Harry Hess and Sea-floor Spreading

Alan O. Allwardt

Unpublished manuscript based on:


Abstract

Harry Hess's hypothesis of sea-floor spreading brought together his long-standing interests in island arcs, oceanic topography, and the oceanic crust. The one unique feature of Hess's hypothesis was the origin of the oceanic crust as a hydration rind on the top of the mantle — an idea that was not well received, even by the early converts to sea-floor spreading. Hess never changed his mind on this issue, and his stubbornness illuminates the logic of his discovery. Published and archival records show that 1) Hess became convinced the oceanic crust was a hydration rind as early as mid 1958, when he was still a fixist, 2) he devised sea-floor spreading in 1960 to reconcile the hydration-rind model with the newly discovered, high heat flow at oceanic ridge crests, and 3) Hess's new mobilist solution did the least amount of violence to his older fixist solution.

Introduction

Harry Hammond Hess (1909-1969), the Princeton University icon, is usually given credit for the idea of sea-floor spreading by earth scientists and historians alike (e.g., Frankel, 1979, 1980; Glen, 1982; Glen and Frankel, 1988; Hallam, 1973; Marvin, 1973; Menard, 1972; Moores and Vine, 1988). The hypothesis of sea-floor spreading brought together three of Hess's long-standing research interests: 1) the structure and evolution of island arcs (beginning circa 1930); 2) the origin of ocean-basin topography (beginning circa 1940); and 3) the nature of the oceanic crust (beginning circa 1950). Three illustrations from papers that Hess wrote in the early 1960s show how he broke from the conventional "fixist" wisdom by integrating these seemingly diverse elements into a single "mobilist" model:
Figure 1, taken from Hess's preprint, "The Evolution of Ocean Basins" (Hess, 1960b; published with minor changes as Hess, 1962) shows an idealized cross section of a mid-ocean ridge. In this diagram, the main layer of the oceanic crust, layer 3, is about 5 km thick and composed of partially (70%) serpentinized peridotite; the mantle below is composed of unaltered peridotite. (The upper crustal layers, together about 1 km thick, are not depicted here: layer 1, composed of unconsolidated sediment; and layer 2, composed of consolidated sediment.) The arrows and flow lines define a pattern of ascending, diverging convection within the crystalline mantle. According to Hess, mantle convection would 1) significantly increase heat flow near the ridge crest (represented on the diagram by the bulge in the 500°C isotherm), and 2) release juvenile water from the mantle, also in the vicinity of the ridge crest (represented on the diagram by the squiggles).

Putting these various elements together, Hess proposed that layer 3 of the oceanic crust is a hydration rind on the mantle, formed at the ridge crest and then transported laterally like a conveyor belt (Hess, 1960b, p. 5-8, 25-28). Laboratory experiments had shown that olivine (the primary mineral in peridotite) reacts chemically with water at temperatures lower than 500°C to form serpentine. Hess reasoned that the base of layer 3 under the ridge crest is the depth at which this reaction commences, as the water released by convection rises to meet the bulging 500°C isotherm (Figure 1). The newly created layer 3, about 5 km thick, is then carried away from the ridge crest on the backs of the diverging convection currents. Note, however, that layer 3 does not become thicker as it moves toward the flanks of the ridge, even though the 500°C isotherm gradually slopes downward: the hydration reaction cannot continue under the flanks of the ridge because there isn't a supply of juvenile water. As such, the base of layer 3 throughout the ocean — the Mohorovicic discontinuity — would represent a "frozen" or "fossil" isotherm inherited at the ridge crest; this accounts for the surprisingly uniform thickness of layer 3.

Figure 2 (from Hess, 1965, Fig. 123, p. 324) shows the crust and mantle "bolted together" to emphasize that they move at the same velocity. In some respects, Hess's oceanic crust — being a hydration rind — could simply be regarded as part of the mantle. He therefore eliminated viscous
drag between crust and mantle, a feature shared by the older, convection-driven tectonic schemes of Holmes (1931, 1944), Vening Meinesz et al. (1934), and Griggs (1939).

Figure 3 (from Fisher and Hess, 1963, Fig. 9, p. 430) is a hypothetical cross section of an island arc and trench, where converging, descending convection currents in the mantle draw the oceanic crust into a tight, vertical downbuckle. Layer 3 of the crust (labeled "70% serpentine") is destroyed when it reaches the 500°C isotherm, because the hydration reaction is reversed. "A great advantage of such a crust," Hess (1965, p. 327) later noted, "is that it is disposable." Layer 2 of the oceanic crust (shown as "consolidated sediments and volcanic debris") is carried to greater depths, where it finally undergoes partial melting. The resulting magma, along with the water released by the deserpentinization of layer 3, reaches the surface by way of the volcanoes in the island arc. Note that the single net effect of Hess's crustal conveyor belt was the eventual release of juvenile water to the hydrosphere, via the atmosphere. As such, sea-floor spreading would cause the volume of seawater to steadily increase.

These, then, were the basic features of Harry Hess's hypothesis of sea-floor spreading — the underlying theme being the serpentinization of ultramafic rocks. Somewhat coincidentally, Hess's fourth long-standing research interest was the mineralogy and petrology of mafic and ultramafic rocks.

Of course, several of the elements that went into Hess's hypothesis had been previously discussed, in slightly different contexts, by other geologists and geophysicists. For example, Holmes (1931) had long before proposed what amounted to convection-driven, sea-floor "stretching" (see Hess, 1968); more recently, Carey, Heezen and Menard had called upon various forms of mid-ocean rifting (for discussion and references, see Glen, 1982; and Menard, 1986). And then there was Robert Dietz, who in 1961 preceded Hess into print with a version of sea-floor spreading so similar to Hess's unpublished model that it led to a protracted priority dispute (see Dietz, 1961, 1964, 1968; Menard, 1986, Ch. 13; Allwardt, 1990, Part II, Note 3). The one unique element in Hess's model was the origin of layer 3 of the oceanic crust as a hydration rind on the top of the mantle. This highlights a curious fact: Although Hess is generally given credit for the idea of sea-
floor spreading, his one unique contribution to the hypothesis was never well received. Many, if not most, of the early converts to sea-floor spreading preferred an igneous origin for layer 3 (i.e., one composed of sheet dikes and gabbros). Clearly, the "conveyor belt" aspect of sea-floor spreading would work equally well no matter what the exact nature of layer 3; yet Hess remained firm and died believing that layer 3 was composed of serpentinite (e.g., Vine and Hess, 1970, p. 606). Hess's stubbornness on this issue reveals something very important about the logic of his original discovery. To begin, therefore, I will return to the early 1950s and trace the evolution of Hess's thought on the nature of the oceanic crust. As such, this historical treatment of Hess will be somewhat unorthodox; previous historians have tended to pay more attention to Hess's work on island arcs and mid-ocean ridges (e.g., Frankel, 1980, 1987; Glen, 1982).

The path of Hess's discovery is best illuminated with a series of revealing quotations, taken from his published and unpublished writings. Most of the unpublished material is part of the Harry H. Hess Collection at Princeton University (see Appendix). These documents constitute a second "paper trail" showing the evolution of Hess's thought on ocean basins and their margins.

The Early 1950s: Momentous Discoveries

By the 1940s, it was widely accepted that the composition of the oceanic crust was fundamentally different from that of the continents. The basic arguments had been around for decades: First, the earth's surface was dominated by two apparently permanent levels, which corresponded with the continental lowlands and the floors of the oceans, respectively. Because the continental crust was wholly or partly composed of sial ("granite"), isostatic considerations dictated that the oceanic crust consist of a denser material, such as sima ("basalt"). Second, all of the volcanic islands in the deep ocean basins were basaltic, which again seemingly reflected the composition of the source areas below.

Not until the early 1950s, however, would it be realized that the oceanic crust was much thinner than the continental crust; Holmes (1944, Fig. 262, p. 506), for instance, had depicted a very thick (tens of kilometers), world-wide basaltic layer, with comparatively thin granitic continents.
embedded in it. The first inkling that the oceanic crust was only a few kilometers thick resulted from the analysis of earthquake surface waves by Maurice Ewing and Frank Press at Lamont (Wertenbaker, 1974, Ch. 5); the confirmation came through shipboard seismic-refraction studies by Russell Raitt of Scripps (Menard, 1986, Ch. 4). As Hess summarized at a meeting sponsored by the Royal Society of London:

The most momentous discovery since the war is that the Mohorovicic discontinuity rises from its level of about 35 km under the continents to about 5 km below the sea floor [Hess, 1954, p. 341-342].

Figure 4 shows Hess's portrayal of this "momentous" discovery. Note especially the two idealized 40-km sections, with calculations to show their isostatic equivalence. In the text, Hess offered his first explanation of the so-called "isostatic link" between the thickness of the continents and the depth of the oceans (Hess, 1954, p. 342-343).

Having established a typical crustal section for the oceans, Hess then considered the most obvious departures from the norm, the oceanic ridges (Hess, 1954, p. 344-346). Hess's working hypotheses for three different types of oceanic ridge are shown in Figure 5 (at this time Hess used the term "oceanic ridge" rather loosely). Case A, designed to represent the Hawaiian chain, was simply a volcanic pile resting on the sea floor. Case B, designed to represent the Mid-Atlantic Ridge, showed the injection of great quantities of basaltic magma into the upper mantle and crust, "perhaps over an upward convection current in the mantle." The topographic elevation of the ridge was due to the relatively low density of the injected material. Case C, representing submarine extensions of continental fold belts, was essentially an application of the tectogene hypothesis that Hess had helped pioneer in the 1930s (Hess, 1938, 1939).

However, there were other submarine topographic features that were not so easily explained: flat-topped plateaus in the mid-Pacific and the Albatross Plateau (not yet recognized as part of the East Pacific Rise). Hess explained the background for this problem most clearly in late 1953, in a status report submitted to the Office of Naval Research, which had funded his investigations:
It has been supposed that such areas which stand high relative to the ocean floor do so because they have a thicker upper layer [basaltic crust] than normal. The thicknesses reported for the upper layer by Scripps, however, do not seem to show any correlation with depth of water. The upper basaltic layer seems to be uniformly from 4 to 6 km thick. This suggests that some mechanism of density variation occurs below the Mohorovicic discontinuity in the substratum [Harry H. Hess, written communication to the Chief of Naval Research, Geophysics Branch, Office of Naval Research, Oct. 23, 1953].

Hess proposed that the density variation below the Moho was caused by the localized hydration of the upper mantle by juvenile water (Hess, 1954, p. 346-347). Laboratory experiments had shown that olivine (the primary mineral in peridotite) reacts chemically with water at temperatures lower than 500° C to form serpentine. Complete serpentinization (which was unlikely) would reduce the density of the upper mantle from about 3.25 to 2.60 and raise the sea floor accordingly. On the other hand, different degrees of partial serpentinization could easily account for the submarine plateaus lying at different levels.

Because the serpentinization reaction was exothermic, Hess also suggested that this mechanism might help explain the anomalously high heat flow from the ocean basins. (Revelle and Maxwell had reported, in 1952, that oceanic heat flow was about equal to continental heat flow. It had long been known that most of the heat flow from the continents could be accounted for by the radioactivity of the thick sialic crust. On the other hand, most of the newly discovered heat flow from the oceans apparently originated below the thin simatic crust, which was considerably less radioactive.) Finally, Hess noted that a periodic reversal of the serpentinization reaction would lead to widespread subsidence of the sea floor — which might explain how guyots had formed in the Pacific (Hess, 1954, p. 347). To summarize, Hess (1954) used the serpentinization mechanism to explain oceanic plateaus but not oceanic ridges like the Mid-Atlantic. The next logical step was soon to follow.
The Mid 1950s: Oceanic Ridges and the Serpentine Welt

In the mid 1950s Hess extended his hypothesis of upper-mantle hydration to include the Mid-Atlantic Ridge and similar features. Using this mechanism to explain a relatively narrow, prominent feature like an oceanic ridge — as opposed to a broader, more subdued submarine plateau — posed an additional complication:

The problem of why serpentinization was concentrated in the Atlantic along a median line can perhaps be explained in several ways. One hypothesis could be that convective circulation in the mantle occurs, and the ridge represents the trace of an upward limb of a cell. In this case water ejected from the top of the column might cause the serpentinization and heat which would move upward much more slowly by conduction might cause the later deserpentinization [Hess, 1955a, p. 404-405].

In other words, Hess predicted that the Mid-Atlantic Ridge is an ephemeral feature. Accordingly, Hess devised a hypothetical evolutionary sequence for oceanic ridges (Figure 6), which he unveiled at the Columbia University symposium on the *Crust of the Earth* (see Poldervaart, 1955).

Panel 1 established the "normal" oceanic situation, with a thin basaltic crust overlying a peridotitic mantle. Given the known range of values for oceanic heat flow, Hess predicted that the 500°C isotherm was 7-19 km below the Moho, with a "most probable" depth of 12 km (Hess, 1955a, Table 1, p. 403).

Panel 2 showed an early stage of mantle convection, in which rising juvenile water causes localized serpentinization of the mantle above the 500°C isotherm. (Heat flow has not yet begun to increase; hence the stable position of the isotherm.) Because the growing mass of serpentinite is considerably less dense than peridotite, the sea floor is pushed up much like a blister or welt. As Hess noted, the amount of juvenile water required to create the Mid-Atlantic Ridge by this mechanism would be roughly equivalent to one percent of the oceans.
Panel 3 depicted an intermediate stage of mantle convection, in which the constructive effect of juvenile water is exactly offset by the destructive effect of increasing heat flow (represented by the rising 500°C isotherm). The serpentine welt migrates upward but maintains a constant volume until it impinges on the base of the crust, as shown. Consequently, the topographic ridge is in steady state.

Panel 4a represented a later stage of mantle convection, with heat flow continuing to increase. The serpentine welt, which can no longer migrate upward, begins to shrink, and the topographic ridge subsides as a result. Ironically, the end result of active mantle convection would be the destruction of mid-ocean ridges, rather than their formation. Hess believed that the Mid-Atlantic Ridge was at a stage somewhere between Panels 3 and 4a.

Panel 4b showed an alternative way to reverse the serpentinization process, which Hess devised to explain the Mid-Pacific Mountains, a region of guyots in the west-central Pacific (see Hess, 1946). In this scenario, prolonged vulcanism along the crest of a pre-existing oceanic ridge would eventually introduce enough heat to destroy the serpentine welt, causing the ridge to subside. Volcanic islands formed in the early stages of vulcanism would sink along with the sea floor and become guyots; the deepest guyots would in general represent the oldest islands. Hess suggested that the chain of events described above might take about 100 million years — an estimate no doubt influenced by the discovery of Late Cretaceous, shallow-water fossils on deeply submerged Pacific guyots (Hess, 1955a, p. 405). And thus was born the 10^5-year life cycle for oceanic ridges — an idea that would be passed back and forth between Hess and Menard for the next ten years. Menard (1958) took the next step by citing the following features as examples of a morphological succession: the broad, low East Pacific Rise (young); the narrow, steep Mid-Atlantic Ridge (mature); and the rugged, discontinuous Mid-Pacific Mountains (old). He later expanded this treatment of the subject in his book, Marine Geology of the Pacific (Menard, 1964). Hess would also elaborate on the life cycle of oceanic ridges in several papers, all of which will be discussed when the time comes.
Returning now to the central theme of this historical analysis — the evolution of Hess's thought on the nature of the oceanic crust — note that Hess (1955a) still depicted the main crustal layer in the oceans as basalt (Figure 7); this, as he had stated earlier at the Royal Society meeting, was "inferred from the seismic velocities and petrologic probabilities" (Hess, 1954, p. 342). Soon, however, Hess (1955b) would re-examine this issue in considerable detail at an "oceanographic convocation" held in Woods Hole:

*It is possible that some serpentinized peridotite such as that found on the mid-Atlantic Ridge[ in dredge samples] is present in the "basaltic" layer. Seismic velocities in two samples of such material were measured in the laboratory for the writer by E.C. Bullard; measurement gave for Vp 5.7 and 6.3 km/sec at atmospheric pressure. These velocities might be 5 to 10% higher at pressures consistent with their depth within the crust [Hess, 1955b, p. 428, emphasis added].*

Bullard's velocities, corrected for pressure, were consistent with measured values for layer 3. This laid the groundwork for important changes to come in the late 1950s.

The Late 1950s: The Nature of the Oceanic Crust and Moho

For the next three years (1956-1958), Hess published nothing on the subjects of the oceanic crust and oceanic ridges. This was undoubtedly a time of major reassessment for Hess, because when he resumed publishing in 1959, his ideas on the oceanic crust had taken a novel turn, and the serpentine-welt hypothesis for oceanic ridges had apparently reached the crossroads. Whatever the exact cause of the three-year hiatus in Hess's publications on these topics, I have been forced to reconstruct the events of this crucial period almost entirely from archival materials.

Hess's (1955b) suggestion that the oceanic crust might contain a significant component of serpentine had left him in a quandary: could he invoke the same hydration reaction to explain both the oceanic crust and oceanic ridges? In other words, were these two applications of his
hypothesis *internally consistent?* In order to adequately address this question, Hess would first have to refine the hydration model in a number of ways:

1) Hess (1955a, 1955b) had depicted an oceanic ridge as a blister or welt forming beneath a *pre-existing* crust. How old was the crust?

2) If the oceanic crust did contain serpentine, what had been the source of the water necessary to drive the hydration reaction? Why hadn't this water been restricted to specific locations, as in the case of the welts beneath ridges? How deep was the 500° C isotherm when the crust formed?

3) Within the context of the hydration model, what was the degree of serpentinization in the crust? In the welt beneath an oceanic ridge?

Clearly, resolving the third point was the best place to start; and as Hess intimated in his publications of 1955, seismic exploration was the most promising route. Bullard's P-wave velocity measurements on serpentine had been encouraging, but now it was time for a more systematic study. Hess had another reason for wanting to know the seismic properties of serpentinized peridotite. Peter H. Mattson, a Princeton graduate student working in southwestern Puerto Rico (near Mayagüez) in 1953-1956, had discovered that the basement rock of the island was serpentinized peridotite (see Mattson, 1960). Then, in early 1955, an extensive seismic-refraction survey of the Caribbean cosponsored by Woods Hole and Lamont had failed to find the Mohorovicic discontinuity beneath Puerto Rico (see Officer et al., 1957). Could the basement of Puerto Rico be highly altered mantle material, which had poked above sea level because of its lowered density?

With these points in mind, Hess sent a suite of samples, representing varying degrees of serpentinization, to Jack Green of the California Research Corporation (a subsidiary of Standard Oil located in La Habra, California), in late 1956 or early 1957. (The exact date is unclear because the only letter to Green in Hess's files at Princeton was written May 13, 1957, *after* Green's P-
wave velocity measurements had been completed.) The data from Green confirmed the suspicion that Hess had been harboring since the Woods Hole Convocation (Hess, 1955b, p. 428; quoted earlier), as he informed Ed Hamilton (U.S. Navy Electronics Laboratory, San Diego) the following year:

You remember that paper I wrote for the Woods Hole Convocation (I'll send you a reprint)? We really do not have very good evidence to assume that the layer under the sediments is "basalt." It might be lithified sediment too or it might be serpentine. Green at La Habra has been running a series of samples I have collected for him representing serpentine to fresh peridotite. The velocities and densities make a straight line curve vs. per cent serpentinization (5.6 to 8.4 km/sec) [Harry H. Hess, written communication to Edwin L. Hamilton, Dec. 23, 1958].

Now Hess could express the hydration hypothesis in more explicit terms. The "normal" mantle, with seismic velocities above 8 km/sec, would be fresh peridotite; the anomalous upper mantle beneath the Mid-Atlantic Ridge, with seismic velocities around 7.4 km/sec (Ewing and Ewing, 1957, 1959), would represent peridotite two-fifths serpentinized; and the main layer of the oceanic crust, with seismic velocities between 6.5 and 7 km/sec, would represent peridotite two-thirds serpentinized.

Of course, the seismic evidence was still compatible with the traditional interpretation — basaltic crust, above a mantle of peridotite (or eclogite), with the anomalous zone beneath the Mid-Atlantic Ridge representing a mixture of basalt and mantle material. Hess needed an independent geological argument showing that his interpretation was better. The first glimmer of such an argument can be found in a manuscript on Princeton's Caribbean Research Project (an outgrowth of Hess's early work on island arcs) submitted for publication on July 8, 1958:

One might ask whether the crust under the oceans which has seismic velocities generally between 6.4 and 6.9 km/sec might not also be peridotite two-thirds
serpentinized rather than basalt. The dredging of serpentinized peridotite from fault scarps on the mid-Atlantic ridge ... suggests this, as does the rather uniform thickness of this layer in all of the seismic profiles at sea. If this is true, confusion resulting from semantics must be avoided. The "crust" would in essence be altered mantle material [Hess, 1960a, p. 237, emphasis added].

The next year Hess made this argument more explicit in a half scientific, half promotional paper on the fledgling Mohole project, the fill-fated deep-drilling program sponsored by the AMSOC Committee of the National Academy of Sciences-National Research Council (see Bascom, 1961; Greenberg, 1967; Lomask, 1976; and Shor, 1985):

The surprising uniformity in thickness of Layer 3 requires that the bottom of the layer represent the position of an isotherm or past isotherm, and that this is a level at which a reaction or phase transition has taken place. If the layer were basalt flows one would expect great variability in the thickness. Flows would be many times thicker near a vent or fissure from which they issued than at greater distances from their source [Hess, 1959b, p. 345].

Note that this argument presumed that the ocean basins were permanent features: the difficulty, as Hess saw it, was imagining how flood basalts could become so evenly distributed on a scale of thousands of kilometers. Hess then elaborated on the "isotherm or past isotherm" that had controlled the position of the oceanic Moho:

[If] the "crust" and material below are peridotitic in composition and an abrupt change from partially serpentinized peridotite to unserpentinized peridotite occurs at the Moho ... [then] the Moho under the oceans would represent some ancient time when the 500° C isotherm stood at this datum plane below sea level [Hess, 1959b, p. 345, emphasis added].
In describing the Moho as a thermal "datum plane" Hess clearly implied that the ocean basins were permanent — but what did he mean by the "ancient time" when the Moho supposedly formed? At the minimum, the Moho would have predated the oceanic ridges that blistered the crust — a time frame of $10^8$ years. At the maximum, the Moho could date to the primordial stages of the Earth — a time frame of $10^9$ years:

Some scientists believe that the Moho is an abrupt change, perhaps representing the original surface of the earth and that the materials immediately above it are later volcanic outpourings. Others believe that the Moho is a transitional zone perhaps representing a phase change or a "frozen isotherm" that developed as the surface of the earth cooled. If the latter is true, then the top of the deep crust rather than the top of the mantle may be the primordial surface of the earth [AMSOC Committee, 1959, p. 10, emphasis added].

Now all of the elements in Hess's hydration model had seemingly come together:

1) The oceanic crust dated to the early Precambrian, when the earth was significantly hotter and vigorously degassing. Under these primordial conditions, the $500^\circ$ C isotherm would have been uniformly shallow, and juvenile water would have been escaping everywhere — allowing a thin hydration rind to form on the peridotite exposed in the previously "crustless" ocean basins.

2) Oceanic ridges would begin forming later, after the rates of global cooling and degassing had declined dramatically. Accordingly, the $500^\circ$ C isotherm would lie considerably deeper than the base of the oceanic crustal "rind," and the release of juvenile water would be much less widespread — perhaps restricted to the sites of upwelling convection currents in the mantle. Under these conditions, serpentine welts could form in the upper mantle, pushing up the pre-existing oceanic crust to form topographic ridges. Oceanic ridges would be ephemeral, with a $10^8$-year life span tied to convection cycles in the mantle. As such, the current crop of ridges should all be Mesozoic in age or younger.
Thus, as the end of the decade neared, Hess had apparently succeeded in using the serpentinization model to explain both the oceanic crust and oceanic ridges. More importantly, he had done it entirely within the context of permanent ocean basins. This "fixist" synthesis would be short-lived, however. The recent discovery of high heat flow at the crests of oceanic ridges would prove fatal to Hess's explanation for their topographic elevation, although he would not fully realize it until 1960. This learning experience would lead Hess to a new synthesis based on sea-floor mobilism.

1959-1960: Hess's Conversion

During Scripps's *Downwind* Expedition, undertaken from October 1957 to February 1958 as part of the International Geophysical Year, Richard von Herzen made 36 measurements of heat flow in the southeastern Pacific. Prior to this expedition, only 25 measurements had been made in the entire Pacific basin, and their average was roughly equal to the heat flow from the continents — slightly more than 1 HFU (see Bullard et al., 1956). Most of von Herzen's new measurements corroborated the previous basin-wide average, but ten values were anomalously high, from 2 to 8 HFU. Using Menard's detailed but largely unpublished sea-floor topography (see Menard, 1986, Ch. 5), von Herzen determined that eight of his values above 2 HFU lay near the crest of the East Pacific Rise, as did three anomalous measurements from the previous studies.

Although von Herzen did not publish his data until March 1959, the gist of the results were known to the "insiders" at Scripps almost immediately. For instance, Menard submitted a "Short Note" on mid-ocean ridges to the Geological Society of America on February 8, 1958, and in it he cited von Herzen's discovery as a personal communication (Menard, 1958, p. 1182). If Hess hadn't heard the news already, he certainly found out when Menard's manuscript was published in September.

In fact, Hess apparently began wrestling with von Herzen's data some months before Menard's paper appeared. In his yearly summary to the Office of Naval Research for fiscal year July 1957-June 1958, Hess reported that:
The hypothesis that the high heat flow from oceanic ridges might be due to the heat produced by serpentinization (100 cal/gm) of the mantle was tested and found to be too low by two orders of magnitude. A similar analysis of the hypothesis that basalt intrusions cause the high flow is too low by a factor of 20. It is concluded that the heat flow is a result of convection in the mantle [Harry H. Hess, written communication to Gordon Lill, Head of Geophysics Branch, Office of Naval Research, Jan. 13, 1959].

This conclusion by Hess was the first step in the eventual demise of his serpentine-welt hypothesis. Remember, Hess (1955a, 1955b) had envisioned oceanic ridges being created during the early stages of convection by the release of juvenile water and then being destroyed during the later stages of convection by increasing heat flow. Yet now Hess was faced with explaining the East Pacific Rise, which had the morphologic characteristics of a young ridge and the heat-flow characteristics of an old ridge.

For a while, it seems, Hess tried to have it both ways. His abstract on the "Nature of the Great Oceanic Ridges," written for the 1st International Oceanographic Congress in New York (Aug. 31-Sep. 12, 1959), was a mass of contradictions. I have reprinted this abstract in full because it aptly symbolizes the watershed Hess was about to cross:

The Mid-Atlantic and Mid-Indian Ocean ridges have long been known as forming conspicuous topographic features on the Earth's surface. Recently Menard has shown that median ridges exist in all oceanic areas. Heezen has pointed out that the Mid-Atlantic ridge has a narrow longitudinal graben along its crest and that the shallow earthquake activity is concentrated below the graben. Investigators from Scripps (Maxwell and von Herzen [1959]) have found that the heat flow on the crests of some Pacific ridges is several times higher than normal. Gravity data obtained by Vening Meinesz and investigators from Lamont (Worzel, Ewing, et al.) show that no conspicuous anomalies are found on ridges. Seismic data
presented by Raitt, M. Ewing, J. Ewing, and others generally show that the M discontinuity cannot be found on the crests of ridges. Certain details of the seismic profiles on the flanks of ridges give a rather clear insight into the nature of the ridges and their probable origin.

There is some evidence that the great oceanic ridges may be ephemeral in nature. "Old" ridges of rather subdued topography such as the Mid-Pacific mountain range have abundant guyots and atolls indicating subsidence of 3,000 to 6,000 feet. On "young" ridges such evidence of subsidence is absent and high heat flow seems characteristic. The seismic profiles on "old" and "young" ridges seem to differ significantly.

The conclusion is drawn that the ridges owe their origin to a volume change below the M discontinuity and that the volume change is reversible. Two hypotheses on volume change may be proposed at the present time, (1) the change from eclogite to basalt and (2) the change from peridotite to serpentinite. The former, however, is rejected as less likely on the basis of petrologic information presently available.

Assuming that the process forming the ridges is serpentinization which involves about 100 cal/g in heat evolved, the possibility was investigated that this might account for the high heat flow. It fails to do so by about two orders of magnitude. Somewhat more heat could be obtained by supposing it resulted from basalt intrusions into the ridge but this too fails by more than an order of magnitude.

The hypothesis is advanced that the ridges represent the trace on the Earth's surfaces [sic] of upward flowing limbs of mantle convection cells. Water released at the top of the column produces the serpentinization, subcrustal drag of the horizontal flow produces extension and the graben on the crest. Heat moving slowly upward by conduction accounts for the high heat flow and ultimately for deserpentinization and subsidence of the ridge [Hess, 1959a, emphasis added].
From all indications, this abstract was a cut-and-paste job. The central theme, as presented in the second, third, and fifth paragraphs, was vintage 1955. Everything else was an attempt to accommodate more recent discoveries, most notably the high heat flow at the crests of "young" ridges. And here, the cut-and-paste approach (and the thinking behind it) obviously got Hess into trouble. I have emphasized two sentences in the abstract that, taken together, are completely contradictory. The first sentence, which states that young ridges have high heat flow but show no signs of subsidence, was clearly based on von Herzen's discovery and Menard's (1958) morphological classification of ridges. The second sentence, which implies that high heat flow is a characteristic of old ridges and the cause of their subsidence, is a leftover from 1955.

Although Hess's error is obvious in retrospect, he did not recognize it until late 1960. The key, apparently, was the seemingly unrelated research that Carl Bowin completed shortly after receiving his doctorate at Princeton in September 1960 (see Allwardt, 1987; Bowin, 1987). Bowin, who stayed on temporarily as an instructor, was using newly available electronic computers to model the growth rates of chiastolite crystals in the wall rock surrounding a gabbro intrusion in northern Maine (an area he had studied for his Master's degree at Northwestern). This classic example of contact metamorphism was in essence a detailed problem in heat flow. As such, Bowin's calculations had broader applications, which he and Hess were quick to appreciate:

The heat-flow conduction, temperature, and time relations being determined also had impact upon our discussions of Hess's serpentinization hypothesis for ridge elevation. The increase in temperature gradient in the wall rock during contact metamorphism translated [in Hess's problem] to an increase in temperature gradient and a rise in the 500°C isotherm above an upwelling convection cell to a very shallow level [Bowin, 1987, p. 475, emphasis added].

In other words, there wasn't any room for the serpentine welt that supposedly caused the topographic elevation of the ridge. At last, Hess could no longer avoid the conclusion that had been staring him in the face since 1958 or 1959, as he admitted in his forthcoming preprint:
Formerly the writer (1955[b], 1959a) attributed the lower [seismic] velocities (ca. 7.4 km/sec) [beneath oceanic ridges] in what should be mantle material to serpentinization, olivine reacting with water released from below. The elevation of the ridge itself was thought to result from the change in density (olivine 3.3 g/cc to serpentine 2.6 g/cc). A 2 km rise of the ridge would require 8 km of complete serpentinization below, however a velocity of 7.4 km/sec is equivalent to only 40% of the rock serpentinized. Thus serpentinization would have to extend to 20 km depth to produce the required elevation of the ridge. But this reaction cannot take place at a temperature much above 500° C which considering the heat flow [at the ridge crest] probably lies at the bottom of layer 3, about 5 km below the sea floor, and cannot reasonably be 20 km deep [Hess, 1960b, p. 10, 13].

Away from the ridge crest, the lesser heat flow still dictated that the 500° C isotherm lie some 10-20 km below the Moho.

We can only imagine the flash of insight that must have occurred next. If the oceanic crust was a serpentine rind (this being the key to the whole argument), then the Moho should represent the position of the 500° C isotherm at the time of its formation. In the late 1950s, as we have seen, Hess believed that the oceanic Moho was a "fossil" isotherm predating the ridge that had blistered the crust; the current position of the 500° C isotherm, after all, seemed to lie well below the Moho. In 1960, however, he realized that there was one important setting in which the current isotherm actually met the oceanic Moho — directly beneath the crest of a mid-ocean ridge. Maybe the serpentine rind wasn't so old after all: suppose it was actively forming at the ridge crest and then being carried away by the same convection currents responsible for the median rift. The oceanic Moho would still be a "fossil" isotherm, getting progressively older with distance from the ridge crest, but now its age would be measured in millions of years instead of hundreds of millions or billions. As Hess explained in the preprint:
It would appear that the highest elevation that the 500° C isotherm can reach is approximately 5 km below the sea floor and this supplies the reason for the very uniform thickness of layer 3 [Hess, 1960b, p. 13].

The topographic elevation of the ridge, he now reasoned, was due to the warmer, less dense mantle material rising convectively beneath the ridge axis. A new mobilist synthesis was thus born, precisely because Hess had tried to reconcile his older ideas with the newly discovered heat flow at ridge crests. Although the serpentine-welt model for oceanic ridges was gone forever, the *serpentine-rind model for the oceanic crust lived on in a new guise*. Ironically, Hess had originally used logic based on the permanence of ocean basins to conclude that the oceanic crust must be serpentine and not basalt — but now his firm belief in this crustal model was a key to the discovery of sea-floor spreading.

The Early 1960s: Patching and Repair

During the preliminary stages of the Mohole project, the composition of the oceanic crust — no matter what its origin — became an issue of practical concern. Any attempt to reach the mantle would have to occur at sea, where the crust is thinnest, and drilling through 5 km of serpentine would pose different technical problems than drilling through an equivalent thickness of basalt. In early 1961, Hess lobbied hard for his crustal model with the members of the AMSOC Site Selection Panel, for which Hess was Chairman:

I believe an experimental hole to reach layer 3 sometime in the next year or two would be critical to our decision on the deep hole. If layer 3 were serpentinized peridotite and the H2O content decreased with depth, do we want to go on and drill four more kilometers until the last drop of water disappears? Personally I would be inclined to stop and drill a second site if this were the case [Harry H. Hess, written communication to AMSOC Site Selection Panel, Jan. 3, 1961; emphasis added].
This was a powerful argument, because one of the charges frequently leveled against the Mohole project was that it would turn into a costly "one-hole stunt." Here, Hess was showing his fellow panel members how intelligent planning could contribute to the ultimate success of the project. (Not to mention the fact that a couple of well-placed preliminary holes would go a long way towards testing the serpentine-rind model of the crust.)

Later that year J. Brackett Hersey of Woods Hole dredged serpentine from a fault scarp in the Puerto Rico trench. Hess's sense of vindication was clear when he reported the news to Gordon Lill, the AMSOC Committee Chairman:

You may have read in the newspaper of Hersey's very important discovery on the north slope of the Puerto Rico trench. He did manage to sample layer 3 as had seemed likely from the previous seismic refraction work. It was serpentinized peridotite as I predicted so I am no longer a minority of one in believing that the "basalt crust" of the ocean floor isn't basalt but serpentine [Harry H. Hess, written communication to Gordon Lill, July 11, 1961].

Acting as Chairman of the Site Selection Panel, Hess soon made a formal proposal to the AMSOC Committee at large:

A hole in the serpentinized peridotite in the vicinity of Mayagüez, Puerto Rico should also be drilled. If the rather similar serpentines dredged by Hersey from the north slope of the Puerto Rico trench represent layer 3, the crust, then experience in drilling this particular type of rock will be urgently needed [Report, Chairman Site Selection Panel, AMSOC, May 16, 1962; emphasis added].

Hess, of course, was very familiar with the basement rock near Mayagüez because Peter Mattson, his student, had worked there.
The "Mohito," a 1000-foot core hole near Mayagüez, was drilled in four weeks in October and November, 1962, for a cost of $35,000. Samples of the 1-7/8" diameter core were distributed to some two dozen geologists and geophysicists (including many who were not AMSOC members) for studies of the petrographic, chemical, and physical properties. Hess, for instance, did some of the mineralogical and chemical analyses. The results of this team effort were compiled by Creighton Burk, the AMSOC Scientific Officer, and released in July 1964 as National Academy of Sciences-National Research Council Publication No. 1188 (Burk, 1964). In addition to his article on mineralogy and chemistry, Hess also wrote the final synthesis for the volume (Hess, 1964).

One of the studies included in this publication was an analysis of the magnetic properties of the core by Allan Cox and Richard Doell, both of the U.S. Geological Survey, Menlo Park, along with George Thompson of Stanford University (Cox et al., 1964). They discovered that the magnetic susceptibility and intensity of remanent magnetism of the serpentinite were quite low—much too low, for instance, to account for the magnetic stripes on the sea floor if layer 3 were composed of similar material. (For a history of investigations on the sea-floor magnetic stripes, including their interpretation by Vine and Matthews in terms of sea-floor spreading, see Glen, 1982.)

In the final paragraph of his synthesis article, Hess noted that:

It is now necessary to modify in detail the general picture for the generation of oceanic crust on mid-ocean ridges as suggested by the writer (1962). The magnetic properties of the Mayagüez serpentinites suggest that a more magnetic material must be present on the ocean floor to account for the magnetic intensity of oceanic anomalies. Therefore it is proposed that "layer 2" of the oceanic crust is basalt, at least in part, but that "layer 3" is serpentinite as before. Basalt, as well as water to produce serpentinization, is released along the axis of the mid-ocean ridges [Hess, 1964, p. 173-174, emphasis added].
To understand the magnitude of this concession we must backtrack a bit. In his initial paper on sea-floor spreading (Hess, 1960b, 1962), Hess presented an oceanic crustal column that showed layer 1 as unconsolidated sediment, *layer 2 as consolidated sediment*, and layer 3 as serpentinite (Figure 8). This column in part reflected Edwin Hamilton's (1959) interpretation of layer 2, which had profoundly influenced Hess's thinking when he as still a "fixist." In this paper Hamilton had explained away the apparent lack of sediment in the deep oceans. This problem had plagued marine geologists ever since the first seismic-refraction studies had shown that layer 1 of the oceanic crust, composed of unconsolidated sediments, was a fraction of a kilometer thick. If the ocean basins were billions of years old, then layer 1 seemingly should have been *several* kilometers thick. Hamilton argued that the seismic velocities in *layer 2* of the oceanic crust could represent *consolidated* sediments instead of volcanic rocks (the latter being the standard interpretation). If Hamilton was correct, then the "anomalously thin" deep-sea sediments might not be so thin after all.

Hess's continuing, tacit acceptance of this crustal model can be seen in the following letter, written (apparently) when he was in the midst of formulating the hypothesis of sea-floor spreading:

> The most critical number two problem other than finding out what the mantle is made of — is how old is the ocean floor? Put another way — how old are the oldest sediments at any given place? What is the age of the sediments *at the base of layer 2*? Is everything in the oceans Mesozoic or younger? [Harry H. Hess, written communication to Willard Bascom, Executive Secretary, AMSOC, Sept. 29, 1960, emphasis added].

Within the context of sea-floor spreading, Hess had all the more reason to believe that layer 2 was composed of consolidated sediment. As Hess saw it, the fundamental process at the crest of a mid-ocean ridge was a hydration reaction; at best, igneous activity should be an *incidental* occurrence. This, undoubtedly, was why Hess (1964) qualified his new position by saying that layer 2 was basalt *at least in part.*
There is a postscript to the story of Hess and the problem of the magnetic stripes. Hess was scheduled to spend the spring of 1965 on sabbatical leave at Cambridge, and shortly before his February departure for England, he received the following letter from Fred Vine:

Dear Professor Hess,

I am now in my third post-graduate year and about to write-up my thesis in order to submit, I hope, by September next. I graduated here in Cambridge as a petrologist and have since been working in the Department of Geophysics on the interpretation of magnetic surveys at sea. My research supervisors have been Dr. Drummond Matthews and Dr. Maurice Hill.

I have one paper in print on magnetic anomalies over oceanic ridges [Vine and Matthews, 1963] and several others in preparation. ...

Having now a vested interest (!) in ocean-floor spreading and hence, continental drift, I am particularly anxious to continue studying the ocean floor (not necessarily "by magnetics alone") believing that it holds the answers to some of the biggest and most fascinating problems of petrology and structural geology. ...

Clearly my interests are very close to your own and I wondered if there might be any possibility of joining your department for a year or two as from October next. I hope that you will not think it strange that I should write to you when you are about to visit us but I thought you might like warning of such audacity [Fred J. Vine, written communication to Harry H. Hess, Jan. 18, 1965].

Remarkably, the "audacious" Vine presumed that Hess hadn't heard of his paper with Matthews. On the other hand, maybe it wasn't so remarkable: by now, Vine was used to negative reactions — or no reaction at all (see Glen, 1982, p. 279-280, 302-304; Frankel, 1982; Menard, 1986, p.
221-222). (Vine would go to Princeton as an instructor in September — but that is getting ahead of the story.)

J. Tuzo Wilson also chose Cambridge for his sabbatical in 1965, arriving before Hess and staying longer (see Glen, 1982, p. 302-311). Two important developments came out of this "auspicious gathering," as Glen called it. First, Wilson formulated the idea of transform faults (Wilson, 1965), apparently in February. As Hess later recalled:

> Several months sharing an office with Tuzo had rather drastic effects on both of us. His transform faults idea I accept completely [Harry H. Hess, written communication to Kenneth S. Deffeyes, Dec. 14, 1965].

Second, Vine and Wilson collaborated on a paper in which they modeled the magnetic anomalies across the Juan de Fuca Ridge, off Vancouver Island (Vine and Wilson, 1965). The magnetic data, they found, was best explained by a thin layer of basalt overlying a thick layer of serpentine, much as Hess (1964) had predicted — or was it? Vine and Wilson were quite willing to make layer 2 of the crust entirely basalt, a step Hess was loath to take. Furthermore, Vine and Wilson's decision on the composition of layer 3 was purely utilitarian: they needed it to be composed of a relatively nonmagnetic material, and Hess's published model was as good as any (Fred J. Vine, oral communication, Dec. 14, 1987). Thus, it would seem that the similarities in the crustal models of Vine and Wilson (1965) and Hess (1964) were somewhat superficial.

When Fred Vine arrived in Princeton, he continued modeling the magnetic anomalies over oceanic ridges, eventually leading to his masterful synthesis paper (Vine, 1966). Throughout this research, his pragmatic crustal model, with a basaltic layer 2, continued to serve him well. Yet Hess remained reluctant to admit what was becoming obvious. Two decades later, Vine still distinctly remembered being perplexed at Hess's continuing struggle with the composition of layer 2 and the location of the magnetic material needed to account for the anomalies (Fred J. Vine, oral communication, Dec. 14, 1987). Why couldn't Hess accept, as Vine had, the simplest explanation of the magnetic stripes — that layer 2 was composed entirely of basalt? The answer, I think, is
one to which I have already alluded. If Hess had accepted layer 2 as *exclusively* basalt, he would have been admitting that igneous activity was an *integral part* of sea-floor spreading. And thus by diminishing the role of hydration, Hess would have been taking the first step towards undermining the entire chain of logic that had led him to sea-floor spreading in the first place.

Before closing, I would like to discuss the fate of Hess's idea of "ephemeral" oceanic ridges. Soon after completing the preprint on sea-floor spreading, Hess wrote to Felix Vening Meinesz:

> The Mid Atlantic Ridge is only about 200 million years old probably. A mid Pacific ridge existed from Chile to the Marianas 100 million years ago but has now subsided (leaving a belt of guyots and atolls). A new ridge is forming from Central America southward and southwestward to below Australia. ... The ocean ridges themselves either are caused by the upperward momentum of the rising column or by the fact that it is warmer and less dense or both. When the convection stops or shifts position the ridges disappear [Harry H. Hess, written communication to Felix A. Vening Meinesz, Feb. 28, 1961].

This letter was a reasonable digest of the portions of the preprint dealing with mid-ocean ridges. First, consider the "old" ridge from Chile to the Marianas. Figure 9, taken from the preprint (Hess, 1960b, Fig. 4, p. 9), showed the outline of this sunken ridge as defined by a wide band of guyots and atolls. Looking at the western end of this band, Hess (1960b, p. 19-25) noted that the guyots of the Mid-Pacific Mountains (which had been so important in shaping his initial ideas on ephemeral ridges) occupied what would have been the northern flank of the ridge, whereas the atolls of Micronesia occupied the southern flank. The volcanoes had presumably been truncated while moving away from the ridge axis on the sea-floor conveyor belt. Hess emphasized that the current belt of guyots and atolls was probably twice as wide as the *active* ridge had been, all because of this conveyor-belt effect. We now know that the guyots and atolls in the western Pacific represent the traces of several hot spots. Hess was wrong about the direction of sea-floor movement within his band of subsidence: the actual movement was roughly *parallel* to the axis of the band, not
perpendicular to it (as hypothesized by Hess). In other words, the "old" ridge from Chile to the Marianas was illusory.

Finally, what about the "new" ridge from Central America to below Australia (the East Pacific Rise)? Figure 10, taken from the preprint (Hess, 1960b, Fig. 10, p. 24), showed the outline of the East Pacific Rise. The most intriguing thing about this illustration was the caption:

Approximate outline of East Pacific Rise which possibly represents a very young oceanic ridge so young that it has not yet developed a median rift zone and pre-Rise sediments still cap most of its crest [emphasis added].

Hess obviously believed that the East Pacific Rise was on the verge of spreading but hadn't really begun. This was a very curious statement, considering that heat-flow measurements from the East Pacific Rise had been the catalyst for Hess's spreading hypothesis. Nevertheless, he pursued this theme at the Colston Symposium (University of Bristol) in 1965:

Here, apparently, is the old Pacific floor on top of the crest and not a newly-formed sequence such as on the Mid-Atlantic Ridge. The presence of the sedimentary layer is particularly indicative of this. ...I would therefore guess that the East Pacific Rise is less than a million years old. It is not only young but almost in a pre-natal stage. In another million years it might progress to a profile such as has been observed on the Mid-Atlantic Ridge; this will then be its state for perhaps several hundred million years before it dies and the ridge disappears [Hess, 1965, p. 329].

In mid 1965, of course, there weren't any decent magnetic profiles in this part of the Pacific to indicate otherwise. By the end of the year, Hess still didn't have any reason to change his mind, as he informed Kenneth Deffeyes, a former Princeton graduate student:
The segment of the East Pacific Rise south of the equator does not have a central rift valley and sediments go right over the top. *I do not have any magnetic data for it.* It looks as I said in the Colston paper "about to be born" [Harry H. Hess, written communication to Kenneth S. Deffeyes, Dec. 27, 1965, emphasis added].

Hess didn't know that the critical magnetic data had just been collected by Walter Pitman, a graduate student at Lamont, on the cruise of the *Eltanin* in September and November. In February 1966, Fred Vine, who was still at Princeton, saw the famous *Eltanin*-19 profile at Lamont and subsequently obtained a copy from James Heirtzler, Pitman's advisor (Glen, 1982, p. 332-337). With some justification, the "revolution" in the earth sciences can be traced to the public unveiling of this profile at the meeting of the American Geophysical Union in April 1966, and subsequent published interpretations by Pitman and Heirtzler (1966) and Vine (1966). *Eltanin*-19 showed that the magnetic anomalies over the southern East Pacific Rise were perfectly symmetrical about the ridge crest for at least 500 km to either side. Clearly, the East Pacific Rise had been actively spreading for some time and wasn't "about to be born," as Hess believed. In the fall Hess wrote to von Herzen, who was now at Woods Hole:

> I am not so sure anymore that the East Pacific Rise is so very young. Perhaps it is, but Fred Vine's magnetic anomaly data suggest 80 million years old so I am on the fence about this [Harry H. Hess, written communication to Richard P. von Herzen, Nov. 9, 1966].

For some reason, Hess had remained "on the fence" six months after the AGU meeting. In his synthesis paper, Vine (1966) showed that the southern East Pacific Rise was spreading about four times faster than the Mid-Atlantic Ridge near Iceland: the anomalies over the two ridges were identical in all respects except for the spacing. Soon it became clear that the morphological differences between the two ridges were a function of *spreading rate*, not age. The life cycle of oceanic ridges, as proposed by Hess and Menard, was thus shown to be invalid.
Conclusions

The introductory paragraph of Harry Hess's preprint on sea-floor spreading (1960b) is famous for the apparent disclaimer, "I shall consider this chapter an essay in geopoetry." Nearly every historian of sea-floor spreading and plate tectonics has made at least passing reference to Hess's use of this phrase — usually implying that Hess himself did not take his own hypothesis very seriously. I think that this interpretation of Hess's "geopoetry" remark is wrong. The sea-floor spreading hypothesis may have been speculative, but it was hardly fanciful. Soon after the paper was published under its new title, "History of Ocean Basins" (Hess, 1962), Hess sent reprints to his masters at ONR and noted, almost apologetically, that "the paper represents a great deal of hard work recorded on a relatively few pages" [Harry H. Hess, written communication to Arthur E. Maxwell, Head of Geophysics Branch, Office of Naval Research, Feb. 22, 1963].

Throughout his career, Hess was adamant that speculative hypotheses were the key to real scientific advance. In 1954, in his initial attempt to synthesize the post-WWII data on the structure of the crust, Hess, the fixist, had built his arguments on what he called "some facts and near-facts" and defended his approach as follows:

Without hypotheses to test and prove or disprove, exploration tends to be haphazard and ill-directed. Even completely incorrect hypotheses may be very useful in directing investigation toward critical details [Hess, 1954, p. 344].

A decade later, as a mobilist, Hess preached a virtually identical message:

To bring problems into focus and guide continued exploration, co-ordinating hypotheses are needed and necessary. Even incorrect or partly incorrect speculations serve to identify the crucial observations needed for progress. Blind, usually called objective or unprejudiced, collection of data without a framework of hypothesis by which it can be tested is wasteful and commonly unproductive, and
leads to an accumulation of an indigestible mass of data of minor significance
[Hess, 1965, p. 317-318].

Hess's approach to scientific research can be traced to Johannes H. F. Umbgrove, who in 1947 published the second edition of his book, *The Pulse of the Earth*, a grand synthesis in the tradition of Kober, Stille, and Bucher — fixist geologists who had sought regularity (even "laws") in the events of global tectonics (Dennis, 1982; Sengör, 1982). Hess held Umbgrove's treatise in such high esteem that, writing as a *mobilist* in 1960, he referred to "Umbgrove's (1947) brilliant summary," even though most of it had (in Hess's mind) failed the test of time. Hess even borrowed the "geopoetry" analogy from Umbgrove:

We may expect to find a similar "geopoetical" aspect in many a geological treatise, in addition to the normal geological prose. However, authors should always keep their theories strictly separated from descriptions and conclusions of a more rigorously documented kind [Umbgrove, 1947, p. 2].

Indeed, Umbgrove's 300-page presentation, which ranged from cosmogony and the gross structure of the earth to the details of crustal deformation, was a carefully reasoned blend of fact and hypothesis.

Returning now to the preprint on sea-floor spreading, Hess (like Umbgrove) began by establishing a basic — although somewhat speculative — cosmogonical framework within which to discuss the subsequent evolution of the ocean basins:

Dozens of assumptions and hypotheses have been introduced in the paragraphs above to establish a framework for consideration of the problem. The writer has attempted to choose reasonably between a myriad of possible alternatives. No competent reader with an ounce of imagination is likely to be willing to accept all of the choices made. But unless some such set of confining assumptions is made,
speculation spreads out into limitless variations and the resulting geopoetry has neither rhyme nor reason [Hess, 1960b, p. 5].

The last line was, again, an echo of Umbgrove, who had warned against being "carried away by an unbridled poetical inspiration." Within this framework Hess reached 19 wide-ranging conclusions, with sea-floor spreading as the centerpiece, and ended the paper as it had begun:

In this chapter the writer has attempted to invent an evolution for ocean basins starting from scratch. It is hardly likely that all of the numerous assumptions made are correct. Nevertheless it appears to be a useful framework for testing various and sundry groups of hypotheses relating to the oceans. It is hoped that the framework with necessary patching and repair may eventually form the basis for a new and sounder structure [Hess, 1960b, p. 33-34].

These are not the words of a man who thought that the hypothesis he had just proposed was nothing more than a fanciful account.

In summary, the passages quoted above reveal the essence of Hess's approach to scientific research: Without speculative hypotheses, or geopoetry, "exploration tends to be haphazard and ill-directed." On the other hand, if such speculation is not carefully structured, then "the resulting geopoetry has neither rhyme nor reason." One sentence from the opening paragraph of "The Evolution of Ocean Basins" gives the historian a clue as to why Hess, in particular, was successful with this approach: "Little of Umbgrove's (1947) brilliant summary remains pertinent when confronted by the relatively small but crucial amount of factual information collected in the intervening years" (Hess, 1960b, p.1, emphasis added). From our vantage point, Hess's perspective might seem odd, because the postwar history of marine geology has usually been described in terms of an information explosion. Evidently, Hess must have regarded most of the postwar research in marine geology as "an accumulation of an indigestible mass of data of minor significance" — from which he had been able to extract a few digestible nuggets. In other words, Hess's genius lay in his ability to reduce complex issues to their fundamental elements.
In reviewing the evolution of Hess's various hypotheses through the late 1950s, it is clear that his basic approach was to alter the current model just enough to accommodate more recent discoveries. We have seen a good example of this in his handling of von Herzen's heat-flow data from the East Pacific Rise. In 1959, Hess struggled to reconcile the serpentine-welt hypothesis for oceanic ridges with the high heat flow at ridge crests but couldn't; this was an important factor in steering him toward sea-floor spreading. However, what makes this episode interesting in the context of Hess's philosophy of " patching and repair" is the resilience of his serpentine-rind hypothesis for the oceanic crust: Hess's new mobilist solution to the problem of the origin of layer 3 was the one that did the least amount of violence to his older fixist solution.

In the midst of the uncertain reception of sea-floor spreading, Hess confided that:

> Whenever I stick my neck out I get it neatly chopped off the next week. Not that I mind particularly. I admit being wrong more times than any other geologist in the 20th Century. Of course many have been wrong more times than I but not so many acknowledge it. I take pride in my past errors and abandon them with ease and almost no embarrassment [Harry H. Hess, written communication to J. Brackett Hersey, Apr. 2, 1963].

Of course, Hess did not always abandon his "past errors" as easily as he implied in this letter. In retrospect, it appears that Hess should have realized the impact of von Herzen's data sooner than he did. Moreover, some of the early converts to sea-floor spreading who favored an igneous origin for layer 3 argued that Hess's serpentine rind had outlived its heuristic function. Menard (1986, p. 218), for example, recalls David Griggs urging Hess to "stop beating a dead horse."

In my introduction I noted that sea-floor spreading brought together three of Hess's long-standing research interests: island arcs, ocean-basin topography (especially ridges), and the nature of the oceanic crust. From the heuristic standpoint, however, we have seen that one important key to the spreading hypothesis was the invalidation of Hess's previous work on ridges. Even more curious:
in his preprint Hess (1960b) barely mentioned the role of island arcs in sea-floor spreading ("the jaw-crusher of the descending limb," p. 33) and referenced none of his previous work on the subject, primarily in the Caribbean (e.g., Hess, 1932, 1937, 1938, 1939, 1950, 1960a; Hess and Maxwell, 1953). Moreover, Hess's first illustration of an island arc in the context of sea-floor spreading (Figure 3) was already obsolete when it appeared in 1963 — much to the chagrin of his own coauthor, Robert L. Fisher of Scripps (oral communication, July 9, 1993). Fisher had numerous seismic profiles contradicting Hess's vertically plunging crustal structure.

What can we conclude from Hess's cursory treatment of island arcs in his new sea-floor spreading model? Despite their importance as disposal sites for the crust generated at oceanic ridges, island arcs were not integral to the logic of Hess's discovery.

In the end, therefore, we are brought back to Hess's one unique, albeit unpopular, contribution: the origin of layer 3 as a hydration rind on the mantle. Hess stubbornly clung to this idea through his conversion from fixist to mobilist in 1959-1960. In the coming decade, as his colleagues began accepting virtually every other aspect of the sea-floor spreading model, Hess remained a "minority of one" on the composition of layer 3. For Hess, abandoning the serpentine rind would have been tantamount to rejecting the entire chain of logic that had led him to sea-floor spreading in the first place. This, I think, is the primary historical lesson to be learned here.

Acknowledgments

This paper is modified from a portion of my dissertation at the University of California, Santa Cruz; I thank the members of my committee, Léo Laporte, William Glen, and James Gill, for their encouragement and helpful suggestions.

Several people provided me with valuable insights into the life and work of Harry Hess: Alfred Fischer, Eldridge Moores, Donald Wise, Carl Bowin, Fred Vine, Jason Morgan, John Dickey, Ronald Oxburgh, Robert Garrison, John Prucha, Thomas Donnelly, Robert Fisher, Joshua Tracey, John Christie, and Bela Csejtey, Jr. I thank them all for their generosity. I also thank the staff at
the Firestone Library, Princeton University, for their assistance during my examination of the Harry H. Hess Collection.
References Cited


Frankel, H., 1979, Why drift theory was accepted with the confirmation of Harry Hess's concept of sea-floor spreading, in Schneer, C.J. [ed.], *Two Hundred Years of Geology in America*, University Press of New England, Hanover, New Hampshire, p. 337-353.


Glen, W., and Frankel, H., 1988, The jubilee of plate tectonics [meeting report], Eos, Transactions, American Geophysical Union, v. 69, no. 19, p. 583-585.


Hess, H.H., 1932, Interpretation of gravity-anomalies and sounding-profiles obtained in the West Indies by the International Expedition to the West Indies in 1932, Transactions, American Geophysical Union, v. 13, p. 26-33.


Appendix: The Harry H. Hess Collection at Princeton University

The Harry H. Hess Collection is located in the Princeton University Library, Department of Rare Books and Special Collections (main office: Firestone Library). The Hess Collection is largely unprocessed and unsorted. When Harry Hess was alive, "he had a piling system, not a filing system," according to a former student (Donald U. Wise, oral communication, September 15, 1986); when he died suddenly in 1969, the contents of his office at Princeton were simply boxed up as necessary and shipped to the library. Hence, the library cataloguing of the Hess Collection is rudimentary. For more details concerning the organization of the collection as of 1987, see Allwardt (1990).
Figure 1 — Hess's cross section of a spreading mid-ocean ridge. Reproduced from Hess, 1960b, Fig. 6, p. 12.

Figure 2 — Hess's diagram of the sea-floor "conveyor belt," with the crust and mantle "bolted together." Reproduced from Hess, 1965, Fig. 123, p. 324.

Figure 3 — Hypothetical cross section of an island arc and trench. Reproduced from Fisher and Hess, 1963, Fig. 9, p. 430.

Figure 4 — Hess's conception of the continental crust and oceanic crust in the early 1950s. Reproduced from Hess, 1954, Fig. 8, p. 342.

Figure 5 — Hess's models in the early 1950s for three types of oceanic "ridge." Reproduced from Hess, 1954, Fig. 10, p. 345.

Figure 6 — Hess's serpentine-welt hypothesis for the origin of mid-ocean ridges. Reproduced from Hess, 1955a, Fig. 6, p. 404.

Figure 7 — Hess's conception of the continental crust and oceanic crust in the mid 1950s. Reproduced from Hess, 1955a, Fig. 5, p. 400.

Figure 8 — Hess's conception of the continental crust and oceanic crust in the early 1960s. Reproduced from Hess, 1960b, Fig. 2, p. 6.

Figure 9 — Hess's map of the supposed location of a "dead" oceanic ridge in the mid Pacific (later called the Darwin Rise). Reproduced from Hess, 1960b, Fig. 4, p. 9.

Figure 10 — Hess's outline of the East Pacific Rise, which he believed was "about to be born." Reproduced from Hess, 1960b, Fig. 10, p. 24.
Diagram to portray highest elevation which 500°C isotherm can reach over the rising limb of a mantle convection cell and expulsion of water from mantle which produces serpentinization above the 500°C isotherm.
Figure 123. The crust and mantle move laterally away from the ridge-axis, both moving at the same velocity so there is no viscous drag effect. The trailing edges of continents are not deformed. The crust and mantle may be considered effectively to be bolted together. The continents move passively until their leading edges arrive at the site of the downward current.
Fig. 9. Supposed structure, with typical seismic velocities ($V_p$ in km/sec), for a hypothetical trench-island arc association.
Figure 8. Diagram to show typical continental and oceanic sectors of the earth's crust.
Figure 10. Diagrams to illustrate working hypotheses on origin of oceanic ridges.
Figure 6.—Consequences of the hypothesis of sub-Mohorovitic serpentinization by water rising from the mantle, deserpentinization by rising isotherms, and the consequent effects on the submarine topography.
Figure 5.—Crustal columns deduced from seismic evidence, petrological inference, and the assumption of isostatic equilibrium.
Figure 2
Balance of oceanic and continental crustal columns
Figure 4

The former location of a Mid Pacific ridge
Figure 10

Approximate outline of East Pacific Rise which possibly represents a very young oceanic ridge so young that it has not yet developed a median rift zone and pre-Rise sediments still cap most of its crest.