EVOLUTION OF THE TECTOGENE
CONCEPT, 1930-1965

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ABSTRACT

The tectogene, or crustal downbuckle, was proposed in the early 1930s by F. A. Vening Meinesz to explain the unexpected belts of negative gravity anomalies in island arcs. He attributed the isostatic imbalance to a deep sialic root resulting from the action of subcrustal convection currents. Vening Meinesz's model was initially corroborated experimentally by P. H. Kuenen, but additional experiments by D. T. Griggs and geological analysis by H. H. Hess in the late 1930s led to substantial revision in detail. As modified, the tectogene provided a plausible model for the evolution of island arcs into alpine mountain belts for another two decades. Additional revisions became necessary in the early 1950s to accommodate the unexpected absence of sialic crust in the Caribbean and the marginal seas of the western Pacific.

By 1960 the cherished analogy between island arcs and alpine mountain belts had collapsed under the weight of the detailed field investigations by Hess and his students in the Caribbean region. Hess then incorporated a highly modified form of the tectogene into his sea-floor spreading hypothesis. Ironically, this final incarnation of the concept preserved some of the weaker aspects of the 1930s original, such as the ad hoc explanation for the regular geometry of island arcs.

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INTRODUCTION

In November 1937, at a symposium devoted to the "Geophysical Exploration of the Ocean Bottom," Harry H. Hess (1906-1969), the young American geologist who would later propose the sea-floor spreading hypothesis, stated:

> Meinesz's discovery of huge negative [gravity] anomalies in the vicinity of island arcs is probably the most important contribution to knowledge of the nature of mountain building made in this century. ... Instead of pure speculation as to what happens in depth during deformation, certain concrete facts may now be substituted which definitely limit and control speculation to great advantage.[2]

The symposium had been organized by the American Geophysical Union and was held at the American Philosophical Society in Philadelphia, with Richard M. Field (1885-1961), Hess's mentor at Princeton University, as chairman. In the early 1930s Field had been instrumental in establishing cooperative research between American universities and the Federal government in the developing field of marine geophysics.[3] Field recognized the importance of an integrated research program employing several promising new exploration methods such as shipboard seismic refraction and gravity surveying by submarine. The latter technique, of special interest to Hess, had been pioneered by Felix A. Vening Meinesz (1887-1966),[4] the Dutch geophysicist, in the early 1920s.

By the time of the symposium in Philadelphia, Vening Meinesz's gravimetric apparatus had been utilized with success on twenty-one cruises involving submarines of the Dutch, United States, Japanese and Russian navies, among others.[5] Vening Meinesz himself had participated in about half of these expeditions, with a variety of Dutch and American collaborators (including Hess), and he was almost singlehandedly responsible for the startling data to which Hess referred above. The narrow, curving gravity anomalies that Vening Meinesz had detected along the seaward margins of active island arcs represented a significant departure from gravitational equilibrium, as defined by the principle of isostasy.[6] After decades of unsubstantiated speculation about the causes of mountain building, this new and unexpected source of data seemingly offered the first reliable indication of the forces actually responsible for crustal deformation on a regional scale.

Briefly, the gravity anomalies were explained in terms of a profound downward buckling of the earth's crust, called a "tectogene." The tectogene remained a useful organizing principle for understanding the origin of island arcs in particular and the evolution of mountain belts in general for nearly three decades. So successful were the applications of this concept that Hess, speaking in 1956 at a jubilee honoring Vening Meinesz in his homeland, simply repeated his pronouncement in 1937 concerning the discovery of the gravity anomalies and concluded, "Twenty years later I feel satisfied with this statement."[7] Hess continued, however, "that now a second great discovery of the 20th century must be added to the above," namely, the surprisingly thin, basaltic crust beneath the oceans that had been detected by shipboard seismic refraction following World War II.

With a considerable degree of prescience Hess also noted at the jubilee that "[t]he interaction of the two great discoveries upon each other has not yet been fully considered," and herein lies one of the central themes of my historical study. Beginning with the origin of the tectogene as an explanation of the negative gravity anomalies near island arcs, I will trace its evolution into a general theory of mountain building through the efforts of Vening Meinesz, Hess, and others. The tectogene provides a revealing case history of the interaction between geologists and geophysicists over an extended period of time, showing how conflicts can sometimes develop and are eventually resolved when the disparate methods of these two groups are brought to bear on the same problem. Finally, although the tectogene concept was never universally accepted, it proved surprisingly resilient during the initial stages of the revolution leading to plate tectonics.

Harry Hess will be the focal point of this history for a variety of reasons. First, this approach provides a sense of continuity through a period of profound upheaval in the earth sciences, in which Hess was a recognized leader with a rare ability to distinguish the essential from the incidental. Second, it will illustrate how, in the hands of one long-term proponent, the tectogene seemingly outlived its utility as an organizing principle -- but in the end actually provided a key element for a far more powerful concept, sea-floor spreading. Third, Hess, as a geologist, sensed the importance of visual representation in the earth sciences and illustrated his papers profusely, unlike many of his geophysical colleagues.

VENING MEINESZ, GRAVITY ANOMALIES AND THE INITIAL SYNTHESIS, 1930-1937

Felix A. Vening Meinesz is famous for having adapted the pendulum gravimeter for use on submarines and then discovering the huge negative gravity anomalies associated with island arcs. Working for the Netherlands Geodetic Commission, he studied these features in greatest detail in the Netherlands East Indies in collaboration with his countrymen, Johannes H. F. Umbgrove and Philip H. Kuenen. He also initiated similar investigations in the West Indies,[8] which were continued by several Americans including Hess and W. Maurice Ewing (1906-1974). For these and related contributions, Vening Meinesz has been deservedly lauded.[9]

Vening Meinesz made his first expedition by submarine to the Netherlands East Indies in 1923 and began mapping a narrow belt of gravity anomalies seaward of the volcanic arc in 1926. As more data was gathered it became apparent that the anomalies were not a function of topography: at different points along its length the belt might coincide with a deep-sea trench, submarine ridge, or string of nonvolcanic islands. Thus, some sort of subsurface disturbance was indicated. By 1930 Vening Meinesz had participated in additional submarine cruises suggesting analogous patterns of anomalies in the East and West Indies (Figure 1),[10] and he was ready to publish a "Tentative Interpretation of the Provisional Results."[11] Vening Meinesz subsequently appeared before the Royal Geographical Society in November 1930, to speak on "Gravity Anomalies in the East Indian Archipelago."[12] In this talk, he attributed the linear negative anomalies, which were typically in the range of -100 mgal to -250 mgal, to a mass deficiency at depth caused by the downward folding of crustal material.[13] Normal gravity is 980,000 mgal, so the anomalies in question represented only one part in 4,000 to 10,000. However, the width of the anomalies was typically on the order of only 100 kilometers, indicating to Vening Meinesz that the depth of the gravitational disturbance was quite shallow, within perhaps fifty kilometers of the surface. In this context the anomalies actually indicated a mass deficiency in the range of one part in ten to twenty, which was certainly significant.

In 1930 Vening Meinesz was unwilling to speculate much beyond the simple mathematical deductions that followed directly from the data:

The hypothesis has not been investigated thoroughly enough to give a clear opinion regarding the way in which the main phenomenon, the downward folding, takes place. ... We will not further speculate on this question and we will temporarily at least retain the term "folding," leaving it undecided in which way the downward disappearance of the crust along the fold-line takes place.[14]

Vening Meinesz continued his field investigations by submarine during the 1930s. In 1934 he and his Dutch colleagues, Umbgrove and Kuenen, published a landmark, book-length report entitled Gravity Expeditions at Sea 1923-1932. Vol. II.[15] At last, Vening Meinesz was ready "to give a clear opinion" by postulating that the crust in the East Indies had buckled downward elastically in response to lateral compression.[16] He envisioned a two-stage process beginning with compression across a zone several hundreds of kilometers wide, creating a series of gentle waves in the presumably rigid crust. Later one of the downward waves would buckle and collapse inward, creating a narrow crustal root with vertical limbs projecting into the denser substratum (or mantle) below. The density contrast between this root and the material it had displaced was responsible for the observed gravity anomalies.

Although Vening Meinesz acknowledged that crustal compression on the scale indicated could be attributed to thermal contraction of the globe, he favored subcrustal convection currents as the cause. However, one aspect of his hypothesis set it apart from the more familiar convection models of Arthur Holmes, David Griggs, and others, who all assumed that the downbuckle and gravity anomalies marked the site of descending currents: Vening Meinesz's crustal buckling was loosely coupled to subcrustal convection. The viscous drag exerted on the base of the crust created a zone of compression several hundreds of kilometers wide, within which a much narrower downbuckle would eventually form. Mechanically speaking, however, the exact site of failure was ultimately controlled by pre-existing weaknesses in the crust, or perhaps localized geosynclinal sedimentation.

Vening Meinesz's surmise of a broad zone of compression soon received additional support from Kuenen, who conducted a series of experiments with scaled models that behaved just as the buckling hypothesis predicted.[17] In Kuenen's experimental apparatus, basically an oversized aquarium with a plunger at one end, the "rigid" crust was represented by varying mixtures of paraffin, vaseline, and oil. The denser, viscous substratum upon which the crust floated was represented by water. Kuenen conducted his experiments without the benefit of a rigorous theory of scale modeling, however, and chose his materials intuitively. By trial and error he adjusted his crustal "recipe" until two requirements were met: floating in the tank-like apparatus, the model crust had to be strong enough to transmit lateral stress; but deprived of support from the substratum, the crust had to be weak enough to collapse under its own weight.

Kuenen's experiments provided the first clear visual representation of Vening Meinesz's crustal downbuckles, or "tectogenes" as Kuenen called them,[18] and in the years to come, photographs and sketches of Kuenen's apparatus would be reproduced again and again.[19] Hess's sketch of Kuenen's simplest experiment is shown in Figure 2 and the accompanying description echoes Vening Meinesz's hypothesis:

> The "crust" first warps into a series of gentle regular anticlines and synclines. With further compression <u>one syncline buckles</u> and will continue to shove downwards so long as the compression is continued. Thus the results [of Kuenen's experiment] <u>duplicate exactly</u> the postulates of [Vening] Meinesz's hypothesis.[20]

Following Vening Meinesz's suggestion that island arcs might be an early stage in the evolution of alpine mountain belts,[21] Kuenen also performed more sophisticated experiments in which he covered the "crust" with layers of weaker sediments; when a downbuckle developed, the "sediments" were first drawn into the core and then squeezed out to form alpine-like structures.[22] The crustal downbuckle, originally postulated simply to explain the gravity anomalies in island arcs, was thus transformed into a general theory of mountain building that would survive, with modifications, until the early 1960s. Hess's illustrations of the initial synthesis as of about 1937 are especially instructive (Figure 3).[23] Note the vertical symmetry of the tectogene and the "two-sided" character of the mountain belt formed above it. Hess also depicts a granitic layer of near-normal continental thickness, reflecting the prevalent view at the time of the crustal structure beneath marginal seas (oceanic areas enclosed by island arcs), if not the true ocean basins themselves.

Modifications to the original synthesis would arise from three sources, each of which will be reviewed in detail below: 1) geological studies documenting the structural evolution of island arcs and mountain belts; 2) refined experimental modeling of the mechanics of downbuckling and tectogene formation; and 3) geophysical studies revealing the uniform crustal structure beneath all oceanic areas, including marginal seas.

GEOLOGICAL PERSPECTIVES ON THE INITIAL SYNTHESIS: UMBGROVE AND HESS

Even as Vening Meinesz and his Dutch colleagues were writing their synthesis volume in 1934, they recognized potentially troublesome geological problems with the buckling hypothesis. On purely mechanical grounds Vening Meinesz realized that a tectogene must be reasonably short-lived in a geological time frame, say on the order of one million years, simply because lateral compression in the rigid crust would be unable to maintain the deep, buoyant root once radioactive heating had weakened its limbs and hinges. The initial geological findings on the evolution of island arcs were not encouraging, however, as noted in the 1934 report. In the chapter entitled "The Relation between Geology and Gravity Field in the East Indian Archipelago," Umbgrove compiled data on Tertiary deformations in the arc to gain a sense of the age of the downbuckle.[24] (His study was limited to the Tertiary in part because the pre-Tertiary geology was not well known.) Almost invariably, the dominant episode of folding and thrusting in the nonvolcanic islands overlying the belt of gravity anomalies occurred in the middle Miocene, on the order of ten million years ago.

In another chapter, Vening Meinesz summarized the geophysical implications of the geological data:

> The coincidence of all these foldings in the same period is of course satisfactory with regard to our hypothesis, but it is remarkable that the phenomenon has taken place so long ago. It appears unlikely that a root formed in the Miocene would still bring about the narrow belt of anomalies that has been found.[25]

An elastic downbuckle as old as the Miocene, he reasoned, should have heated up, lost its strength and spread laterally, with the gravity anomaly becoming progressively broader and less intense.[26] Consequently, Vening Meinesz was forced to postulate a younger tectonic rejuvenation, perhaps Pleistocene in age, to account for the observed characteristics of the gravity field. This compromise, however, left him in the uncomfortable position of disassociating the crustal downbuckle from the geophysical data used to infer its existence in the first place. His problem would soon become even more acute, thanks to Harry Hess.

Hess had been a graduate student at Princeton University, nearing the completion of his dissertation on an altered peridotite intrusion in Virginia,[27] when Field arranged for him to accompany Vening Meinesz in 1932 on the Dutch geophysicist's second gravity survey of the West Indies on an American submarine, the S-48.[28] Hess joined the Princeton faculty in 1934, and with Field's continuing support he collaborated with Maurice Ewing on the gravity cruise of the U.S.S. Barracuda in 1936-1937.[29]

Given his background, Hess was well acquainted with the geological literature documenting the unique spatial relationship between ultramatic intrusions and gravity anomalies in island arcs. Speaking in 1937 before the American Geophysical Union in Washington, D.C. and the International Geological Congress in Moscow, Hess noted that most island arcs and alpine mountain systems contained two belts of serpentinized peridotite, one on each side of the structural axis. The serpentinite, he suggested, had been squeezed up along the limbs of the downbuckle as the crust pushed into the substratum:

> The structural relations of serpentines to island arcs and present mountain systems are the same. Present mountain systems probably went through an island arc stage, but are now uplifted compared to their former positions. ... The downbuckle presents the axis and most important structure of a mountain system. Inasmuch as the serpentine belts are intimately associated with the downbuckle and are of the

> same age as the downbuckle, they serve as a valuable guide to its former location, even after it has disappeared. ... It would appear that many of the Earth's mountain systems have been dated as younger than they actually are, if the formation of the downbuckle is to be considered the major diastrophic act in mountain formation, as the writer believes it should be. This error in dating has come about as a result of the more striking effects of the younger secondary deformations, and because these younger movements probably obscure the slightly older ones.[30]

Hess argued that the Netherlands East Indies represented a case in point:

> Both Umbgrove and Kuenen have considered that the negative strip [of gravity anomalies] was formed in Miocene time, based on the narrow zone of intensely deformed Miocene and older rocks on islands located on the negative strip. The serpentinite intrusions are, however, latest Cretaceous or early Eocene in age, and so probably is the formation of the negative strip. The intense local Miocene deformation is merely the "jaw-crusher" effect accompanying a lesser deformation at that time. How such a down-buckle can be maintained for so many millions of years without disappearing remains an unsolved question. Though Vening Meinesz and other geophysicists are loath to accept this conclusion, it seems inevitable to the geologist.[31]

In other words, the geological problem facing Vening Meinesz and his Dutch colleagues was actually much worse than they had originally thought. How could horizontal compression have sustained an elastic downbuckle since the Eocene or late Cretaceous, more than fifty million years ago? The apparent resolution of this conflict came within the next couple of years, at the hands of the American geophysicist, David T. Griggs.

GRIGGS AND THE REVISED GEOPHYSICAL MODEL OF THE TECTOGENE

David T. Griggs (1911-1974) had a long and distinguished career as an experimental geophysicist, beginning at Harvard University and ending at UCLA. His initial laboratory experiments in the 1930s on the creep of rocks at high temperatures and pressures demonstrated for the first time the plausibility of solid-state convection in the earth's mantle.[32] During this period Griggs also conducted scale-model experiments having a direct bearing on the mechanics of tectogene formation.

Griggs had a distinct advantage over Kuenen, his predecessor in such experiments, because shortly after Kuenen completed his work M. King Hubbert published the first rigorous theory of scale modeling for the geological sciences.[33] In the introduction to his article Hubbert briefly reviewed the literature from the preceding half century and cited nearly a dozen applications of laboratory modeling to problems in structural geology, including Kuenen's recent experiments. Hubbert noted that a common pitfall in these previous efforts had been the failure to appreciate how the element of time entered into the design of scale models. Many of the parameters describing strain response were clearly time dependent, viscosity being a good example, so that the choice of materials for an experiment could affect the validity of the results in unexpected ways. Although Hubbert noted that Kuenen had been "somewhat more nearly correct" in his choice of materials than many other modelers,[34] he left the details to Griggs, who soon wrote a devastating critique of Kuenen's experiments:

> In this experiment of Kuenen's the strengths of the crust and substratum were reproduced approximately to scale, and for the first time in experimental geology the geometrical conditions were favorable to the production of a Tectogene. One important factor was neglected, however -- dynamical similarity. Kuenen used water for his substratum, which because of its extremely low viscosity did not provide sufficient viscous resistance to the over-riding crust to duplicate conditions in the earth.[35]

As Griggs showed with dimensional analysis, the only way Kuenen could have compensated for this choice of materials would have been to conduct experiments lasting only 1/300 second, which would have introduced inertial forces violating the condition of dynamical similarity to the earth. This, however, was not the end of Kuenen's troubles:

It is interesting that when the viscosity of the substratum is chosen for dynamical similarity, the model does not behave in the same way as that of Kuenen. When the crust is compressed by a moving plunger in the same manner as in his experiments, it shows no tendency to develop a downfold, but instead the compression is taken up by thickening of the crust immediately in front of the advancing plunger. The viscous drag of the substratum seems sufficient to prevent the transmission of compressive stresses for long distances through the over-riding crust, and causes local thickening of the crust instead.[36]

This critique was directed not only at Kuenen's experimental apparatus but also the entire conceptual foundation laid by Vening Meinesz. Producing tectogenes by lateral compression in a rigid crust no longer seemed possible, and Griggs devised an alternate scale model with this in mind. Griggs's model was driven by two rotating drums that simulated converging and descending convection currents in the substratum, and the viscous drag from these currents in turn pulled the plastic crust inward and downward to form a tectogene (Figure 4).[37] Griggs used very viscous liquids such as glycerine to represent the substratum and heavy oil mixed with sawdust or sand to represent the plastic crust.

Based on his experimental results Griggs proposed a theory of mountain building tied to cyclic convection in the mantle, with a handful of large convection cells extending from the base of the crust to the core-mantle interface.[38] In many ways his conceptual model recalled the one proposed by Arthur Holmes a decade earlier,[39] except that Griggs placed more emphasis on the rheological aspects and the intermittent nature of the currents. Griggs also revived Holmes's suggestion that downward drag by convection currents explained the gravity anomalies near island arcs,[40] an idea that Vening Meinesz rejected initially and would continue to resist in the coming decades.[41]

Prior to publishing his theory in September, 1939, Griggs presented a talk at the annual meeting of the Geological Society of America in December, 1938, complete with a film of his scale model in action.[42] Griggs's presentation was attended by some of the leading figures in the American geological establishment, whose conservative views tended toward the traditional theory of mountain building by thermal contraction. Harry Hess was also there, however, as chairman of the session, and decades later Griggs clearly recalled what ensued at the conclusion of his talk:

> Harry Hess presided, and endeavored to get favorable discussion of these then controversial ideas, but circumstances prevented him. Andy Lawson, sitting in the front row got up and squeaked, "I may be gullible. I may be gullible! But I'm not gullible enough to swallow this poppycock." After his long tirade, before Harry could do anything, Bailey Willis who was sitting in the second row got up, turned to face the audience and said, "All you here

today bear witness -- for the first time in twenty years, I find myself in complete agreement with Andy Lawson."[43]

Griggs was evidently undaunted and, in Hess, had found a natural ally. The following May, as Griggs began organizing his material for publication, he wrote to Hess with an offer to collaborate:

> I surely was glad to have the opportunity to see you at the meetings and only hope that the fates will permit longer séances in the near future. I am putting together my ideas for publication of this convection current theory as a possible mountain-building mechanism. If you could join in with an article to follow on possible geological interpretations, I should be very glad, but if you decide that you want to get more data before joining me in the big swim, then I should praise your discretion.[44]

Hess replied immediately, and from his letter it was clear that he regarded Griggs's work as the breakthrough explaining the longevity of island arcs, tectogenes, and gravity anomalies:

> Your convection current hypothesis may be the major factor in development of island arc structures. It is the best hypothesis to date. ... The strongest point in its favor is that it explains the maintenance of the downbuckle for a long period of time -- 50 or 100 million years -- whereas no other hypothesis yet advanced does, and the geologic evidence necessitates such a maintenance.[45]

Hess, however, raised another issue in this letter, which related to his firm belief that the paired serpentinite belts in an island arc marked the primary deformation, with the more spectacular folding and thrusting of the sedimentary deposits simply representing a secondary event:

> This brings up one part of your talk which certainly would be jumped on; namely, the part that dealt with a sequence of events in mountain building which started with a geosyncline, proceeded into buckling, and ended with isostatic uplift after the currents stopped. That, to be sure, is the sequence one would get from all the current literature, but it doesn't fit the facts in island arcs. There often is no geosyncline before buckling (in the sense of a basin with thick sediments), and geosynclines do develop after buckling on either side of the buckle it seems.[46]

In this final comment Hess attempted to convince Griggs that the standard orogenic cycle based on the geosyncline concept of James Hall and James Dwight Dana[47] was all but dead. The assurance Hess displayed here seems somewhat curious in retrospect, because when he had previously attempted to buck the establishment on this point in 1937 at the International Geological Congress in Moscow, he was not well received.[48] Perhaps Hess was hoping that he would have more success with Griggs as an ally. Griggs, however, published his paper with the standard orogenic cycle as a cornerstone of his theory.[49]

In the 1960s, of course, Hess would propose a far more radical concept, sea-floor spreading, but by then he was chairman of the Princeton geology department and a respected member of the National Academy of Sciences. He never forgot, however, how he and Griggs had fared as young, relatively unknown scientists with wild new ideas, and he told Eldridge M. Moores, then his graduate student, that "he had learned a hard if valuable lesson -- that a young scientist must first make a reputation in an established field if he/she wants to synthesize credibly in a controversial field."[50]

POSTWAR DEVELOPMENTS:
THE CARIBBEAN RESEARCH PROJECT AND SEISMIC REFRACTION AT SEA

In the late 1930s Hess joined the Naval Reserve to facilitate his access to Navy submarines for gravity surveying. With the coming of the Second World War Hess was activated and eventually assumed command of a transport ship in the Pacific, the U.S.S. Cape Johnson. Hess kept the fathometer running twenty-four hours a day and strayed as much as possible from straight-line courses in the interest of science. In this manner he charted a good deal of submarine topography including the drowned, flat-topped volcanos in the west-central Pacific that he would later call guyots.[51] After the war Hess remained in the Naval Reserve and ultimately attained the rank of Rear Admiral.[52]

Upon returning to Princeton Hess initiated the famed Caribbean Research Project, an ambitious attempt to document the geologic evolution of the region using the tectogene as a working hypothesis. Funded in large part by the Geophysics Branch of the Office of Naval Research (ONR),[53] this project spanned parts of four decades (outliving Hess himself) and produced some three dozen Ph.D. dissertations.[54]

Hess summarized the results of the ongoing project in a series of short progress reports published at irregular intervals.[55] At the onset, Hess's working hypothesis for the region preserved one important aspect of Vening Meinesz's original synthesis, the sequential evolution of island arcs into alpine mountains:

> For this study we have chosen the Caribbean area, an island arc, with attendant large gravity anomalies, volcanism, and earthquakes. In this area we believe we can trace the transition from island arc to alpine mountain system; some portions are far more advanced in their stage of tectonic development than others. Here also a great body of geophysical and geological data are already available.[56]

Now, however, a new discovery based on seismic refraction at sea had to be accommodated:

> The interpretation of the strip of strong negative gravity anomalies in island arcs as given by Meinesz, Umbgrove, and Kuenen (1934) and more explicitly by Hess (1938) is no longer tenable in its original form. There appears to be no

granitic and little basaltic crust under the oceans to downfold and produce the anomalies.[57]

This new generalization, which Hess attributed to the painstaking shipboard studies of Ewing and his colleagues at Lamont and Russell Raitt at Scripps,[58] applied to the marginal seas enclosed by island arcs as well as the true ocean basins. Hess, however, was confident that the tectogene was still a valid model, although now the source of the anomalies would be "much shallower, smaller, but of greater density contrast."[59] Hess did not illustrate this modified tectogene in cross section, but its form can be envisioned by reference to his earlier interpretation as shown in Figure 3. In the original model a granitic layer twenty-five kilometers thick supposedly buckled downward into a basaltic layer thirty-five kilometers thick, and the gravity anomaly represented the density contrast between the granitic root (specific gravity 2.7) and the displaced basaltic material (3.0). The former sedimentary cover squeezed out of the core of the tectogene (Hess's "Alps"), while obviously important in terms of the surface expression of the deep crustal structure, actually had little bearing on the magnitude of the anomaly.

In the revised model the granitic layer was entirely absent and the basaltic layer only five kilometers thick, so to explain the negative anomaly Hess now turned to the deformed and metamorphosed sediments caught in the core of the tectogene by the "jaw-crusher" effect. This root, with a specific gravity comparable to the missing granitic layer (2.7), would undoubtedly penetrate the peridotite (3.3) lying only five kilometers below the sea floor and thus produce the necessary density contrast. Although Hess concluded that "the change from the structural point of view is not great,"[60] this revised crustal model in fact shifted the focus of "tectogene" investigations toward modern deep-sea trenches and their sedimentary fill, which could be explored profitably by a variety of marine geophysical techniques including seismic refraction.

One of the first integrated shipboard geophysical studies of a modern deep-sea trench was conducted in the early 1950s by Ewing's group from Lamont, in the vicinity of the Puerto Rico trench. Ewing and "Joe" Worzel completed topographic, seismic, and gravity profiles across the trench and developed a crustal model to explain the data (Figure 5).[61] They discovered an enormous pile of sediments in the trench, at least six kilometers thick, which could not be completely penetrated by seismic-refraction techniques (Figure 5A). Thus the exact thickness of the sedimentary fill and the crustal structure below remained unknown. However, Ewing and Worzel could make an educated guess regarding the crustal structure because the thicknesses and densities had to fit the gravity data (Figure 5C). Their resulting model (Figure 5B), which was by no means a unique solution, showed a great thickness of sediments (specific gravity 2.30) underlain by a basaltic crust (2.67) of slightly <u>less</u> than normal oceanic thickness, with no hint of a narrow, deep root. The most plausible way of explaining a topographic depression underlain by abnormally thin crust was, of course, rifting in response to crustal tension. Ewing and Worzel thus rejected the tectogene model based on crustal compression:

> The large negative gravity anomaly is attributed to a great thickness of sediments in the trench rather than to a "sialic root" due to a down-buckle of the crust under the trench, as formerly thought.[62]

The Lamont work had been funded largely by ONR, just like the Caribbean Research Project at Princeton. Ewing and Worzel's bombshell appeared in print in February 1954, and the following month an annoyed Hess responded by writing to Gordon Lill, the head of ONR's Geophysics Branch:

> I suppose you have seen the Ewing and Worzel papers on the Puerto Rican trench and surrounding area. While I can't help but praise the fine geophysical data they are obtaining, I think the interpretation which completely omits consideration of the geology is quite untenable.[63]

The following year Hess published his rebuttal, which was prefaced as follows:

> [Ewing and Worzel's] statement that the anomalies can be explained largely by the mass deficiency of the great thickness of sediment, while true in a sense, is misleading, for if the sediments were not present then the anomalies would be larger still.[64]

This quotation captured the essence of the philosophical gulf separating the Princeton and Lamont investigations. As Hess elaborated, the fundamental question was the nature of the force that could have produced the trench while maintaining gravitational disequilibrium for a prolonged period. Hess's geological field studies had indicated that the Caribbean crustal disturbance was tens of millions of years old, and here he judged the Lamont hypothesis of crustal tension to be clearly deficient. If the Puerto Rico trench had been formed by tension, then the anomaly belt should have disappeared as soon as the inward flow of mantle material at depth could compensate for the mass lost through thinning of the crust above. Such a process should have taken only a few thousand years (by analogy with the rapid isostatic rebound on the continents after the Pleistocene ice caps melted), but this had obviously not been the case in the Caribbean. Hess repeated his rebuttal in 1957, beginning with this rather pointed statement:

> It perhaps would not be worth discussing the opinions on this point published by the Lamont group, were it not for the high respect of the geologic profession ... for their past distinguished achievements. Many geologists with little facility in geophysics accept their statements at face value.[65]

Another challenge to Hess's working hypothesis of a vertically symmetrical tectogene was mounted in the mid 1950s by Russell W. Raitt and Robert L. Fisher of the Scripps Institution of Oceanography. Raitt and Fisher, along with Ronald G. Mason, shot seismic lines across the Tonga trench in the western

Pacific, where the sedimentary fill did not obscure the deep crustal structure, and they seemingly detected the oceanic crust dipping beneath the axis of the trench toward the volcanic arc.[66] This finding apparently corroborated the dipping seismic zones beneath oceanic deeps as proposed a few years earlier by Hugo Benioff.[67] The seismic transect across the Tonga trench was somewhat incomplete, however, and for the time being Hess had no difficulty reinterpreting the raw data to fit the tectogene model (Figure 6).[68] Hess was especially pleased that Raitt, Fisher and Mason had documented a thickening of the basaltic crust beneath the trench, as opposed to Ewing's postulated but unobserved thinning beneath the Puerto Rico trench.

By the late 1950s Fisher, Raitt, and others had obtained more conclusive evidence for crustal asymmetry from the Middle America and Peru-Chile trenches, but these data and interpretations were not published until 1961 and 1962, respectively.[69] Until then Hess inexplicably ignored the unpublished data or was unaware of its existence, leading to a classic instance of miscommunication between coauthors. The late Maurice N. Hill, head of Marine Geophysics at Cambridge, asked Fisher and Hess to prepare a joint manuscript on deep-sea trenches for volume 3 of The Sea, for which Hill was editor. They agreed, but once the basic division of labor was settled Fisher and Hess wrote their respective contributions on opposite coasts, with a minimum of interaction because of their busy schedules. Fisher prepared the first half, focusing on the physiography of the world's trench systems, and Hess concluded with a more speculative discussion on their origin and crustal structure.[70] As Fisher recalls, Hess finished his piece and sent it to Hill without first giving Fisher the opportunity for review, an oversight that ultimately proved embarrassing to both.[71] Fisher, of course, included in his half most of the published and unpublished seismic profiles suggesting that the oceanic crust plunged beneath the trench toward the volcanic arc.[72] Hess, on the other hand, had prepared a schematic cross section and block diagram showing a highly modified, vertically plunging tectogene.[73] Ironically, Fisher and Hess's disjointed contribution to The Sea reached the editor in May, 1961, the same month that Fisher's article on the Middle America trench appeared in print. Even though publication of the volume was ultimately delayed until 1963, Fisher and Hess were apparently unable to revise their manuscript for internal consistency.

CONCLUSION

By the late 1950s the cherished analogy between island arcs and alpine mountain belts had collapsed under the weight of the detailed field investigations by Hess and his students in the Caribbean region:

> The idea that island arcs are an early stage in alpine-type mountain building probably is invalid. Instead island arcs and alpine mountain systems represent a case of parallel

evolution. Island arcs develop in oceanic crust and alpine mountain systems develop in areas of thin continental crust along continent margins.[74]

In 1960, however, Hess would become a proponent of sea-floor spreading, in which new oceanic crust was supposedly created along oceanic ridges, transported in the manner of a conveyor belt towards the periphery of the ocean basin, and then destroyed ("subducted," in current terminology) as it plunged into the mantle beneath the trenches.[75] For Hess's purposes, a "bottomless" tectogene would be ideal for disposing of the oceanic crust, and this is exactly what he illustrated in the paper coauthored with Fisher (Figure 7).[76] Note in this drawing that Hess replaced the traditional basaltic crust with one composed of serpentinite, which instead of melting would simply dehydrate as it entered the mantle. In the late 1950s Hess had concluded for reasons unrelated to sea-floor spreading that the oceanic crust was a hydration "rind" on the mantle,[77] and some of his logic can be traced directly to the Caribbean Research Project. This point is worth a short digression. First, Peter H. Mattson, a Princeton graduate student working in southwestern Puerto Rico (near Mayagüez) in 1953-1956, discovered that serpentinized peridotite was not restricted to narrow belts as Hess had predicted but instead formed the basement rock of the entire island.[78] Then, in early 1955, an extensive seismic-refraction survey of the Caribbean (a joint effort between scientists of Woods Hole and Lamont) failed to find the Mohorovičić discontinuity beneath

Puerto Rico.[79] Could the basement of Puerto Rico be highly altered mantle material, which had poked above sea level because of its lowered density? Hess began exploring this possibility, as he later reported to Gordon Lill at ONR:

> Further investigations were made in the Caribbean area during the summer. In particular rock samples for determination of seismic velocities in the laboratory were collected. In particular the peridotites and serpentinized peridotites were obtained to look for analogs of the material below the M discontinuity. It is hoped to correlate velocities with the seismic work at sea being done by Lamont and Woods Hole.[80]

Hess realized that the primary evidence for the basaltic composition of the oceanic crust was its seismic velocity as determined by refraction studies (averaging roughly 6.7 km/sec, compared to 8.1 km/sec for the peridotite in the underlying mantle). However, since the seismic velocity of peridotite decreased as a function of serpentinization (hence the samples Hess collected for the laboratory), there was no compelling reason why the crust could not be hydrated mantle rock instead of basalt. Reviewing the data from Puerto Rico and vicinity, Hess speculated as follows:

> 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.[81]

Here, then, was a key element of his forthcoming hypothesis of sea-floor spreading.[82]

Returning now to the narrative: One problem that a vertically plunging, "bottomless" tectogene did not solve was why Benioff had detected dipping zones of earthquakes beneath trenches and volcanic arcs. In 1965 Hess devised a clever explanation, but the strain on his initial model for subduction was clearly evident (Figure 8).[83] Plate tectonics, based on the concepts of J. Tuzo Wilson and others,[84] was only a couple of years away and with its advent the tectogene would finally be laid to rest.

A final assessment of the tectogene concept was provided by Hess in the last progress report for the Caribbean Research Project before his death:

> During the early stages of the project we had a number of clear-cut hypotheses about island arcs which seemed at that time to simplify and organize geological and geophysical data concerning the West Indies island arc into a consistent pattern. One by one these hypotheses have fallen by the wayside as factual information has increased by an order of magnitude. We depended heavily on analogies between island

arcs and alpine mountain systems, but today the origin and development of alpine mountains is, if anything, more obscure than island arcs. This situation does not discourage me; it presents fascinating possibilities for reorganization of the facts and development of a new theory.[85]

The last sentence in this passage was, of course, an oblique reference to Hess's recent proposal that island arcs acted as disposal sites for the spreading sea floor (a concept familiar to many of his readers but still highly controversial). Over the course of nearly three decades, Hess's working hypothesis for the tectogene had evolved so dramatically that most of the "essential" characteristics of the original were gone. In its final, highly modified form, however, the tectogene supplied a provisional solution to the problem of subduction and thus represented an important link in the early stages of the plate-tectonic revolution.

NOTES

1. Address: Rogers E. Johnson and Associates, 1729 Seabright Avenue, Suite D, Santa Cruz, California 95062, USA. A good deal of the research for this article was conducted during the course of writing 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.

My examination of the Harry H. Hess Collection at Princeton University was made possible by Peggy Cross, of the Department of Geological and Geophysical Sciences; Earle Coleman, the University Archivist; Jean Preston, the Curator of Manuscripts at the Firestone Library; and her assistant, Ann van Arsdale.

2. Harry H. Hess, "Gravity Anomalies and Island Arc Structure with Particular Reference to the West Indies," Proc. Am. Phil. Soc., 1938, 79:71.

3. Richard M. Field, "Symposium on the Application of Geophysics to Ocean Basins and Margins: Introduction," Trans. Am. Geophys. Union, 1932, 13:11-12; idem, "Report of Committee on Geophysical and Geological Study of Oceanic Basins," Trans. Am. Geophys. Union, 1933, 14:9-16; idem, "The Importance of Geophysics to Submarine Geology," Proc. Am. Phil. Soc., 1938, 79:1-7.

4. Meinesz or Vening Meinesz? The confusion over his surname, evident in most Anglo-American literature, has an interesting explanation rooted in traditional Dutch culture:

> A double (and often fancy) family name used to be familiar in aristocratic and patrician families. Usually one name was

added to an already existing family name. The extra name had to be paid for. The name Vening was added by Felix's grandfather. It was the name of the latter's wife who died at a young age. Hence there have been only three generations in the male line carrying the name Vening Meinesz. The name now only survives in an Amsterdam street named after the former mayor of the city, and as the name of the unmarried and aged niece of Felix. It was usual, for simplicity, to abbreviate the double to a single name in daily life, not only in the U.S.A. and England, but also in Holland [Letter, Nicolaas J. Vlaar to William Glen, 1988; copy courtesy of William Glen].

5. For a complete list of these cruises, including itineraries, scientific personnel, and technical notes, see: W. Maurice Ewing, "Marine Gravimetric Methods and Surveys," Proc. Am. Phil. Soc., 1938, 79:52-56.

6. The 19th-century origins of the principle of isostasy and the competing Pratt and Airy models are reviewed by: Mott T. Greene, Geology in the Nineteenth Century (Ithaca: Cornell University Press, 1982), ch. 10.

7. Harry H. Hess, "The Vening Meinesz Negative Gravity Anomaly Belt of Island Arcs 1926-1956," Kon. Ned. Geol. Mijnb. Genoot. Verhand. Geol. Ser., 1957, 18:184.

8. For an account of the circumstances leading to Vening Meinesz's initial expedition on an American submarine, see: Naomi Oreskes, "Weighing the Earth from a Submarine: The Gravity

Measuring Cruise of the U.S.S. S-21," in Gregory A. Good, ed., The Earth, the Heavens, and the Carnegie Institution of Washington (Washington, D.C.: American Geophysical Union, 1994), pp. 53-68; idem, "Gravity Surveys in the Permanent Ocean Basin: An Instrumental Chink in a Theoretical Suit of Armor," in Proc. Fifth Int. Congr. Hist. Oceanogr. (in prep.).

9. G. J. Bruins and J.G.J. Scholte, "Felix Andries Vening Meinesz, 1887-1966," Biogr. Mem. Fellows Roy. Soc., 1967, 13:295-308; J. Veldkamp, History of Geophysical Research in the Netherlands and its Former Overseas Territories (Amsterdam: North-Holland Publishing, 1984), 139 pp.; Rachel Laudan, "Oceanography and Geophysical Theory in the First Half of the Twentieth Century: The Dutch School," in Mary Sears and Daniel Merriman, eds., Oceanography: The Past, Proc. Third Int. Congr. Hist. Oceanogr. (New York: Springer-Verlag, 1980), pp. 656-666.

10. Figure 1 reproduced from: David T. Griggs, "A Theory of Mountain-Building," Am. J. Sci., 1939, 237:615, 617, by permission of the American Journal of Science.

11. Felix A. Vening Meinesz, "Maritime Gravity Survey in the Netherlands East Indies: Tentative Interpretation of the Provisional Results," Kon. Akad. v. Wetensch. Amst., Proc. Sect. Sci., 1930, 33:566-577.

12. Felix A. Vening Meinesz, "Gravity Anomalies in the East Indian Archipelago," Geogr. J., 1931, 77:323-332.

13. Vening Meinesz reported his results as "isostatic" anomalies, taking into account not only the free-air and

topographic reductions but also a number of competing models for crustal compensation at depth (e.g., Pratt versus Airy isostasy). No matter which hypothesis of isostasy Vening Meinesz employed in his calculations, however, the results indicated such a dramatic departure from equilibrium that the possibility of methodological artifacts seemed remote.

14. Vening Meinesz, "Gravity Anomalies in the East Indian Archipelago," pp. 326-327.

15. Felix A. Vening Meinesz, Johannes H.F. Umbgrove, and Philip H. Kuenen, Gravity Expeditions at Sea 1923-1932. Vol. II (Delft: Netherlands Geodetic Commission, 1934), 208 pp.

16. Ibid., pp. 40-52, 117-125.

17. Philip H. Kuenen, "The Negative Isostatic Anomalies in the East Indies (with Experiments)," Leid. Geol. Meded., 1936-1937, 8:169-214.

18. Most English-speaking geologists had never heard of the term "tectogene" before Kuenen adopted it to describe Vening Meinesz's downbuckles. Those unfamiliar with the German origins of the term have generally, but mistakenly, credited Kuenen with its invention, much to his annoyance: Letter, Philip H. Kuenen to Robert S. Dietz, 5 August 1965, Harry H. Hess Collection, Princeton University (Cabinet III, Drawer 2, "K" file). Kuenen cited the original reference as: E. Haarmann, "Tektogenese oder Gefügebildung statt Orogenese oder Gebirgsbildung," Zeitschr. Deutsche Geol. Gesellsch., 1926, 78(3-5):105-107.

19. Hess, "Gravity Anomalies and Island Arc Structure," p. 76; idem, "Island Arcs, Gravity Anomalies and Serpentinite Intrusions. A Contribution to the Ophiolite Problem," International Geological Congress. Report of the XVII Session, 1937 (Moscow, 1939), 2:266; Griggs, "A Theory of Mountain-Building," p. 626; Felix A. Vening Meinesz, "Plastic Buckling of the Earth's Crust: The Origin of Geosynclines," in Arie Poldervaart, ed., Crust of the Earth (New York: Geological Society of America, 1955), p. 326.

20. The explanation of Figure 2 is taken from: Hess, "Island Arcs, Gravity Anomalies and Serpentinite Intrusions," p. 266, emphasis added. The figure itself had appeared previously in: Hess, "Gravity Anomalies and Island Arc Structure," p. 76; reproduced by permission of the American Philosophical Society.

21. Vening Meinesz, Umbgrove, and Kuenen, Gravity Expeditions at Sea, pp. 125-133.

22. Kuenen, "The Negative Isostatic Anomalies in the East Indies," pp. 184-188.

23. Figure 3 is reproduced from: Hess, "Gravity Anomalies and Island Arc Structure," pp. 75 and 79, by permission of the American Philosophical Society.

24. Vening Meinesz, Umbgrove, and Kuenen, Gravity Expeditions at Sea, pp. 140-162.

25. Ibid., p. 132, emphasis added.

26. Ibid., p. 127.

27. Published as: Harry H. Hess, "Hydrothermal Metamorphism of an Ultrabasic Intrusive at Schuyler, Virginia," Am. J. Sci., 1933, ser. 5, 26:377-408. The intrusion studied by Hess consisted of peridotite (the dominant rock in the earth's mantle) altered by hot fluids to serpentinite.

28. See: Harry H. Hess, "Interpretation of Gravity-Anomalies and Sounding-Profiles Obtained in the West Indies by the International Expedition to the West Indies in 1932," Trans. Am. Geophys. Union, 1932, 13:26-33.

29. See: W. Maurice Ewing, "Gravity Measurements on the U.S.S. Barracuda," Trans. Am. Geophys. Union, 1937, 18:66-69; Harry H. Hess, "Geological Interpretation of Data Collected on Cruise of U.S.S. Barracuda in the West Indies -- Preliminary Report," Trans. Am. Geophys. Union, 1937, 18:69-77. Ewing's cautious handling of the empirical data in his article contrasted sharply with Hess's bold geological speculations, providing the first glimpse of the well-known antagonism between the two that developed more fully after Ewing founded the Lamont Geological Observatory in 1949. Indeed, Bruce C. Heezen, Ewing's long-time colleague at Lamont, noted shortly before his death in 1977 that the cruise of the Barracuda was the initial source of the bad blood between Hess and Ewing (Oral communication, Bruce Heezen to William Glen, 23 August 1976).

30. Hess, "Island Arcs, Gravity Anomalies and Serpentinite Intrusions," p. 281, emphasis added. The mode of emplacement for the serpentinized peridotite was somewhat problematic: the field

relations suggested fluid flow, but having a "dry" peridotite magma actually reach the upper crust seemed out of the question because of the high melting temperature of such a composition. Hess would soon argue that a true "serpentine" magma might be possible if its water content lowered the melting point sufficiently: see Harry H. Hess, "A Primary Peridotite Magma," Am. J. Sci., 1938, ser. 5, 35:321-344.

31. Hess, "Geological Interpretation of Data Collected on Cruise of U.S.S. Barracuda," p. 76.

32. David T. Griggs, "Creep of Rocks," J. Geol., 1939, 47:225-251. Earlier proponents of mantle convection, such as Arthur Holmes (1890-1965), had tacitly assumed that a molten or "glassy" condition was a prerequisite for convective motion: see Arthur Holmes, "The Thermal History of the Earth," J. Wash. Acad. Sci., 1933, 23:169-195.

33. M. King Hubbert, "Theory of Scale Models as Applied to the Study of Geologic Structures," Bull. Geol. Soc. Am., 1937, 48:1459-1519.

34. Ibid., p. 1518.

35. Griggs, "A Theory of Mountain-Building," p. 638.

36. Ibid., p. 640, emphasis added.

37. Figure 4: Ibid., p. 642, reproduced by permission of the American Journal of Science. In the caption Griggs makes reference to Leopold Kober, the German geologist who in the 1920s had introduced the terms Orogen and Kratogen (craton) to describe the basic divisions of the continental crust.

38. Griggs, "A Theory of Mountain-Building," pp. 627-637.

39. Henry Frankel, "Arthur Holmes and Continental Drift," Brit. J. Hist. Sci., 1978, 11:130-150.

40. Holmes, "The Thermal History of the Earth," p. 192.

41. Alan O. Allwardt, "Working at Cross-Purposes: Holmes and Vening Meinesz on Convection," Eos Trans. Am. Geophys. Union, 1988, 69(41):899-906.

42. David T. Griggs, "Convection Currents and Mountain Building" [abstract], Bull. Geol. Soc. Am., 1938, 49:1884.

43. David T. Griggs, "Presentation of the Arthur L. Day Medal to David T. Griggs: Response by David Griggs," Bull. Geol. Soc. Am., 1974, 85:1343.

44. Letter, David T. Griggs to Harry H. Hess, 10 May 1939. Copy courtesy of John M. Christie, UCLA. Christie was Griggs's successor at UCLA and remains the keeper of his files.

45. Letter, Harry H. Hess to David T. Griggs, 12 May 1939. Copy courtesy of John M. Christie, UCLA. In this letter Hess also indicated that he would be receptive to a "joint effort on the manuscript," but not before the upcoming summer field season. Hess realized, however, that Griggs might "wish to get the paper out of the way immediately," and so he also offered some suggestions as to the presentation. Griggs followed the latter course and his paper appeared in print barely a month after Hess returned from the field.

46. Ibid.

47. For a discussion of the development of geosynclinal theory, see: Greene, Geology in the Nineteenth Century, ch. 5.

48. Fifteen years later Hess acknowledged the resistance he had encountered in Moscow: Harry H. Hess, "Serpentines, Orogeny, and Epeirogeny," in Arie Poldervaart, ed., Crust of the Earth (New York: Geological Society of America, 1955), p. 391.

49. Griggs, "A Theory of Mountain-Building," pp. 612, 643-646.

50. Paraphrased by Moores in: E. M. Moores and F. J. Vine, "Alpine Serpentinites, Ultramafic Magmas, and Ocean-Basin Evolution: The Ideas of H. H. Hess," Bull. Geol. Soc. Am., 1988, 100:1205. Indeed, John S. Dickey, Hess's last graduate student, suspects that Hess actually pursued his landmark petrologic study on the Stillwater igneous complex in Montana to secure a solid reputation (Oral communication, John S. Dickey to Alan O. Allwardt, 1986).

51. Harry H. Hess, "Drowned Ancient Islands of the Pacific Basin," Am. J. Sci., 1946, 244:772-791.

52. Arthur F. Buddington, "Memorial to Harry Hammond Hess, 1906-1969," Geol. Soc. Am. Mem., 1973, 1:18-26; Harold L. James, "Harry Hammond Hess, May 24, 1906 - August 25, 1969," Biogr. Mem. Nat. Acad. Sci, 1973, 43:109-128.

53. For a brief history of ONR and its commitment to funding research in the basic sciences, see: Harvey M. Sapolsky, Science and the Navy: The History of the Office of Naval Research (Princeton: Princeton University Press, 1990), 142 pp.

54. Thomas W. Donnelly, "Harry Hess and the Caribbean: An Appreciation," in T.W. Donnelly, ed., Caribbean Geophysical, Tectonic, and Petrologic Studies (Boulder, Colorado: Geological Society of America, 1971), pp. vii-x.

55. Harry H. Hess and John C. Maxwell, "Geological Reconnaissance of the Island of Margarita, Part I," Bull. Geol. Soc. Am., 1949, 60:1857-1868; Harry H. Hess, "Catastrophe in the Caribbean," ONR Res. Rev., 1950, 3(2):1-5; Harry H. Hess and John C. Maxwell, "Caribbean Research Project, Bull. Geol. Soc. Am., 1953, 64:1-6; Harry H. Hess, "Caribbean Research Project: Progress Report," Bull. Geol. Soc. Am., 1960, 71:235-240; Harry H. Hess, "Caribbean Research Project, 1965, and Bathymetric Chart," in H. H. Hess, ed., Caribbean Geological Investigations (New York: Geological Society of America, 1966), pp. 1-10. The 1953, 1960, and 1966 summaries prefaced longer articles by Hess's students and included comprehensive bibliographies of work completed and in progress.

56. Hess and Maxwell, "Caribbean Research Project," p. 3.

57. Ibid., p. 3. The publications cited by Hess in this quotation are referenced here in Notes 2 and 15.

58. In his reminiscences of this period, the late Bill Menard of Scripps noted that Hess was unable to cite much of the relevant data (especially Raitt's) because it was still unpublished. Menard concluded that "[h]ere we see the enormous advantage of the members of an invisible college during a scientific revolution. A few insiders had about five years to digest the

implications of a flood of observations before the outsiders ever saw the bare data." See: H. William Menard, The Ocean of Truth: A Personal History of Global Tectonics (Princeton: Princeton University Press, 1986), p. 111. One also has to wonder if Hess's standing in the Naval Reserve afforded him special access to unpublished research funded by ONR.

59. Hess and Maxwell, "Caribbean Research Project," p. 3.

60. Ibid., p. 3.

61. Figure 5 is reproduced from: W. Maurice Ewing and J. Lamar Worzel, "Gravity Anomalies and Structure of the West Indies, Part I," Bull. Geol. Soc. Am., 1954, 65:167, by permission of J. Lamar Worzel.

62. Ibid., p. 165. See also: J. Lamar Worzel and W. Maurice Ewing, "Gravity Anomalies and Structure of the West Indies, Part II," Bull. Geol. Soc. Am., 1954, 65:195-200.

63. Letter, Harry H. Hess to Gordon Lill, 29 March 1954, Harry H. Hess Collection, Princeton University (Cabinet II, Drawer 1, File: ONR Nonr-1858(10), Reports and Correspondence 1954-1959).

64. Harry H. Hess, "The Oceanic Crust," J. Mar. Res., 1955, 14:434.

65. Hess, "The Vening Meinesz Negative Gravity Anomaly Belt of Island Arcs," p. 186-187. Menard, Ocean of Truth, p. 129, suggests that when Hess made this statement he was already aware of new Lamont seismic data from the Puerto Rico trench casting doubt on the tension model. See Note 79, below.

66. Russell W. Raitt, Robert L. Fisher, and Ronald G. Mason, "Tonga Trench," in Arie Poldervaart, ed., *Crust of the Earth* (New York: Geological Society of America, 1955), pp. 237-254.

67. Hugo Benioff, "Seismic Evidence for the Fault Origin of Oceanic Deeps," *Bull. Geol. Soc. Am.*, 1949, 60:1837-1856.

68. Figure 6 (top): Raitt, Fisher, and Mason, "Tonga Trench," p. 253, reproduced by permission of Robert L. Fisher and Ronald G. Mason; Figure 6 (bottom): Hess, "The Oceanic Crust," p. 433.

69. Robert L. Fisher, "Middle America Trench: Topography and Structure," *Bull. Geol. Soc. Am.*, 1961, 72:703-720; Robert L. Fisher and Russell W. Raitt, "Topography and Structure of the Peru-Chile Trench," *Deep-Sea Res.*, 1962, 9:423-443.

70. Robert L. Fisher and Harry H. Hess, "Trenches," in Maurice N. Hill, ed., *The Sea, Volume 3. The Earth Beneath the Sea: History* (New York: Wiley Interscience, 1963), pp. 411-436.

71. Oral communication, Robert L. Fisher to Alan O. Allwardt, 9 July 1993.

72. Fisher and Hess, "Trenches," p. 428.

73. Ibid., pp. 430-431.

74. Hess, "Caribbean Research Project: Progress Report," p. 237; published in 1960 but manuscript submitted in July 1958.

75. Harry H. Hess, "The Evolution of Ocean Basins," unpublished technical report to ONR, December 1960, 38 pp.; Harry H. Hess, "History of Ocean Basins," in A.E.J. Engel, H. L. James and B.F. Leonard, eds., *Petrologic Studies: A Volume to Honor A. F.*

Buddington (New York: Geological Society of America, 1963), pp. 599-620.

76. Figure 7: Fisher and Hess, "Trenches," p. 430, reproduced by permission of Robert L. Fisher and John Wiley & Sons, Inc. ©1963.

77. For example, see Harry H. Hess, "The AMSOC Hole to the Earth's Mantle," Trans. Am. Geophys. Union, 1959, 40:340-345.

78. Later published as: Peter H. Mattson, "Geology of the Mayagüez Area, Puerto Rico," Bull Geol. Soc. Am., 1960, 71:319-361.

79. C.B. Officer, J.I. Ewing, R.S. Edwards, and H.R. Johnson, "Geophysical Investigations in the Eastern Caribbean: Venezuelan Basin, Antilles Island Arc, and Puerto Rico Trench," Bull. Geol. Soc. Am., 1957, 68:359-378. The manuscript for this article was submitted for publication in April 1956, and Hess was acknowledged by the authors for reviewing it.

80. Status Report, Harry H. Hess to Chief of Naval Research, Geophysics Branch, ONR, 15 November 1957, Harry H. Hess Collection, Princeton University (Cabinet II, Drawer 1, File: ONR Nonr-1858(10), Reports and Correspondence 1954-1959).

81. Hess, "Caribbean Research Project: Progress Report," p. 237.

82. The origin of the oceanic crust as a hydration rind on the mantle was one feature of Hess's hypothesis of sea-floor spreading that was never well received. Most of the early converts to sea-floor spreading preferred a basalt/gabbro crust formed by magmatic processes, but Hess refused to change his mind on this issue.

83. Figure 8 is reproduced from: Harry H. Hess, "Mid-Ocean Ridges and Tectonics of the Sea-Floor," in W. F. Whittard and R. Bradshaw, eds., Submarine Geology and Geophysics (London: Butterworths, 1965), p. 323, by permission of the Colston Research Society and the publishers, Butterworth Heinemann Ltd.©

84. See, for example: J. Tuzo Wilson, "A New Class of Faults and Their Bearing on Continental Drift," Nature, 1965, 207:343-347; Dan P. McKenzie and Robert L. Parker, "The North Pacific: An Example of Tectonics on a Sphere," Nature, 1967, 216:1276-1280.

85. Hess, "Caribbean Research Project, 1965, and Bathymetric Chart," p. 5.

FIGURE CAPTIONS

Figure 1: Gravity anomalies in the Netherlands East Indies and the West Indies, mid 1920s - late 1930s.

Figure 2: Hess's sketch of Kuenen's simplest scale-model experiment, mid 1930s.

Figure 3: Hess's interpretation of the tectogene concept, late 1930s.

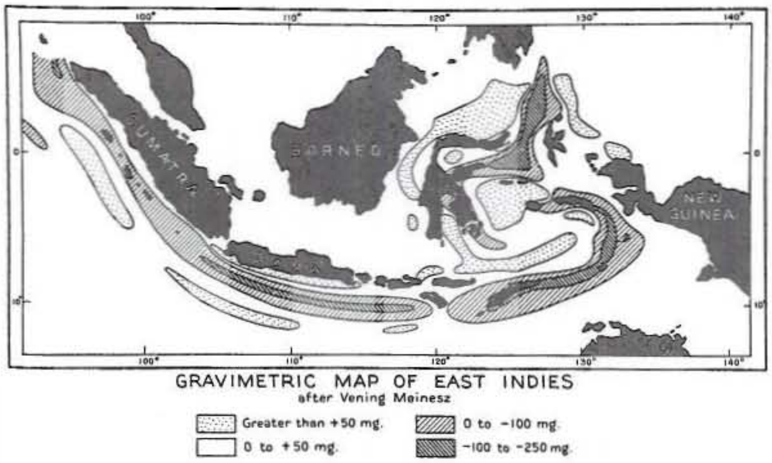
Figure 4: Griggs's scale model designed to simulate the formation of a tectogene, late 1930s.

Figure 5: The Lamont interpretation of the Puerto Rico trench, mid 1950s.

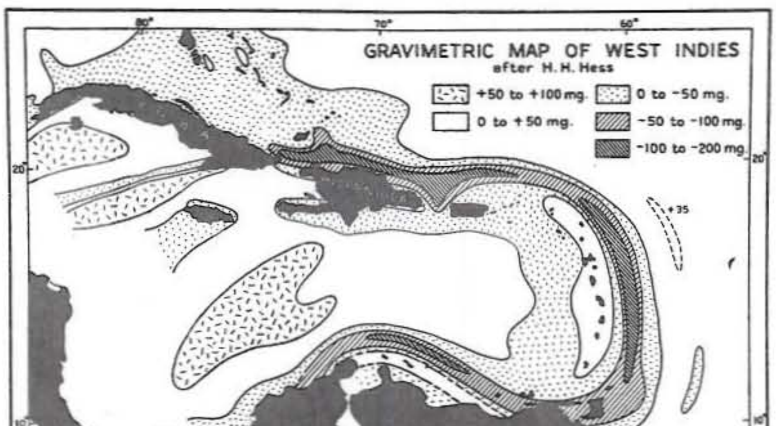
Figure 6: (Top) The Scripps interpretation of the Tonga trench, mid 1950s. (Bottom) Hess's reinterpretation of the Tonga trench based on the Scripps raw data, mid 1950s.

Figure 7: Hess's highly modified, "bottomless" tectogene, early 1960s.

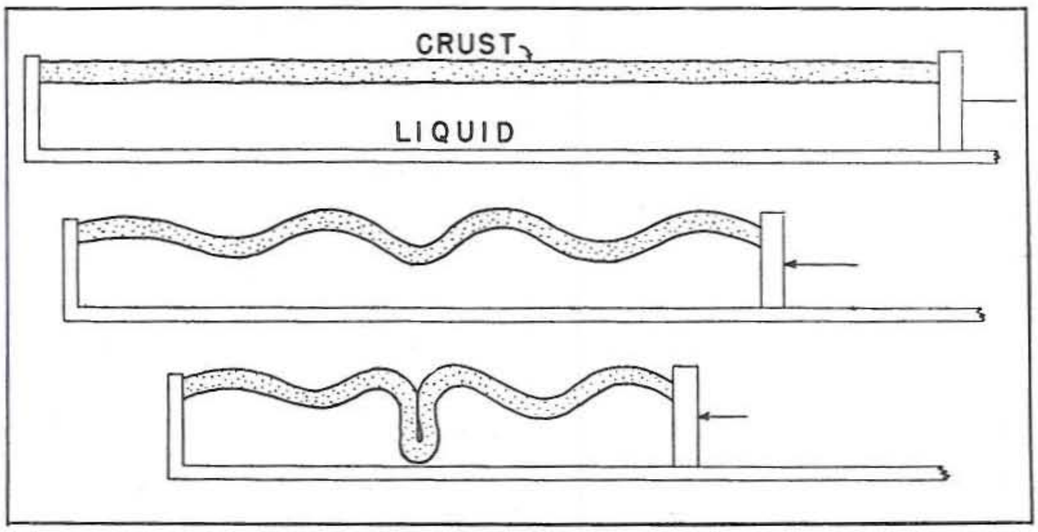
Figure 8: Hess's explanation of dipping Benioff zones as the composite vector of a vertically plunging tectogene being overridden by a horizontally drifting continent, mid 1960s.



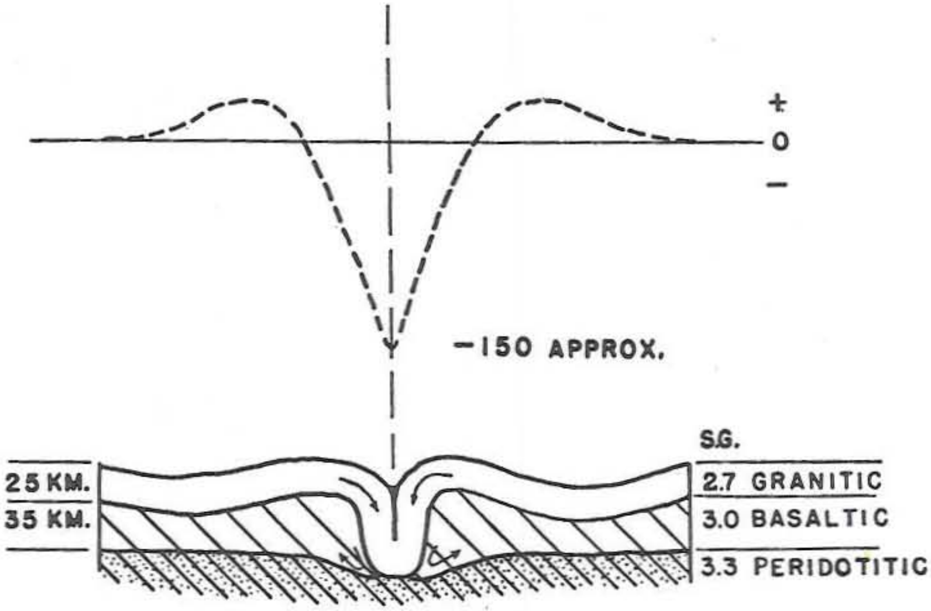
Gravity Anomalies in the East Indies, Showing Belt of Gravity Deficiency Peripheral to the East Indian Archipelago, Flanked by Bands of Positive Anomalies.



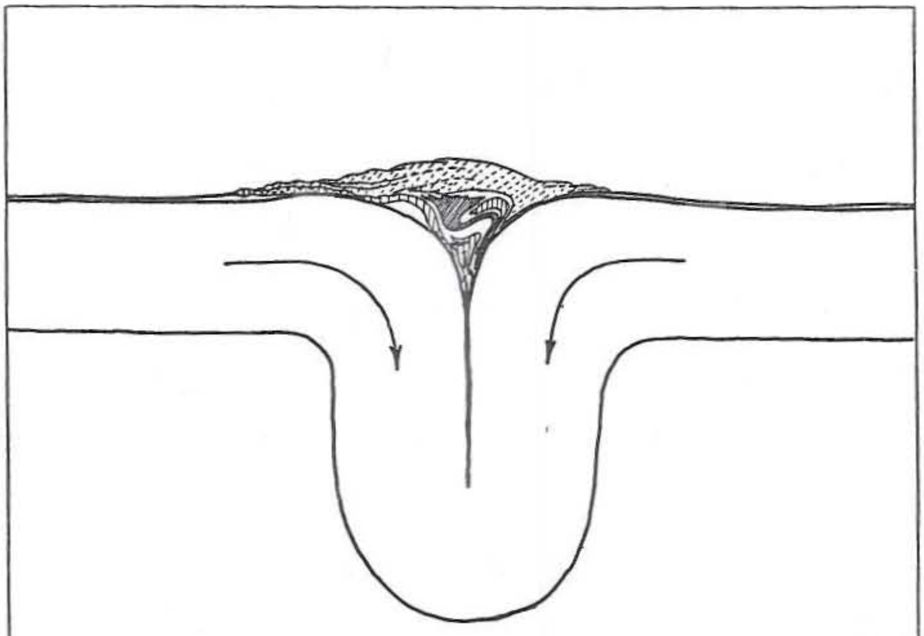
Gravity Anomalies in the West Indies, Showing Similarity to the East Indies.



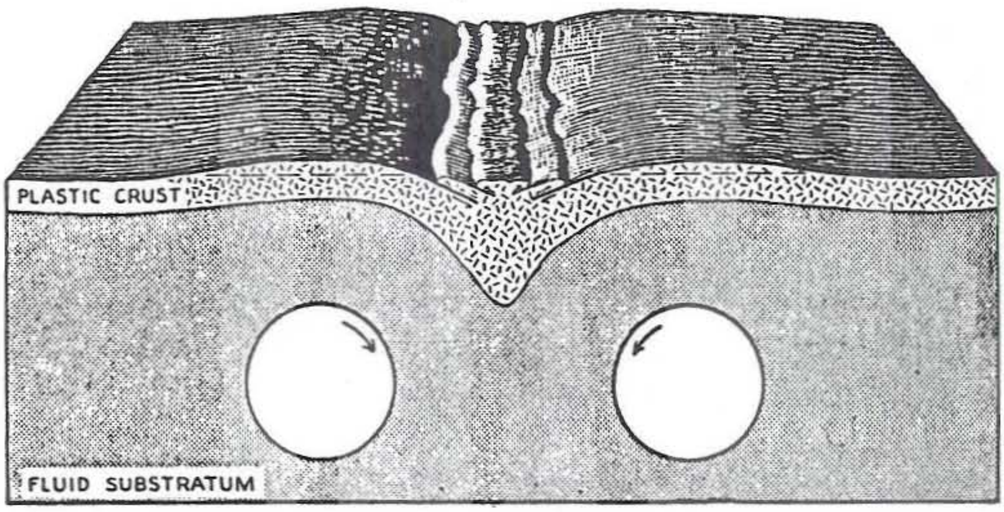
KUENEN'S EXPERIMENTS



Crustal buckle, specific gravity distribution and resultant anomaly curve.

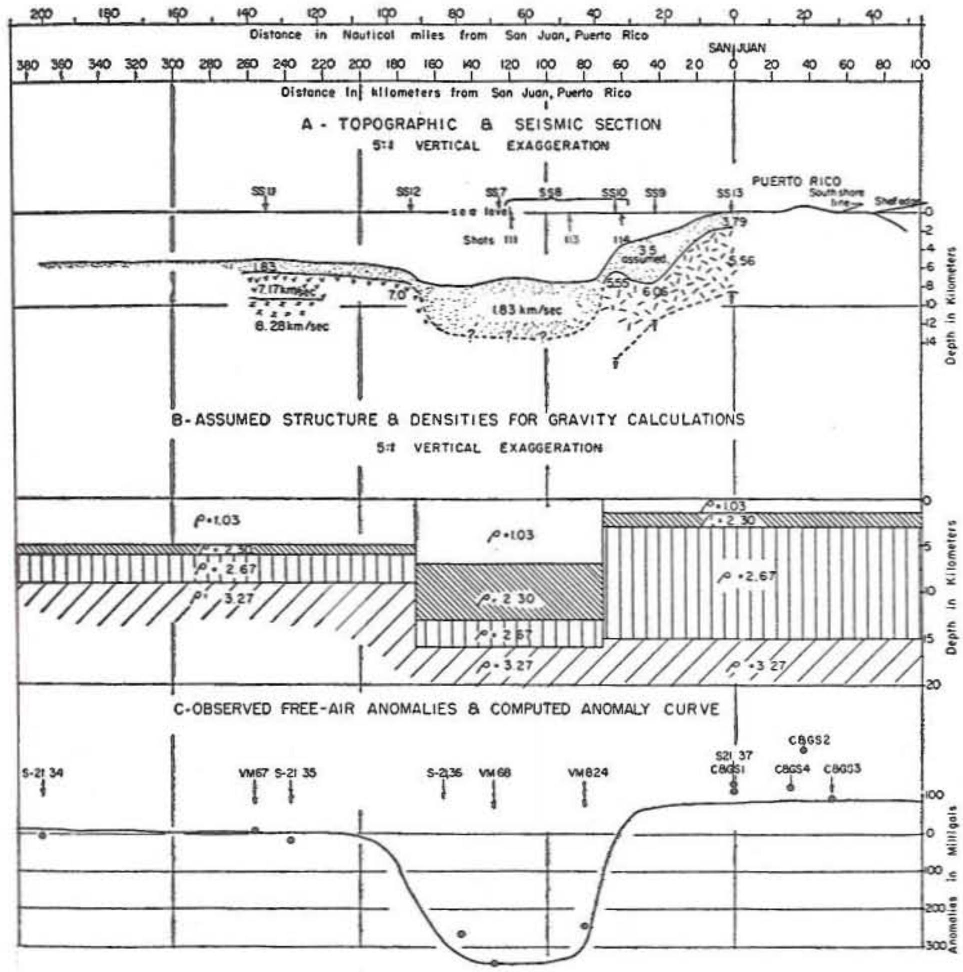


General section of the Alps superimposed on the tectogene. Both features drawn to the same scale with no vertical exaggeration.

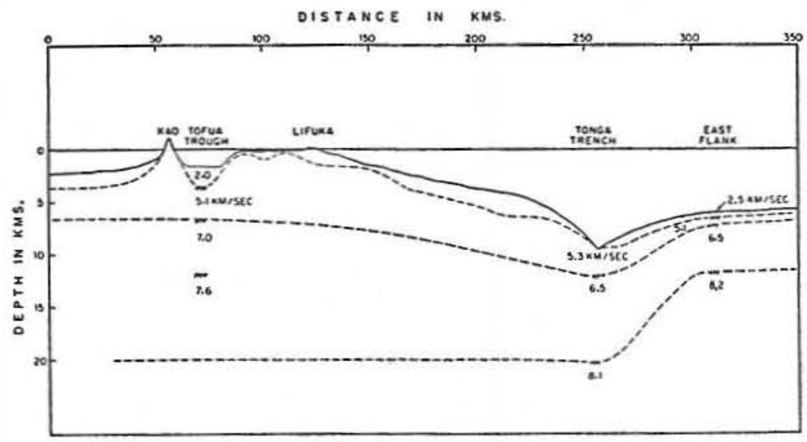


Stereogram of Large Model with Both Drums Rotating, Showing Tectogene and Surface Thrust Masses with Relations Similar to Kober's Orogen.

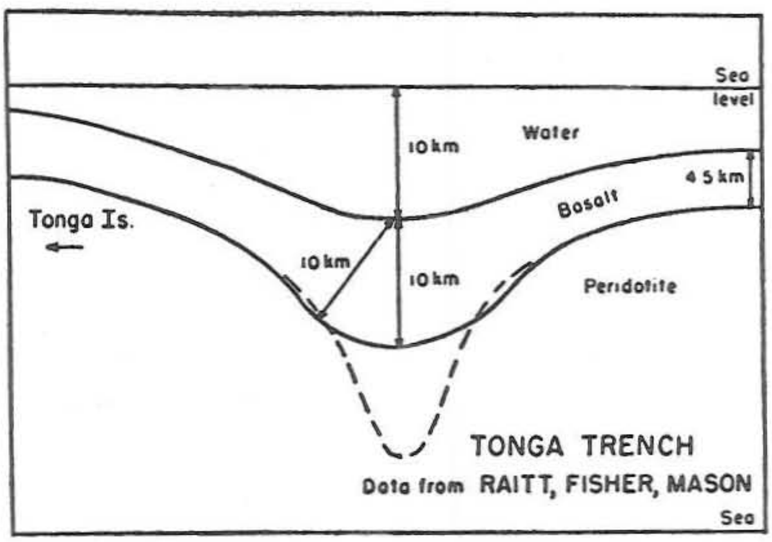
ALLWARDT FIGURE 5

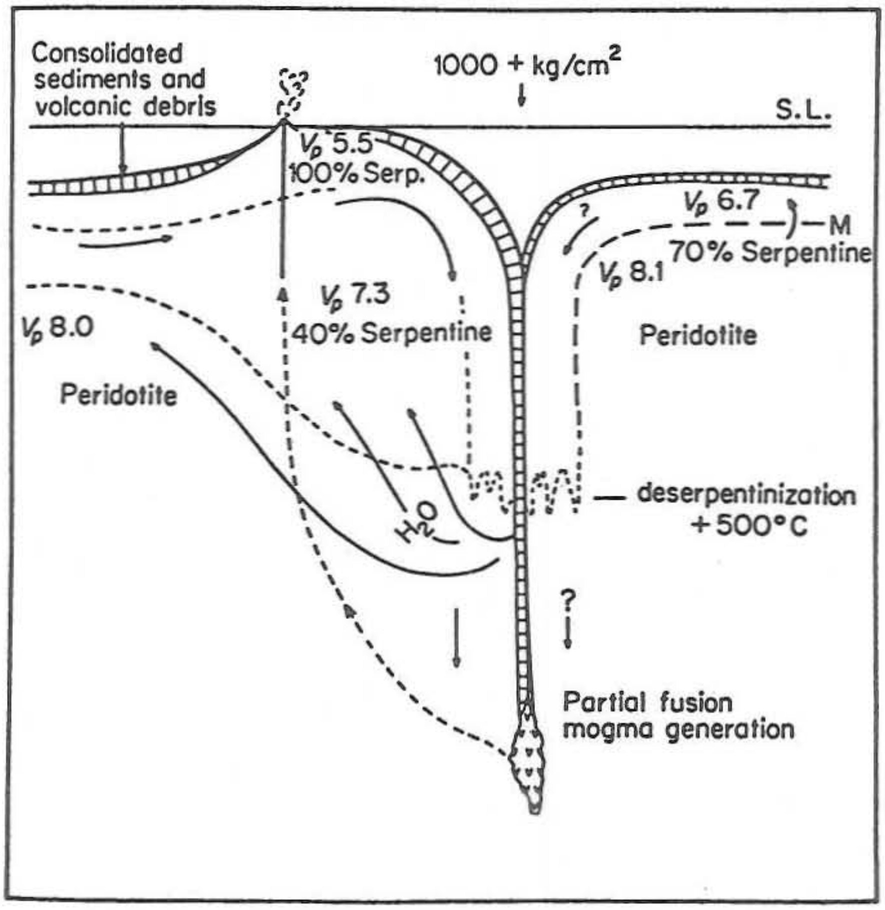


ALLWARDT FIGURE 6

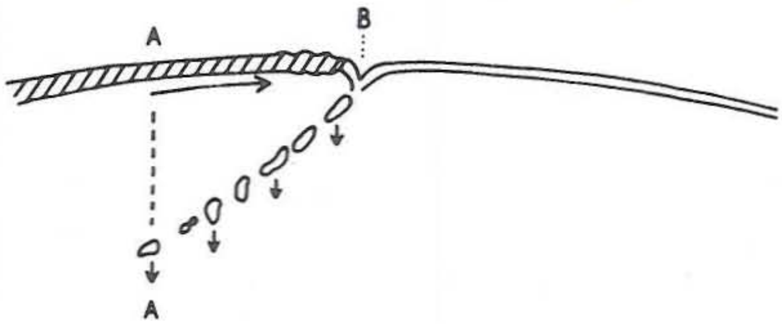


Structure section deduced from seismic and magnetic data





Supposed structure, with typical seismic velocities (V_p in km/sec), for a hypothetical trench–island arc association.



Continent overrides trench forcing it to the right. Settling, heavier, cooler masses move vertically downwards. Trench was at A when heavier mass beneath it was at the surface. Island arcs commonly override their trenches in a similar manner.