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A personal history of the ophiolite concept

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ABSTRACT

Since their original recognition in the early nineteenth century, ophiolites have been an integral feature of European Alpine-Mediterranean tectonic ideas. Ophiolites were not widely accepted by English-writing geologists until the late 1960s. Prior to that time, discussions in the English-language literature centered on the nature of the peridotites and volcanic rocks as separate entities. The development of the ophiolite concept went hand-in-hand with development of ideas of plate tectonics. As with plate tectonics, ophiolites are in a post-revolutionary, "mopping up" phase of scientific development.

My personal experience in ophiolites began in the early 1960s in Greece, and has extended to the Troodos complex, Cyprus, western North America, and other localities. I recount my personal experiences with working on ophiolites during development of the plate tectonic revolution and its aftermath.

Keywords: ophiolite, history, tectonics, Mediterranean, Cordillera.

INTRODUCTION

Ophiolites, fragments of oceanic crust and mantle formed at a spreading center and preserved on land, constitute a major ingredient of the plate tectonic theory and its extension to the continents. The history of the ophiolite concept is interesting not only in its own right, but also in the context of the development and elaboration of the concepts of plate tectonics. Set in this context (see Fig. 1), early developments in ophiolites were part of the fabric of geologic theory that existed before the development of plate tectonics. The development of plate tectonics can be thought of as the outgrowth of five lines of inquiry (see Moores and Twiss, 1995, p. 251–260; Kuhn, 1970): orogenic belts and geosynclines, marine geology and geophysics, continental drift, magnetism/paleomagnetism, and seismology. At the start of the twentieth century, the first subject possessed a unifying world view—geosynclines and their role in orogenic development—which had recently eclipsed an earlier global hypothesis: contraction. The other four were then in an immature state, i.e., they possessed no unifying theory, and they developed separately from one another until the 1960s. The idea of geosynclines went through a period of crisis and overturn as the information from stratigraphy, structure, and continental tectonics were woven together with the other fields to apply the revolutionary plate tectonic ideas to geologic history.

Ophiolites are also instructive from the point of view of the difference in development of concepts with respect to nationality, principal language, and point of origin of the scientist. The ophiolite concept developed principally in Europe, from observation of exposures in western Europe. From the nineteenth century, ophiolites were an important ingredient in the discussion of any orogenic belt in European literature, but were little mentioned in English-language geologic publications until the late 1960s.

This article is a personal history of the ophiolite concept. I have been involved in ophiolite studies since 1963, starting several years before the plate tectonic revolution. I was fortunate to be at an institution (Princeton University) in the 1960s in the early days of ideas of seafloor spreading, and my ophiolite work began against a background of gathering excitement as new results and ideas cascaded in, culminating in 1968–1969 in the plate tectonic revolution.

EARLY DEVELOPMENTS

Ophiolites (from the Greek: *ophis* = snake, and *lithos* = rocks), were first defined by Brongniart (1813), as represent-

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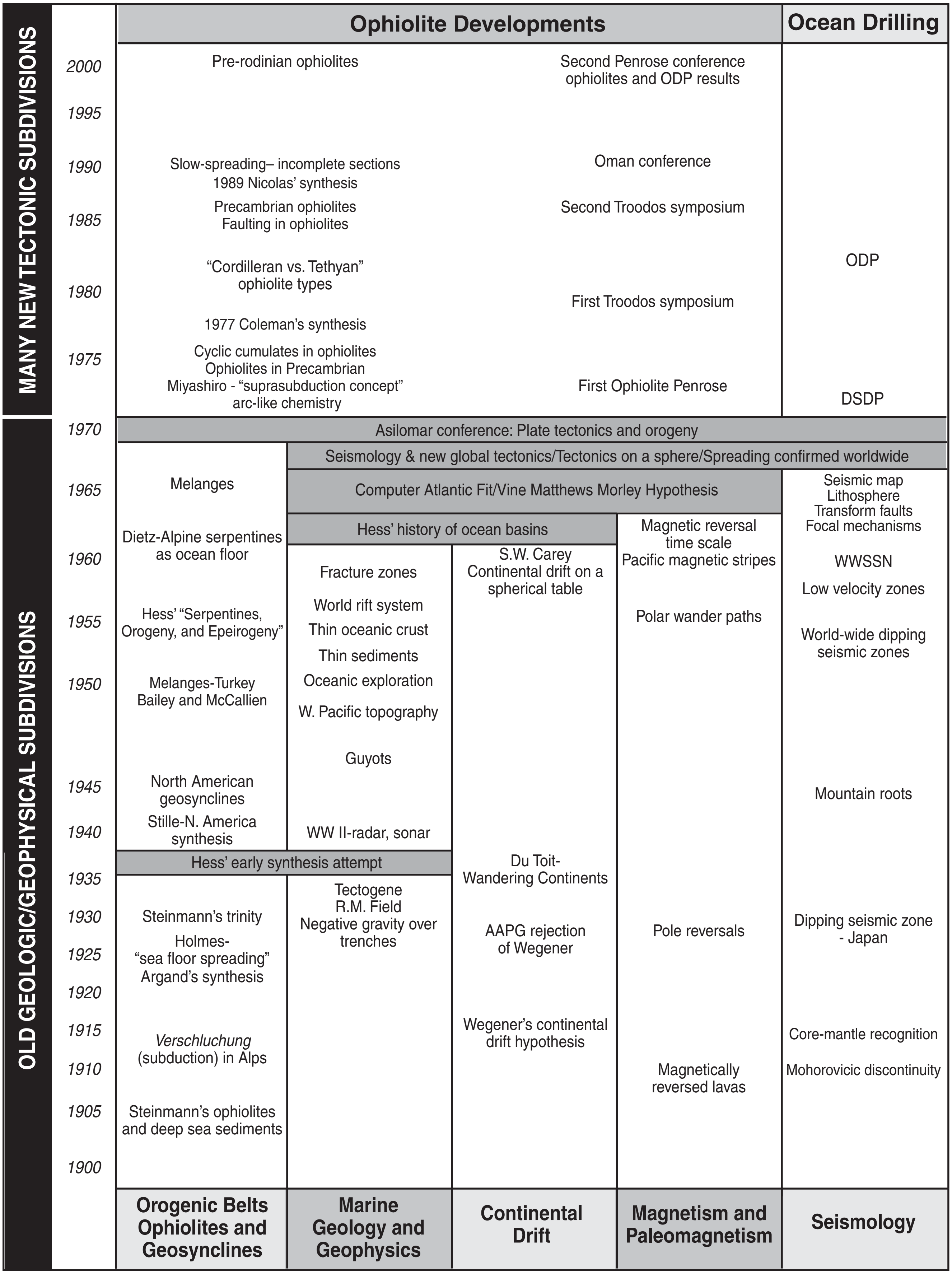

Figure 1. Historical summary of ophiolite developments and ocean drilling in the light of the plate tectonic revolution. Chronologic development of five fields from 1900 on led to the plate tectonic revolution in 1962–1970. New tectonic subdivisions arose out of the revolution.

ing the "common serpentines," the rocks that had been used as dark green-black dimension stone in many Renaissance and pre-Renaissance constructions (eight columns of serpentine breccia collected from Ephesus are present in Aghia Sophia, the church (now a museum) in Istanbul constructed in the sixth century, C.E; Ambriere, 1965, p. 214). In his first writings, Brongniart made no distinction between igneous and metamorphic rocks, but in his later classifications (e.g., Brongniart, 1827), the igneous-metamorphic rock distinction had become clear, and he grouped the ophiolites with igneous rocks. Brongniart also grouped massive altered volcanic rocks and gabbros (eufotides)[1] together with serpentines in his ophiolites, but he did not distinguish any sequence within these rocks.

By the end of the nineteenth century, the presence of "greenstones," "roches vertes," "pietri verdi," "grünsteins," was well-known throughout the world (Suess, 1909). Suess argued that these rocks commonly displayed igneous contacts, they were present as discordant or concordant sheet-like igneous masses in highly folded terranes, and they never cropped out in large batholith-sized masses, in contrast to granitic rocks.

Steinmann (1905, 1927) added considerably to the understanding of ophiolites. Beginning with his work in the late nineteenth century, Steinmann pointed to the ubiquitous association of serpentinite, "diabase" including altered volcanic rocks (spilites, keratophyres), hypabyssal and plutonic mafic rocks, and radiolarian chert. He described this association, which became known as "Steinmann's trinity" in the western Alps and Italy where the sequence abundantly crops out, but generally lacks gabbro and nowhere includes dike complexes (Bernoulli, 2001). Interestingly, Steinmann (1905) also recognized the association in the Golden Gate region, San Francisco, and on Mt. Diablo, California, based upon a field trip led by Andrew C. Lawson of the University of California, Berkeley, in 1892. Steinmann's other important contribution was drawing attention to the deep water environment of the immediately overlying radiolarites. He also observed that shallow water sediments overlay the radiolarites and argued for massive uplift in Cretaceous time and for the ophiolite emplacement during folding. After Steinmann's (1927) synthesis, nearly all workers recognized the importance of the association, but its true significance was not generally recognized until the mid-1960s (Hess, 1965). Steinmann also argued that ophiolites represented the remnants of a 500 to 700 km wide Tethyan Ocean that formerly existed between Africa and Europe (Oreskes, 1999).

Contemporaneously with Steinmann's work, two other lines of thought developed as to the origin and tectonic significance of peridotites and/or serpentines. One was the work of the American petrologist N.L. Bowen. From his experiments as early as 1914 on the crystallization of olivine and the fieldwork by R.A. Daly and others on stratiform mafic-ultramafic complexes, Bowen (1927) argued that peridotites formed chiefly by fractional crystallization of basaltic magma and stratiform accumulation of the early-formed olivine in the bottom of a magma chamber.

A contrasting view was that of the Australian geologist, W.N. Benson. Drawing upon his own fieldwork in eastern Australia and a worldwide survey of peridotite/serpentinite occurrences, Benson (1926) argued that field relations indicated that peridotites were of magmatic origin, and different from peridotites formed by fractional crystallization processes in stratiform mafic-ultramafic complexes. Benson coined the term "Alpine-type" peridotites because of the widespread presence of such peridotite/serpentinite bodies in "Alpine-type" orogenic belts. He argued that peridotites were intruded during the initiation of deformation of an orogen in its marginal parts, as opposed to the central core region where granitic intrusions predominated.

Thus, by 1927, three conflicting lines of opinion existed. The first, the continental European view, emphasized the relationship of peridotites to mafic pillow lavas and radiolarian cherts. The second, an "Anglophone" view espoused principally by workers in the USA, the UK, Australia, and New Zealand, argued that the "Alpine" peridotites were unrelated to the other rocks of Steinmann's trinity, were igneous, and were intruded into the margins of the core regions of "Alpine" mountain systems. The third view, led by Bowen, argued that the temperatures of formation of an olivine-rich magma were too high to explain the field relations of general lack of metamorphism, and that peridotites were formed by fractional crystallization of basaltic magma. This conflict raged on for four decades and was resolved only in the 1960s. With the benefit of hindsight, it is clear that all three lines of thought were partly right. Part of the controversy was cultural, and was aided and abetted by the tendency for authors to cite mainly literature in the language most familiar to them.

Vogt (1924) made an early attempt to reconcile the opposing views. He observed that naturally occurring peridotites contained minor but significant amounts of iron. As Bowen had originally dealt in his experiments with pure Mg olivine (forsterite), Vogt argued that the widespread presence of ~10% Fe might depress the melting temperature of the system below that of Mg- or Fe-olivine so that magmatic peridotites could exist at temperatures where pronounced metamorphic aureoles need not be present. In response, Bowen and Schairer (1935) investigated the system $MgO\text{-}FeO\text{-}SiO_2$ and found that the additions of small amounts of Fe did not reduce the melting temperature nearly as much as Vogt argued and they concluded that "dunites are formed by accumulation of crystals with perhaps some special mode of intrusion of a crystalline mass for those bodies that seem to be special intrusives" or possibly the abundant presence of volatiles (1935, p. 205).

OPHIOLITES AND THE RISE OF MODERN MARINE GEOLOGY AND GEOPHYSICS

At the turn of the twentieth century, little was known about the oceans. The expedition of HMS *Challenger* in the 1870s led

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[1] Interestingly, the term "gabbro" derives from a Tuscan village of the same name, which sits on serpentine.

to recognition of something of the nature of deep-sea sediments and to the presence of relatively shallow depths in the middle of the north Atlantic. Very little was known about the oceans themselves, however, and in the second volume of Eduard Suess's monumental work *Das Andlitz der Erde* (the Face of the Earth), subtitled *Die Meere* (the Seas), Suess (1906) confined his discussion essentially exclusively the geology of the lands surrounding or exposed within the oceans. Extension of the Mid-Atlantic Ridge to the southern hemisphere was an outgrowth of the German *Meteor* expedition in the 1920s (Oreskes, 1999). Argand in 1924 (1977, p. 147) and Holmes (1928) suggested that the Mid-Atlantic Ridge was a "sialic" remnant of the separation of the continents. Dutch investigations of Indonesia in the 1920s revealed the presence of topographic depressions—deep-sea trenches. There was no unifying hypothesis, however, on the origin of the oceans. Alfred Wegener's proposal of continental drift had great implications for the nature of the oceans and their relationships with continents, but the general process was not understood (see discussion in Oreskes, 1999).

The early 1930s saw the rise of modern marine geology and geophysics. R.W. Field, a professor of geology at Princeton University, wrote a report to the American Geophysical Union arguing the scientific rationale for development of a program of exploration of the oceans. It is hard in retrospect to conceive of the ignorance at that time of the submarine realm. Little was known about the topography of the world's oceans; little or nothing was known about broad patterns of sedimentation, the nature of oceanic crust, or the thickness of the crust. Field argued in his report that as the oceans represented such a large portion of Earth's surface and were so little known, some considerable effort was appropriate. Presciently, he observed, in part responding to the debate that had raged over Alfred Wegener's concept of continental drift (Oreskes, 1999):

> The outstanding geological problem is that of the permanency of ocean basins, and it is doubtful whether this problem can be solved by the study of continents and oceanic islands alone. The pressing need is for physiographical and structural data concerning the crust under the Atlantic, Pacific, Indian, and Arctic oceans. Can geophysical methods assist us to obtain this information, by the penetration of the oceanic waters to all depths and by the determination of the composition, thickness, and stratigraphical sequence of the sediments on the sea floor? Most important of all, will these methods enable us to carry our structural studies deep beneath the ocean bottom with the same accuracy which we now attain in the exploration of the continents? (Field, 1938a, p. iv–v.).

As Field made his argument, the Dutch geophysicist F.A. Vening Meinesz was working in marine geophysics with the study of gravity at sea. Beginning first in Indonesia, where topographic trenches were discovered in the early twentieth century, Vening Meinesz extended his studies to the Caribbean in the early 1930s, borrowing a World War I-surplus U.S. submarine for the purpose—arranged for him by Field and Field's friend, Major William Bowie of the U.S. Coast and Geodetic Survey (Hess, 1962a). With him went two young graduate students who were to become giants of the field in the following decades—H.H. Hess and W.M. Ewing. Hess came to marine geology from a background in fieldwork in southern Africa and the Appalachians. He worked with Vening Meinesz on the gravity field of the Caribbean[2]. One of the features that they found during this survey was the negative gravity anomaly extending from the Puerto Rico trench to the Tobago Basin. Hess (in Field et al., 1933; cf. Oreskes, 1999) argued that these negative gravity anomalies were formed by a "tectogene" or a downbuckle in Earth's crust, possibly produced by convection currents within the mantle, a position that was accepted by Vening Meinesz and investigated experimentally by the University of California, Los Angeles geophysicist, David T. Griggs (Oreskes, 1999).

In the mid-late 1930s, Hess began to attempt to integrate his Ph.D. work in the Appalachians and with his work with Vening Meinesz. Hess delivered a paper at the 1937 International Geological Congress in Moscow, which was published in 1939 (Hess, 1939), not a good year for geologic publications from the USSR[3]. In this publication, Hess elaborated on the "tectogene" model for negative gravity anomalies, and suggested that tectogenes were the locus of geosynclines. Hess argued that peridotites were intruded in the initial stages of orogeny and, drawing on his observations from the Appalachians, that they were present in paired belts some 60 to 100 km on either side of the axis of the geosyncline/tectogene. Hess stated that peridotites are intruded in the initial stages of orogeny and are not reintruded in later stages. Ironically, Hess mentioned ophiolites only in the title, but not in the body of the paper. The meaning of Hess's observation on timing of emplacement became clear in the reinterpretation of ophiolites after the plate tectonic revolution. In a companion paper (Hess, 1938), he argued for evidence for the magmatic emplacement of peridotites, and suggested that the temperature problem documented by Bowen and Schairer (1935) could be alleviated by having a hydrous ultramafic magma of serpentine composition. The German geologist Hans Stille (1939) also argued that ophiolites were intruded in the initial stages of deformation of geosynclines, but Stille did not make any connection with the oceans, saying nothing about the oceans, gravity anomalies, or trenches. (One wonders whether Stille had attended the 1937 Moscow congress and heard Hess's presentation). Hess' (1939) work was the first to try to integrate continental geology with new marine geological and geophysical data. On Figure 1, it is illustrated as an early synthesis attempt.

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[2] Hess once recounted an experience on the submarine when through a malfunction it dove to twice its designed maximum depth limit. Only drastic action by the captain saved the ship and its personnel.

[3] Hess observed in the 1960s that his 1939 article had received little attention also because he was a young person just starting out on his career, and he had not yet made his name in an accepted field. His update of same paper after he had established his reputation received wide attention in 1955.

By the eve of World War II (WWII) a great deal of progress had been made in several fields. The Mid-Atlantic Ridge was well-documented, as was the location of earthquakes along it (Heck, 1938). Earthquake seismic work suggested that the oceanic crust was thin (~7 km) relative to continents (Heck, 1938). Nevertheless, Field (1938b, p. 6) pointed out that with regard to the ocean floor that "the major portion of the earth's crust constitutes a vast 'no man's land' as yet practically unexplored…" The 1939 International Geological Congress was scheduled to convene in New York in early September with Richard Field as its Chair. Field was looking forward to a full discussion of Wegener's ideas on continental drift in the light of the new results from marine geological and geophysical work. Unfortunately, just before the Congress opened, Hitler invaded Poland, and the anticipated international colloquium on Wegener's ideas and orogeny never occurred (Oreskes, 1999).

During the Second World War, scientific progress took a back seat to the war effort. Wartime technological developments in navigation and bottom sounding were to contribute greatly to the subsequent discovery of sea-floor spreading and plate tectonics.

Having been commissioned a reserve naval officer in the 1930s to facilitate his work with Vening Meinisz, Hess reported for active duty the day following the Pearl Harbor attack. He served in the Navy throughout the war, ending up as a Captain of an attack transport in the Pacific, the USS *Cape Johnson*. The story goes that Hess kept his depth sounder going at all times, even when in close-in enemy waters. In the process, he discovered a series of flat-topped seamounts, which he named "guyots" (Hess, 1946; Arnold Guyot was the first earth scientist at Princeton, and the namesake of the Princeton geology building, Guyot Hall). Hess originally thought that these mountains were Precambrian in age, in line with the assumed permanence of oceans, but later acknowledged the Cretaceous age fossils discovered on them (Hess, 1955). Hess also acknowledged the contributions of the USS *Cape Johnson* and the battleship USS *Massachusetts* (now a museum in New Bedford, Massachusetts), as having made more soundings in WWII than any other ship. A young Princeton colleague, John C. Maxwell, spent months on New Caledonia, where he investigated and subsequently described the chromite deposits of the huge peridotite complex exposed there, and documented evidence for ultramafic intrusive relations, interpreted as magmatic dikes and sills (Maxwell, 1949).

After the war, Hess and Maxwell analyzed thousands of km of ship-track depth soundings and published large-format bathymetric maps of the western Pacific (Hess, 1948) and the SW Pacific (Hess and Maxwell, 1949). These maps represented unprecedented revelations of the topography of the western Pacific floor. Hess and Maxwell also recognized the dipping seismic zones beneath island arcs in the western Pacific, which subsequently became known as Wadati-Benioff zones.

Meanwhile, debate about the possibility of a peridotite magma continued. Challenged by Hess' 1938 paper, Bowen and Tuttle (1949) published results of an experimental study of the $MgO\text{-}SiO_2\text{-}H_20$ system. Their results indicated that pure Mg serpentine did not exist at temperatures above ~500 °C, but at higher temperatures olivine coexisted stably with H_2O vapor, and that no magma of serpentine composition existed (see Young, this volume, Chapter 4). They concluded inescapably that ultramafics were intruded only in the solid state. These results bore so heavily against Hess's (1938) hypothesis that he ultimately withdrew it (Hess, 1966). Shortly before his death in 1969, however, he was able to participate in a field trip in South Africa where he saw the copious evidence for extrusion of ultramafic magmas in the form of komatiites. In a letter to F.J. Vine, received while Vine and I were working on Cyprus, Hess described the field trip as the most interesting one that he had ever been on. He clearly felt finally vindicated on his arguments for the existence of ultramafic magma. Ironically, the Archean komatiitic magmas of South Africa did not represent the "Alpine-type" ultramafic rocks that Hess had originally considered.

CRISIS, REVOLUTION, AND OPHIOLITES

By the late 1950s it was clear that geology was in a crisis mode. Polar wander paths had been discovered, sediments in the oceans were thin, the magnetic reversal time scale was being worked out, marine seismic work confirmed that oceanic crust was relatively thin and stood isostatically 5 km or so below continents—a result that Hess called "the most momentous discovery since the war" (Hess, 1954, p. 341), magnetic stripes had been discovered off the coast of the western USA, the mid-ocean rift system had been discovered, and high heat flow was discovered on the ridge axis. None of these developments fits the geosynclinal paradigm or the dogma of permanence of ocean basins.

At the same time, the Alpine peridotite-ophiolite controversy was still unresolved. Work by Ross et al. (1954) documented the similarity of composition of fragments of Earth's mantle brought up in basaltic and many "Alpine-type" peridotites. De Roever (1957) argued that Alpine peridotites were tectonically emplaced fragments of the upper mantle. Hess (1955) essentially repeated his 1939 interpretations and argued that calling the various rocks ophiolites confused the issue, as the peridotites and volcanic rocks really were of separate origin. Hess, however, suggested that the Mid-Atlantic Ridge "represents a welt of serpentine…concentrated [by] convective circulation in the mantle…" (1955, p. 404–405). The term "ophiolite" had become almost taboo in North American circles, and remained so until the following decade. Hess acknowledged that: "So the problem remains unsolved. Some vital piece of evidence is still missing" (1955, p. 402).

One vital new idea was soon to emerge. When I enrolled as a graduate student at Princeton University in fall, 1959, one required course was called "Advanced General Geology." In it, several professors lectured on their current research. In his many comments, Hess and his colleague, A.F. Buddington, discussed polar wander paths, paleomagnetism, and rock magnetism. They emphasized the lack of understanding of the origin of rock magnetism and argued that some of the apparent polar wander data could be the result of complications of the magnetization process. Hess also mentioned the seismic results for a thin oceanic crust, the evidence for only thin sediments in the ocean basins, and the recently discovered heat flow results. Despite diligent search, no one could point to convincing modern analogues of the ultramafic-bearing "eugeosynclines" of orogenic belts, although Drake et al. (1959) had suggested the east coast of North America as a modern analogue of miogeosynclines. In retrospect, the "crisis" in the sense of Kuhn (1970) was upon the world.

Late in 1959, the Australian geologist S.W. Carey came through to deliver a lecture on continental drift and earth expansion. His ideas on expansion have been widely discounted and detract from his contributions to continental drift, however. Carey's contribution to the continental drift debate was to construct a spherical table, ~2 m in diameter, on which he plotted the 500 fathom contour, rather than the coastlines, as had Wegener. Carey gave a three-hour spell-binding lecture, ending completely spent, covered with sweat and chalk dust. At the end, we all filed numbly out of the room. Halfway through the talk, however, Hess bolted out of his seat and started pacing up and down the aisle. Thereafter in Advanced General Geology, there was no more talk of problems of paleomagnetism and polar wander paths. Within two months, Hess was circulating a manuscript, entitled "Evolution Ocean Basins" [sic], which was eventually published as "History of Ocean Basins" (Hess, 1962b). This was the key insightful paper that gave rise to the new unifying model of ocean floor spreading, just as Kuhn (1970) suggested would happen in a scientific revolution. I believe that S.W. Carey must be given the credit for "pushing Hess over the edge." In his article, Hess suggested that the oceanic crust was chiefly serpentinized mantle peridotite, that the mid-ocean ridges were the loci of upwelling and divergent motion and that the continents were passive riders on mantle material.

In the early 1960s, the advent of passenger jets made intercontinental travel relatively easy. New results from Mediterranean ophiolites caused the beginnings of a revolution in American understanding of ophiolites. In 1961–62, J.C. Maxwell traveled to Italy on a sabbatical leave, where he worked on both ophiolites and, incidentally, melanges. As detailed by many workers (e.g., Bernoulli, 2001) in Italy and elsewhere in the Mediterranean the close association of the ophiolitic lithologies in a pseudostratiform sequence is essentially unarguable. Maxwell also investigated the work of Brunn (1956, 1959, 1960, 1961) in Greece (Maxwell and Azzaroli, 1962). Brunn (1959) was the first person ever to recognize the possible association between ophiolites and sea-floor spreading centers; however, he and his associates (e.g., Aubouin, 1959), following Routhier (1946, 1953; and Dubertret, 1953; *in* Nicolas, 1989), posited that ophiolites resulted from massive outpourings of mafic magma in "geosynclines" (which they represented as sediment starved submarine troughs as seen in the Alps, but not in North America) with subsequent stratiform crystallization under a self-formed roof. Maxwell and Azzaroli (1962) essentially adopted this view.

In 1962, while finishing my Ph.D. thesis work in Nevada (see Moores et al., 1968), I persuaded Hess and Maxwell to allow me to switch to working on the Vourinos complex. I traveled to Greece in summer, 1963, on an NSF Post-Doctoral Fellowship and an NSF grant, remaining over a year. At the time I began this work, I was buffeted by three rival schools of thought: (1) the view of Hess, that Alpine-type peridotites were of very constant composition, with a Mg/Fe ratio of ~9:1, and represented either hot, diapiric intrusions (MacKenzie 1960) or solid mantle fragments, following De Roever (1957); (2) the view of Thayer (1967), who had long argued for a consanguineous relationship between the peridotite and gabbro of "Alpine-type complexes," but not for the volcanic rocks; and (3) the European (especially French) view that they represented extrusive/intrusive outpourings in geosynclinal troughs (Brunn, 1956; 1960). About this time, Dietz (1963) suggested that "Alpine serpentinites" were fragments of oceanic crust incorporated into continental margin "geosynclines" but Dietz does not mention Hess's already published article (Hess, 1962b, which was widely circulated for over a year prior to its publication) showing ocean crust as serpentinite. Dietz never mentioned the word "ophiolite." In an earlier article he stated "a preprint by H. Hess also (suggested) a highly mobile sea floor. Full credit of priority is to be accorded him for any merit which this suggestion has" (Dietz, 1962, p. 12).

Work in Vourinos led to the key discovery that the lower part of the ultramafic complex was metamorphic tectonite. This tectonite was intruded and overlain by an igneous upper ultramafic-mafic (transition) zone (Moores, 1969a). Davies (1968, 1971) made a simultaneous similar discovery in Papua-New Guinea. This discovery partly resolved the conflict between the Brunn-Aubouin view of ophiolites and that of Anglophone authors, because chemical study of the tectonite showed that they were identical with the "Alpine-type" peridotites by then well-documented by MacKenzie (1960) and Green (1964). A field trip with Harry Hess and John Maxwell to the Vourinos and the Italian ophiolites of Tuscany and Liguria led to a letter from Hess to Maxwell and myself accepting the ophiolite sequence as an inescapable fact and Hess's (1965) acceptance of ophiolites as fragments of ocean floor (Hess, 1965). I was not so sure, and in my article (Moores, 1969a; submitted in spring 1967), I proposed two hypotheses, one that it was a tectonic fragment of ocean floor, and the other that it was a solid/liquid diapiric emplacement of partially molten mantle material. I had still not made the connection to sea-floor spreading.

While writing up my Vourinos work in 1965–1966, I spent considerable time with Fred Vine and Jason Morgan, both young members of the Princeton faculty, discussing what oceanic crust at a spreading center might look like in the field. I heard of a remarkable set of rocks on Cyprus, and obtained copies of the Cyprus Geological Survey Memoirs. Opening the map of Wilson (1959), I was stunned. There in front of me lay a massif of ultramafic rocks, gabbro and related intrusive rocks, a "sheeted complex" of dike-within-dike, gradationally overlain by pillow lavas. The most arresting feature of Wilson's map is the large area of pink-colored "diabase" with dips and strikes all aligned with each other and with dikes in the overlying pillow lavas. I showed it to Fred Vine and asked, "What do you think of this as oceanic crust formed at a spreading center?" That possibility occasioned enough interest on the part of Hess and Maxwell that during the process of a circum-Mediterranean survey of peridotites in summer 1966, J.S. Dickey Jr., and I traveled to Cyprus for two-day reconnaissance of the Troodos complex.

After returning from the Mediterranean and moving immediately to Davis, California, I wrote a report to Hess and Maxwell about Troodos, arguing that it was worth a closer look. A few months later, at a plenary session of the 1966 Geological Society of America Annual Meeting in San Francisco, F.J. Vine, A. Cox, R. Doell, and G.B. Dalrymple all gave talks on summarizing the evidence for sea-floor spreading and magnetic reversals. The "Marine Geological Revolution" (Fig. 1) had arrived. During the meeting, Vine proposed to me that we do a joint study of the Troodos complex. We obtained Hess's blessing, and in early 1967 applied for and were awarded an NSF grant to study the Troodos massif as a product of sea-floor spreading. We delayed going because of political and professional conflicts in summer, 1967.

Simultaneously, Ian Gass, then of University of Leeds, clearly had similar ideas. Gass had been a staff member of the Cyprus Geological Survey and had written a memoir of part of the area (Gass, 1960). Gass (1967) had described the Troodos massif as an assemblage of pillow lavas unconformably overlying the sheeted intrusive complex and plutonic rocks, formed in an oceanic area and thrust over the African continental margin, but not as an ophiolite. In Gass (1968), he drew the analogue between the sheeted complex (Wilson, 1959) and sea-floor spreading, but he did not recognize the tectonite/cumulate contrast in the ultramafic rocks, writing that "…these plutonic rocks belong to a differentiated ultrabasic mass of batholithic dimensions" (Gass, 1968, p. 39).

Fred Vine and I traveled to Cyprus in August 1968, arriving nearly simultaneously with the Soviet invasion of Czechoslovakia and just before the anti-Vietnam War riots at the Democratic Convention in Chicago. We spent much of the first of two field seasons on the island performing field paleomagnetic vector measurements and drilling for paleomagnetic studies. Our rationale for this work was to investigate a possible magnetic reversal that was suggested by existing aeromagnetic survey maps. Though we did not find a reversal, our data were the first to document an ~90° counterclockwise rotation of the complex[4]. In summer 1969, we returned to Troodos and concentrated on the geology of the massif. Our study (Moores, 1969b; Vine and Moores, 1969; Moores and Vine, 1969; Moores and Vine, 1971; Vine and Moores, 1972) recognized the Troodos complex as an ophiolite, and was the first explicit examination of an ophiolite in the light of the concept of sea-floor spreading[5]. Our studies also reversed the sequence of ultramafic units. Following the relationships worked out in stratiform mafic-ultramafic intrusions, earlier workers, including Gass (1968) had assumed that the dunite was at the base of the ultramafic sequence. Vine and I established that the harzburgite represented tectonite mantle, and that the dunite instead was the bottom of the cumulate plutonic rocks, but above the harzburgite. We presented these results at a meeting of the Royal Society, London, in November 1969 (cf. Moores, 1969c).

In early December 1969, a GSA Penrose Conference entitled, "The meaning of the new global tectonics for magmatism, sedimentation and metamorphism in orogenic belts," was held at Asilomar, California, organized by W.R. Dickinson. At that meeting, presentations of the recently published evidence for plate tectonics were compared with evidence for orogenic history in such regions as the Appalachians and the western North America Cordillera. In the course of a few days, geology was transformed, Wegener was vindicated, active subduction zones were recognized as coinciding with dipping seismic zones, and geosynclines were reinterpreted in terms of actualistic models (Dickinson, 1970, 1971).

Ophiolites were a major topic of discussion at the Penrose conference. If they were formed by sea-floor spreading, how were they emplaced? The idea occurred to several workers that emplacement may occur by collision of a continental margin with a subduction zone. Others thought that emplacement could occur simply by "tipping up" of the oceanic edge of the overriding plate, or by incorporation of slices of the down-going plate in a subduction zone.

As the conference was ending, Dickinson rose to summarize aspects of the conference proceedings, and outlined his newly formulated ideas of geosynclines and plate tectonics. It was a tremendous excitement to hear geologic history folded into plate tectonics. A few minutes later, I formulated the general ideas of how plate tectonics, ophiolite emplacement, and the Phanerozoic evolution of western North America might fit together. It was a tremendously exciting moment. After a suitable lag time owing to the then-existing ground rules of a Penrose conference, several ophiolite-related articles appeared. I developed the model for emplacement of ophiolites by collision of a continental margin on the down-going plate with a subduction zone, emplacing a part of the upper plate as an ophiolite complex and applying the concept to a model for the

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[4] At one point, we were accosted by a man who turned out to be the Cyprus Interior Minister. At the end of a long discussion about our studies and why we were drilling, he remarked, "You mean that your government pays you to come over here and drill holes in my island?" Not long afterward, in a reflection of the turbulence of the times, he was assassinated.

[5] I remember continually thinking what an extraordinary experience it was to be studying such a remarkable series of rocks and with such a remarkable person as Fred Vine.

development of the USA portion of the North American Cordillera (Moores, 1970). Coleman (1971) made a global survey of ophiolites and defined the ophiolite emplacement process as "obduction"; and Dewey and Bird (1971) discussed ophiolite emplacement, especially as exhibited in early Paleozoic rocks of Newfoundland.

"MOPPING UP": OPHIOLITES SINCE THE REVOLUTION

Historical Overview

Since the plate tectonic revolution, studies of ophiolites have been in a "mopping up" phase in the sense of Kuhn (1970) as the new paradigm is extended to global occurrences and the entire geologic record. Thousands of studies have been conducted of ophiolite complexes throughout the world, in various tectonic regions, and our understanding of them has improved enormously. Particularly important have been international conferences and field trips investigating ophiolite complexes in various regions around the world, summary monographs devoted to ophiolites, and increasingly detailed comparisons of ophiolites with oceanic crust, particularly as documented through the Ocean Drilling Project (ODP).

A first ophiolite Penrose conference in 1972 consisted of a 1600 mile (2500 km) road trip of newly-recognized ophiolite complexes in the western USA, specifically Oregon and northern California, principally the Canyon Mt. Complex, Oregon, the "type area" of Thayer's "Alpine mafic magma system," several exposures in the Klamath Mountains, including the Trinity ophiolite, exposures in the Sierra Nevada including the Feather River ultramafic belt, and the Del Puerto ophiolite of the California Coast Ranges. The 12 informal seminars during the trip culminated in the so-called "Penrose definition" of ophiolites—"a distinctive assemblage of mafic to ultramafic rocks"—consisting of an ultramafic complex, a gabbroic complex, a mafic sheeted dike complex, and a mafic volcanic complex, commonly pillowed. So-called "associated rocks types" include an overlying sedimentary section of chert, minor shale and limestone (no mention is made of volcaniclastic sediments). The report called for more careful mapping of the various members within ophiolites and more petrologic studies (Anonymous, 1972, p. 25). With the benefit of hindsight, one can note that conspicuously absent from the discussion of this field trip is any mention of the need for consideration of the ophiolite in its regional context.

A second ophiolite conference the following year (May 31–June 14, 1973) in the Soviet Union focused on Hercynian (Paleozoic) ophiolitic complexes in the Alai Range and the Kyzyl Kum Desert, as well as Mesozoic ophiolitic complexes of the Lesser Caucusus. In addition, the conference provided a detailed exchange of views between Soviet and western geologists and introduced many Soviet geologists to plate tectonic concepts (Coleman, 1973).

Several books have been devoted to ophiolites (e.g., Coleman, 1977; Nicolas, 1989). My own modest contributions have included considerations of the tectonic significance of ophiolite emplacement (Moores, 1970, 1982), the re-interpretation of all ultramafic rocks in the light of plate tectonics (Moores, 1973), an early attempt to relate the structure of oceanic crust and ophiolites to spreading rate (Moores and Jackson, 1974). Further studies with students and colleagues of the Vourinos complex led to the recognition of cyclic cumulates in the mafic-ultramafic plutonic section. These have been recognized, but not studied in detail, in other ophiolites, as well (e.g., Vourinos, Oman; Jackson et al., 1975; Harkins et al., 1980; Rassios et al., 1983). Detailed studies of dike complex of the Troodos complex, Cyprus, led to discovery and elaboration of listric normal fault systems in the Troodos (e.g., Moores et al., 1990; Varga and Moores, 1985; Verosub and Moores, 1981) and the Josephine complex, California-Oregon (Harper, 1982).

Studies of ophiolitic rocks in the western North American Cordillera have led to the use of ophiolites in a re-interpretation of the structural evolution of that margin (e.g., Dilek et al., 1990). A global review of ophiolites and their significance led to their separation into "Tethyan" and "Cordilleran" types based upon the presence or absence of a continental substrate, an island arc edifice, or other geologic criteria (Moores 1982). Exploration of the nature of Precambrian, especially Pre-Rodinian (pre 1Ga), oceanic crust has elaborated on the hypothesis of Proterozoic oceanic crustal thinning in the Neoproterozoic (Moores, 1973, 1986, 1993, 2002).

Several major conferences have contributed to our understanding, including conferences on the Troodos ophiolite in 1979 and 1987 (Panayiotou, 1980; Malpas et al., 1990), the Oman ophiolite in 1990 (Peters et al., 1991), a conference at the 29th International Geological Conference, Kyoto (Ishiwatari et al., 1994), and a second ophiolite Penrose conference comparing ophiolites and ODP results on oceanic crust in 1998 (Dilek et al., 2000). The latter conference was particularly valuable, as it brought together workers concentrating on the ODP and those more focused on land-based ophiolite studies. It is through such comparisons that new insights will develop.

A major post-revolution discussion began with Miyashiro's (1973) focus on the arc-like chemistry of the Troodos complex. Most subsequent petrological and geochemical discussions have focused on the geochemical evidence for a mantle source already depleted of its MORB components (e.g., Robinson and Malpas, 1990, Bloomer et al., 1995).

Vourinos Ophiolite

A re-interpretation of the Vourinos ophiolite, northern Greece, epitomizes changes in our understanding of ophiolites. These changes are illustrated in Figure 2. Figure 2A shows a longitudinal schematic cross-section of the Vourinos complex drawn in the late 1960s after recognition of the tectonite/cumulate contrast, but before the plate tectonic revolution. Reinterpretation of

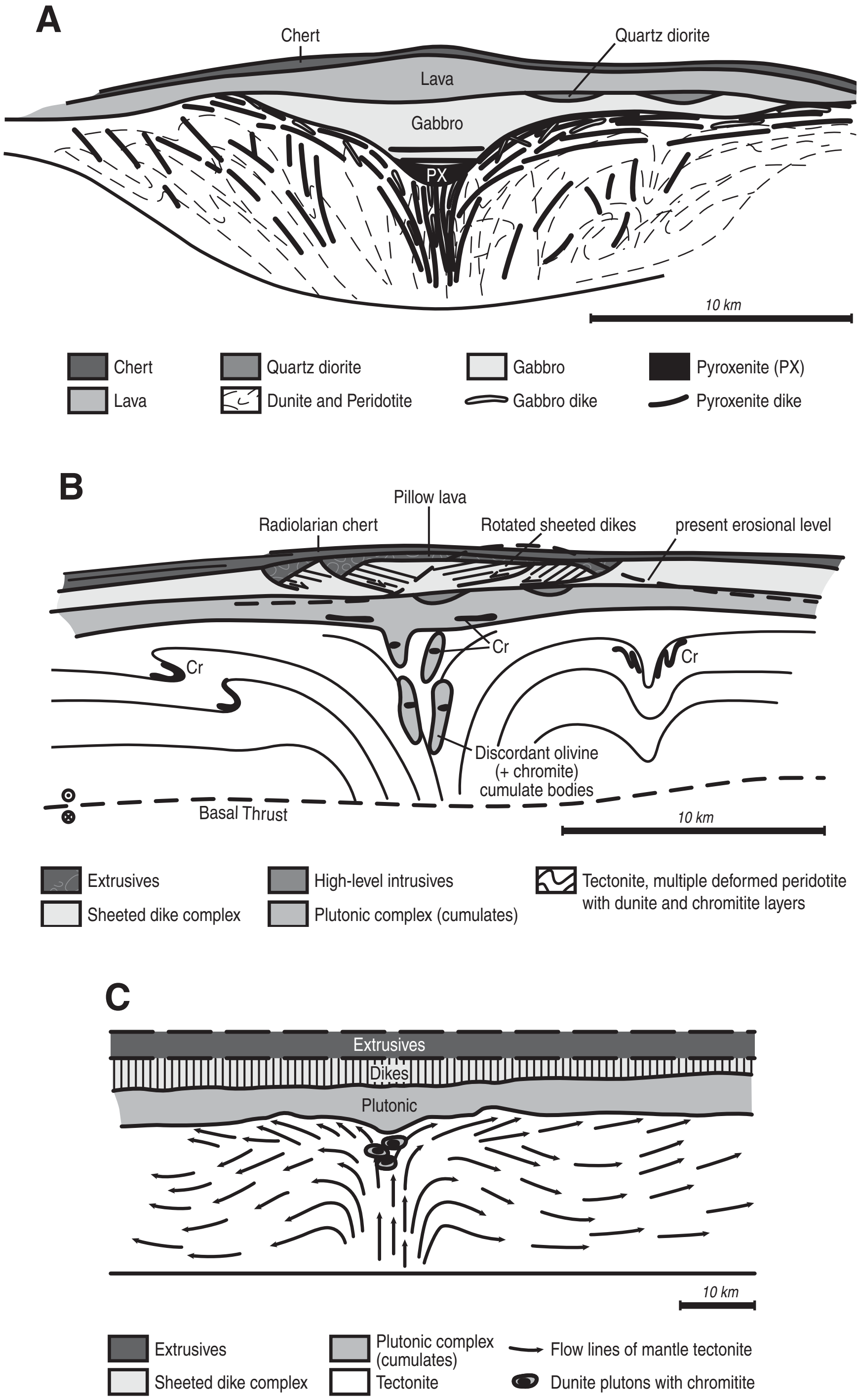

Figure 2. Evolution of ideas on the Vourinos complex, northern Greece. A: Longitudinal interpretative cross-section after Moores, 1969, showing pre-plate tectonic revolutionary interpretation of complex with tectonite dunite and peridotite and overlying magmatic plutonic/extrusive rocks. B: Re-interpretation of A based upon plate tectonic recognition of tectonic emplacement of ophiolites, a sheeted dike complex (Rassios et al., 1983), stratiform mafic-ultramafic cumulates (Jackson et al., 1975) and intrusive dunite (olivine cumulate) and chromite bodies (Harkins et al., 1980). C: Model for diapiric rise and divergence of asthenospheric mantle and development of oceanic crust at a spreading center. Cr—chromitite. (Redrawn after Nicolas and Violette, 1982, *in* Nicolas, 1989, Fig. 9.1, p. 204).

that cross-section in the light of what we know now of ophiolites as tectonic fragments of oceanic crust and mantle would lead to a revised cross-section as shown in Figure 2B. The presence of igneous dunites (Harkins et al., 1980) in the central zone and the overall structure suggests that in the Vourinos complex, one has captured an actual mantle cross-section of a spreading center, as envisioned by Nicolas and Violette (1982; see Fig. 2C).

CURRENT STATUS OF THE OPHIOLITE QUESTION

Although it is beyond the scope of this short reminiscence to discuss fully the status of the ophiolite question and the significance of ophiolites, it is worth summarizing briefly the main points concerning our present understanding. Three points stand out—the environment of formation of ophiolites and what they can tell us about oceanic crust formation; the mechanism of emplacement of ophiolites and its significance for interpretation of the tectonic development of orogenic systems; and the change in ophiolites and thus, oceanic spreading processes through time.

Environment of Formation

Ophiolites represent ocean crust and mantle formed at oceanic spreading centers. These centers occur either at mid-oceanic ridges, in pull-apart intra-arc oceanic basins, at active back-arc basins, at extensional zones in the centers of forearcs during the initial development of island arcs. The nature of the environment of formation of ophiolites cannot be obtained by geochemistry alone; rather a comprehensive set of data including geologic relations, associated deposits, internal structure, and geochemistry are necessary to evaluate the significance of ophiolite complexes. Some ophiolites from magma-rich spreading centers display a complete sequence (Penrose-type ophiolites); this sequence may imply a fast-spreading environment (Dilek et al., 1998). Other complexes, formed in a magma-starved environment, display incomplete sequences, as seen on modern slow-spreading ridges and in the Alps and Apennines where Steinmann did his work (Bernoulli, 2001, this volume, Chapter 7). These "Hess-type" complexes imply a significant portion of the oceanic crust was serpentinized peridotite, as advocated by Hess (1962) and Vine and Hess (1970; see also Moores, 2002; Dilek et al., 1998).

Discovery of faulted structures in ophiolites and oceanic crust strengthens the link between these different environments. Karson's (2002) summary of the structure within exposures of intermediate-fast spreading ridges adds a new complication to interpretation of ophiolite structure, and should be incorporated in new studies of favorably situated complexes. Many other ophiolites may display insights into mantle structure similar to that of Vourinos, as outlined above or the Semail complex, Oman (e.g.; Nicolas, 1989). Other ophiolites may contain more information about the spreading structure and oceanic crustal nature than originally assumed, following the "Penrose definition."

The environment of formation of ophiolites will continue to be controversial. Moores et al. (2000) attempted to resolve the difficulties between universal application of this model and lack of geologic evidence for any island arc in many Tethyan ophiolites by suggesting that the magma source compositions were "historically contingent," that is, a product of prior history, and not necessarily reflective, a priori, of modern environments. Metcalf and Shervais (2001) criticized the "historical contingency" concept on geochemical grounds, but they did not consider many data from the mid-oceanic ridges and from the Tethyan region. This discussion is ongoing. The issues are not whether some ophiolites are "supra-subduction zone," but all; the inadequacy of geochemistry itself to determine the tectonic environment in the absence of geologic evidence; and the increasing evidence for long-lived mantle heterogeneity. Recent discovery of silicic lavas at active mid-oceanic ridges (e.g., Stoffers et al., 2001) at the very least should encourage use of multiple working hypotheses and careful fieldwork before any dogmatic assertion of a tectonic interpretation of a given ophiolite.

Tectonic Significance

The tectonic significance of ophiolite emplacement was signaled early on, principally by Hess's (1939) and Stille's (1939) observations that ophiolites (or ultramafics) were intruded in the initial stages of orogeny; however, tectonics of ophiolite emplacement was not considered in the initial ophiolite Penrose conference (Anonymous, 1972). The tectonic significance of ophiolites have received relatively short shrift in most discussions of ophiolite complexes, which have concentrated on petrology and geochemistry.

Viewed in a plate tectonic context, the issue becomes how to get little-deformed oceanic crust and unserpentinized mantle over continental platforms (in the case of Tethyan-type ophiolites) or island arc crust (in the case of Cordilleran-type ophiolites). Many workers (starting with Temple and Zimmerman, 1969; Moores, 1970, 1973) have argued that such emplacement, given the presence of the topographic difference between continental and oceanic crust, must be by collision of a continental margin with a subduction zone dipping away from the continental margin. This is the "obduction" process of Coleman (1971) and Dewey (1976). Accordingly the basal sole thrusts of ophiolites represent the remnants of former subduction zones; their displacements are indeterminate but may be very large; they cannot be balanced. Synthetic and subsidiary sedimentary fold-thrust belts are secondary to the main ophiolite thrusts. Rare is the synthesis of any orogenic belt that has adequately taken this fact into account.

Of course, slices of down-going oceanic crust easily can have been incorporated into subduction zones. These slices, however, are almost invariably dismembered, incomplete, and can be distinguished from their better preserved counterparts.

Change of Ophiolites through Time

Many authors have suggested (e.g., Sleep and Windley, 1982; Hoffman and Ranalli, 1988) that on theoretical grounds, oceanic crust should be thicker in early earth history than at present. Moores (2002, 1993, 1986, 1973) suggested that oceanic crust thinned abruptly at ~1000 Ma. Recent reports of a ~1020 Ma ophiolite in the East Sayan belt, Siberia (Khain, et al., 2002) may alter the timing of this possible abrupt thinning. Whatever the nature of the pattern, there is increasing recognition that oceanic crustal sequences are present in Proterozoic and Archean terrains. Thus, interpretation of Proterozoic-Archean orogens can find guidance from the consideration of much better-known ophiolites in Phanerozoic orogens.

THE FUTURE OF OPHIOLITE STUDIES

Future ophiolite studies ideally will be guided by the two cardinal facts of ophiolite occurrences: (1) that they are the only guide to oceanic crust prior to 200 Ma; and (2) that ophiolite emplacement is the initial and perhaps most important tectonic event in the development of an orogenic system, as Hess first pointed out over 60 years ago. In working out oceanic spreading history, it would be encouraging to see a decline in automatic assignment of ophiolites to a back-arc origin; rather, a multiple working hypotheses approach is much preferable. In addition, orogenic models would benefit from more incorporation of models based on on-going processes in the SW Pacific and SE Asia (e.g., Hall, 2002).

Ideally, one would like to see a careful and systematic attempt to unravel the history of pre-Mesozoic ocean spreading and ophiolite emplacement processes. For example, there seem to be clusters of records of spreading and emplacement at, say, early Neoproterozoic, early Ordovician and mid-Carboniferous. These records may be biased by emplacement events, and they may apply more to Tethyan-type than Cordilleran-type occurrences. With patience and an open-minded multidisciplinary approach, we may well see a further revolution in our understanding of the orogenic process, specifically an emerging inference of oceanic spreading processes in pre-200 Ma times, and a re-evaluation of all orogenic tectonic histories using ophiolite emplacement as a primary tectonic indicator.

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