challenges the common assumption that single refutations are epistemic units. Within this enriched account, the ambiguity of falsification comes in degrees, and in suitable conditions, falsification can be deductively isolated. Additionally, problem solving may be more complex and logically constrained than is typically assumed.

The argument proceeds as follows. First, I examine alternative attempts to isolate the ambiguity of falsification. These attempts confront notable obstacles, but their intuitive appeal is clearly encompassed within the programme I develop. Then, I argue that the ambiguity of falsification can only be narrowed by eliminating options from a field of choice, thereby allowing falsification to be isolated to varying degrees. I identify three general conditions wherein options from a field of choice can be eliminated. In traditional approaches to the ambiguity of falsification via the Model of Conjunction, these conditions have limited efficacy. I then identify subsystems, recursive systems, and recursive problems in order to demonstrate that a single falsification problem may be accountable to multiple predictions simultaneously. This improves the capacity for the deductive elimination of options from a field of choice, thereby constraining problem solving and facilitating the isolation of falsification. Finally, I examine a historical case study of the problem of biotic disjuncts to illustrate the historical relevance of this enriched programme of problem solving.

Previous Attempts to Isolate Falsification

Possible limitations upon the ambiguity of falsification have been approached from several different directions.

There are alternative notions of what comprises a falsified *system* in the philosophy of science, and not all these alternatives are mutually exclusive. Duhem claimed that some hypotheses could be tested individually, especially in immature sciences. More mature sciences, however, typically do not contain hypotheses that can be tested individually because there is a need for theoretical interpretation of experimental outcomes.[724] Alternatively, Quine envisioned the whole of science as a single empirical unit.[725] For Quine, single statements do not have empirical content. If a hypothesis seems to confront observation directly, this is only because we ignore a vast collection of other statements that we assume

[724] Duhem, 1991
[725] Quine, 1951

to be true in making that judgement. Quine claimed that some statements in the fabric of knowledge are more structurally central or interrelated than others, and modification of such central statements would have wider reverberations than modification of peripheral statements.

Like Quine, Kuhn considered some statements to be more structurally significant in science than others.[726] Unlike Quine, Kuhn envisioned periodized paradigms to be confined to disciplines or subdisciplines, incommensurable across episodes of change. Lakatos indicated that disciplines and subdisciplines may not be so hegemonic, as multiple research programmes may compete at any given time.[727] Kuhn's claim of incommensurability across paradigms elicited many attempts to identify areas of continuity or translation across scientific change or disciplines. One common approach is to argue that reference may be preserved in some way, even if the language used to address these referents changes. Structural realists may argue that even if reference to things is inconsistent across theory change, the mathematical relationships between them may remain intact. Peter Galison provides examples of scientists in disparate fields working together in successful and meaningful ways through formation of a functional language of exchange.[728] Star and Griesemer point to "boundary objects" as robust enough to maintain a common identity across disparate groups, facilitating communication.[729] More generally, there is wider appreciation, today, that scientific change is not as absolute as Kuhn envisioned. Rather, disciplines and subdisciplines are structured by many quasi-independent theories and methods, and piecemeal change allows for some continuity of meaning.

These alternative views of systems or predictive units in scientific knowledge have bearing on the degree of ambiguity involved in falsification. For Quine, falsification is ambiguous about the whole of human knowledge. For Kuhn, falsification is ambiguous about some discipline or subdiscipline wherein a certain paradigm reigns. For Lakatos, falsification is ambiguous about some research programme, though there are several competing alternatives at any time within a discipline. For more contemporary views of piecemeal change across tangled strands of scientific commitments, the ambiguity of falsification may be even more localized.[730]

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726 Kuhn, 1962

727 Lakatos, 1970

728 Galison, 1997

729 Star and Griesemer, 1989

730 Though these alternative views are relevant to the limits of the ambiguity of falsification, there is a risk of misinterpreting this relevance. Modus tollens forms the basis of the ambiguity of falsification. In this chapter, I write of the ambiguity of falsification as it pertains to the isolation of falsification within an unambiguously falsified system. However, others may speak of the ambiguity of falsification as it pertains to the collection of alternative

Another route of inquiry with bearing upon the ambiguity of falsification pertains to logical constraint upon problem solving imposed by commitments that are retained during problem solving. Kuhn claimed that paradigm commitments are preferentially retained during problem solving, and the work of aligning that paradigm with observation may be highly determined. Similarly, Lakatos claimed that retained hard core commitments come with a positive heuristic that directs possible changes made in the protective belt. Beginning in the 1960s, Adolf Grünbaum argued that these sorts of constraints imposed by commitments that are retained during problem solving might challenge Quine's conclusion that any hypothesis confronting refutation can be retained by modifying alternative commitments.[731]

Consider the following generalization of ambiguous falsification.

$$((P1 \cdot P2) \rightarrow E) \cdot {\sim}E$$

$${\sim}(P1 \cdot P2)$$

Grunbaum characterizes Quine's claim that any statement (P1) can be retained "come what may" as follows.

$$(\exists P2')(P1 \cdot P2' \rightarrow E')$$

However, Grunbaum argues that this conclusion does not follow from the ambiguity of falsification. Though Grunbaum does not explicitly state as much, his implicit claim is that there may be circumstances wherein, given P1 and E', there is no P2' capable of resolving the problem.[732]

$${\sim}(\exists P2')(P1 \cdot P2' \rightarrow E')$$

Grunbaum does not establish when or how such circumstances could be identified.

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possible ways in which a falsification problem can be resolved and the consequences of such resolutions. This, I call the ambiguity of problem solving. Though these are distinct concepts, seminal accounts offered by Duhem and Quine do not mark a strong distinction between these ideas. Consequently, much subsequent work on the units of empirical knowledge or falsification conflates the ambiguity of falsification with the ambiguity of problem solving. For now, it is important to highlight that in the remainder of this chapter, the term "predictive system" will denote a conjunction of propositions delineated by a refuted prediction of interest. A predictive system consists of all propositions used in the formation of that prediction. Symbolic representation of the ambiguity of falsification often adheres to this distinction, but this is not always what is intended by the alternative approaches to systems introduced above. Lakatosian research programmes, for example, consist of sets of propositions that constitute a hard core and the protective belt. However, it is not necessarily the case that all hard core propositions and all protective belt propositions are involved in every prediction to which the research programme may be deemed accountable.

[731] Grünbaum, 1960, 1962, 1973

[732] E' itself may be taken as P2', but this would make the ostensible underdetermination of falsification trivial.

It is sometimes argued that theories of confirmation or alternative standards of epistemic support may restrict the ambiguity of falsification.[733][734] Alternative propositions within a beleaguered system may garner different degrees of epistemic support, and propositions ought to be spared from falsification in a manner that is somehow proportional with this confidence. Scientists surely consider some beliefs to be more secure than others, so this account may have pragmatic relevance to problem solving. However, if observation typically confronts entire systems rather than individual propositions, then an adequate appeal to such differential epistemic support must identify some route through which confidence can be differentially attributed within a system in the first place.

It may be presumed that some analysis of overlapping systems could facilitate such differential attribution of confidence within a system. For example, some component of a falsified system may also be included in an alternative system that is confirmed. This component may seem to obtain some independent epistemic support that alternative propositions within the falsified system lack.[735] However, false propositions may entail confirmed prediction, so this trajectory of inquiry must find some way to avoid affirming the consequent or risk the rejection of perfectly fine hypotheses for the entrenchment of those that may be less adequate. Alternatively, a proposed solution to a falsification problem may confront an alternative refutation in an overlapping system, and it may thereby be presumed that some analysis of refutations and differences between systems would facilitate the isolation of falsification in some way. However, such precipitating problems may be attributed to unshared propositions. As an illustration, consider the following systems.

System S1:

$(P1 \cdot P2) \rightarrow E1$

System S2:

$(P2 \cdot P3) \rightarrow E2$

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[733] Grünbaum 1962; Laudan 1977; Mayo 1997; Strevens 2001; Norton 2008, Rowbottom 2010

[734] Literature on general underdetermination also has bearing at this point. Those who attempt to limit the epistemic significance of underdetermination often argue that, though data may underdetermine theory choice, this is not the only means of establishing epistemic credibility. For example, two competing theories may be empirically adequate, but one may be simpler than another, and simplicity may be deemed to have epistemic relevance. Such alternative methods of establishing epistemic support may pertain to the ambiguity of problem solving by providing differential support to alternative logically permissible resolutions.

[735] This sort of rationale is quite common (at least implicitly) across varied attempts to limit the underdetermination of falsification (Lakatos, 1970; Laudan, 1977, p. 43; Strevens 2001, Howson and Urbach 2006, pp. 109-114; Rowbottom 2010).

Prediction E1 may be refuted, thereby falsifying System S1.

Problem S1:

$((P1 \cdot P2) \rightarrow E1) \cdot \sim E1$

$\sim(P1 \cdot P2)$

To resolve this problem, proposition P2 may be modified to P2'.

System S1':

$(P1 \cdot P2') \rightarrow E1'$

The modification of P2 to P2' may precipitate a problem in System S2.

Problem S2':

$((P2' \cdot P3) \rightarrow E2') \cdot \sim E2'$

$\sim(P2' \cdot P3)$

Even though the modification of P2 to P2' creates a new problem (Problem S2'), this is not fatal for the proposed resolution of Problem S1. Instead, P2' can be retained by directing falsification to P3. In this manner, scientists can "kick the can" of falsification down a sequence of independent falsification problems, resolving each new problem as it arises.[736][737]

From this brief review of attempts to limit the ambiguity of falsification, as well as the introduction to the ambiguity of falsification provided in the previous chapter, it is apparent that refutations are often treated as single epistemic units within the philosophy of science. Though predictive systems are sometimes recognized to be partially overlapping, these overlapping systems are often treated as logically independent from the refutation at hand. A successful resolution of a falsification problem is generally considered to consist of the modification of a falsified system so as to

[736] For an influential illustration of kicking the can of falsification, see Lakatos, 1970, 100-101.
[737] In this chapter, I use the term "precipitating problems" or "precipitating refutations" to refer to problem solving efforts that result in still other problems. Some precipitating problems may be separable from the initial problem, while others may be inseparable. Alternatively, Frankel uses the term "difficulties" to refer to problem solving efforts that results in still other problems. He does not distinguish between separable and inseparable difficulties, however.

eliminate a refutation, sometimes with the additional stipulation that the newly modified system entails the initial refutation such that empirical content expands during problem solving.

Isolating Falsification by Narrowing the Field of Choice

In the previous chapter, I defined the ambiguity of falsification as ambiguity within a field of choice. This established a clear directive for any attempt to isolate falsification: Identify some route through which options in a field of choice can be logically eliminated.[738]

The present task is to identify general conditions wherein options from a field of choice can be logically eliminated, thereby deductively facilitating the isolation of falsification. The general conditions are quite simple and agreeable, yet also capable of providing some purchase to the notions that some sort of analysis of retained commitments or partially overlapping systems may facilitate the isolation of falsification.

<u>Condition 1:</u> Falsification cannot be isolated to a proposition (or component of a system) if that proposition (or component) cannot be negated.

Predictive systems may include analytic statements or statements that cannot be modified within a given language. Additionally, propositions may be subject to a successful argument for realism. Any option within a field of choice that contains such a statement can be eliminated.

<u>Example:</u>

System S:

$((P1 \cdot P2) \rightarrow E) \cdot {\sim}E$

S field of choice:

(P1) - (P2) - (P1)&(P2)

[738] Some attempts to limit the ambiguity of falsification, identified in the previous section, are not suitable for this task. For example, the prospect of differential attribution of confirmation to components within a beleaguered system may direct problem solving, but this does nothing to logically limit the ambiguity. However, some of the approaches identified in the previous section aim toward the logical elimination of options from a field of choice, even if these aims are not fully articulated.

If P2 cannot be negated - for whatever reason - then falsification cannot be isolated to P2. Every option within the field of choice that contains P2 can be eliminated.

S field of choice:

(P1) - ~~(P2)~~ - ~~(P1)&(P2)~~

Condition 2: Falsification cannot be isolated to a proposition (or component of a system) if there is no modification of that proposition (or component) capable of resolving the falsification problem.

In some circumstances, attributing falsification to a certain option within a field of choice will unavoidably precipitate additional problems that can only be resolved by attributing falsification to some alternative option within that field of choice.

Example:

System S:

$((P1 \cdot P2) \rightarrow E1) \cdot {\sim}E1$

S field of choice:

(P1) - (P2) - (P1)&(P2)

During problem solving, falsification may be attributed to P2. However, the conjunction of P1 with ~P2 may entail an alternative refuted prediction.

System S':

$((P1 \cdot {\sim}P2) \rightarrow E2) \cdot {\sim}E2$

S' field of choice:

(P1) - (~P2) - (P1)&(~P2)

If falsification is attributed to ~P2 within the field of choice of S', then the initial problem re-emerges. In such circumstances, it is impossible to attribute falsification to P2 without resulting in an alternative problem that can only be resolved by also falsifying P1. Accordingly, the option to attribute falsification to P2 alone can be eliminated from the field of choice of Problem S.

S field of choice:

(P1) - ~~(P2)~~ - (P1)&(P2)

<u>Condition 3:</u> Falsification must be isolated to a proposition (or component of a system) if that proposition (or component) is unambiguously falsified elsewhere.

If some proposition or component of a system is unambiguously falsified in some way, then falsification must be isolated to that proposition or component if it is found within the field of choice of an alternative problem.

<u>Example:</u>

System S1:

$((P1 \cdot P2) \rightarrow E1) \cdot {\sim}E1$

S1 field of choice:

(P1) - (P2) - (P1)&(P2)

Proposition P1 may be contained within some alternative system, S2.

System S2:

$((P1 \cdot P3 \cdot P4) \rightarrow E2) \cdot {\sim}E2$

S2 field of choice:

(P1) - (P3) - (P4) - (P1)&(P3) - (P1)&(P4) - (P3)&(P4) - (P1)&(P3)&(P4)

As exemplified in Condition 2, it may be the case that the refutation of S1 cannot be avoided by attributing falsification to P2 alone. This eliminates P2 from the field of choice of S1.

S1 field of choice:

(P1) - ~~(P2)~~ - (P1)&(P2)

This unambiguously falsifies P1, since P1 is found within every viable option of the field of choice. Falsification must be attributed to P1 regardless of its location in alternative systems. Accordingly, every option in the field of choice of S2 that does not contain P1 can be eliminated.[739]

S2 field of choice:

(P1) - ~~(P3)~~ - ~~(P4)~~ - (P1)&(P3) - (P1)&(P4) - ~~(P3)&(P4)~~ - (P1)&(P3)&(P4)

When every option in a field of choice remains viable, the field of choice will be called *fully open*. Narrowed fields of choice can be delineated into two general categories. First, a field of choice may be narrowed without isolating falsification to a single proposition. This will be called *partial determination*. Partial determination can eliminate problem solving trajectories, thereby directing problem solving or determining the superiority of one proposed resolution over an alternative. Partial determination may also unambiguously isolate falsification to some component of a system that consists of multiple propositions. Second, a field of choice may be narrowed such that at least one proposition is unambiguously falsified. This may be the case if a field of choice is narrowed to only a single option or if a certain proposition is contained within every viable option of a sufficiently narrowed field of choice. This will be called *full determination*. Full determination unambiguously isolates falsification within a system. The following are examples of fully determined fields of choice.

~~(P1)~~ - ~~(P2)~~ - ~~(P3)~~ - (P1)&(P2) - ~~(P1)&(P3)~~ - ~~(P2)&(P3)~~ - ~~(P1)&(P2)&(P3)~~

In this case, propositions P1 and P2 are both falsified.

(P1) - ~~(P2)~~ - ~~(P3)~~ - (P1)&(P2) - (P1)&(P3) - ~~(P2)&(P3)~~ - (P1)&(P2)&(P3)

In this case, every viable option in the field of choice includes P1, so P1 is unambiguously falsified. Falsification of P2 and P3 remains ambiguous.

Simultaneous Accountability in Problem Solving

As previously indicated, the ambiguity of falsification is generally illustrated by a single system confronting a single refutation. Likewise, the successful resolution of a falsification problem is typically

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[739] Analysis of partially overlapping systems can thereby facilitate the isolation of falsification. Such analysis can be made more precise by combining the fields of choice of partially overlapping systems. This will be introduced shortly.

considered to consist of the modification of a system to avoid or accommodate a single refutation. All examples of falsification problems addressed thus far align with this orthodoxy. The benefit of illustrating the ambiguity of falsification in this way is conceptual simplicity. However, this simplification smooths away the possibility that a single predictive system may entail several alternative predictions.[740]

In the previous chapter, it was noted that prediction testing within Models of Concordance and Discordance erode a strong functional distinction between hypothesis and evidence. Predictions may be tested empirically but may also be tested against other accepted statements or against other untested predictions. Additionally, predictive systems may include hypotheses, but may also include empirical statements. It was also noted that the Models of Concordance and Discordance can be elaborated and combined, in part due to this erosion of a functional distinction between hypothesis and evidence. By elaborating upon these features of predictive networks, I will show that predictive systems and falsification problems may be logically accountable to multiple entailed predictions.

Suppose some system, S, entails prediction E1.

System S:

$(P1 \cdot P2 \cdot P3) \rightarrow E1$

Though each of these three propositions P1, P2, and P3 may be involved in the derivation of prediction E1, it is not necessarily the case that this is the only prediction entailed by this set of propositions. Instead, alternative prediction may be entailed by subsets of these propositions. Such systems, comprised entirely of propositions contained within another system, will be called *subsystems* (denoted as Ss). Subsystems may entail alternative predictions that can be independently tested.[741] Accordingly, a system can be accountable to numerous predictions simultaneously.

System S:

$(P1 \cdot P2 \cdot P3) \rightarrow E1$[742]

[740] It is not entirely novel to suggest that a single system may entail multiple predictions. Even in the seminal account of the underdetermination of falsification, Duhem points toward the possibility that a single system may entail several empirical successes (Duhem, 1991). On occasion, there is implicit suggestion that such alternative predictions may facilitate the isolation of falsification (Hattiangadi 1974; Popper 2002b, 324; Norton 2008). However, this prospect is never expounded with sufficient clarity to be of analytic utility.

[741] The number subsystems contained within a system varies on a case-by-case basis and can be influenced by the delineation of predictive systems. The same is true of recursive systems and recursive problems.

[742] These propositions may be specified as follows.

Subsystem Ss1:

$(P1 \cdot P2) \rightarrow E2$

Subsystem Ss2:

$(P2 \cdot P3) \rightarrow E3$

Subsystems are not the only route through which a single system may entail multiple predictions. Alternatively, a prediction entailed by a system may feedback into that system and, in conjunction with only those propositions contained within the system, entail an additional prediction. This will be called a *recursive system* (denoted as Rs).

System S:

$(P1 \cdot P2) \rightarrow E1$[743]

Recursive system Rs:

$(P1 \cdot E1) \rightarrow E2$

Subsystems may be recursive as well, and alternative subsystems may be recursively inter-related.

Additionally, it may also be the case that multiple predictions are entailed by the collection of statements that comprise a falsification problem. In this regard, an accepted refutation may entail prediction in conjunction with only those propositions contained within the falsified system. This will be called a *recursive problem* (denoted as Rp).

P1: Jim is a man
P2: All men are mortal
P3: All mortals breathe

The conjunction of P1, P2, and P3 entail E1: Jim breathes. Two additional predictions are entailed by this set of propositions. The conjunction of P1 and P2 entails E2: Jim is mortal. The conjunction of P2 and P3 entails E3: All men breathe.

[743] This example may be specified as follows.

P1: Money in Jim's account will accumulate compounding interest at 10% per day
P2: Jim has $100 in his account on January 1

The conjunction of P1 with P2 entails E1: Jim will have $110 in his account on January 2. The conjunction of P1 with E1 entails an additional prediction E2: Jim will have $121 in his account on January 3.

Problem S:

$((P1 \cdot P2) \rightarrow E1) \cdot {\sim}E1$[744]

Recursive problem Rp:

$(P1 \cdot E1') \rightarrow E2$

Recursive problems are somewhat different from subsystems and recursive systems. Rather than a single system entailing multiple predictions, recursive problems illustrate how multiple predictions may be entailed by a single problem. That is, if there is a falsification problem, then additional prediction may be entailed by only those propositions that constitute the problem.

The presence of subsystems, recursive systems, or recursive problems make a falsification problem accountable to multiple predictions simultaneously.[745]

Enriched Criteria of Problem Solving

In the orthodox approach to falsification and problem solving, a successful resolution of a falsification problem eliminates and hopefully accommodates a single refutation. Though problem solving may precipitate alternative refutations, these are typically deemed to be logically independent problems that can be resolved in turn. This orthodox approach to falsification and problem solving does not work when falsification problems are accountable to multiple predictions.

If a system entails multiple predictions, associated problem solving may be accountable to numerous predictions simultaneously.

[744] Developing on the previous footnote, suppose that prediction E1 is refuted by observation E1': Jim has $105 in his account on January 2. The conjunction of P1 with E1' entails an additional prediction E2: Jim will have $115.5 in his account on January 3.
[745] For the sake of illustrative simplicity predictions and recursion have been denoted as "E", but in principle, these additional predictions need not be empirical.

System S:

$((P1 \cdot P2 \cdot P3) \rightarrow E1) \cdot {\sim}E1$

Subsystem Ss1:

$((P1 \cdot P2) \rightarrow E2) \cdot {\sim}E2$

Subsystem Ss2:

$((P1 \cdot P3) \rightarrow E3) \cdot E3$

In this example, System S is refuted by E1', but this single refutation does not encompass the full extent of the falsification problem. Rather, a successful resolution of Problem S must also resolve the falsification of Subsystem Ss1. After all, Ss1 is entirely comprised of propositions contained within System S. If Ss1 is not resolved during problem solving, then some component of the proposed resolution of Problem S remains unambiguously falsified.

A proposed resolution to this problem may modify P1 to P1', and this may avoid the refutation of S and Ss1. However, this modification may precipitate the refutation of Ss2.

System S':

$((P1' \cdot P2 \cdot P3) \rightarrow E1') \cdot E1'$

Subsystem Ss1':

$((P1' \cdot P2) \rightarrow E2') \cdot E2'$

Subsystem Ss2':

$((P1' \cdot P3) \rightarrow E3') \cdot {\sim}E3'$

This precipitated refutation confronts a subsystem of S', the proposed resolution to Problem S. The conjunction of P1' and P3 is unambiguously falsified, so some alternative resolution to Problem S is required.[746] Accordingly, if a system entails multiple predictions, precipitating refutations that take place

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[746] As a specified example of such a precipitated refutation, consider the following.

P1: Jim is a man

P2: All men are mortal

P3: All mortals breathe

The conjunction (P1·P2·P3) entails prediction E1: Jim breathes. As noted in a previous footnote, this system also contains subsystems. The conjunction (P1·P2) entails prediction E2: Jim is mortal, while the conjunction (P2·P3) during problem solving may be inseparable from the initial problem. As previously illustrated, when "kicking the can" of falsification down a sequence of problems, falsification is attributable to propositions that are unshared between partially overlapping systems. However, if problem solving precipitates inseparable problems within subsystems or recursions, then kicking the can of falsification is impossible as there are no unshared propositions to which falsification can be attributed.

In summation, problem solving may be accountable to numerous predictions, a falsification problem may consist of multiple refutations, and the successful resolution of a falsification problem must satisfy several conditions that deviate from the orthodox approach to problem solving. A successful resolution to a falsification problem a) eliminates all refutations confronting the system, b) attributes falsification to one viable option within a field of choice, and c) avoids precipitating refutation of any additional prediction entailed by the proposed resolution.

Narrowing the Field of Choice Again

The simultaneous accountability of a problem to multiple predictions and the associated enriched criteria for a successful resolution of a falsification problem facilitate the identification of more-substantive ways in which the conditions that limit the ambiguity of falsification can be satisfied.

Condition 1: Falsification cannot be isolated to a proposition (or component of a system) if that proposition (or component) cannot be negated.

As previously indicated, some propositions contained within a refuted system may be immune from negation. The refutation of prediction entailed by a recursive problem may *provisionally* satisfy this condition and thereby narrow a field of choice.

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entails prediction E3: All men breathe. Upon testing, prediction E1 may be refuted by E1': Jim does not breathe. During problem solving, P3 may be modified to P3': No mortal breathes. The proposed resolution (P1·P2·P3') eliminates E1' as a refutation. However, the modification of P3 to P3' also influences Subsystem Ss2. In this case, (P2·P3') entails prediction E3': No men breathe. Upon testing, prediction E3' may be refuted, thereby falsifying Subsystem Ss2'. Since P2 and P3' are components of S', the falsification of Ss2' unambiguously falsifies some component of S'. Accordingly, some alternative resolution of Problem S is required.

Example:

System S:

$((P1 \cdot P2 \cdot P3) \rightarrow E1) \cdot {\sim}E1$

S field of choice:

(P1) - (P2) - (P3) - (P1)&(P2) - (P1)&(P3) - (P2)&(P3) - (P1)&(P2)&(P3)

The refutation E1' may entail prediction in conjunction with some component of System S, and this prediction may be refuted.

Recursive Problem Rp:

$((P1 \cdot E1') \rightarrow E2) \cdot {\sim}E2$

Rp field of choice:

(P1) - (E1') - (P1)&(E1')

Falsification Problem S is contingent upon the acceptance of E1'. Accordingly, if there is a problem to be resolved, then falsification cannot be isolated to E1'. In this case, E1' is not logically immune from negation. Instead, Condition 1 is satisfied provisionally, but provisionally upon there being a falsification problem in the first place. Any option in the field of choice that contains E1' can be eliminated.

Rp field of choice:

(P1) - ~~(E1')~~ - ~~(P1)&(E1')~~

Acceptance of refutation E1' thereby fully determines the falsification of P1. By Condition 3, any option within the field of choice of problem S that does not contain P1 can thereby be eliminated.[747]

[747] This example may be specified as follows.

P1: The arrowhead predates the pottery
P2: The bone fragment predates the arrowhead
P3: The bone fragment postdates the burial mound

This system (P1·P2·P3) entails prediction E1: The burial mound predates the pottery. Upon testing, this prediction may be refuted by E1': The burial mound postdates the pottery. This refutation creates a falsification problem but also entails alternative prediction in conjunction with P1. The resulting recursive problem (P1·E1') entails prediction E2: The arrowhead predates the burial mound. Upon testing, this prediction may also be refuted. In this case, acceptance of refutation E1' results in problem S and *also* fully determines the falsification of P1.

S field of choice:

(P1) - ~~(P2)~~ - ~~(P3)~~ - (P1)&(P2) - (P1)&(P3) - ~~(P2)&(P3)~~ - (P1)&(P2)&(P3)

<u>Condition 2:</u> Falsification cannot be isolated to a proposition (or component of a system) if there is no modification of that proposition (or component) capable of resolving the falsification problem.

As previously indicated, retained commitments may facilitate the isolation of falsification by restricting modifications that can be made, should falsification be attributed elsewhere. The restrictions imposed by retained commitments can become more pronounced and complex when analyzing subsystems and recursion.

<u>Example 1:</u>

System S:

$((P1 \cdot P2 \cdot P3) \rightarrow E1) \cdot {\sim}E1$

Subsystem Ss

$(P1 \cdot P2) \rightarrow E2$

S field of choice:

(P1) - (P2) - (P3) - (P1)&(P2) - (P1)&(P3) - (P2)&(P3) - (P1)&(P2)&(P3)

It may be the case that P1, P3, and E1’ establish some P2 or a range of possible P2’s as logical consequences.

$(P1 \cdot P3 \cdot E1') \rightarrow P2'$

During problem solving, propositions P2 may be modified to P2’ in order to eliminate the refutation of E1. However, it may also be the case that P1 and E2 establish some P2 or a range of possible P2’s as logical consequences, and these constraints upon P2 may be discordant.

$(P1 \cdot E2) \rightarrow P2''$

Proposition P2 may thereby be modified to P2’ to eliminate the refutation of E1. However, any permissible modification of P2 to P2’ that avoids refutation E1’ would result in the refutation of Subsystem Ss. In such circumstances, it is impossible to modify P2 without precipitating an additional problem that requires the falsification of P1. Accordingly, falsification cannot be attributed to P2 alone, so this option can be eliminated from the field of choice.[748]

S field of choice:

(P1) - ~~(P2)~~ - (P3) - (P1)&(P2) - (P1)&(P3) - (P2)&(P3) - (P1)&(P2)&(P3)

A single proposition may be contained within multiple subsystems or recursive systems. Accordingly, the modification of such a proposition may have to avoid falsification from many directions simultaneously.

Retained commitments may also designate a set or range of permissible modifications that can be made elsewhere during problem solving. This might make it easier to identify whether all possible modifications of a proposition precipitate inseparable refutation. It may also be the case that inseparable refutation precipitates from all but one (or some narrowed range) of these permissible modifications. In such circumstances, retained commitments may direct or even determine the manner in which a falsification problem must be solved, should falsification be attributed to a certain option within the field of choice.

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748 Consider the following specified example.

P1: The number of candies in the dispenser equals the number of candies added to the dispenser minus the number of candies removed from the dispenser which is equal to twice the number of dollars added to the dispenser (two candies are dispensed for each dollar added).

P2: Three dollars were added to the dispenser

P3: Ten candies were added to the dispenser

The conjunction of these propositions (P1·P2·P3) entails prediction E1: Four candies are in the dispenser. This system contains a subsystem (P1·P2) which entails the prediction E2: Six candies were removed from the dispenser.

Upon examination, E1 may be refuted by E1': Two candies are in the dispenser.

$((P1 \cdot P2 \cdot P3) \rightarrow E1) \cdot {\sim}E1$

$(P1 \cdot P2) \rightarrow E2$

Falsification may be attributed to P2. Given P1 and P3, P2 may state that zero, one, two, four, or five or more dollars were added to the dispenser. In this case, P2 may be modified to P2': Four dollars were added to the dispenser. Given P1 and P3, this is the only possible modification of P2 that can accommodate E1'. However, the modified subsystem (P1·P2') also entails a different prediction E2': Eight candies were removed from the dispenser. It may be the case that E2' is refuted.

$(P1 \cdot P2' \cdot P3) \rightarrow E1'$

$((P1 \cdot P2') \rightarrow E2') \cdot {\sim}E2'$

In this case, we know that P2' was the only possible modification of P2 that would accommodate refutation E1', yet this modification precipitates an inseparable problem in Subsystem Ss'. Accordingly, falsification cannot be attributed to P2 alone.

Example 2:

System S1:

((P1 · P2) → E1) · ~E1

S1 field of choice:

(P1) - (P2) - (P1)&(P2)

System S2:

((P2 · P3) → E2) · ~E2

S2 field of choice:

(P2) - (P3) - (P2)&(P3)

In this example, proposition P2 is included in both System S1 and System S2. In general, we can call a set of such partially overlapping problems a *problem cluster*. The fields of choice of problems within a problem cluster partially overlap and can be usefully combined to articulate the collection of possible ways in which falsification can be isolated to propositions within a problem cluster. Combining the fields of choice of S1 and S2 consists of taking one viable option from the field of choice of S1 and appending one viable option from the field of choice of S2, until all viable option pairs have been exhausted.

Combined fields of choice S1 and S2:

(P1) - (P2) - (P1)&(P2)

(P2) - (P3) - (P2)&(P3)

(P1)&(P2) - (P1)&(P3) - (P1)&(P2)&(P3)

(P2)&(P2) - (P2)&(P3) - (P2)&(P2)&(P3)

(P1)&(P2)&(P2) - (P1)&(P2)&(P3) - (P1)&(P2)&(P2)&(P3)

By removing redundancies, we reach the field of choice of this problem cluster.

PC field of choice:

(P2) - (P1)&(P2) - (P1)&(P3) - (P2)&(P3) - (P1)&(P2)&(P3)

Like the field of choice of a falsification problem, the field of choice of a problem cluster is an illustration of the ambiguity of falsification therein. The field of choice of this problem cluster consists of only five options. These are the possible ways in which falsification can be isolated therein. In this case, it is not possible to resolve both problems S1 and S2 by isolating falsification to P1 alone or P3 alone.

It may be the case that retained commitments impose restrictions upon permissible modifications when analyzed in a problem cluster that are not apparent when analyzed individually. For example, it may be the case that there is some possible modification of P2 that will resolve problem S1, and some modification of P2 that will resolve problem S2, but no possible modification of P2 that will resolve both S1 and S2.

In such circumstances, falsification cannot be isolated to P2 alone, but this only becomes apparent when analyzing the entire problem cluster. This eliminates an option within the field of choice of a problem cluster but does not eliminate an option within the field of choice of either S1 or S2. Rather, this narrowed field of choice in the problem cluster reflects the fact that if falsification were attributed to P2 in either S1 or S2, then it would also have to be attributed to an alternative unshared proposition.

PC field of choice:

~~(P2)~~ - (P1)&(P2) - (P1)&(P3) - (P2)&(P3) - (P1)&(P2)&(P3)

Analysis of problem clusters can invalidate proposed problem solutions even without isolating falsification, by identifying precipitating inseparable refutation. Consider the following example.

System S1:

$((P1 \cdot P2) \rightarrow E1) \cdot {\sim}E1$

System S2:

$((P2 \cdot P3) \rightarrow E2) \cdot E2$

System S3:

$((P1 \cdot P3) \rightarrow E3) \cdot E3$

Falsification may be attributed to P2, to resolve problem S1.

$((P1 \cdot P2') \rightarrow E1') \cdot E1'$

The modification of P2 to P2' may result in the refutation of System S2'.

$((P2' \cdot P3) \rightarrow E2') \cdot {\sim}E2'$

Falsification may be attributed to P3 to resolve this problem, by kicking the can of falsification.

$((P2' \cdot P3') \rightarrow E2) \cdot E2$

However, the modification of P3 to P3' may result in the refutation of System S3'.

$((P1 \cdot P3') \rightarrow E3') \cdot {\sim}E3'$

In this example, the orientation of these three systems with respect to one another may prohibit the possibility of "kicking the can" of falsification, as retained commitments and modifications made during problem solving may result in circular accountability across partially overlapping systems. In this case, problem S3 requires the falsification of either P1 or P3', even though P1 is a component of the resolution of problem S1, and P3' is a component of the resolution of problem S2. In this case, no options in a field of choice seem to be eliminated. Rather, proposed solutions to falsification problem S1 and S2 result in some inseparable precipitated problem requiring some alternative resolution.

<u>Condition 3:</u> Falsification must be isolated to a proposition (or component of a system) if that proposition (or component) is unambiguously falsified elsewhere.

As previously indicated, falsification must be isolated to propositions or components of a system that are unambiguously falsified. The refutation of subsystems or recursive systems satisfies this condition.

<u>Example 1:</u>

System S:

$((P1 \cdot P2 \cdot P3) \rightarrow E1) \cdot {\sim}E1$

S field of choice:

(P1) - (P2) - (P3) - (P1)&(P2) - (P1)&(P3) - (P2)&(P3) - (P1)&(P2)&(P3)

The falsification of S may be attributed to P1, P2, P3, or some combination of these propositions. However, System S may contain a subsystem.

Subsystem Ss:

$(P1 \cdot P2) \rightarrow E2$

Supposing this subsystem is refuted, the falsification of S cannot be accounted by attributing falsification to P3 alone. Either P1, P2, or both P1 and P2 must be falsified to account for all refutations confronting System S.[749]

S field of choice:

(P1) - (P2) - ~~(P3)~~ - (P1)&(P2) - (P1)&(P3) - (P2)&(P3) - (P1)&(P2)&(P3)

Example 2:

System S:

$((P1 \cdot P2 \cdot P3) \rightarrow E) \cdot {\sim}E$

S field of choice:

(P1) - (P2) - (P3) - (P1)&(P2) - (P1)&(P3) - (P2)&(P3) - (P1)&(P2)&(P3)

It may be the case that the refutation of E entails prediction in conjunction with some component of System S, and this prediction may contradict some alternative component of that system.

Recursive Problem Rp:

$(P1 \cdot E') \rightarrow P2'$

In this case, the refutation of E begets an additional refutation within Problem S. Should P1 be retained, then E' necessitates the falsification of P2, since P2' contradicts P2. Alternatively, P2 may be retained if falsification is attributed to P1. Together, this recursive problem isolates falsification to either

[749] As a specified illustration, consider the following three propositions.

P1: Jim is a swan
P2: All swans are white
P3: Nothing in the southern hemisphere is white

This system (P1·P2·P3) entails the prediction E1: Jim is not in the southern hemisphere. Upon observation, it may be the case that Jim is in the southern hemisphere. Subsystem (P1·P2) entails the prediction E2: Jim is white. Upon observation, prediction E2 may be refuted. This unambiguously falsifies P1, P2 or both P1 and P2.

P1, P2, or both P1&P2. Accordingly, any option within the field of choice of S that does not attribute falsification to either P1 or P2 can be eliminated.

S field of choice:

(P1) - (P2) - ~~(P3)~~ - (P1)&(P2) - (P1)&(P3) - (P2)&(P3) - (P1)&(P2)&(P3)

Should P1 be retained during problem solving, the conjunction of P1 with E' not only necessitates the falsification of P2 but also determines the manner in which P2 must be modified. In this case, attempts to resolve Problem S that do not attribute falsification to P1 must modify P2 to P2'.[750]

As demonstrated, when the scope of analysis of a falsified system is expanded to include subsystems, recursive systems, and recursive problems, the conditions that facilitate the isolation of falsification become more readily and rigorously applicable.

Complex Problems and Overview

For the sake of illustrative simplicity, the general examples of problems and problem solving provided to this point have most closely applied to the Model of Conjunction. However, the framework developed in this chapter still applies when problems and problem solving are modeled by alternative Models of Discordance or elaborations and combinations thereof. For example, some Problem S may combine the Model of Conjunction with the Model of Corroboration.

Problem S

((P1 · P2 · P3) → E1) · ((P4 · P5) → ~E1)

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[750] Suppose we use the tangent function to model the relationship between the angle of the sun above the horizon, the height of a particular flagpole, and the length of its shadow.

P1: The tangent of the angle of the sun above the horizon equals the height of the flagpole divided by the length of its shadow
P2: The angle of the sun above the horizon is 30 degrees
P3: The flagpole is 15 meters tall

This system (P1·P2·P3) entails prediction E: The shadow is 26 meters long. Upon examination, this prediction may be refuted by E': The shadow is 45 meters long. Refutation E' entails a recursive prediction. One such recursive problem (P1·P2·E') entails the prediction: The flagpole is 26 meters tall. In this case, refutation E' results in Problem S and also determines the modification that must be made to P3 should P1 and P2 be retained during problem solving.

This problem does not require empirical testing in order to be identified and for problem solving to proceed. Problem S still results in a field of choice, with limited possible ways in which falsification can be logically isolated. An adequate solution to this problem requires elimination of the discordance (and any other refutation or discordance entailed by this set of propositions) by attributing falsification to one option within that field of choice, while avoiding the precipitation of inseparable refutation.

Such problems may still be accountable to multiple predictions, and this may facilitate narrowing of the field of choice. In this example, the discordant systems S1 and S2 may contain subsystems or recursion in a familiar way. However, subsystems and recursion may also be comprised of alternative subsets of propositions between S1 and 2.

System S1

$(P1 \cdot P2 \cdot P3) \rightarrow E1$

System S2

$(P4 \cdot P5) \rightarrow {\sim}E1$

Subsystem Ss

$(P1 \cdot P5) \rightarrow E2$

Recursive system Rs

$(P4 \cdot E2) \rightarrow E3$

These additional predictions entailed by subsets of propositions that constitute problem S may facilitate the elimination of options from the field of choice and constrain problem solving.

The conditions and general examples provided in this chapter are highly distilled to illustrate specific concepts. The implicit lesson, now made explicit, is that these constrains upon the ambiguity of falsification and problem solving are not mutually exclusive but can compound upon and reinforce one another. When problem solving is accountable to numerous predictions, the field of choice of a single problem can be narrowed from many directions simultaneously.

The ambiguity of falsification comes in degrees. Seminal works of Duhem and Quine, and much subsequent work on the ambiguity of falsification and its consequences, including the consensus problem, applies most fully to circumstances wherein a field of choice is fully open. A fully open field of choice, however, is only a special case within a more general framework for the analysis of falsification and problem solving. Partial determination limits the ambiguity of falsification and may eliminate the consensus problem (on a case-by-case basis) by facilitating eliminative induction toward an agreeable resolution. In the strongest conditions, falsification may even be fully determined. Additionally, the framework developed here provides insight into provisional constraints upon problem solving, which may have particular significance when problem solving within complex predictive networks.

Accumulating Constraint: The Historical Case of Biotic Disjuncts

The account provided above smooths away the complex reality of scientific work. In practice, the propositions that constitute a predictive system need not be entirely or immediately apparent. Concordances and discordances with other commitments within the corpus of scientific knowledge may also not be immediately apparent. Even when such commitments are clear, entailed prediction is not necessarily forthright, and the capacity to test prediction may lag far behind its formulation. Similarly, the full set of predictions entailed by a proposed resolution may not be immediately derived nor tested. Additionally, the corpus of scientific knowledge is constantly in flux, so apparent concordances and discordances related to some problem or proposed solutions may change due to scientific work elsewhere. In sum, systems, predictions, testing, and problem solving are inculcated in complex, diverse, and overlapping processes of actual scientific work. When combined with the analytic account developed previously, these complexities support an enriched view of scientific change.

Contrary to typical approaches to analysis of falsification and problem solving, the history of science is not adequately construed as a sequence of independent problems and solutions devised in turn. Rather, single problems can stretch across extended periods of diversifying scientific work. At first, the contours of the problematic system may be imprecise, and the set of predictions entailed by that problem may be unidentified or untested. Falsification may be highly underdetermined, and a great deal of freedom may be available for initial problem solving efforts. When a prospective resolution is first conceived, the full set of predictions entailed by that resolution may not be immediately derived or tested. As scientific work (pertaining to system articulation, prediction testing, and problem solving) takes place, constraint upon problem solving may accumulate. Resolutions that once seemed tenable and perhaps even highly agreeable may be invalidated by the eventual identification of inseparable precipitating refutation. Problems thought to be solved might re-emerge. Options in a field of choice may be eliminated, thereby limiting the underdetermination of falsification.

A falsified system may contain a grand theory that is preferentially retained during problem solving (akin to Kuhn or Lakatos). By stipulating ontology, universals, or other prescriptions, such commitments may impose strong restrictions upon the permissible modifications that can be made elsewhere. Also, when grand commitments are retained, some degree of subsystem or recursive consistency may be expected across proposed resolutions. Accordingly, preferential retention of grand commitments may be conducive to the sorts of analytic circumstances that invalidate proposed resolutions and eliminate options from a field of choice. The accumulation of such constraint may facilitate eliminative induction toward an agreeable resolution. Accumulating constraint may eventually eliminate all proposed resolutions to a falsification problem that retain the grand theory. In such circumstances, modification of preferentially retained commitments may become agreeable. Under sufficiently strong conditions, accumulated constraint might even fully determine the falsification of preferentially retained commitments. Even grand scientific change may be highly determined.

Accumulating constraint is apparent in historical efforts to resolve the problem of biotic disjuncts across continents. During the 19th century, paleontologists and biogeographers recognized that life forms are sometimes more similar with forms on opposing sides of oceans than neighboring forms on the same continent. Sometimes, this seemed to be the case for a single genus.[751] Sometimes, this seemed to be the case for larger taxa or even biomes. Given assumptions about descent with modification and dispersal capability of alternative biological forms, these patterns of biotic disjuncts posed a challenge to alternative predictive systems pertaining to the Earth and its physical history.[752]

Champions of contractionism could accommodate biotic disjuncts by postulating the existence of subsided continents or land bridges. For example, Eduard Suess claimed that a continent previously connected India, Madagascar, Africa, and Australia. The continent then subsided, forming the Indian Ocean, resulting in biotic disjuncts. Early proponents of permanentism, like Dwight Dana, were not as invested in biotic disjuncts, but this problem garnered significant attention in the first few decades of

[751] Fossilized plants called Glossopteris were found in Australia, India, and Africa, but not in Asia. Mesoraurus fossils were known only in South Africa and South America.

[752] A fully detailed account of accumulating constraint in this case would require dozens or hundreds of pages. The particular problem of biotic disjuncts is made more complex because it confronted multiple alternative grand models of the Earth, and these models were sometimes combined or distinguished by different individuals in different ways.

the 20th century. Of course, permanentists did not endorse the idea of sunken continents since oceans were deemed to be archaic. Instead, they relied on the notion of smaller land bridges or thin isthmian links between continents to account for biotic disjuncts. Thus, though contractionism and permanentism offered somewhat different systems of global physical history, their respective approaches to solving the problem of biotic disjuncts were fundamentally quite similar: land connections between continents had subsided. The timing, location, and number of these links was subject to debate. Still, all major proponents of land links between continents recognized that many such connections were needed, and the history of these connections would be highly complex.[753]

In his 1915 work, *The Origin of Continents and Oceans,* Alfred Wegener argued that the notion of subsidence of continental crust to a depth of many kilometers was isostatically impossible, like an iceberg sinking to the bottom of the ocean.[754] In the early 20th century, support for isostasy was increasing, and isostasy was increasingly integrated into global systems of physical history.[755][756] Efforts were thereby directed toward modification of the notion of subsidence to concord with commitments to isostasy. However, this task proved to be quite challenging, as proposed solutions precipitated still other inseparable problems that invalidated problem solving trajectories and narrowed the field of choice.

Two general routes could be pursued to align the notion of subsided continental links with the principle of isostasy within a permanentist framework. The first general approach could account for subsidence due to some process other than isostatic adjustment. For example, one possible route toward such a solution would be to postulate historical changes in sea level, resulting in emergence and subsidence of intercontinental links. This approach was taken for particular regions, such as a proposed land bridge connecting Siberia and Alaska during periods of glaciation, subsequently subsumed by rising sea levels.[757][758] However, as far as I am aware, this was never seriously championed on a global scale to account for biotic disjuncts. This may be because precipitating inseparable problems were readily

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[753] Arldt, 1907; Von Ihering, 1907; Grabau, 1909; Willis, 1909; Schuchert, 1909
[754] Wegener, 1924, 15
[755] See, for example, Chamberlin and Salisbury, 1907; Kober, 1921, 1928; Stille, 1924; Jeffreys, 1924
[756] Wegener thereby highlighted that the proposed resolution to biological disjuncts precipitated an inseparable discordance. This precipitated problem did not require empirical testing to be identified but was instead an entailed discordance between isostasy and postulated subsidence of continental links.
[757] Daly, 1929b; Antevs, 1935; Hopkins, 1965
[758] Increasing sea levels would result in isostatic adjustment but could subsume land even when that adjustment is minimal.

apparent.[759] In this case, it was well known that ocean basins were several kilometers deep, such that changes in sea level capable of opening connections across the Atlantic or Indian oceans would be incapable of producing required geographic and temporal distribution of intercontinental links.[760]

The second, more frequently pursued approach, consisted of the postulation of processes that could account for subsidence as a consequence of isostatic adjustment. Several such processes were proposed.[761] One influential example will suffice for present purposes. In 1932, American Geologist Bailey Willis and Paleontologist Charles Schuchert argued that biogeographical and paleontological data could be accounted by the subsidence of slender isthmian links of oceanic rock.[762][763] Willis proposed that these isthmian links were the product of eruptive or upwelled mases of basaltic oceanic rock which would cool and solidify and thereby increase in density. If the upwelling processes acted relatively swiftly, then accumulation of eruptive mass could outpace compensatory isostatic adjustments. This could result in the formation of land connections between continents that would subside only when the upwelling processes were outpaced by isostatic adjustment, as would surely be the case when such processes ceased. These isthmian links could avoid some of the problems that precipitated from subsidence of expansive continental land bridges. For example, subsidence of slender isthmian links could be accounted by smaller-scale and simpler geological processes than required to account for the subsidence of continents. Additionally, subsidence of large land bridges would result in a substantial concurrent global decrease in sea level, but evidence for such drastic change in sea level was not

[759] Global fluctuations in sea level were at times proposed, but not specifically to solve the problems of biotic disjuncts. For example, Grabau proposed a sea level "pulsation theory" which he linked to Joly's theory of thermal cycles (Grabau, 1934-1937, 1938, 1940).

[760] Another possible approach was to postulate crustal thinning through some process of crustal tension (Still 1944, 1955; Beloussov and Ruditch, 1961; Termier and Termier, 1963; Talwani 1962, Fairbridge, 1965). Proponents could point toward regions of the ocean where seismic data indicated crust of intermediate thickness, and presence of non-volcanic islands which showed gradual subsidence over time. Fairbridge, in 1965, argued for subsidence by thinning but also endorsed relative lateral drift as a consequence (Fairbridge, 1965).

[761] Jeffreys proposed the crystallization of denser materials upon lighter continental crust, resulting in subsidence (Jeffreys, 1924). Stille proposed the possibility of displaced mass at the lower level of the crust, resulting in subsidence (Stille, 1955).

[762] Willis, 1932; Schuchert, 1932

[763] Charles Schuchert was Professor Emeritus at Yale and 74 years old in 1932. Willis was Professor Emeritus at Stanford. Both were members of the National Academy of Sciences, both had served as President of the Geological Society of America, and both were prominent American opponents to Wegener's theory of drift. Schuchert had previously endorsed expansive land bridges and continued to do so even after his work with Willis (Schuchert, 1909, 1924; Schuchert and Dunbar, 1941).

apparent.[764] Slender isthmian links would displace less water, and thereby avoid the precipitated problem of sea level changes.

If isthmian links crossing ocean basins had subsided, as Willis proposed, then this would result in several additional predictions within a permanentist framework. Isthmian links were envisioned to be the product of rapid crustal thickening followed by isostatic adjustment. Elevation changes on the seafloor, consonant with locations of postulated isthmian links, would thereby be expected, at least for the most recently subsided isthmuses.[765] Additionally, gravimetric and seismic measurements could, in principle, identify regions of varying crustal density and thickness.[766] Willis argued that bathymetric evidence of subsided isthmuses from the Mesozoic could be identified between Africa and South America, Australia and Asia, South America and Antarctica, and also between India, Madagascar, and Africa.[767] He also claimed that the Mid-Atlantic Ridge was a subsided isthmus.

Charles Schuchert supported Willis' general notion of isthmian links, but also claimed that more expansive land bridges were required to account for certain biotic disjuncts.[768] In the 1940s, George Gaylord Simpson, perhaps the world's foremost expert on mammalian paleontology, improved upon Willis' proposed solution.[769] First, Simpson argued that different sorts of links between continents would result in alternative biogeographical patterns. Examination of biogeographical and paleontological data could thereby facilitate inferences about the geographical scale of subsided continental links. Simpson distinguished broad land bridges from narrower links which he associated with Willis' isthmuses and claimed that such narrow links may be sufficient to account for all mammalian biogeography. In part, Simpson argued, this was due to sloppy data and misinterpretations among those who aimed to solve the problem of biotic disjuncts.[770] In part, this was also due to gaps in the fossil record, as biological forms may have been far more widely dispersed than indicated by available paleontological data. In

[764] Wegener, 1924, 21-22
[765] Isostatic adjustment would retain some elevation difference, depending on the density of accumulated rock. In Willis' case the rock was of similar density to that of the seafloor.
[766] Willis explicitly appealed to gravimetry to argue that positive gravity anomalies over volcanic islands hinted toward future subsidence (Willis, 1932).
[767] On the poorness of Willis' bathymetric support, see Krill, 2011.
[768] Schuchert, 1932
[769] Simpson, 1940, 1943
[770] Simpson, 1943

effect, Simpson argued that fewer links between continents were required to account for biotic disjuncts and that isthmian links would be sufficient for all mammalian distribution.[771]

In general, alternative proposed solutions for biotic disjuncts from the 19th century into the middle of the 20th century, pushed many of their precipitating predictions to the bottom of the ocean, a location that was not readily testable.[772] As understanding of the earth's crust and seafloor improved, especially following WWII, constraints upon problem solving accumulated and many alternative solutions to the problem of biotic disjuncts were found to precipitate inseparable problems.

As previously noted, isthmian links entailed rapid crustal thickening followed by isostatic subsidence, and this entailed a host of additional predictions pertaining to seafloor structures. Improved bathymetric, gravimetric, seismic, and petrological data, however, did not reveal the anticipated remnants of isthmian links crossing ocean deeps. Gravimetry showed notable negative anomalies near trenches and island arcs, and slight positive anomalies around volcanic islands, but extended lateral anomalies across ocean basins were not found.[773] In conjunction with seismic studies, gravimetry also resulted in more sophisticated understanding of oceanic crust toward the middle of the 20th century. The ocean crust was of highly uniform thickness and consisted of a few layers with different seismic properties indicative of a certain range of petrological composition.[774] Large deviations in crustal thickness running laterally across ocean basins, or deviations in the thickness of alternative layers indicative of many kilometers of upwelled basalt (required for an isthmus to breach the surface) were not apparent.[775] The crust under the Mid-Atlantic Ridge was not thicker than under other parts of the

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[771] By focusing only on mammalian paleontology, Simpson's conclusions were largely limited to the Cenozoic (Du Toit, 1944).
[772] Some predictions entailed by proposed solutions also pertained to paleontology, but incompleteness of the fossil record could be cited to account for apparent precipitating problems (Simpson, 1943).
[773] See Chapter 3, pages 46-47 on initial measurements of Vening Meinesz and page 52-53 on subsequent utilization of such measurements to argue for downwelling of convection currents in the mantle, resulting in isostatic adjustments.
[774] Speculation upon crustal composition and distinction between continental and oceanic rock types began in the 19th century but improved notably around the middle of the 20th century. Based on seismic refraction and earthquake seismology studies, continental crust was known to be several dozen kilometers thick, while oceanic crust at abyssal plains were only around 5km thick. Continental crust and oceanic crust also propagate seismic waves at different velocities. Large scale seismic studies facilitated the generalization of petrological differences apparent between dredged oceanic basalts and lighter continental granites. Intermediary regions of shallower oceans were identified and sometimes found to propagate seismic waves at intermediate velocities, suggestive of some mixed composition crustal material (Gaskell, 1962).
[775] Guyots were recognized to be subsided islands of oceanic rock with characteristic flat tops due to erosive wave action. Dredged material eventually showed that some guyots were as old as the Cretaceous. Guyots could thereby provide insight into the degree of isostatic adjustment involved in subsidence of oceanic rock. However, while

ocean.[776] Ridges formed a global network, typically at positions median between continents, rather than intersecting with continents at locations required by isthmian links.[777] Long, parallel, magnetic anomalies ran across ocean basins, and could be tracked at times for thousands of kilometers.[778] This was known to be the product of seafloor spreading by the end of the 1960s, and large-scale eruptive activity such as that proposed by Willis would result in extensive linear disruptions to these anomalies, but this was not found.[779][780]

Additionally, during the 1950s it became increasingly apparent that the seafloor was younger than anticipated by permanentists. This was the result of several lines of evidence, most notably the thinness of accumulated sediment and the lack of pre-Cretaceous fossils on the seafloor.[781][782] The young age of the seafloor precluded the possibility of isthmian links between continents prior to the cretaceous, thereby posing a problem to the isthmian link solution of biotic disjuncts. Additionally, the young age of the seafloor seemed to directly refute higher-level commitments of permanentism pertaining to the archaic age of ocean basins. Permanentists thereby pursued alternative adjustments to retain the archaic age of the seafloor, while also accounting for its apparent youth. Thin surface sediments may be underlain by thick consolidated sediments, sedimentation rates may have been quite different in the past, some processes might remove sediment from the seafloor, or ancient fossils may be found in time. Each of these possible problem solving trajectories became problematic by the end of

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Guyots were readily apparent from bathymetric data, especially in the pacific, subsided intercontinental links crossing ocean basins were not.

[776] Talwani, Heezen and Worzel, 1961.

[777] See Chapter 3, pages 53-55 for an account of the development of the notion of a global ridge network.

[778] See Chapter 3, pages 58-61 and Chapter 5, pages 99-113 for an account of the identification of magnetic anomalies and their interpretations.

[779] Magnetic anomalies showed complex structures, and deviations from parallelism, especially with increasing distance from ridges. Additionally, near the magnetic equator, where the inclination of remanent magnetism is lowest, magnetic anomalies are not apparent. Some ridges (of slow spreading rate) also exceeded the resolution limits of magnetometry and thereby were not associated with anomalies. Still, the matter of relevance is that pronounced distortions in anomaly patterns would be expected if volcanic action had a role in subsidence of intercontinental links (as proposed by Willis), but such distortions were not apparent at suitable locations.

[780] As illustrated in Chapter 3, during the 1950's, ocean features were increasingly recognized to be systematically distinct from features of continental geology. This posed a clear problem to contractionism which did not mark a strong distinction between oceanic and continental geology. Additionally, by the end of the 1960s, seafloor spreading became widely accepted, and the youth of ocean basins was discordant with core commitments of permanentism.

[781] In 1953, Arthur Holmes raised the youthful age of the seafloor as evidence against the archaic origin of ocean basins and the prospect of subsided land bridges (Holmes, 1953). The scale of this problem to both contractionism and permanentism only increased over time. Contractionists may be fine with thin sediment, but not the absence of ancient fossils.

[782] See Chapter 3, pages 56-58 on the apparent youth of the seafloor inferred from sediment and alternative theoretical interpretations.

the 1960s. Seismic and gravimetric data showed that sediment was not underlain by consolidated sediment of sufficient thickness to account for an archaic seafloor. Coring work also showed that sediments were underlain by basalt. Retrieved sediment cores could be used to date the oldest seafloor sediment at different locations,[783] and pre-Cretaceous fossils were not found even in the lowest sediment layers. The research of Neil Opdyke facilitated the absolute dating and correlation of seafloor biostratigraphy, which provided insight into regional fluctuations in sedimentation rates and also allowed for regional dating of sediments even in the absence of fossils.[784] In sum, the possible routes through which permanentism and isthmian links could be aligned with apparent youth of the seafloor were eliminated.[785]

The global models first employed to account for Earth's physical history were not just accountable to a single prediction. Rather, these were complex systems comprised of many layers of interrelated subsystems. These systems were comprised of commitments pertaining to petrology, volcanism, thermodynamics, erosion, tectonics, isostasy, paleontology, evolutionary theory, and more. These commitments also evolved over time as systems were extended. Initial attempts to resolve the problem of biotic disjuncts preferentially retained many of these commitments by postulating the existence of subsided intercontinental links. Recursion of biotic disjunct anomalies even facilitated the identification of locations and ages of these links. However, to accord with isostasy, this subsidence could only take place in a limited number of ways. Either subsidence was due to isostatic adjustment via some geophysical process that influenced the thickness or relative density of the crust, or subsidence was not isostatically produced, but the product of some alternative process related to changes in the relative height between sea level and the subsided link. Alternative proposed solutions often precipitated additional predictions within the preferred global system. Some of these predictions pertained to paleontology and biogeography, petrology, geology, or climatology, and precipitating refutations compelled the refinement of proposed solutions. Thus, Harold Jeffreys attempted to update the theory of subsided land bridges to accord with available seismic data, while G.G. Simpson attempted to update the interpretation of biogeographical and paleontological data to limit the scale and number of intercontinental links. Many of these precipitated predictions pertained to features of the seafloor which were not readily testable until the maturation of marine geology following WWII. Upon testing,

[783] The Mohole is a well-known example of deep drilling. The JOIDES deep sea drilling project beginning at the end of the 1960s also found that base sediment increased in age with distance from mid-ocean ridges (Sclater and Detrick, 1973).
[784] See Chapter 5, pages 108-111, 114 on Opdyke's work on seafloor sedimentation (especially Opdyke, 1972).
[785] Theories of crustal stretching and subsidence were also found to be problematic for these same reasons.

these precipitated predictions were roundly refuted. The task of problem solving, in this case, was not simply to account for biotic disjuncts. Instead, the task was to eliminate the problem of biotic disjuncts without disrupting harmonies elsewhere in the preferred global system. When such harmonies were disrupted, proposed solutions had to be reworked.

The collection of predictions that had bearing upon the problem of biotic disjuncts changed over time as systems and proposed resolutions were articulated and relevant predictions tested. At first, the problem of biotic disjuncts pertained to biogeographical and paleontological data. Simpson later attempted to discard many of the apparent disjuncts, citing sloppy work and incompleteness of the fossil record. Alternative problem solving trajectories introduced new predictions of relevance, like bathymetric traces, properties of the Earth's crust, seafloor fossils, or global sea level changes. On occasion, lack of reliable observation limited the capacity to determine the viability of proposed resolutions. Suess proposed sunken continents to resolve the problem of biotic disjuncts, but it took nearly 60 years before sufficient understanding of the Earth's crust raised a precipitating problem with respect to isostasy. Willis attempted to resolve the problem of biotic disjuncts by proposing submerged isthmian links but precipitating predictions about seafloor structures could only be tested by the 1950s. Logical constraint upon problem solving can thereby accumulate over time. Additionally, problems that seem to be resolved may re-emerge following the identification of inseparable precipitating refutation.[786] Problem solving can be a very extended process. For about a century, many prominent paleontologists, biogeographers, geophysicists, and geologists attempted to resolve the problem of biotic disjuncts by postulating subsumed intercontinental links. At just about the same time that many of these problem solving trajectories were found to be insufficient, grand changes took place across the earth sciences with the rise of plate tectonics.[787]

As detailed in Part 1, Henry Frankel develops the notion of a difficulty-free solution, but he identifies these solutions empirically, without consideration of the possible epistemic mechanisms by which consensus forms. In this chapter, I have provided a partial account of these mechanisms. Adequate solutions to falsification problems must eliminate the refutation by attributing falsification to one viable option in a field of choice and avoid the precipitation of inseparable refutation. Problem

[786] Isthmian links garnered widespread support in the 1940s, even swaying strong supporters of mobilism like Arthur Holmes.
[787] Additionally, though not elaborated here, permanentism and contractionism confronted other problems pertaining to geological disjuncts, paleomagnetism, and the age of the seafloor. Constraint also accumulated on efforts to solve these problems.

solving efforts may be guided by narrowing of the field of choice or by constraints imposed by retained commitments. Otherwise, consensus formation during problem solving is based on the elimination of conceived solutions. Much of the historical account provided by Frankel can be read in this context: Permanentism and contractionism seemed to be incapable of accounting for an expanding collection of research by the end of the 1960s as conceived problem solving trajectories within these frameworks were definitively eliminated as non-viable. The formation of consensus around some proposed solution despite persistence of difficulties (or precipitated problems) is not fully accounted in this chapter. Such an account will be developed in Chapter 8.

Chapter 8: Snapping Together

Introduction

In the second half of the 19th century, Lord Kelvin estimated the age of the Earth based on a set of commonly accepted assumptions pertaining to thermodynamics, the physical properties of the Earth, and temperature gradient with depth. His estimates were subsequently roughly corroborated by several other prominent efforts to establish the age of the Earth, including John Joly's work on ocean salinity and William Johnston Sollas' work on the maximum thickness of the geological column. In the first half of the 20th century, research in radioactivity and convection currents became discordant with many of Kelvin's assumptions. Alternatively, assorted radiometric dating methods became increasingly coherent and sophisticated while establishing ages of minerals and rocks that greatly exceeded Kelvin's estimates of the age of the Earth. The work of Kelvin, Joly, Sollas, and others became increasingly obsolete.

In 1947, Keith Runcorn set out to test Blackett's theory of magnetism by measuring change of geomagnetic intensity with depth. His initial results seemed to confirm Blackett's theory. These results were also corroborated by independent work undertaken in South Africa and published in 1949. By 1951, however, Runcorn argued that these initial results did not adequately account for nearby magnetic fields and that depth had no measurable effect on the Earth's magnetic field.

In 1957, Allan Cox found that paleopole determinations of Eocene lava flows in Oregon deviated significantly from previous work on North American paleopoles. Cox thereby considered paleopole measurements to be of questionable reliability. Ted Irving, however, claimed that Cox's sampling region had undergone local rotation with respect to the rest of the continent, thereby accounting for the Cox's discordant measurements. If most paleopole inferences in the 1950s were unreliable, as Cox supposed, it is not difficult to imagine how similar adjustments to those endorsed by Irving could contribute to the apparent corroboration of paleopole determinations within continents.

In 1965, Frederick Vine and Tuzo Wilson published a magnetic profile of the Juan de Fuca Ridge. The profile did not clearly align with the reversal timescale in development at the USGS and ANU. By making arbitrary adjustments to the postulated seafloor spreading rate, Vine was able to make the profile align with the available reversal timescale. Indeed, by selecting only the most symmetrical profiles, and by adjusting seafloor spreading rates, Vine could, in principle, account for any arbitrary reversal timescale. Vine's work was not particularly convincing to opponents of seafloor spreading who could, presumably, recognize that suitable adjustments in seafloor spreading rate could accommodate any reversal timescale, even if seafloor spreading was false. Indeed, this soon became apparent to all upon the publication of an updated reversal timescale in 1966. Had the timescale not been updated so quickly, it is not difficult to imagine how Vine's notion of variable spreading rates might have been employed in the interpretation of other magnetic profiles, or models of convection, or seismic data, or elsewhere.[788]

These historical examples illustrate that successful predictive networks may contain false commitments and that scientists are aware of this possibility. In some cases, large chunks of a once-successful predictive network may be recognized as false. In other cases, alternative commitments between individuals or groups may result in the formation of alternative competing predictive networks. Regardless, such historical examples raise the prospect that any predictive network, regardless of falsity therein, may still be capable of indefinite growth and predictive success.

In this chapter, I identify the formation of certain network structures as *snapping together* events. In the strongest cases, snapping together can prohibit the falsity of certain commitments therein. In somewhat weaker conditions, snapping together may justify the conclusion that falsity within a network structure is implausible. Thus, in some circumstances, networks that contain some false commitment(s) may be incapable of certain kinds of network growth. In other circumstances, networks that contain some false commitment(s) may be less capable of certain kinds of network growth.[789] Consideration of network structures may thereby provide epistemic support to commitments therein, in excess to that which may be identified from the consideration of the individuated commitments or concordances that comprise that network.[790]

The argument in this chapter proceed as follows. First, I examine the snapping together of small network structures. Small structures are comprised of a small set of known interconnected concordances. In the strongest possible circumstances, small structures may prohibit the falsity of commitments therein. This insight may pertain to single commitments, sets of commitments, or all commitments within a structure. In less stringent circumstances, small structures may justify the

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[788] In 1965, Vine claimed sudden variation in seafloor spreading rates should be expected. After the discovery of the Jaramillo event resulted in realization that seafloor spreading rates were fairly consistent, Vine described his prior expectation of inconsistent spreading to be "bloody silly" (Frankel, 2012, Volume IV, 346).

[789] Inversely, networks that contain false commitments are prone to network growth that does *not* snap together.

[790] The term "snapping together" is intended to illicit an expectation of well-fitting parts that suddenly come together to form strong connections. In a general intuitive sense, when a network snaps together, components of that network may seem to be perfectly suited to one another, as if the network has anticipated its own growth. When a network snaps together, this can result in sudden appreciation of profound epistemic significance.

conclusion that falsity, therein, is implausible. I also examine the snapping together of large network structures. Large structures may contain large sets of commitments and interconnected concordances that may not be clearly defined. The snapping together of large structures can justify the conclusion that it is implausible for the relevant network to be substantially false. Establishing the impossibility of falsity within a structure requires the identification of compounded restrictions upon provisional falsity within constituent concordances. Alternatively, the implausibility of falsity in both small and large structures can be established when the growth of that structure requires the coincidence of concordances, were falsity contained therein. Finally, I illustrate the relevance of snapping together to the historical case study examined in Part 1. I argue that research in paleomagnetism, marine geology, and geochronology came together in such a way as to produce a series of snapping together events that convinced researchers of continental mobilism. I highlight the snapping together of commitments related to seafloor-spreading and the Vine-Matthews hypothesis from 1965-1966. Those most familiar with the relevant research were rapidly convinced of seafloor spreading. In 1967, snapping together of commitments related to seafloor spreading and crustal plates on a spherical surface directly inculcated continental mobilism. A series of additional snapping together events took place thereafter, contributing to the surging support for continental mobilism.

The Impossibility of Falsity within Small Structures

In this subsection, the idea of snapping together will be approached in two stages. In the first stage, I argue that concordances may provide insight into the provisional falsity of commitments therein. More specifically, the provisional falsity of some commitment(s) within a concordance may be restricted in known ways by that very concordance. Alternatives to some provisionally false commitment(s) can be definitively eliminated if their provisional truth would preclude the initial concordance. Additionally, the provisional falsity of some commitment may be known to require the complementary falsity of some concordant commitment(s). Factors such as these can provide insight into provisional falsity of commitment(s) within a concordance that would not be apparent from consideration of the provisional falsity of individuated commitments. In the second stage, I argue that interconnected sets of concordances allow for such restrictions upon provisional falsity to be compounded upon one another. I model alternative network structures that are conducive to such compounding restrictions, and I identify conditions wherein falsity within such structures can be known to be impossible.

Concordances can provide insight into the provisional falsity of commitments therein. If some commitment(s) within a concordance is provisionally taken to be false, then viable alternatives to the false commitment(s) *cannot* preclude the initial apparent concordance.

Suppose that a fruit company distributes boxes of bananas to grocery stores. Though all boxes are identical, each box is filled with either 20 bananas or 10 bananas. A grocery store receives an order from this company, and a grocer asks a cashier to open the box and price the contents. The grocer suspects that the box contains 20 bananas. We will call this P1. The grocer also knows that every piece of fruit in the store is priced at either $1, $2, or $3 per item. The grocer thinks that a banana is priced at $2. We will call this P2. The grocer thereby suspects that the contents of the box will be priced at $40. We will call this E. Having checked the box, the cashier informs the grocer that the contents of the box cost more than $20. Suppose a customer then tells the grocer that bananas aren't priced at $2 each. The grocer considers the possibility that the customer is correct by speculating upon the falsity of P2. In this case, the grocer recognizes that if the price of each banana is not $2, then it must be $3. The grocer knows that the conjunction of P1 and P2 aligns with available evidence E, and the only way that this concordance could take place, despite the falsity of P2, is if the price of each banana was $3.[791]

In this scenario, the concordance of P1 and P2 with respect to E provides insight into the provisional falsity of P2. This is a highly prescribed scenario wherein the box is known to contain 20 bananas or 10 bananas, the price of each banana is known to be either $1, $2, or $3, and the cashier is taken as the authority on the cost of the bananas in the box. It is by virtue of knowing that the contents of the box cost more than $20 that the grocer can establish that the provisional falsity of P2 requires that bananas cost $3, regardless of the falsity of P1. Similar insights about provisional falsity of commitments within a concordance can be reached even in the absence of definitive evidence statements. This may be illustrated by considering the consequences of provisional falsity within a corroborative relationship, wherein predictions are tested against one another.

Suppose we have two clocks, one digital and one mechanical. When the two clocks were purchased, they were both synchronized to a local clock tower reference at midnight. The mechanical clock is kept in a drawer, out of sight. The digital clock is then used to coordinate daily activities. Whenever the digital clock indicates that it is 12 O'clock, we open the drawer to check this time against the mechanical clock. We then put the mechanical clock back in the drawer. Over the course of several

[791] If, instead of costing $3, each banana cost $1, then it would not be possible for the conjunction of P1 and P2 to concord with respect to E. For any possible value of P1, P1 and P2 would be discordant with respect to E.

days, every time that we compare the digital clock to the mechanical clock, they agree. Because of this, we suspect that the digital and mechanical clocks are synchronized, and we may also suspect that they correspond with the local clocktower. In consulting the user manual for the digital clock, however, we learn that an occasional production error results in the digital clock running at an inconsistent rate wherein the clock will run slowly after midnight and then run quickly after noon. In such circumstances, the digital clock would show the correct local time only once a day, at midnight. We also learn that occasional manufacturing errors can result in the mechanical clock running slowly by 1 minute per hour, or the clock may stop entirely.

We may inquire into the provisional falsity of our assumption that each of the two clocks are keeping accurate time. If the mechanical clock is not keeping accurate time, then it may either be losing one minute each hour, or it may be stopped completely. Regardless of whether the digital clock is keeping accurate time, a slow mechanical clock would not corroborate the digital clock at midnight each night. If the digital clock were accurate, the two clocks would disagree for approximately 30 days, at which point the mechanical clock would be a full 12 hours behind. The clocks would then corroborate once and disagree for another 30 days. Alternatively, if the digital clock were not keeping accurate time, then the two clocks might agree twice in succession, before becoming discordant. For example, at midnight, both clocks may agree, and as the mechanical clock runs slowly in the morning, so too may the digital clock. At noon, the two clocks may corroborate again. However, after noon, the digital clock would run quickly and deviate from the mechanical clock. The two clocks would thereafter remain discordant for 30 days.

If, rather than running slow, the mechanical clock was stopped entirely at the 12 O'clock position, then anytime the digital clock reads 12, this would be corroborated by the mechanical clock, regardless of the accuracy of the digital clock. Thus, in this circumstance, it can be known that if the mechanical clock is not keeping accurate time, it must be stopped at the 12 O'clock position. Any possible alternative would not allow for the apparent corroboration between the two clocks.[792]

In the example of the grocer, a concordant conjunction facilitated certain conclusions about provisional falsity therein. In the clock example, successful corroboration facilitated certain conclusions

[792] It is also clear, in this case, that insight into provisional falsity within a concordance can be influenced by the nature of the concordance, along with the specific commitments involved. In this case, if the clocks were checked against each other only once every 30 days rather than every 12 hours, then we could not be certain that the mechanical clock has stopped.

about provisional falsity therein. In both cases, alternatives to the commitment taken to be provisionally false can be eliminated when the provisional truth of these alternatives would prohibit the initial concordance.[793][794]

In both examples above, insight into provisional falsity is facilitated by enumeration of known alternatives. It is by virtue of knowing that bananas cost either $1, $2, or $3 and that each box contains either 20 bananas or 10 bananas that the grocer can establish that the price of each banana cannot be $1, regardless of the number of bananas in the box. Similarly, it is by virtue of knowing that the mechanical clock can either be accurate, run slowly, or stop entirely, and that the digital clock may either be accurate or run inconsistently that we are able to recognize that a slow running mechanical clock could not result in the apparent corroboration with the digital clock. In each case, possible alternatives to provisionally false statements have simply been provided as parts of highly prescribed scenarios. However, such possible alternatives may be identifiable in other ways. For example, some collection of data may facilitate a knowable set of possible conclusions, or a particular statement may be known to be either true or false. Possible alternatives to a provisionally false statement may also reside within a known range, even if there are infinite possibilities within this range. What is important, for the purpose of identifying knowable consequences of provisional falsity within a concordance, isn't necessarily that all possible alternatives to a statement can be enumerated, but that alternatives are known to be unique.

As an illustration, we may return to the grocery store. Suppose that a box contains 8 candies (P1). A grocer may believe that there are 10 candies in that box (P1'), while a customer may maintain that the same box contains only 4 candies (P1''). Both the grocer and customer hold false commitments about the number of candies in the box, but their commitments are false in unique ways. The grocer overestimated the number of candies by 2. The customer underestimated the number of candies by 4. Suppose that the grocer and the customer go to the cashier to finalize the purchase. The cashier counts the candies and charges the customer $4 (E), since each candy costs 50₵ (P2). The grocer, still intent that

[793] If the cashier provided the grocer with different information, then provisional falsity may not be restricted in the same way. Similarly, if the two clocks were checked against one another only once every thirty days, then their corroboration would not provide any insight into provisional falsity therein. In both cases, it is the manner in which specific propositions concord that provides insight into provisional falsity.

[794] Additionally, in both examples, there are two commitments that may each be taken as provisionally false, independently of the other, yet regardless of where provisional falsity is attributed in these examples (P1 or P2 or both) restrictions upon provisional falsity are only apparent in one of the two commitments (P2). This was intentionally designed for illustrative simplicity and is not a general feature of restrictions upon provisional falsity within concordances.

the box contains 10 candies, may suppose that each candy costs 40₵ (P2’). The customer, still intent that the box contains only 4 candies, may suppose that each candy costs $1 (P2’’). In order to accord with the cashier’s charge, the customer and grocer can adjust their commitments in alternative ways. In this case, those adjustments uniquely compensate for the unique falsity of their P1 commitments.[795]

With the benefit of knowing the actual number of candies in the box, the unique differences between the false commitments of the grocer and the false commitments of the customer are apparent to the cashier. Things are not quite so clear to the grocer and the customer, because they do not know the actual number of candies in the box, so they do not know where false commitments may reside. Still, both the grocer and the customer would know that if a commitment about the number of candies in the box were false, then in order to align with the cashier’s charge of $2, the commitment about the cost of each candy would also have to be false in a complementary way. More specifically, the grocer and the customer would know that if the number of candies was underestimated, then the price per candy must be proportionally overestimated, and if the number of candies was overestimated, then the price per candy must be proportionally underestimated. Unlike the previous two examples, the set of possible alternatives to P1 and P2 commitments are not clearly enumerated in this case. Additionally, provisional falsity within this concordance does not eliminate or establish any specific alternative(s) to either P1 or P2. Still, it is apparent that the provisional falsity of either one of these commitments requires the falsity of the other, and that the provisional falsity of these commitments can be defined in relation to one another.

In this candy example, provisional falsity in one commitment is known to require complementary falsity in another concordant commitment. In this case, for any P1 there is a unique P2 that will entail E, and for any P2 there is a unique P1 that will entail E. When a commitment is taken as provisionally false, this requirement of complementary falsity within a concordance may become apparent in other ways. For example, it may be known by enumeration that if P1 is false, then the conjunction of P1 with P2 would result in discordance unless P2 were also false. Provisional falsity may also be known to require falsity elsewhere due to logical relationships between concordant commitments. For example, provisional falsity of some commitment that is the consequent of an entailed concordance would require the falsity of the antecedent.[796]

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[795] In this example, for any P1 there is a unique P2 that will entail E. In general, this need not be the case.
[796] Recall from Chapter 7 that falsification problems have a field of choice that can be narrowed. In these examples, there is not necessarily a falsification problem to be solved. Rather, these are successful concordances wherein

To recapitulate what has been covered to this point, concordance can provide insight into the provisional falsity of commitments therein. When inquiring into the provisional falsity of some commitment(s), alternatives to that commitment can be eliminated if their provisional truth would make the concordance of the initial statements impossible. In some cases, provisional falsity of some commitment(s) may require the falsity of some alternative concordant commitment(s), such that the false commitments complement one another.

Now, by expanding our view from single concordances to sets of overlapping concordances, it is possible to identify circumstances wherein such restrictions upon provisional falsity from one concordance may be compounded by restrictions imposed by another concordance. I will identify four general models of small network structures that facilitate such compounding of restrictions. These will be called Radial Concordance, Duplicated Concordance, Circular Concordance, and Tiered Concordance.

When some commitment concords with two or more alternative commitments, this network structure will be called Radial Concordance.

Examples:

- $[((P1 \cdot P2) \rightarrow E1) \cdot E1] \cdot [((P1 \cdot P3) \rightarrow E2) \cdot E2]$
- $(P1 \rightarrow P2) \cdot (P1 \rightarrow P3)$
- $[(P1 \rightarrow E1) \cdot (P2 \rightarrow E1)] \cdot [(P1 \rightarrow E2) \cdot (P3 \rightarrow E2)]$

For the sake of illustrative simplicity, the notation "$\leftrightarrow$" may be used to represent any of the three modeled concordance relationships from Chapter 6. The general form of Radial Concordance may then be expressed as follows.

$$P2 \leftrightarrow P1 \leftrightarrow P3$$

This simplified notation facilitates the illustration of more complex Radial structures.

$$\begin{array}{ccccc} & & P4 & & \\ & & \updownarrow & & \\ P2 & \leftrightarrow & P1 & \leftrightarrow & P3 \\ & & \updownarrow & & \\ & & P5 & & \end{array}$$

speculation into provisional falsity takes place. Logical relations between subsystems or recursion may have bearing, but there is no "refutation" which would otherwise facilitate the isolation of falsification.

In a Radial structure, restrictions upon the provisional falsity of the central commitment (P1 in the two examples above) may be restricted in multiple ways by alternative concordances. For example, suppose that some proposition P1, concords with another, P2.

$$(P1 \rightarrow E1) \cdot (P2 \rightarrow E1)$$

This concordance may restrict the provisional falsity of P1 in some known way. For example, if P1 is false, then there may be a limited set of viable alternatives to P1 that would not preclude the apparent concordance of P1 with P2.

Suppose that P1 also concords with P3.

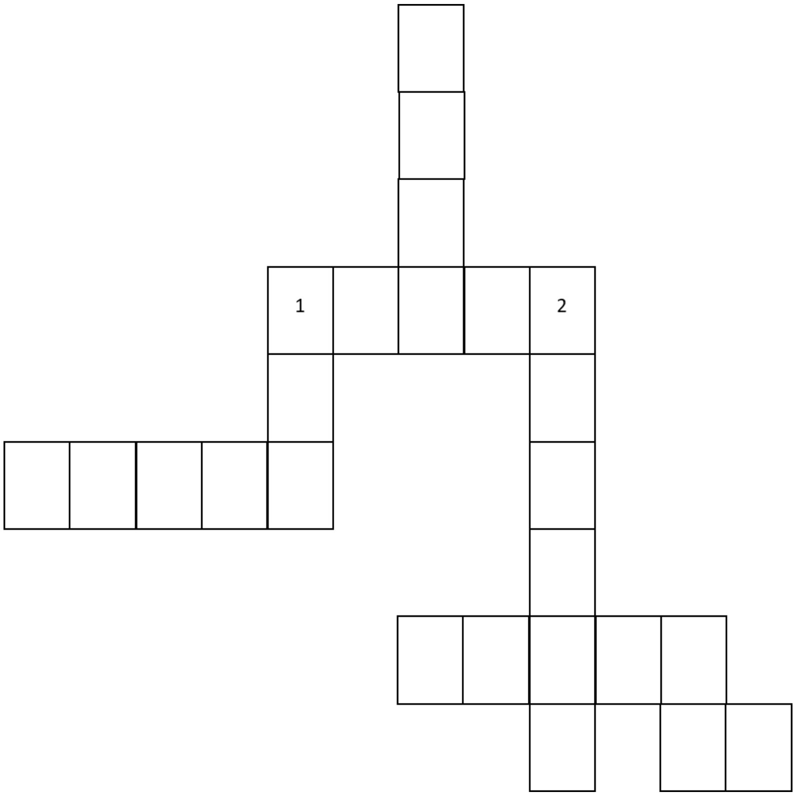
$$(P1 \rightarrow E2) \cdot (P3 \rightarrow E2)$$

This concordance may also restrict the provisional falsity of P1 in an alternative way. It may be the case that the set of viable alternatives to P1, apparent from its concordance with P2 is different from that set of viable alternatives to P1, apparent from its concordance with P3. In such circumstances, if P1 were false, then the only viable alternatives to P1 will be those contained within the overlap of these two sets.

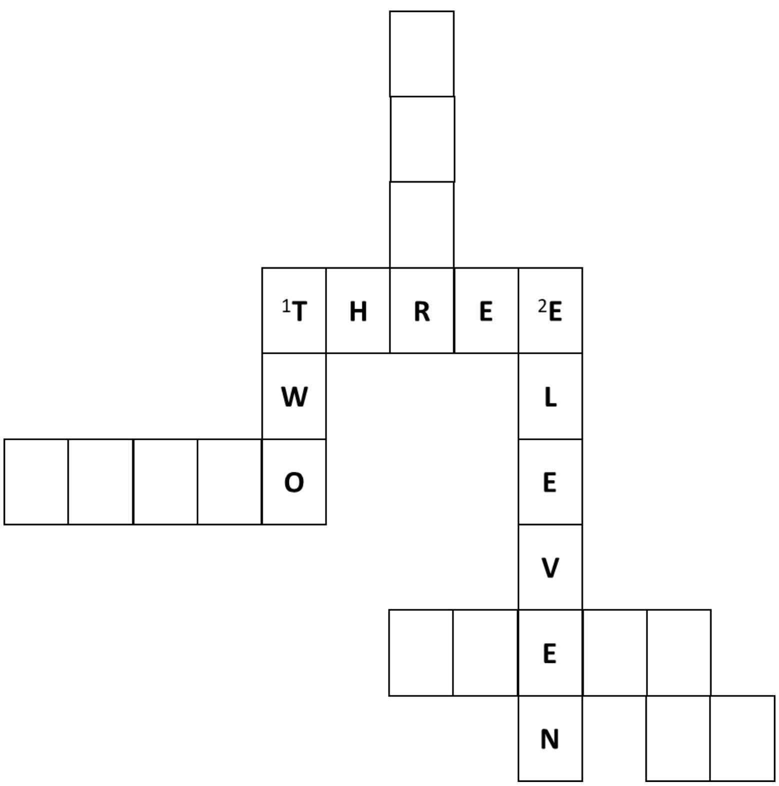
Consider the following illustrative example. A crossword puzzle consists of a set of clues, each of which corresponds to a certain word of designated length, but there may be many words that satisfy these criteria. The words in a crossword puzzle intersect one another at specified locations, such that both words share a common letter at a designated point of intersection. Solving a crossword thereby involves the formation of a predictive network. The words that are entered into the puzzle are individual commitments that concord with one another by corroborating specific letters. Individual clues and designated word lengths contribute to judgements about the suitability of individual words, but so too do other parts of the puzzle that are already (perhaps tentatively) completed.

Clues:

1. Across: A number
1. Down: A number
2. Down: A number



Suppose that we have started to solve this crossword puzzle by entering THREE for 1. Across, TWO, for 1. Down, and ELEVEN for 2. Down.



This section of the puzzle seems to be adequately solved for now, so we move on to work elsewhere. Suppose, however, that our puzzle solving progress eventually stops before the puzzle is completed. We may then return to this completed portion of the puzzle and inquire into the possible falsity of these three words.

In this case, we can easily enumerate all possible alternatives to each of the three words, based only on the available clue and the designated word length.

- Alternatives to 1. Across: SEVEN, EIGHT, FORTY, FIFTY, SIXTY
- Alternatives to 1. Down: ONE, SIX, TEN
- Alternatives to 2. Down: TWELVE, TWENTY, THIRTY, EIGHTY, NINETY

By considering designated overlaps between words, some options in these lists can be eliminated because they would unavoidably result in discordance. Considering the overlap between 1. Across and 1. Down, *if* THREE is false, then SEVEN and SIXTY would be viable alternatives, but EIGHT, FORTY, and FIFTY would unavoidably result in discordance. Alternatively, *if* THREE is false, then the designated corroboration by 2. Down would make SEVEN and EIGHT the only viable alternatives, as FORTY, FIFTY, and SIXTY would unavoidably result in discordance. In this case, the additional insight that is apparent from Radial Concordance is obvious. SIXTY could align with 1. Down, but not 2. Down, and EIGHT could align with 2. Down, but not 1. Down. Accordingly, *if* THREE is false, then SEVEN is the only possible alternative. Every other option would unavoidably result in discordance. This then determines that 1. Down must be SIX, and 2. Down must be NINETY.

In this example, Radial Concordance can provide insight into the provisional falsity of each crossword clue. Additionally, this insight would not be apparent by considering individual words, or individual concordances, alone. By comparing restrictions upon provisional falsity within one concordance against another, we know that there are two entirely mutually exclusive networks that could satisfy all available criteria for this section of the crossword puzzle.

Duplicated Concordance applies when two or more commitments concord in two or more ways.

Examples:

- $[(P1 \rightarrow E1) \cdot (P2 \rightarrow E1)] \cdot [((P1 \cdot P2) \rightarrow E2) \cdot E2)]$
- $[((P1 \cdot P2) \rightarrow E1) \cdot E1)] \cdot [((P1 \cdot P2 \cdot P3) \rightarrow E2) \cdot E2)]$

Duplicated Concordance may be represented as follows.

$$P1 \leftrightarrow P2 \leftrightarrow P1$$

The similarity to Radial Concordance is apparent. If each concordance is represented with its own set of double arrows, then more complex Duplicated structures can be represented as well.

$$P1 \leftrightarrow P2 \leftrightarrow P3$$

Duplicated Concordance can provide additional insight into provisional falsity.

Suppose that some proposition P1, concords with another, P2. This concordance may restrict the provisional falsity of P1 in some known way.

$$(P1 \rightarrow E1) \cdot (P2 \rightarrow E1)$$

Suppose that P1 and P2 concord elsewhere as well.

$$(P1 \cdot P2) \rightarrow E2) \cdot E2$$

This alternative concordance may restrict the provisional falsity of P1 in an alternative known way. These alternative restrictions may compound upon one another, thereby restricting the provisional falsity of P1 in a manner that would not be apparent from consideration of individual concordances. Like Radial Concordance, restrictions upon the provisional falsity of P1 may then restrict provisional falsity of P2. Unlike Radial Concordance, these restrictions upon P2 may then iteratively restrict P1 still further.[797]

Circular Concordance applies when a commitment within a chain of Radial Concordances concords with an earlier link in that chain.

Examples:

- $[((P1 \cdot P2) \rightarrow E1) \cdot E1] \cdot [((P2 \cdot P3) \rightarrow E2) \cdot E2] \cdot [((P1 \cdot P3) \rightarrow E3) \cdot E3]$
- $(P1 \rightarrow P2) \cdot (P2 \rightarrow P3) \cdot (P3 \rightarrow P1)$
- $[(P1 \rightarrow E1) \cdot (P2 \rightarrow E1)] \cdot [(P2 \rightarrow E2) \cdot (P3 \rightarrow E2)] \cdot [(P3 \rightarrow E3) \cdot (P1 \rightarrow E3)]$

With generalized notation, Circular Concordance may be represented as follows.

[797] A specific example of such a structure would be a Circle-In-The-Square crossword puzzle, wherein designated boxes within a crossword puzzle contain a circle. When the crossword is complete, the circled letters then spell out a word that corresponds with some clue or theme.

$$\begin{tikzcd}
& P1 \arrow[dl, leftrightarrow] \arrow[dr, leftrightarrow] & \\
P2 \arrow[rr, leftrightarrow] & & P3
\end{tikzcd}$$

More complex Circular Concordance may be expressed as follows.

$$\begin{tikzcd}
& P1 \arrow[dl, leftrightarrow] \arrow[dr, leftrightarrow] & \\
P2 \arrow[dr, leftrightarrow] & & P3 \arrow[dl, leftrightarrow] \\
& P4 &
\end{tikzcd}
\qquad\qquad
\begin{tikzcd}
& P1 \arrow[dl, leftrightarrow] \arrow[dr, leftrightarrow] & \\
P2 \arrow[rr, leftrightarrow] \arrow[dr, leftrightarrow] & & P3 \arrow[dl, leftrightarrow] \\
& P4 &
\end{tikzcd}$$

Suppose that some statement P1, within a Circular Concordance structure is taken to be false. The falsity of P1 may require complementary falsity of statement P2 in order to concord with P1. The falsity of P1 may also require complementary falsity of statement P3. In Circular Concordance, these required complementary falsities can be directly tested against one another. This would not be possible within a Radial Concordance structure.[798]

In Radial Concordance, the removal of the central statement would destroy the radial structure, but the removal of a peripheral statement would only influence a single concordance. In Circular Concordance, the removal of any one statement would result in the loss of Circular Concordance, yet retention of a Radial structure. Tiered Concordance applies when there are multiple tiers of concordances within a set of networked statements, such that the removal of some statement would destroy the tiered structure.[799] This may be the case if some concordance tests a prediction from an alternative concordance.

Examples:

- [(P1 · P2) → P4] · [(P2 · P3) → P5] · [((P4 · P5) → E) · E]
- [(P1 · P2) → E1] · [((P3 · E1) → E2) · E2]
- ((P1 · P2) → E) · ((P3 · P4) → E)

To represent Tiered Concordance in generalized form requires a slight addition to the generalized notation.

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[798] Radial Concordance is best suited to restricting provisional falsity of the proposition at the center of the structure. This may then have ramifications upon peripheral commitments. Circular Concordance has no center.
[799] The distinction between tiers is based on levels of entailed relationships.

$$\begin{array}{c} P1 \leftrightarrow P2 \\ \updownarrow \\ P3 \end{array}$$

In this case, the bisected concordance arrow between P1 and P2 indicates that their concordance is contingent upon mutual concordance with P3.

More complex Tiered Concordances may then be represented as follows.

$$\begin{array}{ccccc} P1 & \leftrightarrow & P2 & \leftrightarrow & P3 \\ & \updownarrow & & \updownarrow & \\ & P4 & \leftrightarrow & P5 & \end{array} \qquad\qquad \begin{array}{c} P1 \leftrightarrow P2 \\ \updownarrow \\ P3 \leftrightarrow P4 \end{array}$$

Tiered Concordance can provide additional insight into provisional falsity.

Suppose that the conjunction of propositions P1 and P2 entails a prediction that is corroborated by the conjunction of P3 and P4.

$$((P1 \cdot P2) \rightarrow E) \cdot ((P3 \cdot P4) \rightarrow E)$$

Suppose that statement P1 is taken to be false. The concordance of P1 with P2 may restrict the provisional falsity of P1 in some known way. The additional concordance of P1 (in conjunction with P2) with P3 and P4 may impose additional restrictions.

Each of the four network structures identified above can be elaborated or combined to model more complex circumstances in which additional insight into provisional falsity may be possible.[800]

The function of identifying Radial, Duplicated, Circular, and Tiered Concordance is to highlight that network structures may facilitate insight into provisional falsity of commitments therein that may not be apparent from consideration of individuated concordances. In the strongest possible circumstances, such compounding restrictions upon the provisional falsity of some commitment(s) may eliminate every possible alternative. Stated alternatively, provisional falsity within a network structure may unavoidably result in discordance that can only be resolved by restoration of the commitment(s) taken as false. In such circumstances, the network structure prohibits the falsity of some commitment(s) therein. This may pertain to single statements, sets of statements, or all statements contained within

[800] It may seem as though these four structures are already highly complex, but this is only due to the detail with which they have been examined. When solving a crossword puzzle, or sudoku, or certain other puzzles, fruitful consideration of provisional falsity within a network is quite simple and commonplace.

the structure. When a structure prohibits provisional falsity of commitments therein, the entire structure may be said to *snap together*.

Consider the following example of Radial Concordance

$$[((P1 \cdot P2) \rightarrow E1) \cdot E1] \cdot [((P1 \cdot P3) \rightarrow E2) \cdot E2]$$

The concordance of P1 with P2 may restrict the provisional falsity of P1 to a limited set of viable alternatives. Additionally, the concordance of P1 with P3 may also restrict the provisional falsity of P1 to a limited set of viable alternatives. These sets may not overlap. In such circumstances, the network structure prohibits the falsity of P1 since there would be no viable alternative to P1.

Consider the following example of Circular Concordance.

$$[((P1 \cdot P2) \rightarrow E1) \cdot E1] \cdot [((P2 \cdot P3) \rightarrow E2) \cdot E2] \cdot [((P1 \cdot P3) \rightarrow E3) \cdot E3]$$

We may inquire into provisional falsity within this structure, by first focusing on the two conjunctions of which P1 is a part.

$$((P1 \cdot P2) \rightarrow E1) \cdot E1$$

$$((P1 \cdot P3) \rightarrow E3) \cdot E3$$

If P1 is false, it may be known that P2 must be false in a complementary way. If P1 is false, then it may be known that P3 must be false in a complementary way as well. The falsity of P2 may thereby be defined with a specific relation to P1, and the falsity of P3 may be defined with a specific relation to P1. It may be the case that the conjunction of such a P2 with such a P3 would be known to unavoidably result in discordance. Of course, it is apparent from the network structure that P2 and P3 concord with respect to E2. Thus, the network structure prohibits the falsity of P1. A similar impossibility result may also apply to the provisional falsity of P2 and/or P3.[801]

The example of Radial Concordance pertains to circumstances wherein a set of alternatives to some provisionally false commitment(s) can be defined. Alternatively, the example of Circular Concordance pertains to circumstances wherein provisional falsity is defined in relation to some other

[801] In this case, the provisional falsity of commitments within each concordance are defined in relation to one another. Even so, the Circular Concordance structure facilitates absolute conclusions. The impossibility of the falsity of P1 may also contribute to limiting the provisional falsity of P2 and/or P3.

commitment(s). In principle, impossibility results may be identifiable for either sort of insight into provisional falsity within each of the four models of small structures.[802]

The Implausibility of Falsity within Small and Large Structures

The conditions that are required to identify the impossibility of falsity within a structure may be quite stringent. Specific commitments must be known, the concordances involved must be known, and the insight into provisional falsity within these concordances must be such that the compounding of such restrictions eliminates every possible alternative to the commitment(s) taken to be false. The recognition of such impossibility is most readily achieved when dealing with small and simple network structures, wherein alternatives to constituent commitments can be readily enumerated or when the provisional falsity of commitments are known to be defined in relation to one another in a certain way. Provisional falsity within network structures can also be recognized to be highly implausible. The ways in which implausibility of falsity within a structure might be established are quite different from the previous approach to impossibility and may be more broadly applicable.

In Chapter 6, I described the epistemic significance of concordances between independently accepted commitments. The epistemic significance of such concordance, I argued, may come in degrees. After all, independently established commitments can coincidentally concord when one or more of those commitments are false. I claimed that concordance that is implausible, were commitments therein false, may warrant greater epistemic significance, and that this implausibility is related to the specificity of the concordance and the diversity of possible alternatives to the commitments contained therein. When there are many possible ways for some commitment(s) to be false, and relatively few ways for such falsity to concord, the coincidence may be deemed implausible.[803]

In the previous section of this chapter, I described how concordances may combine or overlap to form different types of small structures. When concordances combine or overlap, restrictions upon provisional falsity within each of these concordances may be compounded. Even commitments that are not directly concordant may restrict one another by restricting the provisional falsity of a mutually

[802] Radial Concordance is less conducive to impossibility results wherein provisional falsity is defined in relation to some other commitment. Radial Concordance is also more conducive to impossibility results that apply to an individual commitment (the central commitment) within a structure, rather than sets of commitments.
[803] In Chapter 6, I focused on the bearing that this had upon the epistemic significance of a concordance, by establishing the “remarkableness” of a concordance.

concordant intermediary. In the previous section, this route of inquiry was directed toward establishing that provisional falsity of some commitment(s) within a structure can be known to be impossible. Even when such impossibility results do not obtain, overlapping or combined concordances may still impose restrictions upon provisional falsity therein. Suppose that some such network structure was formed by concordances between independently accepted commitments and that this structure contains some false commitment(s). In such circumstances, not only would some coincidence be required for the formation of the concordance(s) associated with the false commitment(s), but also an additional coincidence may be required for these concordance(s) to combine or overlap elsewhere in that structure.

As an illustration of this, suppose that P1, P2, and P3 are independently accepted statements that are each false. P1 is found to concord with P2, but the coincidence required for this concordance is small and the concordance of P1 with P2 limits the possible alternatives to P2 only slightly. P2 is also found to concord with P3, and the coincidence required for this concordance is also slight. Though in each case the coincidence required for concordance is slight, this may not be the case for the overlap of these two concordances. In this case, restrictions upon provisional falsity of P2 imposed by concordance with P1 may limit possible alternatives to P2 in such a way as to make the coincidental concordance of P2 with P3 *less* plausible, were P2 false. The matter of relevance, here, is that provisional falsity within a concordance may require a coincidence in the formation of that concordance, but also that provisional falsity within a structure may require *additional* coincidence in the formation of that structure, and this would not be apparent by consideration of only individual concordances therein. Network structure may thereby add to the epistemic significance of concordances therein.

There is an alternative way in which concordances can be formed, which I will call *special fitting*. Special fitting takes place when a commitment is formed to concord with some other, already accepted commitment.[804] A commitment that is formed by special fitting may go on to form additional concordances or obtain empirical support. Such concordances are not as epistemically significant as concordance of independently accepted commitments.[805] Were a concordance formed by special fitting

[804] This notion of special fitting can also apply during problem solving. Problem solving may proceed by adjusting some commitment(s) until the problem is resolved by the (re)formation of concordance. Even if the discordant commitments were independently accepted, the adjustments made during problem solving could be due to the pursuit of concordance.
[805] In Chapter 6, I noted that independent epistemic support for concordant commitments influences epistemic significance.

to contain some false commitment(s), the formation of such a concordance may not require a coincidence.

Consider a simple concordance between two propositions.

P1 ↔ P2

This concordance may be the result of special fitting. P1 may have been contained within a corpus of accepted statements, and P2 may have been formed to concord with P1. If P1 were false, then P2 could be formed specifically to complement this falsity. If P2 were false, then this falsity would complement P1, since P2 was formed to concord with P1.

Network structures grow by the incorporation of new concordances, so certain network growth may be due to special fitting, or it may be said that special fitting has had a certain amount of influence upon the development of a certain network. By special fitting, false commitments may accrue concordances. Even a network that contains many falsities may continue to grow, and such growth may not require the coincidence of concordances therein.

Consider a simple Radial Concordance that may be represented as follows.

P1 ↔ P2 ↔ P3

In this example, P1 may have been contained within a corpus of accepted statements, and P2 may have been added by special fitting. With P2 in place, P3 may then have been added by special fitting. Supposing that P1 is false, P2 could be formed to concord with the falsity of P1. Supposing P2 is false, then P3 could be formed to concord with the falsity of P2. In this case, the formation of the Radial structure by special fitting does not require the coincidence of concordances therein, and a chain of concordances could continue to grow by special fitting, even if the chain contains numerous false statements.

Network growth by special fitting depends upon the specific commitments contained therein, which may include false commitments. For example, network growth by the addition of concordances to some statement, P1, may involve special fitting that would be quite different from that required for some alternative P1’. Supposing that some network includes statements P1, P2, and P3, network growth by the addition of concordances to P1 may involve special fitting that would be quite different from that required if, instead of P2 and P3, the network contained statements P2’ and P3’.[806] Finally, supposing that some statement P1 concords with P4, the growth of such a network by special fitting may be quite different from that required if P1 concorded with P4’.[807] In general, it may be said that network growth by special fitting results in a network that is contextually tailored to the unique set of commitments and concordances contained therein. Locally, specific commitments are tailored to one another, to form concordances while avoiding discordance elsewhere. Globally, a network that has grown by special fitting will be highly contingent upon an evolving idiosyncratic set of commitments therein.

Sometimes, network growth by special fitting may be readily apparent.[808] For example, in 1965, Vine recruited the notion that seafloor spreading rates are variable, in order to align measured magnetic profiles with the available reversal timescale. The variations in spreading rates endorsed by Vine were specifically developed to align with the available reversal timescale, and he offered no additional defense or evidence for these variations. Sometimes, the role of special fitting in the growth of a predictive network may not be so easily established.[809]

Provisional falsity within a network structure may be deemed implausible when the formation of that structure requires the coincidence of concordances (and avoidance of discordance), were falsity contained therein, regardless of the possibility of special fitting. This takes place when network growth in one part of a network anticipates growth of a different kind elsewhere in that network. Network growth may be described as anticipatory when potential network growth by special fitting taking place in multiple areas within the same network yields mutual concordance despite contextual differences between that special fitting. Such circumstances may be identified when potential special fitting in one area is arbitrary with respect to potential special fitting elsewhere within that structure, or when concordances within a structure are asymmetrical.[810]

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[806] In this case, special fitting to P1 that would avoid discordance to P2 and P3 (or vice versa), may be different from special fitting that would avoid discordance with P2’ and P3’.

[807] In this case, if network growth resulted in discordance with P1, then the flexibility of P1 during problem solving would be limited by concordance with P4. Adjustments that could be made to P1 that would avoid discordance with P4 may be different from adjustments that could be made to P1 that would avoid discordance with P4’.

[808] Kuhn might argue that the great majority of scientific work involves special fitting. Kuhn’s notion of normal science exemplifies special fitting, wherein a set of paradigm commitments are taken to be true and subsequent scientific work expands the applications of this paradigm (Kuhn, 1962).

[809] The independence of epistemic support may even come in degrees, meaning that special fitting may also come in degrees.

[810] Suppose we have three watches, all of which agree on the time. We may inquire into the possibility that such a structure grew by special fitting. The first watch may have been used to set the time of the second watch, and the second watch may have been used to set the time of the third watch. Supposing that the first watch was 20

The formation of Circular and Tiered Concordance requires anticipatory network growth. In both cases, each commitment within the structure forms multiple concordances with alternative commitments that are structurally connected elsewhere.[811] To illustrate the requirement of anticipatory network growth in the formation of such structures, consider the following example. Suppose some commitment P1 is accepted. P2 may concord with P1 by special fitting. P3 may also concord with P1 by special fitting. Each of these alternative commitments, established by special fitting, may then serve as the foundation for extended chains of concordances. The formation of a Circular structure would consist of the formation of concordance between these two chains. If falsity were contained anywhere therein, this would require the coincidence of concordances, since the growth of each chain would be contextually distinct. Likewise, in the case of a Tiered structure, concordances within one chain would be contingent upon concordances within the other, and this would require the coincidence of concordances since the growth of each chain would be contextually distinct.[812] When the formation of a Circular or Tiered Concordance is recognized to anticipate growth of a different kind elsewhere – as would be the case if potential special fitting in one area of a structure is recognized to be arbitrary with respect to potential special fitting elsewhere – then provisional falsity within the structure may be deemed to be implausible.[813]

Small structures are comprised of known sets of commitments and concordances. Other network structures may contain many commitments, yet the full set of commitments and concordances therein may not be clearly known. Such large structures may be representative of scientific disciplines, consisting of large collections of commitments contained within complex yet ambiguous evolving networks. Provisional falsity within large structures may be deemed implausible in suitable circumstances. There are

minutes slow, the second and third watch would also be 20 minutes slow. A circular structure could thereby form by special fitting, and this would not require a coincidence between concordances. However, it is apparent that the potential special fitting between the first and second watch is *not* arbitrary with respect to the potential special fitting of the second and third watch. It is also apparent that all concordances within this structure are symmetrical. Each commitment is concordant with every other commitment, and the concordances are all corroborative.

[811] This is not necessarily the case in Radial and Duplicated Concordance.

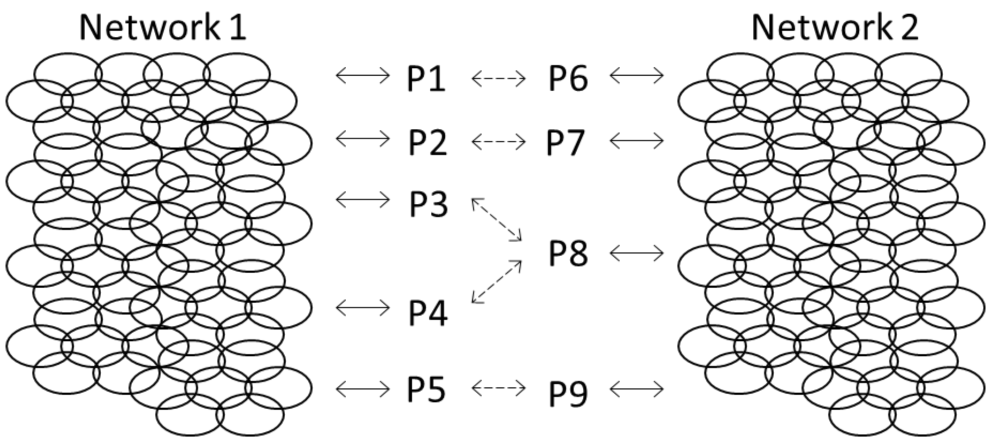
[812] In some cases, it could be difficult to even imagine how a Tiered structure could be formed by special fitting at all. An explanation for this requires more detailed consideration of different kinds of special fitting, distinguishing special fitting that is entirely unconstrained by anything other than the commitments therein, from special fitting that is constrained by unshared concordances or empirical support.

[813] A specific example of such apparent arbitrariness will be described in the forthcoming historical case study. In general, arbitrariness may be identified from historical research contexts. Uncertainties and debates in historical research contexts can establish the parameters wherein special fitting may take place. When such uncertainties and debates do not overlap across alternative contexts of potential special fitting (within a small or large structure) then arbitrariness may be historically apparent. In the case of Circular and Tiered Concordances, asymmetry of concordances may or may not take place. I consider asymmetry to be most relevant in large structures.

two general models for the growth of large structures that are particularly significant. These will be called Network Enmeshment and Lock-and-Key Growth.

Enmeshment takes place when growing independent networks combine by the formation of many concordances between these networks.

Figure 1: Enmeshment



In this figure, Network 1 and Network 2 may be conceived of as independent predictive networks. Each network is known to contain many interconnected concordances, but the details of these relationships may not be precisely mapped.[814] This ambiguity is represented by the bubbled regions in the figure. However, Network 1 may be known to contain many commitments, including P1, P2, P3, P4, and P5. Likewise, Network 2 may be known to contain many commitments, including P6, P7, P8, and P9. These commitments may be intertwined with their respective networks in assorted complex ways.[815] This figure simplifies this possibility by indicating simple concordance arrows between these propositions and their respective networks of complex yet uncertain structure. The dashed concordance

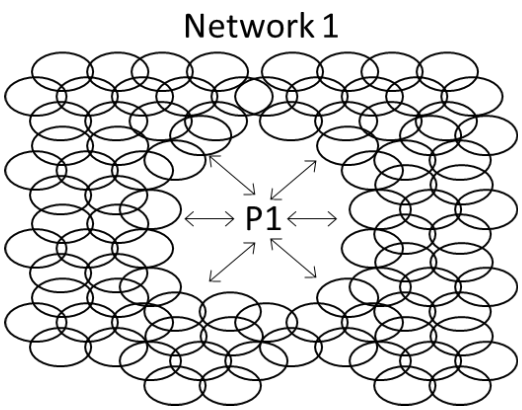
[814] Different researchers may have different impressions of these networks. A single researcher may have different impressions of possible alternative sets of commitments and structures within these networks. Additionally, these networks may be constantly evolving, as discordances are identified, problem solving efforts take place, and network growth takes place due to the addition of new concordances.

[815] The matter of importance in this section on large structures is to highlight how imprecisely known network structures may still result in epistemic insights pertaining to implausibility of provisional falsity. Though the figure indicates that Networks 1 and 2 are entirely ambiguous this need not be the case. Some or all of the small structures associated with propositions P1 through P9 may be known, even when the broader network remains ambiguous. It may also be known when a proposition is centrally interconnected within a network, or whether it is peripheral, forming very few connections elsewhere within that network.

arrows running down the middle of this figure represent the formation of concordances when these previously independent networks are combined.

Lock-and-Key Growth takes place when the introduction of some commitment(s) produces many new concordances within some network(s).

Figure 2: Lock-and-Key Growth



In this figure, the introduction of P1 to a large structure results in the formation of many new concordances at many locations within that network.[816] The introduced commitment, P1, may result in the formation of many small structures within this network. However, like the previous illustration of Enmeshment, this figure simplifies this possibility by indicating simple concordance arrows.

In the case of Enmeshment, concordances are identified between propositions that were previously incorporated into alternative networks. In the case of Lock-and-Key Growth, it is the introduction of some new commitment(s) that produces new concordances within some previously established network(s). Though the figure provided above illustrates Enmeshment between two networks, Enmeshment can also take place between many networks, or between alternative lobes of a single network. Additionally, though Lock-and-Key Growth is illustrated within a single network, it can also take place between multiple networks.

Enmeshment and Lock-and-Key Growth have epistemic significance, not only to the commitments contained within the newly formed concordances, but also to the broader network. As an intuitive illustration of the epistemic significance of Enmeshment, imagine that two individuals are

[816] One circumstance wherein Lock-and-Key Growth may take place, is if a network confronts multiple discordances that are each resolved in the same way.

attempting to solve the same crossword puzzle. The first individual starts at the top and works downward, while the second individual starts at the bottom and works upward. Eventually, they both reach a degree of completion where the combination of their efforts may result in many words intersecting one another. Even if the puzzle is still far from complete, these individuals may find that all these points of intersection agree at all designated letter overlaps. Both puzzle solvers may recognize that there is more puzzle left to solve, and they may not be surprised if certain portions of their completed work needs to be adjusted, but this Enmeshment may convince them that their efforts, to this point, are *largely* correct.

As an intuitive illustration of Lock-and-Key Growth, again imagine solving a crossword puzzle. Suppose that a small chunk of the puzzle has been provisionally completed, and this facilitates entry of a very long word that intersects in many locations with both the completed section of the puzzle and also the incomplete section. As puzzle solving continues elsewhere, collections of discordances become apparent. The very long word does not seem to accommodate the newly completed portions of the puzzle, even though these portions were strongly determined by completed sections of the puzzle elsewhere. An alternative very long word is then found that retains all the established concordances, while also resolving all the apparent discordances. This Lock-and-Key Growth may result in a sudden increase in the conviction that puzzle solving efforts, to this point, are *largely* correct.

Enmeshment and Lock-and-Key Growth may result in the formation of small structures that are, themselves, conducive to impossibility or implausibility results. These small structures may be identifiable between the commitments directly involved in Enmeshment or Lock-and-Key Growth. Alternatively, these structures may be identifiable within the broader network. Additionally, Enmeshment and Lock-and-Key Growth include the formation of multiple concordances within or between some already established network(s). Accordingly, *some* Circular Concordance will assuredly be formed during Enmeshment or Lock-and-Key Growth, even if these small structures are not clearly identified within the broader network. The greater the number of concordances that are formed during Enmeshment or Lock-and-Key Growth, the more Circular Concordances will necessarily be formed therein.

The contextual nature of network growth by special fitting also has relevance to Enmeshment and Lock-and-Key Growth. Enmeshment or Lock-and-Key Growth of some network(s) that contains many false commitments or centrally integrated false commitments would require the coincidence of concordances. Consider the Enmeshment from *Figure 1*. The growth of Network 1 by special fitting would produce a network contextually tailored to the particular set of false commitments contained therein. Enmeshment would require that Network 2 also be well tailored to the false commitments contained within Network 1, so as to produce many concordances while avoiding discordance when the two networks are combined. This anticipatory network growth would be coincidental since Network 1 and Network 2 developed independently. Consider the Lock-and-Key Growth from *Figure 2.* Such Lock-and-Key Growth consists of the introduction of some novel commitment(s) that results in the formation of many concordances within a network all at once. Even if this novel commitment was established by special fitting to some local context within the network, the formation of many concordances elsewhere within that network, while avoiding discordance, would only be possible coincidentally. In either case, difference in kind of network growth may be established by apparent arbitrariness of potential special fitting, or by asymmetries within the resulting structure. Indeed, in a sufficiently complex large structure, differences in kind of network growth may simply be assumed.

Like the snapping together of small structures, the implausibility of falsity within large structures is based on a requirement for coincidence of concordances, were the network to contain some false commitment(s), even if growth was due to special fitting. However, conclusions about the implausibility of falsity within large structures has a slightly different character to that of small structures. In small structures, the requirement of coincidental concordance may justify the conclusion that any provisional falsity within the structure is implausible. Alternatively, when networks undergo Enmeshment or Lock-and-Key Growth, it may be said that it is implausible that the relevant network(s) is *substantially* false. The falsity of many commitments within the network(s) or centrally connected commitments may be deemed implausible, even if falsity elsewhere in the network is not implausible. Snapping together of large structures may thereby indicate that the growing network(s) is on the right track, even if provisional falsity *somewhere* remains plausible.[817]

The fundamental reason for this difference in the character of implausibility is that in small structures, all commitments and concordances are known. In a large structure, there is ambiguity. Though Enmeshment and Lock-and-Key Growth may result in the formation of many small structures wherein falsity is impossible or implausible, such conclusions would only pertain to the commitments contained within those small structures, and these need not encompass *all* commitments contained

[817] Returning to an intuitive crossword example, Enmeshment of Lock-and-Key Growth may facilitate the conclusion that the completed portions of a crossword puzzle are largely correct. This does not mean, however, that portions of the completed network cannot be changed.

within a large structure. Additionally, as the proportion of false commitments within a network increases and/or as the proportion of concordances that include some false commitment(s) within a network increases, the potential ramifications of such falsity to special fitting within the network also expands, so network growth by Enmeshment or Lock-and-Key Growth becomes increasingly coincidental.[818]

To recapitulate, snapping together pertains to circumstances wherein network structures or the formation of these structures prohibit or greatly limit the possibility of falsity, therein. The snapping together of a network structure greatly increases the epistemic support of commitments therein, and this support is only apparent from consideration of structural relationships. Identifying the impossibility of falsity within a structure requires knowledge of the limitations upon provisional falsity within individual concordances therein. Alternatively, the implausibility of falsity within a structure may be established when the formation of a structure requires coincidences between concordances that would be implausible, were commitments therein false. This takes place when network growth in one area anticipates growth of a different kind elsewhere. In the case of small structures, the formation of Circular and Tiered structures requires anticipatory network growth, and this may be deemed implausible when potential special fitting in one part of the structure is arbitrary with respect to potential special fitting elsewhere. In the case of large structures, Enmeshment and Lock-and-Key Growth may be deemed implausible, were the network to contain a high proportion of false commitments or centrally integrated false commitments. Accordingly, in the case of small structures, the implausibility of falsity pertains to all commitments therein, but the implausibility of falsity within large structures is less definitive. When large structures snap together by Enmeshment or Lock-and-Key Growth, it may be said that it is implausible that the relevant network(s) is *substantially* false.

The implausibility of provisional falsity within a structure may come in degrees, as related to the plausibility of the coincidence between concordances therein. The relevance of arbitrariness has already been introduced, especially with respect to small structures. Practical judgements of such arbitrariness may come in degrees, and this may influence judgements pertaining to the degree to which falsity is deemed implausible. The relevance of the proportion of false commitments and the central interconnectedness of false commitments has already been introduced, especially with respect to large

[818] In small structures, a false commitment has nowhere to hide because it is clearly integrated into a known structure in a known way. Alternatively, in a large structure, some false commitment(s) may have limited influence upon broader network growth.

structures. Obviously, the proportion of false commitments within a large structure may come in degrees, and so too does the proportion of concordances within a structure that include some false commitment(s). Accordingly, snapping together of large structures may very strongly justify the claim that it is implausible for the network to contain many falsities, or centrally connected falsities, but the prospect of fewer falsities or tangentially connected falsities may not be as implausible. Other factors that may have influence upon judgements of implausibility include the number of concordances formed during a snapping together event, the diversity of possible alternatives to commitments contained within a structure, and the specificity of concordances therein.

Implausibility judgments are fallible. Even when falsity within a structure seems highly implausible, there is a chance that network growth in one area that includes some false commitment(s) *just so happens* to anticipate growth of a different kind elsewhere. However, snapping together events may overlap, or may be contained within other snapping together events. Accordingly, a single network structure may be the product of numerous snapping together events. The epistemic significance of snapping together to commitments within a structure may thereby be compounded.[819] Such compounding of snapping together events can result in virtual certainty that commitments contained within the relevant structure are not false.

Conversely, as false commitments accumulate within a growing network, not only does the prospect of snapping together decline, but also continued growth by special fitting requires ever greater care to avoid discordance. The set of commitments within such a network must be contextually tailored in order to avoid discordance, and were discordance to arise, the network may not be conducive to problem solving efforts that are minimally disruptive to existing structures. Instead, as a structure grows, commitments therein become less flexible to adjustments that may be involved in problem solving, and as false commitments accumulate within a growing network, the prospect of widespread disruption therein increases.

The best predictive networks snap together, and by snapping together they provide predictive scaffolding that facilitates additional network growth, whether by special fitting or otherwise. This subsequent network growth may yield still more snapping together events. If the network is largely false, this limits the prospect of snapping together, and increases the prospect that discordances will

[819] Indeed, snapping together of large structures will inherently involve such compounding due to the unavoidable formation of Circular Concordances therein.

emerge that require widespread modification of the network.[820] My historical argument, to which we now turn, is that during the 1960s, the coming together of paleomagnetism, marine geology, and geochronology resulted in a series of snapping together events that greatly increased the epistemic support for mobilism.

Snapping Together and the Plate Tectonics Revolution

Toward the end of Part 1, I used the words "perfect suitability" to describe a series of events wherein paleomagnetism, marine geology, and geochronology came together in support of mobilism. I claimed that contemporaneous researchers recognized the great epistemic significance of these events, thereby increasing the conviction of proponents of mobilism and swaying opponents. Having introduced the idea of snapping together, a conceptual framework is now in place that may facilitate a more-detailed historical account of this epistemic significance.

My historical argument is that a series of snapping together events facilitated the rapid acceptance of continental mobilism in the second half of the 1960s. The snapping together of a small structure including the Vine-Matthews hypothesis and the geomagnetic reversal timescale was of central importance. Researchers who were most familiar with those structures that snapped together were able to appreciate their epistemic significance. Accordingly, each snapping together event broadened the range of researchers who were strongly invested in the growing network. In the case of

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[820] The discussion of consilience in Chapter 6 mainly pertained to modeling the sorts of relationships between commitments that might satisfy Whewell's notion of consilience or the intuitions thereof. It may be claimed that consilience is not entirely conducive to such simplified modeling and that understanding consilience requires specification of different kinds of commitments. William Harper, for example, conceives of consilience as a lawlike relation between alternative inductions, but claims that these inductions must be universal generalizations of natural kinds and the lawlike relation must be a unifying causal explanation (Harper, 1989). It may very well be the case that alternative kinds of commitments within predictive networks may have epistemic relevance, and this trajectory of inquiry might enrich the idea of predictive networks. However, the sophistication of the analytic framework developed to this point should not be overlooked. In Chapter 6, I claimed that the Models of Concordance could be conceived as modeling consilience. Toward the end of that chapter, I introduced the idea of predictive networks, consisting of sets of partially overlapping concordances wherein individual commitments may directly concord with many other commitments (or sets of commitments) and may also directly discord with many other commitments (or sets of commitments). In Chapter 7, I examined the logical isolation of falsification within predictive systems. Individual commitments may be contained within many different predictive systems that confront discordance and falsification may be isolated within these systems to varying degrees. Additionally, in this chapter, I introduced the notion of snapping together, wherein network structures have epistemic bearing upon commitments therein. When all of these features of predictive systems are taken into consideration, appeals to complications like different kinds of commitments may not be necessary to make sense of even complex or nuanced epistemic intuitions.

mobilism, some snapping together events were sufficiently simple as to be of intuitively appreciable significance and were heavily emphasized in communications that spanned disciplinary boundaries exactly for that reason. Consequently, even those researchers who were not immediately involved in research that snapped together could still recognize the epistemic significance of some of these events. Additionally, the network structure produced by snapping together events facilitated productive integration and reinterpretation of diverse research across the environmental sciences.[821]

I will begin by identifying the snapping together of a small structure in 1966 that convinced researchers at Lamont, Scripps, and elsewhere of seafloor spreading. I will then elaborate upon this small structure to identify additional layers of snapping together events, including both small and large structures.[822]

In the early 20th century, research in paleomagnetism resulted in the recognition of the phenomenon of polarity reversals in rock magnetism. By the 1950s, two competing frameworks were often employed to account for this. Some proposed that apparent reversals were the result of certain collections of minerals that could take on polarity opposite to that of the ambient field. Others proposed that the Earth's magnetic field periodically reversed, and apparent polarity reversals were the product of such field reversal.[823] These alternative hypotheses remained in competition into the 1960s.

Paleomagnetists, Allan Cox and Richard Doell, set out to decide between these two hypotheses by attempting to construct a field reversal timescale. If the geomagnetic field were reversed in the past, and polarity reversals were a product of this, then reversals should be globally consistent across rocks of the same age. Alternatively, if polarity reversals were due to self-reversal, then efforts to construct such a timescale should fail to produce globally coherent results. The absolute age of sampled rock could be

[821] Once a structure snaps together, it may strongly determine subsequent network growth, which may result in a flurry of epistemically significant research. Though this research need not include snapping together events, it is my contention that snapping together is conducive to additional snapping together events. In this historical case, the acceptance of plate tectonics resulted in widespread unification and reinterpretations across environmental sciences. Though this is outside of the scope of this project, some of this growth may include snapping together events, while some of this growth may be limited to the epistemic significance of concordances and problem solving as detailed in Chapters 6 and 7.

[822] Two points should be made on the historical adequacy of this account. First, historical events can be interpreted in multiple ways. I offer one interpretative structure, but other interpretations are possible even within the conceptual framework of concordances and predictive networks. Second, the notation developed in this chapter to represent small structures applies most-adequately to pairwise concordances, even though concordant relationships are not limited to only pairs of commitments. This notation was selected for the sake of illustrative simplicity, and many of the concordances outlined in the following historical analysis are concordant pairs. However, in some cases the notation simplifies more complex structure.

[823] See Chapter 2 for more detail, especially pages 38-39.

established by potassium-argon dating, which could then be used to establish historical concurrency of polarity reversals across geographically distant sites.[824]

Potassium-argon dating consisted of measuring the ratio of radioactive potassium and radiogenic argon within a sampled mineral or rock. This ratio could then be used to establish age based on the decay rate of potassium and assorted other assumptions related to the retention of parent and daughter isotopes within the sample. Efforts to date rock by measuring the potassium-argon ratio began in the 1950s, and by the early 1960s, this dating method showed promise when applied to basic igneous rock of low potassium content.[825] Still, it was unclear whether potassium-argon dating could be made sufficiently precise as to facilitate the construction of a field reversal timescale.

At the USGS in Menlo Park, Cox and Doell recruited Brent Dalrymple, who specialized in Cenozoic potassium-argon dating. Additionally, Ian McDougall (who specialized in potassium-argon dating of basic rock) collaborated with paleomagnetist John Tarling in a similar effort to construct a field reversal timescale at ANU. Initial results from both groups were published in 1963.

In 1960, Harry Hess proposed the hypothesis of seafloor spreading to account for the origin of ocean basins. He claimed that mid-ocean ridges are tensional structures where new seafloor is created. The newly created seafloor moves laterally away from the ridge and in opposite directions on either side of the ridge. Thus, the seafloor gets progressively older when moving away from a mid-ocean ridge. Hess proposed that convection currents in the mantle drive the process of seafloor spreading and later claimed that ocean trenches are sites where oceanic crust descends into the mantle. Seafloor spreading was proposed to account for the apparent youth of the seafloor, as indicated by the thinness of ocean sediments and the absence of ancient fossils. However, the youth of the seafloor was still subject to debate into the 1960s, and several other prominent attempts to account for the origin of ocean basins were also proposed around this time. Like its competitors, seafloor spreading was recognized to be highly speculative.[826]

In 1963, Frederick Vine and Drummond Matthews proposed that marine magnetic anomalies – which were first identified in the 1950s – were the product of seafloor spreading. They proposed that new ocean crust obtains remanent magnetism in the direction of the Earth's magnetic field as the crust solidifies. Recruiting the hypothesis of geomagnetic field reversals, Vine and Matthews claimed that

[824] See Chapter 5, especially pages 94-99.
[825] See Chapter 4, especially pages 87-89.
[826] See Chapter 3, especially pages 56-58.

differentially magnetized adjacent blocks of seafloor would be created as new seafloor spreads away from a ridge. The Vine-Matthews hypothesis combined field reversals with seafloor spreading to account for marine magnetic anomalies, but this was widely recognized to be a highly speculative hypothesis.[827] Alternative accounts of marine magnetic anomalies typically appealed to petrological differences of the seafloor.[828]

The USGS and ANU groups refined their reversal timescales in the years following 1963. Occasional discordances resulted in successful problem solving. A general challenge that confronted this collective effort to produce a reversal timescale was that available data points were known to be highly discontinuous and thereby conducive to many possible interpretations about the history of the Earth's magnetic field.[829] In 1965, Vine used the USGS reversal timescale to model the magnetic profile across the Juan de Fuca Ridge, as anticipated by the Vine-Matthews hypothesis. This modeling involved a set of additional assumptions related to electromagnetism and the magnetic properties of the ocean crust. Vine argued that seafloor spreading rates varied over time. In this modeling work, Vine used the USGS timescale published in 1964, wherein the most recent reversal event was considered to have take place approximately 1 million years ago.[830] In 1965 and 1966, the USGS group made important updates to their timescale. The youngest reversal event was identified at about 0.7 million years ago, which marks the beginning of a reversal epoch. Additionally, the Jaramillo event was identified as a reversal event of short duration that took place around 0.9 million years ago.[831] The Jaramillo event was corroborated by the ANU group in 1966.

In 1966, Walter Pitman used the updated USGS timescale to model a magnetic profile of the Pacific-Antarctic Ridge. Pitman showed that seafloor spreading at a constant rate of about 4.5cm/yr would produce a modeled profile strikingly similar to those obtained from the Eltanin cruise, especially the Eltanin 19 profile. Also in 1966, Vine modeled profiles for the Reykjanes Ridge, the Juan De Fuca

[827] See pages 99-102.
[828] See Chapter 3, especially pages 57-61.
[829] See pages 94-99.
[830] See pages 106-107.
[831] See pages 107-108.

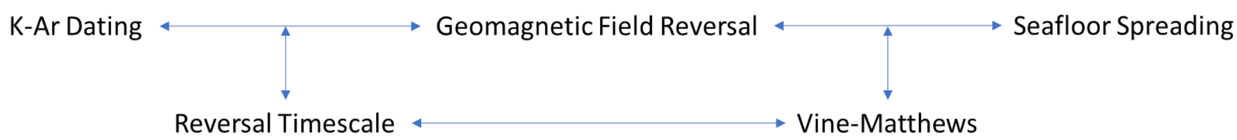
Ridge, the East Pacific Rise, the Carlsberg Ridge and the Mid-Atlantic Ridge, showing strong similarity between these models and measured magnetic profiles.[832][833]

Measured magnetic profiles could then be used to improve the geomagnetic reversal timescale, by entailing predictions that could be corroborated either by study of reversals in igneous rock or by the study of reversals in seafloor sediment. Pitman collaborated with Neil Opdyke on such work in 1965. Indeed, Opdyke identified the Jaramillo event independently from the USGS group and also confirmed reversal events first identified in marine magnetic profiles. In his 1966 paper, Pitman used the Eltanin profiles to speculate on reversal events that were later confirmed by Cox and Dalrymple. Pitman also extrapolated the reversal timescale back to 10 million years, whereas the USGS timescale extended to only 4 million years. Vine extrapolated the timescale still further, based on his review of magnetic profiles, and he also identified new reversal events confirmed by McDougall later that year. Thus, by 1966, reversal events identified by the study of igneous rock at USGS and ANU could be corroborated by marine magnetic profiles and by research on seafloor sediments. Furthermore, Opdyke's work on seafloor sediments provided a more-continuous record of reversal events than the discontinuous igneous record and could also be used to corroborate reversal events inferred from magnetic profiles that predated the available igneous record.[834]

[832] See pages 109-112.
[833] An alternative possible representation of this historical case study is that the discovery of the Jaramillo event exhibits Lock-and-Key Growth, wherein a single commitment results in the formation of many concordances across multiple networks.
[834] See pages 111, 114.

This collection of research briefly recounted above may be diagramed as a Tiered Concordance with the following structure.[835][836]

Figure 3: Snapping Together from Magnetic Profiles



[835] This structure may be modeled in the following, simplified, way.

[((P1 · P2) → P4)] · [(P4 → E1) · E1)] · [((P2 · P3) → P5)] · [(P5 → E2) · E2)] · [((P4 · P5) → E3) · E3]

In this model, the conjunction of potassium-argon dating (P1) with the geomagnetic field reversal hypothesis (P2) entails that a globally coherent reversal timescale (P4) can be produced. This was tested (E1), independently from Vine-Matthews, by research on igneous formations, undertaken by the USGS and ANU research groups. Alternatively, the conjunction of the field reversal hypothesis (P2) with seafloor spreading (P3) entails the Vine-Matthews hypothesis (P5). The Vine-Matthews hypothesis was tested (E2), independently from the reversal timescale, by identifying linearity in marine magnetic anomalies, the parallel orientation of these anomalies to ridge axes, symmetry of these anomalies about ridge axes, and correlations between magnetic profiles between ridges. The field reversal timescale (P4) was then applied to the Vine-Matthews hypothesis (P5) to model marine magnetic profiles (E3). In 1966, this modeling was confirmed by measurements of magnetic profiles across multiple ridges.

This model includes important simplifications. P1, P2, and P3 represent sets of commitments. P4 and P5 also represent sets of commitments, only some of which are directly entailed by the commitments contained within P1, P2, and P3. For example, the Vine-Matthews hypothesis includes the implicit assumption that oceanic rock has sufficient magnetic susceptibility to account for marine magnetic anomalies, and this assumptions concorded with measurements pertaining to the magnetic susceptibility of marine basalts. Details such as this are not included in the model. Rather, the matter of importance is that some core commitment(s) of Vine-Matthews was entailed by the conjunction of seafloor spreading with the field reversal hypothesis, and some core commitment(s) of a globally coherent field reversal timescale was entailed by the conjunction of the field reversal hypothesis with a sufficiently reliable dating method like potassium-argon dating, and the conjunction of these core commitments of Vine-Matthews with the field reversal timescale entailed prediction that was confirmed by measurement of magnetic profiles.

Additionally, E1, E2, and E3, in this model, may also represent sets of commitments, and these predictions may be tested by still other sets of commitments not specified in this model. For example, testing of E1 may include commitments related to fieldwork methods and specific potassium-argon ratio measurements and paleomagnetic polarity measurements, as undertaken by the USGS and ANU research groups during the formation and refinement of the reversal timescale. Alternatively, testing of E2 may include commitments related to alternative fieldwork methods, as well as bathymetric, seismic, and magnetic measurements of the seafloor, as undertaken by researchers at Scripps, Lamont, Cambridge, and elsewhere. Testing E3 may include commitments involved in obtaining magnetic profiles, but also additional commitments involved in modeling magnetic profiles, such as presumed distances between alternatively magnetized bodies, their width, depth, shape, and the magnetic susceptibility, strike, distance, and inclination of the rock.

[836] For the sake of illustrative simplicity, *Figure 3* omits the Duplicated Concordance between Vine-Matthews and the reversal timescale via corroborations of novel reversal events. This Duplicated Concordance is included *Figure 5*. This figure also omits the relevance of Opdyke's work on seafloor sediments, though this is also included in *Figure 5.*

Upon its inception, the reversal timescale aimed to test the geomagnetic field reversal hypothesis by pushing the precision of potassium-argon dating. Prior to 1966, it obtained epistemic support from organized inquiry of igneous formations by the USGS and ANU research groups. Alternatively, upon its inception, the Vine-Matthews hypothesis was a speculative attempt to account for marine magnetic anomalies by combining seafloor spreading with the geomagnetic field reversal hypothesis. Prior to 1966, the Vine-Matthews hypothesis obtained support from apparent parallel orientation of magnetic anomalies following the trend of mid-ocean ridges, the symmetry of magnetic profiles about the ridge, and correlations of magnetic profiles between ridges. These measurements were made by researchers at Lamont, Scripps, Cambridge, and elsewhere, and Vine recruited these measurements as evidence for the Vine-Matthews hypothesis. The conjunction of the Vine-Matthews hypothesis with the reversal timescale was confirmed by numerous measured magnetic profiles in 1966. This not only had bearing upon the epistemic strength of the Vine-Matthews hypothesis, and the reversal timescale, but also upon seafloor spreading, the geomagnetic reversal hypothesis, and the reliability of potassium-argon dating.[837] Additionally, this epistemic significance was not *only* due to individual concordances, but also due to the apparent anticipatory network growth required to form this small structure. Were this structure to contain some false commitment(s), then the formation of the structure would require some coincidence between concordances.

Suppose that the geomagnetic field reversal hypothesis was false and/or that potassium-argon dating methods were unreliable for dating basic igneous rock from the Pliocene and Pleistocene. In such circumstances, the USGS and ANU research groups might still have constructed a coherent reversal timescale by special fitting. Assumptions pertaining to potassium-argon dating,[838] or the field reversal hypothesis[839] could have been adjusted in some way to accomplish this.[840] However, whatever special

[837] By 1966, seafloor spreading and Vine-Matthews were likely in a weaker epistemic position than the field reversal hypothesis and reliability of potassium-argon dating methods. Accordingly, the snapping together of this Tiered structure may have had greater epistemic impact on the seafloor spreading hypothesis and the Vine-Matthews hypothesis.

[838] In principle, arbitrary adjustments to certain parameters involved in potassium-argon dating might facilitate the formation of a globally coherent reversal timescale regardless of the falsity of commitments therein. However, the USGS and ANU groups did not adjust assumptions pertaining to general potassium-argon dating methods.

[839] The USGS and ANU groups modified the field reversal hypothesis over the course of their research, endorsing irregularity of reversals over periodicity. They also identified dated rock of intermediate polarity, which provided some insight into the duration of reversal events.

[840] The commitments involved in research on igneous formations could also be adjusted by special fitting in order to construct a coherent reversal timescale. Indeed, multiple discordances were identified between the USGS and ANU research groups between 1963 and 1966. These discordances motivated the modification of both potassium-argon measurements and paleomagnetic polarity measurements. It is also conceivable that the USGS and ANU groups omitted discordant data or accepted and published only those results that were most conducive to the

fitting may have been involved in the research conducted at the USGS and ANU, such special fitting would be arbitrary with respect to the Vine-Matthews hypothesis. The available parameters which could be subjected to special fitting by the USGS and ANU groups had no bearing upon marine magnetic anomalies, and the USGS and ANU groups were not involved in the application of their research to the interpretation of marine magnetic anomalies.[841]

Alternatively, suppose that the geomagnetic field reversal hypothesis was false and/or that seafloor spreading was false. In such circumstances, special fitting by Vine and others, may still have facilitated the identification of linearity of anomalies with respect to ridge axes, symmetry of magnetic profiles, or correlations across ridges. Assumptions pertaining to the field reversal hypothesis,[842] or seafloor spreading[843] may have been adjusted to this end.[844] However, in such circumstances, whatever special fitting may have been involved in the work of Vine and others, this special fitting would be arbitrary with respect to the formation of a globally coherent reversal timescale in the study Pliocene and Pleistocene igneous rock. The available parameters which could be subjected to special fitting to identify linearity, symmetry, or correlations of magnetic anomalies had no bearing upon the USGS and

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formation of a reversal timescale. Afterall, as illustrated in Part 1 of this work, there was always ample ambiguity involved in the process of selecting suitable sampling locations, establishing the suitability of rock for potassium-argon dating, and establishing the stability of remanent magnetism. One historical issue with this possibility, however, is that the USGS and ANU groups routinely relied on potassium-argon dates and polarity measurements obtained by researchers who were not aware that their work would be subsequently used in the formation of a reversal timescale.

841 Electromagnetic theory had bearing upon research involved in the development of the geomagnetic field reversal timescale and also marine magnetic measurements and modelling. In principle, this is a parameter that could have been modified by special fitting to facilitate the construction of a coherent reversal timescale. However, the USGS and ANU groups did not modify electromagnetic theory in their work.

842 The identification of parallel linearity, symmetry, and correlations of marine magnetic anomalies did not, in fact, involve modification of the field reversal hypothesis. As long as seafloor spreading was symmetrical, linearity, symmetry, and corroborations across ridges would be expected, regardless of the nature and details of field reversals.

843 Offsets in the linearity of anomalies and zig zag patterns could be attributed to faulting. Backus added the possibility of inconsistent seafloor spreading rates along the length of a ridge due to spherical geometry, but Vine did not recognize the importance of this insight until 1966.

844 Commitments involved in marine magnetic measurements could be adjusted by special fitting to facilitate the identification of parallel linearity, symmetry, and correlations. For example, researchers examining the relationship between magnetic anomalies and ridges often emphasized some magnetic profiles over others. Vine often published or highlighted those profiles that most clearly demonstrated symmetry or correlations across ridges, while discordant profiles were omitted or attributed to local variations in physical history. Similarly, marine magnetic anomalies in certain regions like the Northeast Pacific were known to include complex patterns that deviated from linearity, but this too was often attributed to local variations in physical history. Thus, discordant data may have been omitted or marginalized, while concordances were emphasized to support the Vine-Matthews hypothesis. One historical issue with the this is that many of the researchers involved in obtaining relevant marine magnetic measurements were explicitly opposed to the Vine-Matthews hypothesis and seafloor spreading.

ANU research groups. Accordingly, were this structure to contain some false commitment(s), the concordance of the Vine-Matthews hypothesis with the reversal timescale in 1966 would require a highly implausible coincidence.[845] The matter of importance is that the research on the reversal timescale and the Vine-Matthews hypothesis were arbitrary with respect to one another, yet still yielded concordance, such that an implausible coincidence of concordances within the Tiered structure would be required, *even if* special fitting was somehow involved in the formation of this structure.

As examined in Chapter 5, the research outlined above had tremendous influence upon the convictions of many of those researchers most intimately familiar with the commitments involved. However, Vine was already convinced of the Vine-Matthews hypothesis by this time. Though, at first, Vine recognized the Vine-Matthews hypothesis to be highly speculative, he became convinced that the hypothesis was correct in 1965. The events surrounding Vine's conviction will be briefly examined.

In 1965, Tuzo Wilson developed the notion that the Earth's crust is comprised of several rigid plates, the edges of which form a global network of mobile belts where relative motion between these plates takes place. The hypothesis of the transform fault was entailed by the conjunction of Wilson's notion of crustal plates and seafloor spreading.[846] The conjunction of crustal plates and seafloor

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[845] In this case, the Vine-Matthews hypothesis and the reversal timescale have independent support, and this provides a basis upon which to establish the arbitrariness of potential special fitting in this case. However, *Figure 3* could also be modeled in an even more simplified manner as follows.

$$[((P1 \cdot P2) \rightarrow P4)] \cdot [((P2 \cdot P3) \rightarrow P5)] \cdot [((P4 \cdot P5) \rightarrow E3) \cdot E3]$$

This model removes some of the context that is helpful in establishing the arbitrariness of potential special fitting that has historical relevance in this case. However, even if the historically relevant independent support for the reversal timescale and the Vine-Matthews hypothesis are ignored, the requirement of implausible coincidence of concordances within this structure can still be identified. In 1965, Vine used the available reversal timescale, in conjunction with the Vine-Matthews hypothesis, to model magnetic profiles, but found that measured profiles deviated from expectation. Vine thereby endorsed the notion of inconsistent seafloor spreading in an effort to resolve this falsification problem. Alternatively, researchers at the USGS and ANU identified the Jaramillo event and thereby amended the reversal timescale. This research was not at all motivated by an interest in Vine's falsification problem. The USGS and ANU groups worked on terrestrial igneous formations and did not pay attention to the possible bearing of marine magnetic anomalies. Any special fitting that may have been involved in the USGS and ANU groups' research would have been arbitrary with respect to Vine's problem solving efforts. Even so, it was the discovery of the Jaramillo event and associated revisions of the reversal timescale that ultimately resolved Vine's falsification problem.

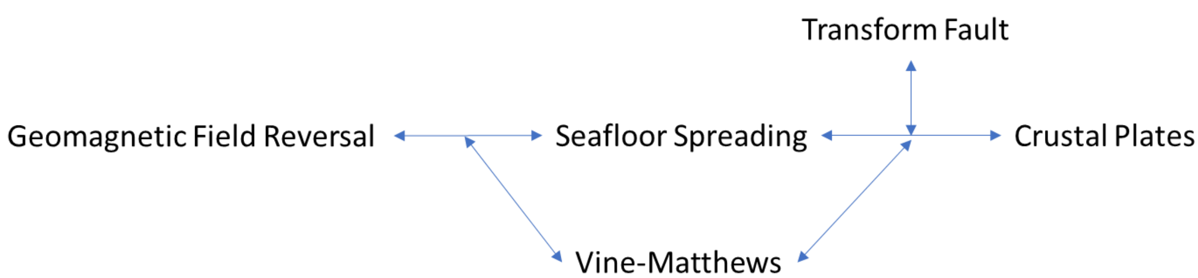
Consideration of historical research contexts shows that Vine's 1965 endorsement of inconsistent seafloor spreading was not arbitrary with respect to the field reversal timescale. However, research contributing to the discovery of the Jaramillo event and subsequent revision of the field reversal timescale was arbitrary with respect to Vine's problem solving efforts, and this independent research ultimately established the concordance between the reversal timescale and the Vine-Matthews hypothesis.

[846] See pages 103-105.

spreading also predicted that an undiscovered ridge resided off the coast of Vancouver Island.[847] Wilson, along with Vine and Hess, corroborated this prediction while at Cambridge by consulting a map of magnetic anomalies of the Northeast Pacific. The map showed magnetic anomalies that paralleled the trend of Wilson's postulated ridge. Additionally, Wilson, Vine and Hess identified symmetry within the anomaly pattern, thereby using the Vine-Matthews hypothesis to localize the position of the postulated ridge. This concordance between the Vine-Matthews hypothesis and Wilson's theory of crustal plates and mobile belts convinced Vine that the Vine-Matthews hypothesis was correct.[848]

The small structure that Vine and Wilson considered so compelling may be diagramed as a Tiered Concordance.[849]

Figure 4: Snapping Together from the Juan de Fuca Ridge



The network structures modeled in *Figure 3* and *Figure 4* were among the first instances of the snapping together of structures associated with seafloor spreading. Those most intimately involved in the research that contributed to the formation of these structures immediately recognized their

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[847] Wilson categorized the San Andreas Fault as a transform fault running from the East Pacific Rise to an unidentified ridge in the Northeast Pacific.

[848] Researchers at Lamont had already identified this ridge through other means.

[849] This may be represented as follows (not including the transform fault hypothesis).

$$[((P1 \cdot P2) \rightarrow P4)] \cdot [((P2 \cdot P3) \rightarrow E)] \cdot [P4 \rightarrow E]$$

The conjunction of the field reversal hypothesis (P1) with seafloor spreading (P2) entails the Vine-Matthews hypothesis (P4). The conjunction of seafloor spreading (P2) with Wilson's description of crustal plates (P3) predicts (E) that a ridge with a certain trend resides off the west coast of Vancouver Island. Based on a magnetic map of the Northeast Pacific, the Vine-Matthews hypothesis also predicted that a ridge with the anticipated trend resides off the west coast of Vancouver Island. In this case, it is important to note that the map of marine magnetic anomalies was constructed well before Vine's work on the Vine-Matthews hypothesis and Wilson's work on crustal plates and mobile belts. Additionally, the Vine-Matthews hypothesis was first proposed in 1963, prior to Wilson's 1965 work on crustal plates and mobile belts, and Wilson's work on the subject was not motivated by nor seemingly relevant to the Vine-Matthews hypothesis.

Though not included in *Figure 4*, the transform fault hypothesis also facilitated the interpretation of marine magnetic anomaly maps, and this was also apparent to Wilson, Vine, and Hess when consulting the magnetic map of the Northeast Pacific.

epistemic significance.[850] Accordingly, these small structures are of great importance to the tectonics revolution, but many additional snapping together events followed in quick succession.

In 1967, Jason Morgan and Dan McKenzie and Richard Parker extended Wilson's hypothesis of crustal plates and mobile belts to the geometry of a spherical surface. The motion of a rigid plate moving on a spherical surface may be represented as a rotation about a fixed pole. Portions of the plate nearest to the pole of rotation move at a slower rate than portions of the plate nearest to the equatorial region. Based on assumptions about plate boundaries and poles of rotation, Morgan modeled anticipated variations in seafloor spreading rate along the length of the Mid-Atlantic Ridge. He then confirmed this prediction by comparing alternative magnetic profiles across the Mid-Atlantic Ridge, as interpreted by the Vine-Matthews hypothesis. Alternatively, McKenzie and Parker used seismic data to infer poles of rotation and, based on deviations in this seismic data, they proposed the existence of a small plate in the Northeast Pacific with a subduction zone along the coast of Oregon. Also, using seafloor spreading rates, as established by Vine, and assumptions about plate boundaries and poles of rotation, both Morgan and McKenzie and Parker predicted seafloor spreading rates that were not yet measured. In 1967, Xavier Le Pichon elaborated upon these efforts to show that seafloor spreading rates established by Vine were globally consistent when applied to spherical geometry. Seafloor spreading rate at any one mid-ocean ridge was the sum of the spreading rates at other ridges.[851][852]

Beginning in the 1950s, paleomagnetists endorsed the relative motion of continents to account for paleomagnetic measurements and associated inferences.[853] By 1965, Ted Irving[854] and Ken Creer[855] argued that Africa, Antarctica, Australia, India, and South America were united near the geographic South Pole in the Early Permian. This was informed by paleolatitude inferences and corroborated by

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[850] To be precise, mobilism was not directly involved in either of the snapping together events represented in *Figure 3* or *Figure 4*. Hess endorsed a network wherein seafloor spreading concorded with mobilism. However, in principle, seafloor spreading could take place even if continents were fixed. Seafloor spreading was, however, often taken to entail drift due to the apparent geological and biotic similarities between continents separated by a mid-ocean ridge, as well as the congruency of continental shelves across the Atlantic as indicated in the "Bullard fit" (see footnote 534). As we will see, mobilism became directly inculcated in snapping together events only in 1967.

[851] See pages 115-118.

[852] Note that the motion of crustal plates on a spherical surface can be tested in conjunction with the Vine-Matthews hypothesis by consideration of changes in magnetic profiles across the length of a mid-ocean ridge. Of course, the Vine-Matthews hypothesis also concords with crustal plates via interpretations of seafloor magnetic anomalies, as previously illustrated with respect to the Juan de Fuca ridge. Circular Concordance is thereby apparent in this small structure. Numerous such Circular Concordances will become apparent in *Figure 5.*

[853] See Chapter 2, especially pages 30-38.

[854] Irving, 1964

[855] Creer, 1965

paleoclimate indicators. Paleomagnetic measurements from India indicated rapid subsequent decrease in paleolatitude, with Australia exhibiting a similar trajectory thereafter. Measurements from South America[856] also indicated a decrease in paleolatitude during the Middle Permian, at which point latitudinal change largely ceased. Alternatively, paleomagnetic measurements from Africa[857] indicated northward motion during the Permian and early Mesozoic, at which point latitudinal change largely ceased.[858] Such changes in paleolatitude could be established quite precisely. Relative motions of continents could also be inferred, though less precisely, by establishing the degree of deviation of alternative paleopole determinations between continents over time. Along with economy of motion and the matching of geological features and coastlines, this provided a means of establishing relative longitudinal motions between continents over time. South America and Africa, for example, were typically recognized to have moved apart in a roughly East-West direction following the Jurassic period.[859] Such paleogeographic inferences from paleomagnetism corroborated seafloor spreading orientations and rates, as established by magnetic profile modeling. Relative motions of crustal plates, as established by methods developed by Morgan, McKenzie and Parker, and Le Pichon also facilitated corroborations with paleomagnetism as well as more detailed paleogeographic reconstructions.[860]

Seafloor spreading rates and paleogeographic reconstructions were also corroborated against alternative methods used to measure the age of the seafloor or ocean basins.[861] Radiometric dating of seafloor rock also corroborated seafloor spreading rates.[862][863] Similarly, the lowest layer of sediment, resting on top of the seafloor, could be dated by biostratigraphy[864] or by measurement of the paleomagnetic reversal pattern therein. Such dating methods could be used to establish the minimum possible age of underlying seafloor.[865] Paleomagnetic measurements of radiometrically dated

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[856] Creer, 1965
[857] Gough, Opdyke and McElhinny, 1964
[858] In a general way, the paleogeographic reconstruction of Gondwana resembled that proposed by Wegener based on paleoclimate inferences.
[859] Creer 1958; Gough, Opdyke and McElhinny1964
[860] See Heirtzler, et al., 1968
[861] By the 1970s, marine magnetic anomaly dating could be used to date portions of seafloor to around 75 million years.
[862] See Fisher, Engel and Hilde, 1968; McDougall and Van Der Lingen, 1974
[863] Marine conditions may influence retention of parent or daughter isotopes, and thereby influence ratio measurements in a manner that deviates from terrestrial measurements. Research into retention of radiogenic argon in marine conditions took place during the 1960s and into the 1970s. This research was facilitated by corroboration against biostratigraphy, marine magnetism, and alternative isotopic dating methods.
[864] Heirtzler, et al., 1973; Sclater and Detrick, 1973
[865] In principle, it is possible for older sediment to accumulate on top of a younger seafloor. Opdyke's paleomagnetic measurements, however, require that sediments remain static since the time of their deposition.

formations along the margins of ostensibly displaced continents could also be used to establish the absolute age of ocean basins.[866] Corroborations of paleogeography were also readily apparent from long-established geological and biogeographical disjuncts, as well as paleoclimate reconstructions. Radiometric dating was also employed to identify geological disjuncts between continents separated by a spreading ridge.[867][868]

Of course, many of the commitments centrally involved in this growing structure had independent origins within largely independent disciplinary networks.[869] As illustrated in Chapter 2, Paleomagnetism developed rapidly in the 1950s and was based around measuring magnetic properties of rock of defined age and location. The functionality of instruments and immediate interpretation of results was based around electromagnetic theory. Higher-level interpretations were offered with respect to the geomagnetic field, petrology, and ferromagnetism. The geomagnetic field reversal hypothesis was interconnected with this large structure, as a hypothesis that could account for apparent polarity reversals in rock magnetism. Concordance of this hypothesis with seafloor spreading or potassium-argon dating was only apparent subsequently. Additionally, the determination of paleolatitudes was a component of this large structure, as was Opdyke's research on seafloor sediments.

As illustrated in Chapter 3, during the 1950s, marine geology was largely directed toward the measurement of physical properties of the seafloor and the identification of patterns within this data. Increasingly, efforts were directed to the development of physical explanations for these patterns. Gravimetry was based on the physics of gravity. Heat flow measurements relied on thermodynamics. Seismology was based on the physics of waves. In marine geology, magnetometers were used to measure the magnetic field above a particular geographic region, rather than the magnetic properties of

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[866] See Dalrymple, Gromme and White, 1975

[867] See Hurley, et al., 1967.

[868] Corroborations of paleogeography and seafloor spreading rates were widespread in all areas noted above, but discordance was common too. Discordances, however, often resulted in successful, minimally disruptive problem solving.

[869] There was some overlap between practices in paleomagnetism, marine geology, and geochronology with respect to biostratigraphy and principles of relative dating. Theory on electromagnetism was important in paleomagnetism and marine geology. Arthur Holmes was deeply involved in 20th century geochronology, but also a notable pioneer and proponent of convection currents, which was of great relevance in marine geology. Edward Bullard was deeply involved in marine geology during the 1950s and 1960s, but also engaged in early pioneering work in paleomagnetism. Despite these slight overlaps, paleomagnetism, marine geology, and geochronology may be treated as largely independent networks. In a more precise or complete formulation, these fields may be considered lobes of a larger network, wherein the lobes are more thoroughly intertwined than the connections between them.

rock samples. Higher-level physical interpretations often relied on the Earth's internal heat, internal convection currents, isostatic equilibrium, erosion and deposition, and the composition and motion of the Earth's crust. Hess' seafloor spreading hypothesis was devised to account for heat flow, gravimetric, and geological features of (especially) mid-ocean ridges, as well as their median position between continents, and the apparent youth of the seafloor. Marine magnetic anomalies were identified and mapped by marine geologists and typically considered to be of secondary importance well into the 1960s. Wilson's notion of crustal plates was based on emerging patterns of physical structures, especially on the seafloor, but was also related to seismic and geological data.

As illustrated in Chapter 4, geochronologists aim to establish the absolute or relative age of rocks, fossils, geological structures, and even the Earth itself. The relative dating of geological formations, structures, and rocks within a stratigraphic column can be established by generalized principles related to deposition and consolidation, and by the identification distinctive patterns of fossils therein. The discovery of radioactivity and associated developments in atomic physics transformed the study of geochronology during the 20th century by facilitating absolute age determinations and the development of an absolute geological timescale. In radiometric geochronology, functionality of instruments and immediate interpretation of data was based on the physics of radioactivity, atomic physics, and spectroscopy. Assumptions pertaining to potassium-argon dating methods were interconnected with this large structure. The viability of potassium-argon dating was established by corroboration against established relative age dating methods and against other absolute dating methods.

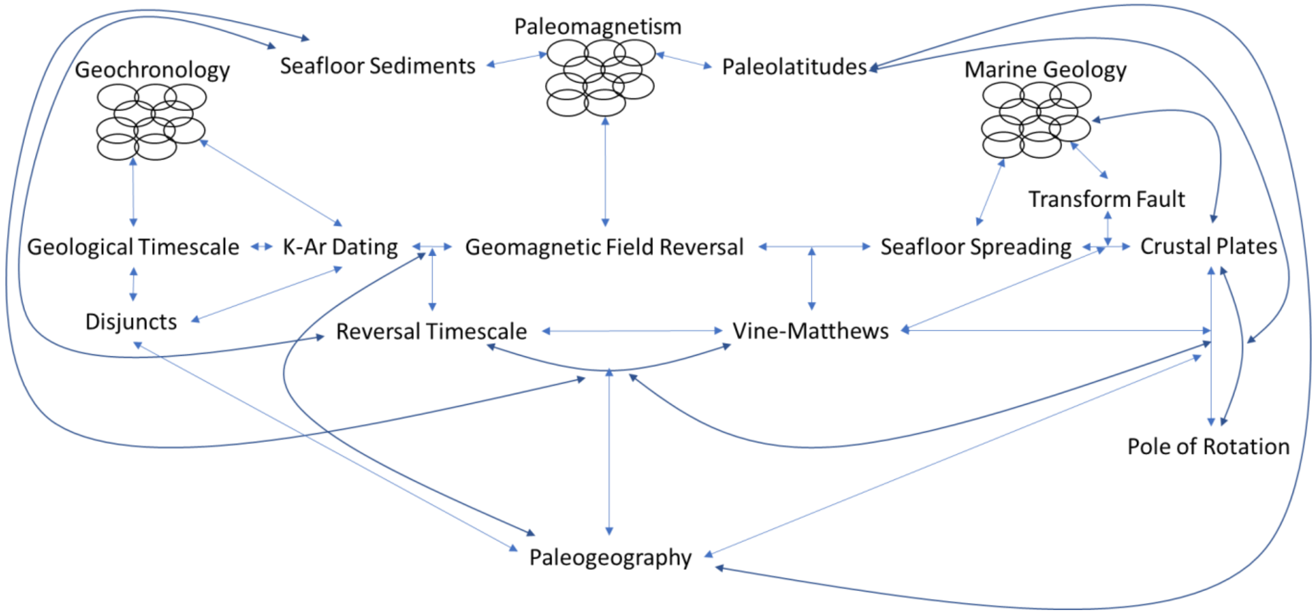
The snapping together of small structures identified previously, inculcated these larger network structures. The Vine-Matthews hypothesis combined research on paleomagnetism with research from marine geology, the field reversal timescale combined research on paleomagnetism with research from geochronology, and the assorted other concordances briefly examined above resulted in still more interdisciplinary connections between these large structures.

In Part 1 of this work, attention was directed to historical contexts of research in paleomagnetism, marine geology, and geochronology leading into the 1960s, with particular emphasis directed to ambiguities and debates. A sufficiently motivated critic might argue that progress within these fields, as detailed in Chapters 2, 3, and 4, could be the product of special fitting: Ambiguities and debates within each field may become functionally resolved or worked through, and researchers may even reach consensus or move on to new problems, but this does not necessarily mean that the ambiguities and debates were settled in an epistemically justified way. However, it is apparent from Chapters 2, 3, and 4, that the ambiguities and debates within paleomagnetism, marine geology, and geochronology were largely distinct from one another leading into the 1960s, by which time many of the commitments eventually involved in snapping together events were already established. The potential special fitting that might have been involved in relevant research in paleomagnetism, would thereby be arbitrary with respect to the possibility of special fitting involved in relevant research in marine geology, or geochronology, and vice versa.[870] This historically apparent arbitrariness of potential special fitting limits the capacity for the special fitting of false commitments to account for the snapping together events of the second half of the 1960s, thereby facilitating appreciation of the implausibility of these snapping together events, were commitments therein false.

The following figure provides a simplified summary of the small and large structures described above.

[870] In the *Figure 5*, relevant commitments developed within paleomagnetism include the geomagnetic field reversal hypothesis, methods used to measure reversals of rock magnetism in igneous formations and seafloor sediment, and inferences of paleolatitudes and associated paleogeographic reconstructions. Relevant commitments developed within marine geology, include the hypothesis of crustal plates, seafloor spreading, and methods used to measure seismic phenomena and marine magnetic anomalies. Relevant commitments developed within geochronology include methods used to measure potassium-argon ratios, methods used to measure other radiometric ratios, and commitments pertaining to the development of an absolute geological timescale. All of these commitments are integrated into a series of snapping together events in the second half of the 1960s, even though the contextual development of this research consisted of largely distinct sets of debates and ambiguities.

Figure 5: Snapping Together of Plate Tectonics



This figure may serve as a first approximation of the network structure that was formed when research in paleomagnetism, marine geology, and geochronology came together (especially in the second half of the 1960s) in manner that greatly increased the epistemic support for mobilism.[871] This figure could be expanded to include many additional areas of relevant research. For example, biogeography, paleontology, paleoclimatology, seismology, and the fit of continental margins are not included in this figure even though research in each of these areas would interconnect with this structure. Electromagnetism, principles of relative geological dating, and commitments pertaining to biostratigraphy would also be deeply interconnected with this structure. The concordances identified in this figure could also be diagramed with greater precision. For example, Hess' seafloor spreading consisted of a number of hypotheses about physical processes that changed over the course of the 1960s.[872] Potassium-argon dating consists of a set of commitments pertaining to instruments, methods,

[871] It is not the case that every concordance added to this network constituted a snapping together event. Rather, I have emphasized some of those concordances that produced Tiered and Circular structures. The arbitrariness of potential special fitting was thoroughly examined in *Figure 3,* but not in *Figure 4* or *Figure 5.* However, the detailed consideration of historically independent research contexts within paleomagnetism, marine geology, and geochronology in Part 1 of this work should provide ample demonstration that the formation of the structures represented in *Figure 4* and *Figure 5* would have required highly implausible coincidences between concordances, were falsities contained therein.

[872] Hess' theory of seafloor spreading included the notion that the process was driven by internal convection currents. However, the epistemic significance of snapping together of small structures applies *only* to those commitments that are integral to the relevant structure. Convection currents were not integral to the Vine-Matthews hypothesis, nor the concordance of seafloor spreading with Wilson's hypothesis of crustal plates. The

and interpretations. Concordances confirmed by empirical testing could also be highlighted, if so desired. Also, in principle, concordance arrows could be distinguished by year or perhaps, by publication, in order to provide more detail into simplifying generalizations.[873]

To summarize, *Figure 5* illustrates the snapping together of research in paleomagnetism, geochronology, and marine geology. Historically influential snapping together events took place especially between 1965-1967, and the resulting structure continued to snap together as additional layers of concordances were added thereafter. These snapping together events were recognized to be epistemically significant by those researchers most intimately familiar with the commitments therein. As additional layers of concordances snapped together, the implausibility of falsity therein compounded, and the group of researchers whose work was convincingly aligned with mobilism expanded. This resulted in the surging support for mobilism in the second half of the 1960s.

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historical matter of whether it could be immediately recognized that convection currents were not integral to the snappings together turns out to be a moot point. By 1967, upwelling convection currents beneath mid-ocean ridges seemed to result in discordances elsewhere in this structure.

[873] For example, disjuncts are shown to concord with paleogeography as derived from the interpretation of magnetic profiles (via Vine-Matthews and the reversal timescale). Such work was completed over a period of many years, involving many different researchers.

Chapter 9: Conclusion

In Chapter 6, I argued that entailed relationships between statements have epistemic bearing upon those statements. I identified Models of Discordance and Models of Concordance as alternative models of prediction testing. These models of prediction testing were identified from a deductive capacity for falsification. Overlapping concordances may be said to form predictive networks. This notion of a predictive network can synthesize many other ideas in the epistemology of science and is also compatible with other accounts of scientific knowledge as networked.[874] Answers to epistemic questions may depend upon the way in which a predictive network is dissected and analyzed.

A general conclusion that may be pulled from Chapter 6 is that epistemic support is not unidirectional. Falsification may eventually be isolated asymmetrically, but in principle, any commitment(s) within a discordance may be at fault. Similarly, concordances often form epistemically symmetrical relationships, such that all commitments involved in a concordance obtain epistemic support from that concordance. Empirical statements, instrumental measurements, theoretical generalizations, causal hypotheses, and commitments pertaining to functionality and reliability of instruments, methods, and practices may be constituents within predictive networks and may be accepted, modified, or discarded in relation to concordances and discordances therein. Additionally, ambiguity during problem solving or plurality of scientific inquiry may allow for the formation of mutually exclusive networks, built around mutually exclusive commitments. Accordingly, a particular statement or set of statements (including empirical statements) may obtain differential epistemic support within alternative networks.

In Chapter 7, I argued that predictive networks facilitate the formation of an enriched programme for the analysis of falsification and problem solving. The apparent ambiguity of falsification, often articulated with respect to predictive holism, results in the consensus problem which is often resolved by appealing to the intervention of non-logical factors during problem solving. I argue that falsification can be isolated to varying degrees, and a successful solution to a falsification problem necessarily avoids the inseparable precipitation of discordance. Predictive relationships between the commitments that constitute a falsification problem may facilitate the identification of such inseparable

[874] Predictive networks are not mutually exclusive with other views of scientific knowledge as networked. In Chapter 6, I briefly address Quine's view of semantic relationships and examine Thagard's explanatory coherence in greater detail. Predictive networks build off some of the same intuitions involved in these alternative accounts but identify different sorts of networked relationships as having epistemic bearing.

discordance during problem solving. Apparent constraint upon problem solving may accumulate over time, and this may facilitate the isolation of falsification or limit problem solving flexibility. The accumulation of constraint may eventually demand modifications that are highly disruptive to a predictive network. Consensus formation during problem solving does not necessarily require intervention of non-logical factors.

Historians of science often examine controversies or debates as particularly revealing periods of scientific inquiry. Often, such accounts identify some point at which a debate becomes closed, at which point the outcome of the debate is accepted without question by subsequent researchers. However, the account developed in Chapter 7 shows that problem solving can be a protracted process. Constraint may accumulate over decades or centuries. Then again, problem solving may remain highly ambiguous and this may result in the diversification of competing predictive networks. Proposed solutions may eventually precipitate separable discordances that can be quickly resolved in a minimally disruptive way or may precipitate concordances that were not previously apparent. This may provide positive epistemic support to one proposed solution over a competitor. Alternatively, a problem long-thought to be solved may re-emerge due to the eventual identification of some precipitating inseparable discordance that was not previously apparent. The apparent resolution of debate does not necessarily mark an end of the problem solving process nor ongoing testing of a favored resolution.

In Chapter 8, I identified epistemically significant network structures that I called snapping together events. If, as I established in Chapter 6, epistemic support within a predictive network is not unidirectional, then some process of circular justification, which I called special fitting, might facilitate the indefinite growth of a predictive network, despite the falsity of commitments therein. Snapping together events identify conditions wherein provisional falsity within a structure can be deemed impossible or implausible, regardless of the possibility of circular justification in the growth of that network.[875]

Some predictive networks may undergo a series of snapping together events as they grow. Other networks may grow but fail to snap together, and problems may result in the accumulation of constraint until the network is forced to undergo large scale change. In the early 20th century, divergent approaches to problem solving resulted in the growth of alternative predictive networks of mobilism and fixism. In the 1960s, mobilism snapped together. Accumulating constraint in fixist networks during

[875] Snapping together may thereby have bearing upon a host of other topics in general epistemology and analytic philosophy.

the 1950s and 1960s, however, did not result in snapping together events. Instead, due to narrowing of problem solving trajectories and elimination of options from the field of choice, modification of central fixist commitments became increasingly necessary. When mobilism snapped together, researchers who were invested in fixist networks adopted mobilist commitments.

Throughout this work, I argued that snapping together contributed to the surging support for mobilism in the second half of the 1960s. I identified numerous snapping together events involved in the combination of research in paleomagnetism, marine geology, and geochronology, and I highlighted two of these events as particularly formative. The first event took place in 1965 when Vine and Wilson combined Wilson's hypothesis of crustal plates and mobile belts with the Vine-Matthews hypothesis and thereby discovered the Juan de Fuca Ridge. The second event, with broader significance, took place in 1966 when Pitman and Vine used the geomagnetic reversal timescale, following the discovery of the Jaramillo event, to model magnetic anomalies across ridge axes, and these models were confirmed by measured magnetic profiles from multiple ridges. I then examined how these formative events snapped together with still other commitments to produce a large network structure that strongly supported mobilist commitments. This is why support for mobilism rapidly expanded in the 1960s. Snapping together events unified research interests and carried remarkable epistemic significance that convinced researchers of mobilism, and the resulting network was conducive to further growth, thereby integrating an expanding range of research and a growing collection of researchers.

Henry Frankel developed the idea of the difficulty-free solution to make sense of the mobilism controversy.[876] He identified four difficulty-free solutions in the mobilism debate. In the second half of the 1950s, mobilism provided a difficulty-free solution to the interpretation of some paleomagnetic measurements. The Vine-Matthews hypothesis became difficulty-free in 1966 following Vine's application of the geomagnetic reversal timescale to model global seafloor spreading rates. Transform faults became difficulty-free in 1966 following the seismic measurements of Sykes. Finally, plate tectonics was difficulty-free upon its initial formulation in 1967 by Morgan, McKenzie, and Parker. These difficulty-free solutions marked a transition in problem solving efforts, wherein some difficulties confronting an accepted solution were deemed to be adequately resolved while unresolved difficulties

[876] Frankel, 2012

were no longer considered to weigh against that solution.[877][878] Frankel identifies difficulty-free solutions empirically, by the formation of consensus among researchers centrally involved in problem solving efforts.

The analytic framework developed in Part 2 of this work culminates with an account of why researchers might form consensus, despite persistent difficulties during problem solving. I argued that snapping together events can have profound epistemic importance. Snapping together raises the epistemic support for commitments therein and may pledge that a growing network is on the right track. This may result in growing confidence that persistent difficulties do not weigh upon those commitments that snapped together, and that falsification can be attributed elsewhere, even if a fully adequate solution has yet to be proposed. I showed how the Vine-Matthews hypothesis, Wilson's hypothesis of crustal plates and mobile belts, and plate tectonics snapped together with a large collection of other commitments in the second half of the 1960s.[879] So impressive were these series of snapping together events, that explicit support for mobilism rapidly expanded. Some researchers expressed amazement and delight at predictive successes and recognized the significance of ongoing research. Others, previously committed to fixist networks, described their transition to mobilism as deeply shocking, world shattering, yet impossible to avoid.[880] The convincing power of the research was often explicitly attributed to the remarkable fit across multiple lines of research.

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[877] Several early accounts of the plate tectonics revolution, including Anthony Hallam's *A Revolution in Earth Sciences* and Menard's *The Ocean of Truth,* also note that mobilism and associated commitments like plate tectonics and seafloor spreading confronted anomalies even after they were widely accepted (Hallam, 1973; Menard, 1986). These particular works rely on Kuhnian ideas of paradigm changes and normal science to account for difficulties that are no longer considered to weigh upon an accepted hypothesis.

[878] In Chapter 7, I did not rely on Frankel's terminology of "difficulties" but instead referred to distinct and inseparable refutations or discordances. Distinct refutations allow for "kicking the can" of falsification down a sequence of problems that can be treated independently. Alternatively, inseparable refutation or discordances invalidate a proposed solution. It may be possible to retain some commitment(s) within a proposed solution that confronts inseparable refutation if the field of choice allows for falsification to be attributed elsewhere. Frankel refers to all such refutations or discordances as difficulties.

[879] Though I am ambivalent about the difficulty-free status of paleomagnetism in the 1950s, I argued that paleogeographic reconstructions from paleomagnetism also snapped together with a growing mobilist network in the second half of the 1960s. My historical inquiry is directed toward the surging support for mobilism in the 1960s, and the account of paleomagnetism that I provided in Chapter 2 is not sufficiently detailed to identify possible snapping together events therein during the 1950s.

[880] Early accounts of the plate tectonics revolution sometimes refer to uptake of mobilism in the USA, especially, as being akin to a religious conversion. For example, in *The Ocean of Truth*, Menard claimed that uptake of mobilism at Lamont was, "more like the Protestant Reformation than a scientific revolution" (Menard, 1986, 264). He also described the tectonics revolution as more violent than often presumed (Menard, 1986, 256).

The analytic account that I offer is also able to complement and incorporate some of the central arguments endorsed by other historians, with respect to the growing support for mobilism during the 1960s. In *The Road to Jaramillo*, William Glen claimed that a conflation of evidence from independent origins resulted in the rapid uptake of mobilism during the 1960s.[881] In *Drifting Continents and Shifting Theories*, Homer Le Grande claimed that a growing confluence of many steams of knowledge were integrated into mobilist research during the 1960s and into 1970s, and this broadened the relevance of mobilism across many specialties, thereby facilitating uptake.[882] Naomi Oreskes views scientific knowledge as networked, emphasizing the role of methods in scientific change, and the broad influence of sociological forces upon such methods.[883] Oreskes also claims that the unmaking of old scientific knowledge is a necessary step in the formation of new scientific knowledge. For Oreskes, this unmaking takes place gradually as an interwoven fabric of methods and beliefs is unwoven and restitched.[884] Likewise, Le Grande argues that uptake of mobilism was a gradual process, spanning decades but accelerating and broadening in the 1960s. Though I have emphasized that snapping together events are sudden, I have also illustrated that accumulation of constraint upon fixist networks as well as the research that contributed to the snapping together of mobilist networks developed gradually over decades.

Throughout this work, I focused primarily on large scale theoretical changes in Earth sciences. I examined the accumulation of constraint upon fixism and the snapping together of mobilism to make sense of the plate tectonics revolution. However, the analytic framework developed in Part 2 of this work may have broad utility elsewhere and may provide insight into other instances of large-scale theory change. Predictive networks, accumulating constraint, and snapping together may also have bearing on the epistemic support of observations, measurements, methods, causal hypotheses, expectations about reliability, notions of functionality, and more.[885]

Those historians, philosophers, and sociologists who study science often aim to account for both the epistemic significance and historical contingency of scientific knowledge. Throughout this work, I

[881] Glen, 1982
[882] Le Grande, 1988
[883] Oreskes, 1999
[884] Oreskes, 1999, 316
[885] For example, rather than examining the rapid uptake of mobilism in the second half of the 1960s, the analytic framework developed in Part 2 could be used to examine the acceptance of potassium-argon dating methods, or the possible snapping together of radiometric dating with the geological timescale. Radiometric dating may seem to be more about measurement and instrumentation, and the geological timescale may seem to be more about categorization or ontology.

have argued that close attention to the history of science is required in order to establish the epistemic strength of commitments within a predictive network. Leading into the 1960s, research in paleomagnetism, marine geology, and geochronology was packed with ambiguity and debate, and the epistemic strength obtained by mobilism near the end of the 1960s was very much dependent upon this. In Chapter 6, I argued that concordance is epistemically significant when constituent commitments have independent epistemic support. Independence and plurality of research contexts is conducive to the formation of epistemically significant concordances. Establishing the epistemic support of concordances within a predictive network thereby requires detailed consideration of such alternative research contexts. In Chapter 7, I showed that attempts at problem solving can facilitate the elimination of problem solving trajectories and the isolation of falsification by narrowing a field of choice. Diversity and plurality of problem solving efforts facilitate the accumulation of constraint and may thereby be conducive to consensus formation. Establishing the degree to which falsification is isolated, or the necessity of non-logical factors in consensus formation, requires consideration of previous and concurrent problem solving efforts. In Chapter 8, I noted that ambiguities and debates establish the possible parameters of network growth by special fitting. If the set of ambiguities involved in alternative directions of network growth do not overlap, then snapping together of these lines of research may be deemed implausible, were falsity contained therein. Establishing the epistemic support of snapping together events may thereby require detailed knowledge of the ambiguities and debates involved in the historical development of a network structure.

An implicit argument, throughout this work, is that judgements of epistemic strength among scientists depends, in part, upon historical knowledge pertaining to such independence, ambiguity, diversity, and plurality, and this historical knowledge contributed to the plate tectonics revolution. By virtue of contributing to knowledge of the history of science, historians, sociologists, and philosophers may influence the epistemic judgements of scientists and may also facilitate the identification of the epistemic limitations of such judgements. Additionally, historical knowledge may, itself, form predictive networks. The analytic framework developed in Part 2 of this work may thereby provide insight into the epistemic strength of knowledge claims endorsed among historians, sociology, and philosophers of science.

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